MARKETING THEORY

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SHELBY D. HUNT

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This book is dedicated to
Marguerite and Donald C. Hunt,
my parents,
who persevered,
with extraordinary courage,
in times of great adversity.

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PREFACE

Cynical observers claim that science in the traditional sense is already corrupted, spoiled, and lost. However, realism is a philosophy which encourages us to fight for science, for its methods and ethics. If anything, this is a good social reason for keeping up the high spirit of critical realism about science.
—Ilkka Niiniluoto

The first version of this work was entitled *Marketing Theory: Conceptual Foundations of Research in Marketing* (Hunt 1976a). Often referred to as the "little green book," its purpose was to explore philosophical issues in marketing and provide a "tool kit," based on the philosophy of science, for developing and analyzing marketing theory. The slender, 150-page monograph was not to be *on* the philosophy of science, or *about* it, but rather to apply the philosophy of science to issues in marketing theory. The second edition, often referred to as the "big red book," was entitled *Marketing Theory: The Philosophy of Marketing Science* (Hunt 1983b). Although much larger than the little green book, it continued to use the philosophy of science to explore and explicate the nature of such concepts as "explanation," "laws," and "theories" in marketing theory and research.

The third version was entitled *Modern Marketing Theory: Critical Issues in the Philosophy of Marketing Science* (Hunt 1991a). The "blue book" differed markedly from its predecessors. Part I of the "blue book" continued the applications orientation of its forerunners, which focused on the traditional topics in the philosophy of marketing science. However, Part II addressed the philosophy debates that were emerging in marketing. Stated briefly, many marketing scholars in these debates (1) questioned whether there can possibly be genuine progress in the development of knowledge in any science, (2) argued for relativism as a philosophical foundation for marketing, and (3) argued against epistemic values such as *truth* and *objectivity* in marketing theory and research.

By the late 1980s, it was clear that the philosophy debates were becoming increasingly unproductive: discussions of ideas were shifting toward *ad hominem* discourse, epistemology was morphing into "epistobabble" (Coyne 1982), honest mischaracterizations were becoming "nastiness and purposeful distortions" (Hirschman 1989a, p. 209), and a concern for civility was being replaced with "ridicule" (Pechmann 1990, p. 7). Furthermore, by then the nihilistic implications of relativism were evident. Because a major factor contributing to the muddled status of the philosophy debates was a lack of

understanding of the various "isms" in the philosophy of science (e.g., logical positivism and logical empiricism), Part II of Hunt (1991a) included four new chapters in a section entitled "Philosophy of Science: Historical Perspectives and Current Status." Its aim was to use a historical approach to raise the level of debate to a more informed level.

The structure of the fourth version of *Marketing Theory* resulted from discussions I had with instructors who used the blue book. It seemed that some instructors wanted a book that focused exclusively on the traditional marketing theory topics, as did the green and red books. Others seemed more interested in the philosophy debates and wished to focus exclusively on them, as did Part II of the blue book. And still others wanted text material that would cover both the traditional topics and the philosophy debates. Accordingly, *Foundations of Marketing Theory: Toward a General Theory of Marketing* (Hunt 2002a) addressed the interests of the first group. A companion volume, *Controversy in Marketing Theory: For Reason, Realism, Truth, and Objectivity* (Hunt 2003a), addressed the interests of the second group. Instructors in the third group could use the two books together.

The structure of this fifth version of *Marketing Theory* results from recommendations of instructors who have used *Foundations* (2002a) and *Controversy* (2003a). Four have been prominent. First, instructors have suggested that the next version should include *both* the traditional topics of philosophy of marketing science and the issues involving the philosophy debates (as did the "blue" book). Second, they suggested that the historical material in Chapters 1 through 5 of *Controversy,* though interesting*,* was probably more than their students needed in order to understand marketing's philosophy debates. Therefore, they suggested that the next version should shorten *Controversy*'s Chapters 1 through 5. Third, since its first publication in Hunt and Morgan (1995) and its brief discussion in Chapter 9 of *Foundations* (2002a), an extraordinary interest in resource-advantage (R-A) theory has developed among marketers and other academics. Therefore, instructors suggested that more attention be given to the view that R-A theory is toward a general theory of marketing. Fourth, modern marketing, both practice and theory, has taken a sharp turn toward strategic issues. Instructors suggested that increased attention be given to how R-A theory provides a theoretical foundation for marketing strategy.

Accordingly, this fifth version of *Marketing Theory* has four parts. Part 1, The Nature of Marketing and Science, uses the Three Dichotomies Model to explore key issues concerning the conceptual domain of marketing and the key characteristics of science. Part 2, The Foundations of Marketing Theory, uses the "toolkit" of philosophy of science to explore the morphology of scientific explanation, scientific laws, and theoretical constructions. Part 3, Controversy in Marketing Theory, focuses on the philosophy debates in marketing, with particular attention devoted to scientific realism, the science/nonscience controversy, qualitative methods, truth, and objectivity. Part 4, Toward a General Theory of Marketing, develops arguments for viewing the resource-advantage (R-A) theory of competition as being toward a general theory of marketing. Specifically, this section (1) overviews R-A theory, (2) details why R-A theory is a general theory of competition, (3) shows how R-A theory extends Wroe Alderson's theory of market processes, and (4) explicates how R-A theory provides a theoretical foundation for research in, and the teaching of, business and marketing strategy.

All those who have attempted to find publishers for works that have small markets know the difficulty of the task. Therefore, I thank M.E. Sharpe for agreeing to publish this work and, in particular, I thank Harry M. Briggs, Executive Editor, for encouraging me to once again revise *Marketing Theory.*

In an extraordinary display of intercollegial generosity, numerous philosophers of science agreed graciously to review drafts of materials that formed the basis for the discussions in Chapters 9–12. These philosophers include Robert L. Causey (University of Texas, Austin), Martin Hollis (University of East Anglia, United Kingdom), Evan K. Jobe (Texas Tech University), John Kekes (State University of New York at Albany), Michael Krausz (Bryn Mawr College), Jarrett Leplin (University of North Carolina at Greensboro), Michael E. Levin (City College, City University of New York), Steven Lukes (Oxford University), (the late) Paul E. Meehl, formerly at the University of Minnesota, Jack W. Meiland (University of Michigan), Dennis C. Phillips (Stanford University), Hilary Putnam (Harvard University), Israel Scheffler (Harvard University), and (the late) David Stove, formerly at the University of Sydney. Given the ambitious nature of Chapters 9–12, these reviewers' assistance and encouraging comments are most appreciated. Indeed, they significantly improved this work by correcting numerous errors. Although any remaining errors are my responsibility alone, I gratefully thank all of these philosophers for their help.

One philosopher of science, more so than any other, has influenced the content of this book. That philosopher is Harvey Siegel (Philosophy Department, University of Miami). My own work has benefited enormously from Professor Siegel's many writings. His incisive analyses of extraordinarily complex issues in the philosophy of science are consummate examples of true scholarship.

Many colleagues have encouraged me to write this (final?) edition of *Marketing Theory* and/or provided suggestions as to specific topics to be included, excluded, or revised. I gratefully acknowledge the encouragement, comments, and suggestions of Dennis Arnett (Texas Tech University), Danny Bellenger (Georgia State University), Terry Clark (Southern Illinois), Donna Davis (Texas Tech University), Adam Finn (University of Alberta), Michael Hyman (New Mexico State University), Robert Lusch (University of Arizona), Carl McDaniel (University of Texas, Arlington), Sreedhar Madhavaram (Cleveland State University), Robert Morgan (University of Alabama), William Pride (Texas A&M University), Arturo Vasquez-Parraga (University of Texas, PanAmerican), Scott Vitell (University of Mississippi), and Arch Woodside (Boston College).

Over the years, my doctoral students, as well as students in theory courses at other universities, have provided numerous helpful suggestions. I thank them all. Finally, but very importantly, thanks go to Sherry Fowler, Cassie Myatt, and Ashley Weaver for their extensive assistance in manuscript preparation.

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MARKETING THEORY

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PART 1

THE NATURE OF MARKETING AND SCIENCE

This introductory section explores the nature of marketing and science. Chapter 1 examines the following questions: (1) Is marketing a science? (2) Should marketing be broadened to include nonprofit organizations? (3) What is the proper conceptual domain of the construct labeled "marketing"? (4) What is science? (5) Do the various sciences require different scientific methods? The chapter addresses these questions using a model of the scope of marketing that has come to be known as the Three Dichotomies Model, which was first proposed in an article entitled "The Nature and Scope of Marketing" (Hunt 1976b).

Chapter 2 continues the introductory material, first by exploring the nature of the marketing *discipline* using a perspective that has come to be known as the "responsibilities framework" (Hunt 2007b). Second, it will examine the controversy concerning the definition of marketing and conclude that the definition that the American Marketing Association (AMA) adopted in 2007 has much to recommend it, especially for any marketing practitioner or academic who views (1) marketing practice as a profession and (2) marketing academe as a professional discipline. Third, using the Three Dichotomies Model developed in Chapter 1 as an analytical tool, the chapter explores the controversy related to the AMA definition of *marketing research.*

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1

INTRODUCTION

Question: Prove to me I should study logic!
Answer: How would you know that it was a good proof?
—Epictetus

Marketing research books usually contain sections on issues such as experimental research designs, data collection procedures, the availability and desirability of secondary data, sampling methods, data analysis, and the writing of research reports. Because this monograph discusses none of these topics, how can it claim to be about *marketing research*? Most books on *advanced* research topics in marketing discuss items such as factor analysis, multiple discriminant analysis, cluster analysis, multiple regression, dummy variable regression, canonical correlation, and structural equation modeling. Because there are few equations in this monograph, why have graduate students referred to this work as *advanced* issues in marketing research? Finally, students often describe contemporary works on marketing theory as "impractical," with no relevance to the real world. How can this monograph concern marketing theory and further assert that the study of theory is the most *practical* intellectual pursuit of anyone interested seriously in marketing research? Is this entire monograph an inherent contradiction in terms? Let's examine these contradictions and determine whether they are real or only apparent.

1.1 THREE CONTRADICTIONS?

Few students of marketing would deny that much marketing research attempts to explain, predict, and understand marketing phenomena. Thus, much research is directed at explaining why some products have failed and attempting to predict which new products will succeed; explaining why certain retail institutions have declined and predicting which retail institutions will emerge; explaining why some promotional programs have succeeded and predicting the characteristics of successful future programs; and explaining why consumers have allocated their expenditures according to certain patterns and predicting how consumers will purchase in the future. Thus, explanation, prediction, and understanding are fundamental to marketing research.

Care should be taken to distinguish between marketing research and market research. *Marketing* research (or, alternatively, scholarly research in marketing) always seeks to

expand the total knowledge base of marketing. In general, *market* research attempts to solve a particular company's marketing problem. To evaluate a particular department store's image would be a market research problem. To explore whether department stores have images *at all* is a marketing (scholarly) research problem. To attempt to determine the best location for a particular warehouse is a market research problem. To attempt to develop a model for locating warehouses in *general* is a marketing research problem. The following question can serve as a litmus test for differentiating *market* research from *marketing* research: "After conducting this research project, what will we then know about marketing in *general* that we do not know now?" In short, "What will be the contribution of this research to knowledge about marketing?" Unfortunately, many dissertation research proposals and even some completed dissertations fail this test. Although the line differentiating marketing research from market research may sometimes be fine, the distinction is useful and conceptually important.

Myers, Massy, and Greyser (1980) have drawn similar distinctions among basic research, problem-solving research, and what they refer to as "problem-oriented" research. They suggest that problem-oriented research lies between basic research and problem-solving research and "may be fundamental or highly applied, but its driving force is the desire to make a contribution to the solution of an important practical problem" (1980, p. 157). Does problem-oriented research "lie between" basic research and problem-solving research? If "basic" research is considered to be roughly synonymous with "marketing" research and "problem-solving" research is considered to be roughly synonymous with "market" research, then problem-oriented research is *not* "between" the two. Problem-oriented research is a subclass of *marketing* research because it is research directed at general *classes* of marketing problems and because it is generalizable *across* different firms. Problem-oriented research is, simply, a kind of basic research in marketing that is normative-driven rather than positive-driven (see section 1.3.1). It seeks answers to normative questions such as "How *should* retail establishments price their merchandise?" rather than answers to positive questions such as "How *do* retail establishments price their merchandise?" Both questions are appropriate for "basic" or "marketing" research.

The first apparent contradiction dissolves if we note that this monograph is substantially concerned with exploring the basic methodological issues attendant on the explanation, prediction, and understanding of marketing phenomena. These basic methodological issues are customarily given only cursory treatment, at best, in most marketing research texts. Such texts focus primarily (and probably justifiably, given their target markets) on the conventional topics previously mentioned (data collection, sampling, etc.). Fortunately for the present endeavor, many of the basic methodological issues in research and scientific inquiry have been extensively developed in the philosophy of science and are applicable to marketing research. *A major purpose of Parts 1, 2, and 3 of this monograph will be to draw upon the vast storehouse of analytical methods in the philosophy of science in order to systematically explore the basic methodological issues underlying marketing research.* The philosophical orientation of this monograph, sometimes referred to as "contemporary empiricism" or "modern empiricism," may be described as a combination of critical pluralism and scientific realism.[1] Critical pluralism is the view that,

because both dogmatism and relativism are antithetical to science, we should both (1) adopt a tolerant, open posture toward new theories and methods and (2) subject all such theories and methods to critical scrutiny—nothing is, or can be, exempt (Hunt 1991b). Claims of "incommensurability" represent neither a state of nature to be accommodated nor a problem to be addressed—they are a convenient catchall for squelching debate or avoiding critical scrutiny.

Scientific realism, following Hunt (1990b), is the view that:

- the world exists independently of its being perceived (classical realism);
- the job of science is to develop genuine knowledge about the world, even though such knowledge will never be known with certainty (fallibilistic realism);
- all knowledge claims must be critically evaluated and tested to determine the extent to which they do, or do not, truly represent, correspond, or accord with the world (critical realism); and
- the long-term success of any scientific theory provides reason to believe that something like the entities and structure postulated by that theory actually exists (inductive realism).

Note that a philosophy encompassing critical pluralism and scientific realism is open, without being anarchistic; it is critical, without being nihilistic; it is tolerant, without being relativistic; it is fallible, without being subjectivistic; it is absolutist, without being ABSOLUTIST.

As used here, to be "absolutist" is to maintain that there exist trustworthy criteria for evaluating the merit of competing knowledge claims. To be "ABSOLUTIST" is to maintain that one knows with certainty that one's criteria will guarantee the production of true knowledge claims. Thus, "ABSOLUTISM" equates with Siegel's (1987) "vulgar absolutism."

The second apparent contradiction is (a) that "advanced" topics in marketing research universally seem to be quantitatively sophisticated and (b) that quantitative techniques are conspicuous by their absence in this monograph, yet (c) students who have used this work generally consider it advanced. The contradiction is illusory. Quantitative techniques represent a tool kit for conducting research. Many mathematical and statistical models are difficult to understand and, hence, *advanced.* Similarly, the philosophy of science is a tool kit that students may perceive as being relatively advanced. Students may find the tool kit to be of moderate difficulty for two reasons. First, few students have been formally exposed to the philosophy of science, and the first exposure to new material is always the most difficult. The reader not only must comprehend the *substance* of the tool kit but also must learn the *vocabulary.* Every effort has been made to "dejargonize" the presentation. Nevertheless, just as students must understand terms such as *differentiation* and *integration* to learn the role of calculus in marketing research, so must they understand terms such as *retrodiction* and *deductive-nomological explanation* to appreciate the usefulness of the philosophy of science tool kit in marketing research.

Some marketing commentators have charged that the history of marketing (not unlike the history of other social sciences) can be interpreted as a history of marketing fads. Every few years a new tool kit appears that promises to be the key to marketing problems. Thus, marketing has been blessed with motivation research, operations research, Markov processes, systems analysis, the behavioral sciences, mathematical models, multidimensional scaling, psychographics, conjoint analysis, structural equation models, and multiattribute models. Although each tool kit has value in conducting research in marketing, advocates of the various tool kits often tend to oversell and overpromise. Therefore, a caveat concerning the philosophy of science seems appropriate. Just as marketing research problems are not solved by restating our ignorance in mathematical symbols, so, too, the present philosophy-of-science tool kit provides no panaceas, no magic formulas.

The second reason some students may find this presentation moderately difficult is that we shall attempt to analyze rigorously a topic (often referred to as "the scientific method") about which students have some notoriously nonrigorous (though often firmly held) notions. Unfortunately, rigor and difficulty often travel in tandem. If the analysis is both complete and clear (rigorous), this will maximize the opportunity for others to point out errors. When analyses are incomplete and ambiguous, the temptation is often strong for authors to dismiss their errors as misinterpretations. Because ambiguity should never be confused with profundity, I plead guilty to the charge of attempted rigor.

The last apparent contradiction is (a) students believe that theory is impractical, yet (b) this book concerns theory, while (c) claiming to be devoted to a practical intellectual pursuit. The fallacy lies in the false dichotomy of theoretical-practical. Almost all marketing practitioners, most marketing academicians, and, sadly, too many marketing researchers perceive theoretical and practical as being at the opposite ends of a continuum. This perception leads to the conclusion that as any analysis becomes more theoretical, it must become less practical. To puncture this misperception, one need only note that a theory is a systematically related set of statements, including some lawlike generalizations, that is empirically testable. The purpose of theory is to increase scientific understanding through a systematized structure capable of both explaining and predicting phenomena. Thus, any structure that purports to be theoretical must be capable of explaining and predicting phenomena. Any structure that has neither explanatory nor predictive power is not a theory. Because the explanation and prediction of marketing phenomena are eminently practical concerns, the study and generation of marketing theory are practical pursuits of the first order.

The *theoretical-practical* issue is not the only false dichotomy in marketing. Consider the *behavioral-quantitative* classification. Incredibly, some marketing educators still inquire of prospective faculty whether they are quantitative *or* behavioral. This false dichotomy automatically presumes that no one can be both behaviorally oriented and at the same time be well-grounded in quantitative methodology. The presumption is, of course, unfounded. Likewise, the *rigor-relevance* dichotomy has been shown to be false on the grounds that it wrongly assumes that research cannot be both rigorous *and* relevant (Kassarjian 1989; Hunt 1989a).

1.2 OBJECTIVES OF MONOGRAPH

The primary objective here will be to explore systematically some of the basic methodological issues underlying marketing research. As discussed earlier, the analytical methods to be developed and employed will be drawn from the tool kit of critical pluralism and scientific realism, with insights from logical empiricism, critical rationalism (falsificationism), and pragmatism, where appropriate. Numerous other tool kits exist in the philosophy of science: classical empiricism, phenomenalism, rationalism, instrumentalism, logical positivism, conventionalism, relativism, constructivism, and "Weltanschauungen-ism." The differentiating characteristics of these various "isms" need not detain us, because this work, unlike Hunt (2003a), is not *on* the philosophy of science, but, rather, attempts to *use* the philosophy of science. This is not a philosophy of science book disguised in the trappings of marketing research. Much of the philosophy of science is not even mentioned, let alone developed, in this work. Philosophy of science issues and methods are introduced and discussed only when they are deemed useful for explicating some particular methodological issue in marketing research.

One way to clarify the purpose of this work, especially Parts 1 and 2, is to give some examples of the basic methodological issues that will be explored. Although certainly not exhaustive, the following list should prove reasonably representative of these issues:

1. How does one scientifically explain marketing phenomena?
2. Is it possible to be able to explain marketing phenomena without being able to predict them?
3. Is functionalism a different method of explaining phenomena?
4. How does explanation differ from causation?
5. Can one understand marketing phenomena without being able to explain or predict them?
6. What is the role of laws and lawlike generalizations in marketing research?
7. How do empirical generalizations differ from laws?
8. Are the axioms in a theory "assumed to be true"?
9. How do universal laws differ from statistical laws?
10. What is theory, and what is its role in marketing research?
11. How can formalization help in analyzing marketing theory?
12. Why must theories contain lawlike generalizations?
13. Why must theories be empirically testable?
14. How can marketing phenomena best be classified?

Before analyzing these questions, some preliminary matters require attention. These preliminary issues can be best examined in the context of the "Is marketing a science?" controversy. The controversy was sparked by an early *Journal of Marketing* article written by Converse (1945), entitled "The Development of a Science of Marketing." Prominent writers who then entered the debate included Bartels (1951), Hutchinson (1952), Baumol (1957), Buzzell (1963), Taylor (1965), and Halbert (1965). After raging throughout most

of the 1950s and 1960s, the controversy has since waned. The waning may be more apparent than real because many of the substantive issues underlying the marketing science controversy overlap with the 1970s "broadening the concept of marketing" debate and the 1980s–1990s "positivism-relativism" controversy. Fundamental to these controversies are some radically different perspectives on the essential characteristics of both *marketing* and *science.* An exploration of the basic nature of both these notions will provide a frame of reference for the rest of this monograph.

1.3 THE NATURE OF MARKETING

What is marketing? What kinds of phenomena are appropriately termed marketing phenomena? How do marketing activities differ from nonmarketing activities? What is a marketing system? How can the marketing process be distinguished from other social processes? Which institutions should one refer to as marketing institutions? *In short, what is the proper conceptual domain of the construct labeled "marketing"?*

Prior to the definition of marketing formulated in 1985, the American Marketing Association (AMA) defined marketing as "the performance of business activities that direct[s] the flow of goods and services from producer to consumer or user." This position came under attack from various quarters as being too restrictive. Specifically, a position paper by the Marketing Staff of the Ohio State University (1965, p. 43) suggested that marketing be considered "the process in a society by which the demand structure for economic goods and services is anticipated or enlarged and satisfied through the conception, promotion, exchange, and physical distribution of goods and services." Note the conspicuous absence of the notion that marketing consists of a set of business activities (as in the AMA definition). Rather, marketing is viewed as a societal *process.*

Next to plunge into the semantic battle were Philip Kotler and Sidney Levy. Although they did not specifically propose a new definition of marketing, Kotler and Levy (1969a) suggested that the concept of marketing be broadened to include nonbusiness organizations. They observed that nonbusiness organizations, including churches, police departments, and public schools, have products and customers and use the normal tools of the marketing mix. Therefore, Kotler and Levy concluded that these organizations perform marketing activities, or at least marketing-like activities. Thus, "the choice facing those who manage nonbusiness organizations is not whether to market or not to market, for no organization can avoid marketing. The choice is whether to do it well or poorly, and on this necessity the case for organizational marketing is basically founded" (1969a, p. 15). In the same issue of the *Journal of Marketing,* William Lazer discussed the changing boundaries of marketing. He pleaded that "what is required is a broader perception and definition of marketing than has hitherto been the case—one that recognizes marketing's societal dimensions and perceives of marketing as more than just a technology of the firm" (1969, p. 9). Thus, Kotler and Levy desired to broaden the notion of marketing by including not-for-profit organizations, and Lazer called for a definition of marketing that recognized marketing's expanding societal dimensions.

David Luck (1969, p. 54) took sharp issue with Kotler and Levy by insisting that

marketing be limited to those business processes and activities that ultimately result in a *market* transaction. Luck noted that even thus bounded, marketing would still be a field of enormous scope and that marketing specialists could still render their services to nonmarketing causes. Kotler and Levy (1969b, p. 57) then accused Luck of a new form of myopia and suggested that "the crux of marketing lies in a *general idea of exchange* rather than the narrower thesis of market transactions." They further contended that defining marketing "too narrowly" would prevent students of marketing from applying their expertise to some of the most rapidly growing sectors of the society.

Other marketing commentators began to espouse the dual theses that (1) marketing be broadened to include nonbusiness organizations and that (2) marketing's societal dimensions deserve scrutiny. Thus, Robert Ferber (1970) prophesied that marketing would diversify into the social and public policy fields. And Robert Lavidge sounded a similar call to arms by admonishing marketers to cease evaluating new products solely on the basis of whether they *could* be sold. Rather, he suggested that they evaluate new products from a societal perspective, that is, *should* the products be sold?

> The areas in which marketing people can, and must, be of service to society have broadened. In addition, marketing's functions have been broadened. Marketing no longer can be defined adequately in terms of the activities involved in buying, selling, and transporting goods and services. (Lavidge 1970, p. 27)

The movement to expand the concept of marketing probably became irreversible when the *Journal of Marketing* devoted an entire issue in 1971 to marketing's changing social/environmental role. At that time, Kotler and Zaltman coined the term *social marketing,* which they defined as "the design, implementation, and control of programs calculated to influence the acceptability of social ideas and involving considerations of product planning, pricing, communication, distribution, and marketing research" (1971, p. 5). In the same issue, marketing technology was applied to fund-raising for the March of Dimes (Mindak and Bybee 1971), health services (Zaltman and Vertinsky 1971), population problems (Farley and Leavitt 1971), and the recycling of solid waste (Zikmund and Stanton 1971). Further, Dawson chastised marketers for ignoring many fundamental issues pertaining to the social relevance of marketing activities:

> Surely, in these troubled times, an appraisal of marketing's actual and potential role in relation to such [societal] problems is at least of equal importance to the technical aspects of the field. Yet, the emphasis upon practical problem-solving within the discipline far outweighs the attention paid to social ramifications of marketing activity. (Dawson 1971, p. 71)

Kotler then expanded his earlier position concerning broadening the concept of marketing and articulated a "generic" concept of marketing. He proposed that the essence of marketing is the *transaction,* defined as the exchange of values between two parties. Kotler's generic concept of marketing states, "Marketing is specifically concerned with

how transactions are created, stimulated, facilitated, and valued" (1972b, p. 49). By the mid-1970s empirical evidence indicated that, at least among marketing educators, the broadened concept of marketing represented a *fait accompli.* A study by Nickels (1974) showed that 95 percent of marketing educators believed that the scope of marketing should be broadened to include nonbusiness organizations. Similarly, 93 percent agreed that marketing goes beyond just economic goods and services, and 83 percent favored including in the domain of marketing many activities whose ultimate result is not a market transaction. Also by the mid-1970s, the American Marketing Association was calling for a reevaluation and modification of its formal definition of marketing. Today it is noncontroversial that marketing has an important role to play in nonbusiness organizations.

Three questions are central to understanding the definition (broadening the concept) of marketing controversy. First, what kinds of phenomena and issues *do* the various marketing writers perceive to be included in the scope of marketing? Second, what kinds of phenomena and issues *should* be included in the scope of marketing? Third, how can marketing be defined both to encompass systematically all of the phenomena and issues that should be included and, at the same time, to exclude systematically all other phenomena and issues? That is, a good definition of marketing must be both properly inclusive and exclusive. All three questions cry out for rigorous analysis. However, because a complete explication of questions 2 and 3 depends in part on a satisfactory exposition of question 1, the present analysis will begin by examining the various kinds of phenomena and issues that marketing writers often seem to put within the confines of marketing. In short, what is the scope of marketing?

1.3.1 The Scope of Marketing

The scope of marketing is unquestionably broad. Often included are diverse subject areas such as consumer behavior, pricing, purchasing, sales management, product management, marketing communications, comparative marketing, social marketing, the efficiency/productivity of marketing systems, marketing ethics, the role of marketing in economic development, packaging, channels of distribution, relationship marketing, marketing research, societal issues in marketing, retailing, wholesaling, the social responsibility of marketing, international marketing, brand equity, commodity marketing, and physical distribution. Though lengthy, this list of topics does not exhaust the possibilities. Not all writers would include all of the topics under the general rubric of marketing. However, the point deserving emphasis here is that different commentators on marketing would *disagree* as to which topics should be excluded. The disagreement stems from fundamentally different perspectives and can be best analyzed by developing some common ground for classifying the diverse topics and issues in marketing.

During a presentation at the 1972 fall conference of the American Marketing Association, Philip Kotler made some observations concerning how to classify marketing phenomena using the concepts *micro, macro, normative,* and *positive.*[2] These observations spurred the development of the classificatory schema detailed in Table 1.1. The schema proposes that all marketing phenomena, topics, and issues can be categorized using the

three categorical dichotomies of (1) profit sector/nonprofit sector, (2) micro/macro, and (3) positive/normative. The three categorical dichotomies yield $2 \times 2 \times 2 = 8$ classes or cells in the schema. Thus, the first cell includes all marketing topics that are micro-positive and in the profit sector. Similarly, the second cell includes all marketing activities that are micro-normative and in the profit sector, and so on throughout the table. This model of the scope of marketing was first proposed in an article entitled "The Nature and Scope of Marketing" (Hunt 1976b). The model has come to be known as the Three Dichotomies Model (Hunt 1978).

Some definitions are required to interpret properly the schema presented in Table 1.1. *Profit sector* includes the study of organizations or other entities whose stated objectives include the realization of profit. Also included are studies that adopt the *perspective* of profit-oriented organizations. Conversely, *nonprofit sector* includes the study and perspective of all organizations and entities whose stated objectives do not include the realization of profit. *Positive* marketing adopts the perspective of attempting to describe, explain, predict, and understand the marketing activities and phenomena that actually exist. This perspective examines what *is.* In contrast, *normative* marketing adopts the perspective of attempting to prescribe what marketing organizations and individuals ought to do or what kinds of marketing systems a society ought to have. That is, this perspective examines what *ought to be* and what organizations and individuals *ought to do.* The grounds for the "ought" may be ethical/moral or instrumental/rational (see section 8.2).

Of the three dichotomies proposed to organize the total scope of marketing, the micro-macro dichotomy is probably the most ambiguous. Drawing upon the distinction between microeconomics and macroeconomics, the original paper distinguished between micromarketing and macromarketing on the basis of aggregation: *micro* referred to the marketing activities of individual units (firms and consumers or households), while *macro* referred to a higher level of aggregation, either marketing systems or groups of consumers. However, as was pointed out, topics such as "Does marketing have special social responsibilities?" would not fit the macro label on the basis of a level of aggregation criterion. Given that most marketers desire to classify topics similar to the "social responsibilities" issue as *macro,* how should the specification of *macro* be modified?

Some marketers suggested an "internalities versus externalities" classification. That is, micromarketing focuses on the internal marketing interests of firms; whereas macromarketing focuses on the interests of society with regard to marketing activities. Specified in this way, macromarketing would include topics such as "social responsibilities," efficiency, and productivity. And this is all to the good. However, the specification would not encompass topics such as the legal aspects of marketing, comparative marketing, and relationships in channels of distribution. None of these topics necessarily focuses on the "interests of society," yet many marketers would like to include them under the *macro* rubric because the topics are very different from *micro* topics such as "How do (or should) firms determine their advertising budgets?" Therefore, an "interests of society" criterion is not sufficient.

Thus, it would appear that macromarketing is a multidimensional construct. Therefore, I argue, a complete specification of macromarketing would (should) include the

Table 1.1

The Three Dichotomies of Model of Marketing

Positive	Normative
Profit sector	
Micro	
1. Problems, issues, theories, and research concerning: a. Individual consumer buyer behavior b. How firms determine prices c. How firms determine products d. How firms determine promotion e. How firms determine channels of distribution f. Case studies of marketing practices	2. Problems, issues, normative models, and research concerning how firms *should*: a. Determine the marketing mix b. Make pricing decisions c. Make product decisions d. Make promotion decisions e. Make packaging decisions f. Make purchasing decisions g. Make international marketing decisions h. Organize their marketing departments i. Control their marketing efforts j. Plan their marketing strategy k. Develop and manage relationships with stakeholders l. Manage retail establishments m. Manage wholesale establishments n. Implement the marketing concept o. Be market-oriented p. Manage brand equity
Macro	
3. Problems, issues, theories, and research concerning: a. Aggregate consumption patterns b. The institutional approach to marketing c. The commodity approach to marketing d. The functional approach to marketing e. Legal aspects of marketing f. Comparative marketing g. The efficiency of marketing systems h. Whether marketing spurs or retards economic development i. Power and conflict relationships in channels of distribution j. Whether the marketing concept is consistent with consumers' interests	4. Problems, issues, normative models, and research concerning: a. How marketing can be made more efficient b. Whether distribution costs too much c. Whether advertising is socially desirable d. Whether consumer sovereignty is desirable e. Whether stimulating demand is desirable f. What kinds of laws regulating marketing are optimal g. Whether vertical marketing systems are socially desirable h. Whether marketing should have special social responsibilities

Nonprofit sector

Micro

5. Problems, issues, theories, and research concerning:
   a. Consumers' purchasing of public goods
   b. How nonprofit organizations determine prices
   c. How nonprofit organizations determine products
   d. How nonprofit organizations determine promotion
   e. How nonprofit organizations determine channels of distribution
   f. Case studies of public goods marketing

6. Problems, issues, normative models, and research concerning how nonprofit organizations *should*:
   a. Determine the marketing mix (social marketing)
   b. Make pricing decisions
   c. Make product decisions
   d. Make promotion decisions
   e. Make packaging decisions
   f. Make purchasing decisions
   g. Make international marketing decisions (e.g., CARE)
   h. Organize their marketing efforts
   i. Control their marketing efforts
   j. Plan their marketing strategy
   k. Develop and manage relationships with stakeholders

Macro

7. Problems, issues, theories, and research concerning:
   a. The institutional framework for public goods
   b. Whether television advertising influences elections
   c. Whether public service advertising influences behavior (e.g., Smokey the Bear)
   d. Whether existing distribution systems for public goods are efficient
   e. How public goods are recycled

8. Problems, issues, normative models, and research concerning:
   a. Whether society should allow politicians to be "sold" like toothpaste
   b. Whether the demand for public goods should be stimulated
   c. Whether "low informational content" political advertising is socially desirable (e.g., ten-second "spot" commercials)
   d. Whether political advertising should be held to the same standards as commercial advertising

Source: Hunt (1991a). Reprinted by permission of the author.

following criteria: Macromarketing refers to the study of (1) marketing systems, (2) the impact of marketing systems on society, and (3) the impact of society on marketing systems. Criterion (1) is a level of aggregation criterion that allows the inclusion of topics such as comparative marketing, the institutional structure of marketing, and relationships in channels of distribution. Criterion (2) is a generalized "interests of society" criterion that brings in topics such as "social responsibilities" and the role of marketing in economic development. Criterion (3) recognizes that society impacts on marketing and would include such topics as the legal aspects of marketing and the consequences for marketing of different political and social value systems. This multidimensional perspective has been adopted by the *Journal of Macromarketing* (Fisk 1982, p. 3).

An examination of Table 1.1 reveals that most of the early (circa 1920) approaches to the study of marketing reside in cell 3, profit sector/macro/positive. The institutional, commodity, and functional approaches analyzed existing (positive) business activities (profit sector) from a marketing systems (macro) perspective. However, not all of the early marketing studies were solely profit/macro/positive. L.D.H. Weld's (1920) classic, *The Marketing of Farm Products,* not only examined existing distribution systems for farm commodities but also attempted to evaluate normative issues such as "Are there too many middlemen in food marketing?"[3] Thus, Weld's signally important work was both profit/macro/positive and profit/macro/normative. Similarly, the Twentieth Century Fund study, *Does Distribution Cost Too Much?* (Stewart, Dewhurst, and Field 1939), took an essentially profit/macro/normative perspective. Other important works that combined the profit/macro/positive and the profit/macro/normative perspectives include those of Harold Barger (1955), Reavis Cox (1965), and Neil Borden (1942).

Although the profit/micro/normative (cell 2) orientation to marketing can be traced at least back to the 1920s and notable works such as Reed (1930) and White and Hayward (1924), the movement reached full bloom in the early 1960s with proponents of the managerial approach to marketing such as McCarthy (1960). The managerial approach adopts the perspective of the marketing manager. Often the marketing manager is positioned within a manufacturing corporation, though the approach is not (and *should* not be) restricted to manufacturers. Therefore, the emphasis is micro and in the profit sector. The basic question underlying the managerial approach are questions such as: "What is the optimal marketing mix?" "What segments should be targeted?" and "How should customer relationships be managed?" Consequently, the approach is unquestionably normative.

During the mid-1960s, writers such as Lazer and Kelley (1962), Adler (1967), and Fisk (1967) began advocating a *systems approach* to marketing. Sometimes the systems approach used a profit/micro/normative perspective and applied to marketing certain sophisticated optimizing models (such as linear and dynamic programming) developed by the operations researchers. Other writers used the systems approach in a profit/macro/positive fashion to analyze the complex interactions among marketing institutions. Finally, some used the systems approach to include the profit/macro/normative:

> The method used in this book is called the general systems approach. In this approach the goals, organization, inputs, and outputs of marketing are examined to determine how efficient and how effective marketing is. Constraints, including competition and government, are also studied because they affect both the level of efficiency and the kinds of effects obtained. (Fisk 1967, p. 3)

During the late 1960s, the *environmental approach* to marketing was promulgated by writers such as Holloway and Hancock (1964, 1968) and Scott and Marks (1968). This approach emphasized an analysis of the environmental constraints on marketing activities. These constraints included consumer behavior, culture, competition, the legal framework, technology, and the institutional framework. Consequently, this approach may be classified as profit/macro/positive.

Two trends surfaced in the 1970s. The first was the trend toward *social marketing* as proposed by Kotler (1972b), Kotler and Levy (1969a), and Kotler and Zaltman (1971) and as promulgated by others.[4] Social marketing, with its emphasis on the marketing problems of nonprofit organizations, is nonprofit/micro/normative. The second can be termed the *societal issues* trend. It concerns topics as diverse as consumerism, ethics, marketing and ecology, the desirability of political advertising, social responsibility, and whether the demand for public goods should be stimulated.[5] All of these works share the common element of *evaluation.* They attempt to evaluate the desirability or propriety of certain marketing activities or systems, and, therefore, should be viewed as either profit/macro/normative or nonprofit/macro/normative.

In 1985, two decades after the Ohio State University position paper, the debate within the American Marketing Association resulted in its appointing a committee, chaired by O.C. Ferrell, to evaluate how marketing should be defined. The committee recommended and the AMA Board approved the following definition: "Marketing is the process of planning and executing the conception, pricing, promotion, and distribution of ideas, goods, and services to create exchanges that satisfy individual and organizational objectives" (Bennett 1988, p. 115). The definition did not completely satisfy all those who participated in the debate. For example, I believe that the words "in organizations and society" should be inserted after "the process." Nonetheless, most agreed that it was a substantial improvement over its predecessor. We should note in particular the emphasis on *exchange* and the "broadening" of marketing to include "ideas." Thus, suitably interpreted, the definition accommodated all eight cells of the Three Dichotomies Model. (The American Marketing Association returned to the issue of how to define marketing in 2004 and 2007. We will revisit the subject of defining marketing in Chapter 2.)

Vargo and Lusch (2004) identify seven research streams that emerged in the 1980s and continue through the 2000s that view marketing as a social and economic process. These research streams are (1) market orientation (e.g., Kohli and Jaworski 1990; Narver and Slater 1990), (2) services marketing (e.g., Grönroos 1994; Zeithaml, Parasuraman, and Berry 1985), (3) relationship marketing (e.g., Berry 1983; Gummesson 1994, 2002a, 2002b; Morgan and Hunt 1994; Sheth and Parvatiyar 2000), (4) quality management

(e.g., Hauser and Clausing 1988; Parasuraman, Zeithaml, and Berry 1988), (5) value and supply-chain management (e.g., Normann and Ramirez 1993; Srivastava, Shervani, and Fahey 1999), (6) resource management (e.g., Constantin and Lusch 1994; Day 1994; Hunt 2000b; Hunt and Morgan 1995), and (7) network analysis (e.g., Achrol 1991; Achrol and Kotler 1999; Webster 1992). These seven research streams are merging toward what Vargo and Lusch (2004) call a "service-dominant logic."

For Vargo and Lusch (2004, p. 6), marketing *should* shift toward a service-dominant logic, in which "value is defined by and co-created in concert with the consumer." This customer-centric, market-driven, services-centered view (1) focuses on specialized skills and knowledge as operant resources that provide competitive advantage, (2) strives to maximize consumers' involvement in developing customized offerings, and (3) seeks to be "the predominant organizational philosophy . . . [that] leads in initiating and coordinating a market-driven perspective for all core competences" (p. 13). Furthermore, marketing scholars should "lead industry toward a services-centered model of exchange," teach principles courses that "subordinate goods to service provision," and teach marketing strategy courses that "center on resource-advantage theory" (p. 14).

The service-dominant (S-D) logic, as modified and detailed in Vargo and Lusch (2008, p. 7), has ten foundational premises. These are (1) service is the fundamental basis of exchange; (2) indirect exchange masks the fundamental basis of exchange; (3) goods are a distribution mechanism for service provision; (4) operant resources are the fundamental source of competitive advantage; (5) all economies are service economies; (6) the customer is always a co-creator of value; (7) the enterprise cannot deliver value, but only offer value propositions; (8) a service-centered view is inherently customer oriented and relational; (9) all social and economic actors are resource integrators; and (10) value is always uniquely and phenomenologically determined by the beneficiary.

With respect to the relationship between S-D logic and the Three Dichotomies Model, readers should note that specific research projects guided by S-D logic, depending on the particular context of the project, can fit into any of the eight cells in the Three Dichotomies Model. This is because the foundational premises of S-D logic provide an undergirding for various forms of marketing research. Indeed, Lusch and Vargo (2006, p. 406) argue that the S-D logic "may be able to serve as a foundation for the development of a general theory of marketing."

In conclusion, it is possible to classify all the approaches to the study of marketing and all the topics usually considered within the scope of marketing by using the three categorical dichotomies of profit sector/nonprofit sector, positive/normative, and micro/macro. This does not imply that reasonable people cannot disagree as to which topics should fall within the scope of marketing. Nor does it even imply that reasonable people cannot disagree as to which cell in Table 1.1 is most appropriate for each topic. (For example, the study of the efficiency of marketing systems may have both positive and normative aspects.) Rather, Table 1.1 provides a useful analytical framework, as shall be demonstrated in the next section.

1.3.2 Is Marketing a Science?

The previous discussion on the scope of marketing enables us to clarify some of the issues with respect to the definition (broadening the concept) of marketing controversy and the "Is marketing a science?" debate. Most marketing practitioners and some marketing academicians perceive the entire scope of marketing to be profit/micro/normative (cell 2 of Table 1.1). That is, practitioners often perceive the entire domain of marketing to be the analysis and improvement of the decision-making processes of marketers. Most marketing academicians—but assuredly not all—would chafe at delimiting the proper subject matter of marketing to simply the profit/micro/normative dimensions. Most would, at the very least, include all the phenomena, topics, and issues in the top half of Table 1.1 (i.e., cells 1 through 4). As a result of the "broadening the concept of marketing" debate, including in the definition of marketing *all* eight cells in Table 1.1 has probably become the majority position in marketing academe.

Now, returning to the "Is marketing a science?" controversy, the preceding analysis suggests that a primary factor explaining the nature of the controversy is the widely disparate notions of marketing held by the participants. The common element shared by those who hold that marketing is not (and cannot be) a science is the belief that the entire conceptual domain of marketing is cell 2—profit/micro/normative. Hutchinson clearly exemplifies this position:

> There is a real reason, however, why the field of marketing has been slow to develop a unique body of theory. It is a simple one: marketing is not a science. It is rather an art or a practice, and as such much more closely resembles engineering, medicine, and architecture than it does physics, chemistry, or biology. The medical profession sets us an excellent example, if we would but follow it; its members are called "practitioners" and not scientists. It is the work of physicians, as it is of any practitioner, to apply the findings of many sciences to the solution of problems. . . . It is the drollest travesty to relate the scientist's search for knowledge to the market research man's seeking after customers. (Hutchinson 1952, p. 287)

Note first that Hutchinson confuses problem-solving or *market* research (seeking after customers) with problem-oriented, basic or *marketing* research (expanding the knowledge base of marketing). And no one would deny that the "seeking after customers" is not science. Second, if, as Hutchinson implies, the entire conceptual domain of marketing is profit/micro/normative, then marketing is not and (more important) probably *cannot* be a science. However, if the conceptual domain of marketing is expanded to include both micro/positive and macro/positive phenomena, then marketing *could* be a science. That is, if phenomena such as consumer behavior and systems of distribution are included in the conceptual domain of marketing, there is no reason why the study of these phenomena could not be deserving of the designation "science."

Other disciplines, including economics, psychology, sociology, and philosophy, have experienced similar discipline-definitional controversies. It is not at all uncommon for

an economist to review a colleague's work and proclaim, "This isn't *really* economics." Several decades ago, a definitional debate raged in philosophy. Some philosophers preferred a narrow approach, confining the philosophy of science to the method of investigating ordinary language systems. Other philosophers, including Karl R. Popper (1959, p. 19), rebelled at such an emasculation of philosophy of science. Popper noted that all definitions of disciplines are largely arbitrary in content. That is, they primarily represent an agreement to focus attention on some problems, issues, and phenomena to the exclusion of others. Popper believed that to so define philosophy of science would almost, *by definition,* preclude philosophy from making substantive contributions to the total body of scientific knowledge.

A major problem with narrowly circumscribing the appropriate subject matter of a discipline is that it can seriously trammel research and other scientific inquiry. Kaplan (1964, p. 70) refers to the problem as "premature closure." When some marketers confine their conceptualization of the total scope of marketing to its micro-normative dimensions, they *prematurely close* their thinking and the thinking of others over which they have influence. This may be a particularly pernicious process for marketing because, it may be argued, studies that attempt to describe, classify, explain, and predict the micro-positive and macro-positive aspects of marketing may do more for the micro-normative dimension of marketing in the long run than will studies that are specifically restricted to micro-normative marketing. As Kaplan has observed, "Tolerance of ambiguity is as important for creativity in science as it is anywhere else" (1964, p. 71).

Is marketing a science? Differing perceptions of the scope of marketing have been shown to be a primary factor in the controversy over this question. The second factor contributing to the controversy has been the differing perceptions concerning the basic nature of science, a subject to which we now turn.

1.4 THE NATURE OF SCIENCE

The question of whether marketing is a science cannot be adequately answered without a clear understanding of the basic nature of science. So what is a science? Some relativist writers claim that there is nothing that distinguishes science from any form of inquiry: "[S]cience is whatever society chooses to call science" (Anderson 1983, p. 26). An evaluation of the relativist view is deferred to section 10.1. Most marketing writers adopt the nonrelativist position that there are certain characteristics that distinguish the sciences. Many marketers cite the perspective first proposed by Robert Buzzell, which maintains that a science is:

- a classified and systematized body of knowledge,
- organized around one or more central theories and a number of general principles,
- usually expressed in quantitative terms,
- knowledge which permits the prediction and, under some circumstances, the control of future events. (Buzzell, 1963, p. 37)

Buzzell then proceeded to note that marketing lacked the requisite theory and principles to be termed a science.

Although the Buzzell perspective on science has much to recommend it, the requirement "organized around one or more central theories" seems overly restrictive. This requirement confuses the successful culmination of scientific efforts with *science itself.* Was the study of chemistry not a science before discoveries such as the periodic table of elements? The major purpose of science is to develop laws and theories to explain, predict, understand, and control phenomena. Withholding the label "science" until a discipline has "central theories" would not seem reasonable.

The previous comments notwithstanding, requiring a science to be organized around one or more central theories is not completely without merit. There are strong honorific overtones in labeling a discipline a science. The label often signifies that the discipline has "arrived" in the eyes of other scientists. In large part, the label "science" is conferred upon a discipline only when the discipline has matured enough that it contains several "central theories." Thus, physics achieved the status of a science before psychology, and psychology achieved it before sociology. However, the total conceptual content of the term "science" is decidedly not just honorific. Marketing does not, and should not, have to wait to be knighted by others to be a science. How, then, do sciences differ from other disciplines, if not by virtue of having central theories?

Consider the discipline of chemistry—unquestionably a science. Chemistry can be defined as "the science of substances—their structure, their properties, and the reactions that change them into other substances" (Pauling 1956, p. 15). Using chemistry as an illustration, three observations will enable us to clarify the distinguishing characteristics of sciences. First, a distinct science must have a distinct subject matter, a set of phenomena that serves as a focal point for investigation. The subject matter of chemistry is *substances,* and chemistry attempts to understand, explain, predict, and control phenomena related to substances. Other disciplines, such as physics, are also interested in substances. However, chemistry can meaningfully lay claim to being a separate science because physics does not *focus on* the reactions of substances.

What is the basic subject matter of marketing? Most marketers now perceive the ultimate subject matter of marketing to be the *transaction.* Harking back to the chemistry analogue, marketing might then be viewed as the science of transactions—their structure, their properties, and their reactions with other phenomena. Given this perspective, the subject matter of marketing would certainly overlap with that of other disciplines, notably economics, psychology, and sociology. The analysis of transactions is considered in each of these disciplines. Yet, only in marketing is the transaction the *focal point.* For example, transactions remain a tangential issue in economics, where the primary focus is on the allocation of scarce resources (Leftwich 1966, p. 2). *Therefore, the first distinguishing characteristic is that any science must have a distinct subject matter.* To the extent that transactions—whether they be single transactions or repeated transactions, transactions among anonymous parties or parties that have long-term relationships—are the basic subject matter of marketing, marketing would seem to fulfill this requirement.

A distinct subject matter alone is not sufficient to distinguish sciences from other

disciplines because all disciplines have a subject matter (some less distinct than others). The previously cited perspective of chemistry provides additional insight into the basic nature of science. Note the phrase "their structure, their properties, and [their] reactions." Thus, the second distinguishing characteristic is that every science seeks to describe and classify the structure and properties of its basic subject matter. Likewise, the term *reactions* suggests that the basic subject matter of chemistry is presumed to be systematically interrelated. Thus, the third distinguishing characteristic: *Every science presupposes the existence of underlying uniformities, regularities, and causal structures among the phenomena that comprise its subject matter.*[6] *The discovery of these underlying uniformities yields empirical regularities, lawlike generalizations, laws, principles, and theories.*

The basic question for marketing is not whether there now exist several central theories that serve to unify, explain, and predict marketing phenomena. Rather, the following should be asked: "Are there underlying uniformities, regularities, and causal structures among the phenomena that constitute the subject matter of marketing?" This question can be answered affirmatively on two grounds—one *a priori* and one empirical. Marketing is a discipline investigating human behavior. Insofar as numerous uniformities and regularities have been observed in other behavioral sciences, there is no *a priori* reason for believing that the subject matter of marketing is devoid of uniformities and regularities. The second ground for believing that the uniformities exist is empirical. In the past five decades, the quantity of scholarly research conducted on marketing phenomena probably exceeds the total of *all* prior research in marketing. Efforts in the consumer behavior dimension of marketing have been particularly prolific. Who can deny that *some* progress has been made or that *some* uniformities have been identified? In short, who can deny that there exist uniformities and regularities in the subject matter of marketing? I, for one, cannot.

The task of delineating the basic nature of science is not yet complete. Up to this point, we have utilized chemistry to illustrate that all sciences involve (1) a distinct subject matter, (2) the description and classification of the subject matter, and (3) the presumption that underlying the subject matter are uniformities, regularities, and causal structures that science seeks to discover. The chemistry example provides a final observation. Note that "chemistry is the *science* of . . ." This suggests that sciences can be differentiated from other disciplines (and pseudo-sciences) by the method of analysis. At the risk of being somewhat tautologous, sciences employ a set of procedures that are commonly referred to as the scientific method. The historical significance of the development and acceptance of the method of science cannot be overstated. The scientific method has been called "the most significant intellectual contribution of western civilization" (Morris 1955, p. 63). Is the method of science applicable to marketing?

One way of interpreting this monograph is to view it as an articulation of the application of the scientific method to marketing. Therefore, comments here on the scientific method should be viewed as strictly introductory in nature. One immediate observation is that there is no reason to presume that the scientific method of analysis is any less appropriate to marketing than it is to other disciplines. Similarly, scholarly researchers in marketing, though often holding rather distorted notions concerning topics such as the role of laws and theories in research, seem to be at least as technically proficient as researchers in other areas.

The second observation concerning the scientific method involves the "unity of scientific method controversy." Three questions frame this issue: Is there a single scientific method? Do different sciences require different methods? Are several scientific methods appropriate for the same science? Zaltman, Pinson, and Angelmar (1973, p. 93) state that *the* scientific method is a myth. Conversely, Bergmann (1957, p. 164) states that "there is one and only one scientific method." Similarly, whole volumes have been written on the unity of the scientific method (Neurath, Carnap, and Morris 1955; Jeffrey 1966; Kaiser 1993; Snyder 1978). The unity of scientific method controversy has far-reaching implications for marketing. In fact, a basic understanding of the foundations of this controversy is a precondition to comprehending fully the rest of this monograph. Consequently, the next section will be devoted to an exposition of this issue. The analysis will draw heavily from the works of Gustav Bergmann, Carl Hempel, Harvey Kyburg, Richard Rudner, and Wesley Salmon.[7]

1.5 THE UNITY OF SCIENTIFIC METHOD

The claim is sometimes made that marketing and other social sciences require a scientific method that differs somewhat from the method of the physical sciences. Do the various sciences require different scientific methods? Are there several equally appropriate methods for any particular science? Those who respond affirmatively to these questions support the Multi-Scientific-Method (MSM) thesis. Those who respond negatively support the Single-Scientific-Method (SSM) thesis. Such is the substance of the unity of science issue. Analyzing it requires an appreciation for (1) the differences between the *methodology* of a discipline and the *techniques* of a discipline and (2) the importance of carefully distinguishing between issues in the context of discovery versus issues in the context of justification. (The context of justification is sometimes referred to as the context of validation.)

The techniques of a discipline are the specific tools and apparatus, both conceptual and physical, that researchers in a discipline have found useful in the conduct of inquiry. Marketing uses devices such as consumer panels, questionnaires, pupilometers, Likert scales, multiple regression, multidimensional scaling, surveys, random sampling, and multiple classification analysis. Some of these tools are conceptual, and some are physical. The tools used encompass the *techniques* of marketing research. Chemistry employs test tubes, thermocouples, spectrometers, and cyclotrons. Some techniques, such as strictly experimental research designs, are much more common in the physical sciences than in marketing or most of the social sciences. Unfortunately, many advocates of the MSM thesis point to these differences in techniques to support their position. However, the scientific method is not restricted to certain kinds of hardware (test tubes, cyclotrons, pupilometers), to techniques of gathering data (experiments, surveys), to techniques of measuring phenomena (thermometers, Likert scales), or, most certainly, to techniques of analyzing data (regression, structural equation modeling, multiple classification analysis, canonical correlation). Astronomy is unquestionably a science, and yet its techniques are in some respects closer to those of marketing than to those of physics (note the con-

spicuous lack of laboratory experimentation in astronomy). To the extent that advocates of the MSM thesis rely on these differences in techniques for evidential support, their position becomes either (a) trivial or (b) untenable: trivial, because different sciences obviously use different techniques in research; untenable, because the *techniques* of a science should not be confused with the *methodology* of a science. What, then, is the methodology of science?

Philosophers of science agree that *the methodology of science is its logic of justification.* That is, the scientific method consists of the rules and procedures on which a science bases its acceptance or rejection of its body of knowledge, including hypotheses, laws, and theories.[8] To the extent that advocates of the MSM thesis are not simply referring to different techniques, they are really claiming that different sciences have (or should have?) different bases on which to assess the truth content of their disciplines. This, indeed, is a radical claim and one that has been discredited (Rudner 1966, p. 4). As Hempel has observed:

> The thesis of the methodological unity of science states, first of all, that, notwithstanding many differences in their techniques of investigation, all branches of empirical science test and support their statements in basically the same manner, namely by deriving from them implications that can be checked intersubjectively and by performing for those implications the appropriate experimental or observational tests. This, the unity of method thesis holds, is true also of psychology and the social and historical disciplines. In response to the claim that the scholar in these fields, in contrast to the natural scientists, often must rely on empathy to establish his assertions, logical-empiricist writers stressed that imaginative identification with a given person often may prove a useful heuristic aid to the investigator who seeks to guess at a hypothesis about that person's beliefs, hopes, fears, and goals. But whether or not a hypothesis thus arrived at is factually sound must be determined by reference to objective evidence: the investigator's empathic experience is logically irrelevant to it. (Hempel 1969, p. 191)

As Siegel (1985) argues, the methodological criteria of science that collectively constitute the scientific method (SM) can best be expressed as a "commitment to evidence," as exemplified by "a concern for explanatory adequacy, however that adequacy is conceived; and insistence on testing, however testing is thought to be best done; and a commitment to inductive support, however inductive inference is thought to be best made" (p. 528). Thus, "science's commitment to evidence, by way of SM, is what justifies its claim to respect—science is to be taken seriously precisely because of its commitment to evidence" (p. 530). However, the preceding conclusions by Siegel about scientific method do not imply that he claims that science is the *only* domain in which the epistemic worthiness of beliefs, hypotheses, or claims may be putatively established on the basis of evidence. Rather, "SM extends far beyond the realm of science proper. But this is only to say that SM can be utilized widely. It is still properly labeled *scientific* method" (p. 530).

Therefore, the unity of science stems from the common acceptance by the sciences of a methodology for the justification (confirmation, validation, corroboration) of knowledge.

One of the primary objectives of this monograph will be to explore systematically this logic of justification as applied to marketing. That is, we shall explore the basic nature of the scientific understanding, explaining, and predicting of marketing phenomena. In the process, the unique roles of hypotheses, laws, lawlike generalizations, empirical generalizations, and theories will be developed.

This section began by delineating two rival theses: the Multi-Scientific-Method thesis and the Single-Scientific-Method thesis. Supporters of the MSM thesis often have been shown to confuse the techniques of a science with its methodology. A second shortcoming of the MSM thesis is even more important: a*dvocates of the MSM thesis often confuse the context of discovery with the context of justification.*

1.5.1 Discovery Versus Justification

How does one go about discovering scientific hypotheses, laws, and theories? What kinds of tools and procedures will assist the researcher in uncovering them? Are some procedures better than others? Are some procedures correct and others incorrect? Is there a single procedure that is guaranteed to produce results? Should a theory or law be evaluated on the basis of how that theory or law was generated? What was the genesis of Reilly's Law? How did John Howard and Jagdish Sheth create their theory of buyer behavior? All these issues and questions should be considered in the *context of discovery.* If there existed a set of systematic rules and procedures that were optimal for the discovery of hypotheses, laws, and theories, this set of rules and procedures would constitute a *logic of discovery.*

How does one scientifically *explain* marketing phenomena? Can one explain marketing phenomena without being able to *predict* them? What are the roles of laws and theories in explaining and predicting phenomena? Must theories be empirically testable? Are the axioms of a theory assumed to be true? Must theories contain lawlike generalizations? These issues and questions belong in the *context of justification.* As previously noted, the set of rules and procedures that delineate the criteria for accepting or rejecting knowledge (hypotheses, laws, and theories) in science constitutes its *logic of justification.*

Salmon has observed that treating issues that appropriately belong in the context of discovery as if they belong in the context of justification often leads one to commit the "genetic fallacy." He offers the following statement as an extreme illustration of the genetic fallacy: "The Nazis condemned the theory of relativity because Einstein, its originator, was a Jew" (1963, p. 12). Obviously, in this extreme example, the fact that Einstein was a Jew should have been ignored in assessing the validity of the theory of relativity. Unfortunately, in the literature of science, dividing issues into discovery and justification is frequently much more difficult, and, thus, a trap is laid for the unsuspecting researcher.

Bergmann (1957, p. 51) has noted that many philosophers of science, including Hegel and John Dewey, have confused the discovery of scientific knowledge with its justification. The confusion of discovery with justification seems widespread in all the sciences, and marketing is no exception. This writer (1971) has previously observed that Robert Bartels's (1968) "The General Theory of Marketing" should perhaps be evaluated in the context of

discovery, rather than justification. Similarly, Bartels (1970) has offered a metatheory of marketing comprising seven axioms. To what extent are these seven axioms proposed as rules for *discovering* marketing theory, and to what extent are they to be used to *evaluate* marketing theory? Likewise, William Lazer (1962) believes that there are two approaches for developing models and theories in marketing—*abstraction* and *realization.* Abstraction begins with "perceptions of marketing situations," whereas realization begins with "theoretical and abstract statements about marketing." Which aspects of these approaches belong in the context of discovery, and which in the context of justification? Similarly, in discussing the scientific method, Zaltman, Pinson, and Angelmar (1973, p. 12) propose a model of the research process composed of the following nine states:

1. Assessment of relevant existing knowledge
2. Concept formation and specification of hypotheses
3. Acquisition of meaningful data
4. Organizing and analyzing data in relevant ways
5. Evaluation of and learning from results
6. Dissemination of research information
7. Providing explanations
8. Making predictions
9. Engaging in necessary control activities

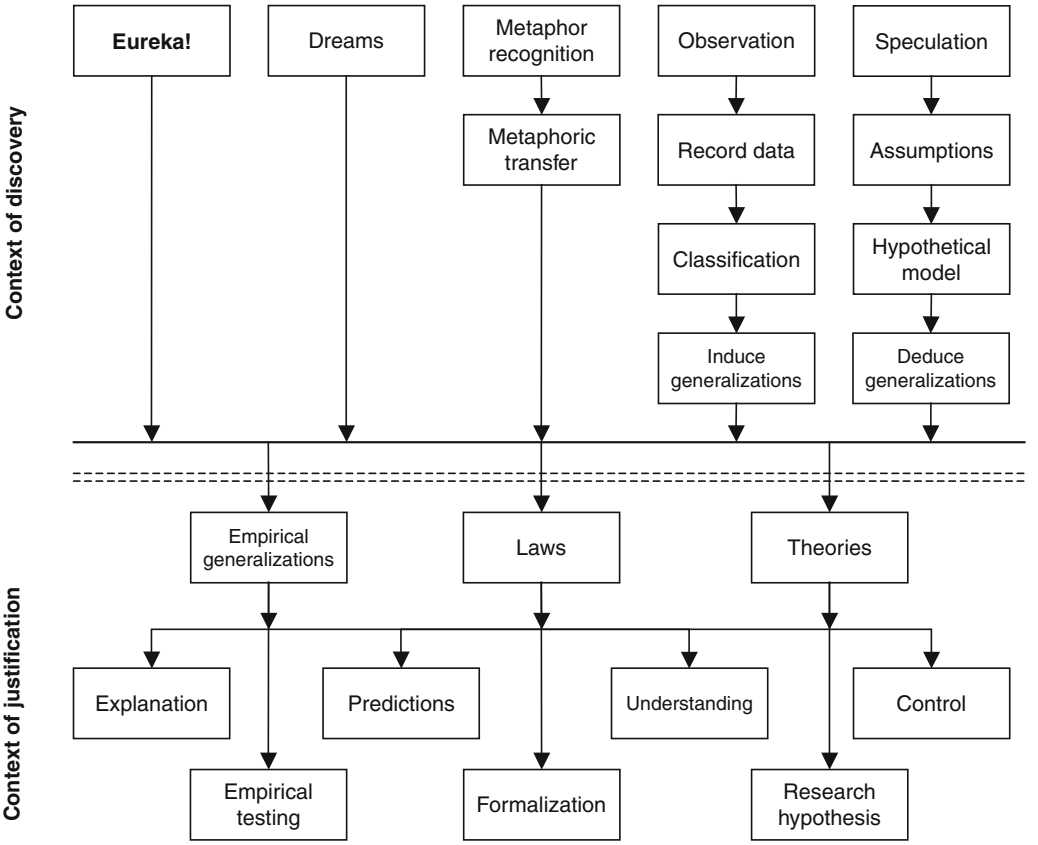
The reader should attempt to determine which of these stages should be evaluated in the context of discovery, and which in the context of justification. It is hoped that the chart in Figure 1.1 will be of some assistance.

Figure 1.1 attempts to provide guidance as to which issues are in the context of discovery and which are in the context of justification. The top half of the chart shows five procedures for generating (discovering) empirical generalizations, laws, and theories. These procedures are simply illustrative and by no means exhaust the possibilities. The bottom half details a variety of issues in justification, and, again, the issues by no means exhaust the subject. Because the entire remainder of this monograph will be devoted to exploring the issues in the bottom half of Figure 1.1, our attention here will be directed to the top half, that is, the alternative routes to the discovery of scientific knowledge.

One route in Figure 1.1 shows that dreams sometimes play an important role in scientific discovery. Hempel relates the story of how the chemist Kekulé discovered the structure of the benzene molecule:

> He had long been trying unsuccessfully to devise a structural formula for the benzene molecule when, one evening in 1865, he found a solution to his problem while he was dozing in front of his fireplace. Gazing into the flames, he seemed to see atoms dancing in snakelike arrays. Suddenly, one of the snakes formed a ring by seizing hold of its own tail and then whirled mockingly before him. Kekulé awoke in a flash: he had hit upon the now famous and familiar idea of representing the molecular structure of benzene by a hexagonal ring. (Hempel 1966, p. 16)

Figure 1.1 **Discovery Versus Justification**



Source: Hunt (1991a). Reprinted by permission of the author.

Similarly, it is claimed that the goddess of Namakkal visited the great Indian mathematician Ramanujan (1887–1920) and revealed mathematical formulas to him (Salmon 1963, p. 10). Thus, sometimes a dream can prompt a scientific discovery.

A second path to discovery, the eureka route, can be illustrated by Archimedes' (287–212 B.C.) principle. Supposedly, Archimedes noticed that his bathwater rose in height when he immersed himself. Shouting "Eureka!" he proclaimed that any body immersed in a fluid would be "buoyed up" with a force equal to the weight of the displaced fluid. Likewise, schoolchildren are told the story of how Newton discovered the universal law of gravitation when an apple dropped on his head. The point of these examples (fables?) concerning the dream and eureka routes is that serendipity plays a prominent role in scientific discovery. *Many, if not most, major scientific discoveries are flashes of perceptual insight and are not the result of following some rigorously prescribed procedure.*

A third path to discovery is metaphor recognition, followed by a subsequent metaphoric transfer.[9] A metaphor is a literally false, declarative assertion of existential equivalence

that compares two concepts or things, where one concept, called the primary concept, is claimed to be another, the secondary concept. For example, in the metaphor "strategic alliances are marriages," *strategic alliances* constitute the primary concept and *marriages* constitute the secondary. It is crucial to keep in mind that all metaphors are denotatively false, though—at least potentially—connotatively true. Indeed, the creative power of metaphors to foster theory development springs from, paradoxically, the very falseness of metaphors.

The denotative, literal meanings of words in all natural languages (e.g., English) change through time by adding new meanings and, at times, deleting old ones. Therefore, in addition to "live" metaphors, we have "extinct" and "dormant" ones (Boyd 1979). Whereas live metaphors are readily recognized and acknowledged by all members of a common language community, an *extinct* metaphor is an expression whose metaphorical origins are so deeply buried in the history of a particular language that current speakers would not recognize it as a metaphor without professional etymological help. For example, few now recognize that the English "muscle" comes from the Latin "musculus," and its metaphorical meaning of "little mouse." *Dormant* metaphors, though not yet extinct, are not customarily recognized until the metaphorical aspect is pointed out by the metaphor's author, usually by use of quotation marks, e.g., "channels" of distribution and "live" metaphors.

When researchers in a discipline recognize that a metaphor from another discipline may prompt theory development, the researchers in the borrowing discipline effect a metaphoric transfer. In such a transfer, researchers in the adopting discipline are invited to borrow theoretical structures from the source or "parent" discipline (Mirowski 1989). The borrowing may be in the form of ontology, concepts, lawlike generalizations, theories, and values. For example, from the marriage literature, the "strategic alliances are marriages" metaphor invites researchers of strategic alliances to consider theorizing about strategic alliances using (1) the marital ontology of spouses, family, children, and relatives, (2) the concepts of relationship, partners, divorce, reproduction, extramarital affairs, and child support, and (3) the values of commitment, trust, harmony, financial security, and procreation. The "marketing is war" metaphor invites marketers to use (1) the ontology of armies, allies, combatants, and divisions, (2) the concepts of intelligence, deployment, espionage, and strategy, and (3) the values of honor, defense, victory, and conquest. Therefore, a third path to discovery is a metaphoric transfer. Some metaphoric transfers are fruitful; others are barren (or should I say "barren"?).

Consider, now, the fourth route in Figure 1.1, which starts with *observation,* and the fifth route, which starts with *speculation.* The fourth route is a variant of the generalized *inductivist* route, and the fifth is a form of the generalized *deductivist* route. The deductivist route plays a prominent role in much economic theory, while marketing theoreticians have historically advocated an inductivist route. Thus, McGarry (1936), in the very first volume of the *Journal of Marketing,* maintained that the "scientific method" in marketing involves the four steps of (1) selecting facts, (2) registering these facts, (3) rearranging the facts so as to bring order out of chaos, and (4) finding a formula or conclusion. Similarly, three decades later, Schwartz (1963, p. 135) argues that "there appears to be

no way of avoiding the laborious empirical method in discovering marketing theory." Conversely, Fisk (1971, p.10) notes that "the first step in applying the scientific method is to think of an hypothesis." Both "grounded theory" (e.g., Glaser and Strauss 1967; Strauss and Corbin 1998) and "data mining" (e.g., Piatetsky-Shapiro, Fayyad, and Smith 1996; Piatetsky-Shapiro and Frawley 1991) are associated with the inductivist approach. Is the *strict inductivist* route the preferred procedure for marketing, as many marketing theorists and others suggest? To answer this question, we must examine more carefully the basic nature of this route.

Let us suppose that a marketing theorist desires to explore phenomena relating to consumer purchase behavior. In particular, let us suppose that the theorist has decided to focus on the phenomenon of brand loyalty as the "dependent" variable. The inductivist route suggests that the first steps for the theorist would be observation and the recording of data. What would the theorist observe? "Everything," the strict inductivist might reply. Yet, we might then point out that observing and recording everything would be impossible because the number of potential phenomena that could be observed and recorded at any point in time is virtually infinite (phase of the moon, height of the tides, temperatures, etc.). The inductivist would probably then become somewhat defensive, chastise us for taking his statement "too literally," and respond, "Of course, you should not observe and record all phenomena! Just observe and record all data *relevant* to the problem of brand loyalty." Readers should attempt to place themselves in the role of the bewildered research assistant who has just been instructed to observe and record all data *relevant* to the problem of brand loyalty and ignore all phenomena *irrelevant* to brand loyalty. By what criterion is the research assistant to separate relevant phenomena from the irrelevant variety? The strict inductivist will finally admit that the charge to the research assistant must be first to think of some *a priori* hypotheses concerning which phenomena might be systematically related to brand loyalty and then to make observations and record data *relevant* to those hypotheses. That is, data are never *a priori* relevant or irrelevant to a *problem* such as brand loyalty. Rather, data can only be *a priori* relevant or irrelevant to hypotheses (however crude or tentative) concerning certain phenomena and brand loyalty.

After observation and the recording of data comes the classification stage in the inductivist route. By now the reader should recognize that the appropriate question is, "On what basis should the data be classified?" Again, because the number of ways a set of data can be classified is virtually unlimited, the data must be classified on some basis that is likely to be useful for exploring *a priori* hypotheses. In conclusion, the strict inductivist approach to theory building is untenable, because speculation and the creation of *a priori* hypotheses are absolutely essential parts of any *systematic procedure* of theory discovery and creation. Actual research does not proceed according to the stages suggested by the strict inductivists, and the inductivist route certainly cannot be defended as the *preferred* procedure.

Given the previous analysis of the strict inductivist route for generating theory, why do so many marketing theorists tout its virtues? Perhaps many marketing theorists have reacted (overreacted?) to certain perceived deficiencies in mainstream, neoclassical, economic theory. Neoclassical economic theory is perceived to be deductive in nature.

It is also often thought to be unrealistic and divorced from the real world. As Alderson (1965, p. 18) once noted, "economists have shown a notable preference for elegance over relevance." Nevertheless, it is erroneous to believe that the deficiencies of neoclassical economic theory are to be found in the context of discovery. That is, to the extent that economic theory is deficient, the cause lies not in the economists' deductive procedures for generating theory. Rather, the truth of economic theory must be analyzed and evaluated in the context of justification. To what extent has neoclassical economic theory been tested and found to depict the real world accurately? These and other questions are appropriate to ask, but have to do with the context of justification, not discovery.

This section began by proposing that advocates of the Multi-Scientific-Method thesis often confuse issues and procedures in the context of discovery with issues and procedures in the context of justification. Proponents of MSM point out the wide variety of "methods" for the discovery of laws and theories in science. We must grant their premise, but not their conclusion. Granted, there exists no single set of procedures that is guaranteed to lead to the discovery of laws and theories. Also, there exists no single set of procedures that can be defended as optimal. *Therefore, there is no single logic of discovery.* Nevertheless, the conclusion that many scientific methods exist does not necessarily follow, because the scientific method concerns the context of justification, not discovery, and *there does exist a single logic of justification that is common to all science.* The reader should clearly recognize the difference between *discovery* and *justification* because we shall be referring to it on numerous occasions throughout this monograph.

1.6 CONCLUSIONS ON MARKETING SCIENCE

The purpose of these introductory sections has been to detail the objectives of this monograph and to discuss some preliminary issues. The primary objective has been stated to be the utilization of the tool kit of the philosophy of science for systematically exploring some of the basic methodological issues underlying marketing research. The preliminary issues explored have been in the context of the "Is marketing a science?" controversy. In attempting to explore this controversy, we have found it necessary to delve into the basic nature of both marketing and science and to distinguish between methodology and techniques and between discovery and justification. The analysis suggests several conclusions.

The scope of the area called marketing has been shown to be exceptionally broad. Marketing has micro/macro dimensions, profit sector/nonprofit sector dimensions, and positive/normative dimensions. Reasonable people can disagree as to which combination of these dimensions (refer to the eight cells in Table 1.1) represents the *appropriate* total scope of marketing. If marketing were restricted to the profit/micro/normative dimension (as many practitioners, as well as some academics, would do), then marketing would not be a science and could not become one. Sciences involve the explanation, prediction, and understanding of phenomena, and, therefore, any discipline that is purely evaluative (normative) is not a science. At least for marketing academe, restricting the scope

of marketing to its profit/micro/normative dimension is unrealistic, unnecessary, and, without question, undesirable.

Once the appropriate scope of marketing has been expanded to include at least some *positive* dimensions (cells 1, 3, 5, and 7 in Table 1.1), then the explanation, prediction, and understanding of these phenomena could be a science. The question then becomes whether the study of the positive dimensions of marketing has the requisite characteristics of a science. Aside from the strictly honorific overtones of nonmarketers accepting marketing as a science, the substantive characteristics differentiating sciences from other disciplines have been shown to be (1) a distinct subject matter, (2) the description and classification of the subject matter, (3) the presumption of underlying uniformities, regularities, and causal structures in the subject matter, and (4) the adoption of the method of science for studying the subject matter. The *positive* dimensions of marketing have been shown to have a subject matter properly distinct from that of other sciences. The marketing literature is replete with description and classification. There have been discoveries (however tentative) of uniformities, regularities, and causal structures in marketing phenomena. Finally, though it was once fashionable for researchers such as Longman (1971, p. 10) to deplore "the rather remarkable lack of scientific method employed by scientists of marketing," I suggest that marketing researchers are at least as committed to the method of science as are researchers in other disciplines. Therefore, we can conclude that the study of the *positive* dimensions of marketing can be appropriately referred to as *marketing science.*

1.7 THE THREE DICHOTOMIES MODEL: AN EVALUATION

Since its first publication in 1976, the "Three Dichotomies Model of Marketing" has evoked substantial interest and significant controversy. For an early review and analysis of the controversy, see Arndt (1981a). Many marketing academicians have expressed views such as "I basically agree with the model except for the following characteristic . . ." This section will address the major "except fors" that have been proposed. Although the positive/normative dichotomy has sparked the most spirited comments, it is fair to say that all aspects of the model have been questioned. And this is as it should be, because models in general are not (a positive observation) and should not be (a normative observation) immutably inscribed in stone.

Although numerous colleagues have volunteered their observations on the Three Dichotomies Model, the "except fors" discussed here have come primarily from (1) the participants at various conferences, especially the Macromarketing Conferences, the first of which was held at the University of Colorado in 1976 (Slater 1977), (2) letters to the editor of the *Journal of Marketing* by Donald P. Robin (1977, 1978) and Michael Etgar (1977), and (3) an article by Michael R. Hyman, Robert Skipper, and Richard Tansey (1991). These writers have charged that the positive/normative dichotomy is (1) false, (2) dangerous, (3) unnecessary, (4) meaningless, and (5) useless. It is further claimed that (6) all of marketing thought is normative. Each charge will be discussed, in turn. First, however, it will be useful to provide a brief review of the positive/normative dichotomy in the philosophy of science.

1.7.1 The Positive/Normative Dichotomy in Philosophy of Science

David Hume (1711–1776) is generally given credit for being the first philosopher to point out that statements concerning the verb "is" are different in kind from statements containing the verb "ought." In particular, Hume observed that no set of statements containing only descriptive terms and no copula except "is" can logically yield a conclusion containing an "ought." The positive/normative dichotomy is the version of Hume's "is/ought" dichotomy discussed in John Neville Keynes's (1891) classic work, *The Scope and Method of Political Economy.* There he defined a positive science as "a body of systematized knowledge concerning what is" and a normative science as "a body of systematized knowledge discussing criteria of what ought to be" (pp. 34–35). Keynes pointed out that "confusion between them is common and has been the source of many mischievous errors" (p. 46). The *Dictionary of Philosophy* clarifies the view in philosophy about the is/ought dichotomy:

> **Is/Ought Dichotomy.** Also, *fact/value dichotomy.* Statements containing the verb *is* are related to descriptive or factual claims and are of a different order from those containing the verb *ought* (*should*), which are related to judgments, evaluations, or commands. It is impossible (logically, formally, conceptually) to derive an "ought" (or "should") statement from an "is" (factual) statement, a normative statement from a statement of facts; it is impossible to have a valid deductive argument in which the premises state descriptions and the conclusion states prescriptions or imperatives. (Angeles 1981, p. 138)

The "is/ought fallacy" is one of the most widely accepted ideas in philosophy and is related to the "naturalistic fallacy" in the area of ethics, which is: "The fallacy of deriving (deducing) ethical statements from nonethical statements" (Angeles 1981, p. 186). Indeed, the is/ought fallacy and the naturalistic fallacy are two of only four fallacies that are given special attention under the heading of "Philosophical Fallacies" in *The Encyclopedia of Philosophy* (Edwards 1972, p. 178).

So, the is/ought (i.e., positive/normative) dichotomy forms the foundation for one of the most important classes of fallacies in all of philosophy. It is a dichotomy that has proved its usefulness in many disciplines for several centuries. Yet, critics contend that when the dichotomy is applied to the marketing discipline, it is, among other things, so ambiguous as to render it useless. On *prima facie* grounds such a conclusion is implausible. Specific analyses of specific criticisms provide other grounds.

1.7.2 Is the Positive/Normative Dichotomy False?

Some writers have suggested that *the positive/normative dichotomy is a false dichotomy because we cannot escape from our own value systems.* This premise is probably true: we probably cannot escape from our value systems. Nevertheless, the premise does not imply that the positive/normative dichotomy is false; to accede to this conclusion would be to capitulate to despair and to commit the "philosophers' fallacy of high redefinition" (see section 11.5).

There is an analogous problem in other disciplines. Consider journalism. It stresses *objectivity* in reporting: journalists attempt to keep their personal value systems out of their *news* writing. All knowledgeable people recognize the impossibility of keeping the news columns completely free of editorializing. Nevertheless, the goal of separating news from opinion remains one of the ethical pillars of journalism. It does so because the credibility of journalists would be irreparably damaged if they abandoned the *goal* of objectivity simply because they found its complete *achievement* to be unattainable. Furthermore, holding the goal of objectivity in high regard is itself a *value*—and, for many, a highly important value. (Alas, for some "journalists" it is quite unimportant.)

So it is in marketing. Granted, marketing research cannot be value-free. But this does not imply that we should not *attempt* to separate the positive issue of whether marketers perceive themselves as having "social responsibilities" from the normative issue of whether marketers *should* have social responsibilities. Nor does it imply that we should not attempt clearly to separate the issue of whether the poor do in fact "pay more" from the issue of whether it is *wrong* for them to pay more. Nor, finally, does it imply that we should not attempt to separate how managers do in fact make marketing decisions from how they *should* make those decisions. The importance of the positive/normative dichotomy as a goal for clear thinking and analysis is in no way impaired because that goal is, in principle, not completely attainable.

1.7.3 Is the Positive/Normative Dichotomy Dangerous?

A few colleagues have proposed that *the positive/normative dichotomy is dangerous because it may lead people to downgrade the importance of micro/normative marketing.* However, the model does not imply that micro/normative marketing is unimportant or unworthy of attention. On the contrary, the study of how marketing decisions *should* be made is extremely important. To believe otherwise is to grossly misinterpret the model. Some marketers, upon reviewing the taxonomic schema shown in Figure 1.1, may perhaps choose to deemphasize micro/normative marketing. Furthermore, this deemphasis *may* have some unfortunate consequences. Nevertheless, many marketers *still* consider the entire scope of marketing to be its micro-normative dimension, and this *has had* demonstrably unfortunate consequences, as pointed out by Wilkie and Moore (2003).

The positive/normative dichotomy puts micro/normative marketing "in its place." It does not imply that its "place" is unimportant. The potential advantages of awakening marketing practitioners and academics to the fact that there are dimensions to marketing *beyond* the micro/normative dimension greatly outweigh any possible dangers of downgrading the importance of micro/normative marketing.

1.7.4 Is the Positive/Normative Dichotomy Unnecessary?

Robin charges that *the positive/normative dichotomy is "unnecessary" for considering the "Is marketing a science?" controversy because "using scientific explanation in marketing*

simply requires that the normative statements be used as antecedent conditions" (1977, p. 136). To illustrate his point, he offers the following "explanation" as an example:

> *Antecedent Conditions*
>
> C_1 = Long-run profit maximization is the primary objective of the organization.
> C_2 = One or more competitors of approximately equal economic strength exist in the market.
> C_3 = The organization has the opportunity of offering several different variations of a new product, at varying prices, with different promotional possibilities, and through a variety of channels.
> C_4 = The products of the firm are such that buyers can adequately determine their value (functional and/or psychological).
>
> *Generally Accepted Propositions*
>
> L_1 = Strong competitors can and will produce products desired by buyers if a large enough number desire them.
> L_2 = Buyers purchase goods in a manner that they perceive to be in their best interest at the time of the purchase.
>
> *Thus:* Given C_1, C_2, C_3, C_4, L_1, and L_2, the firm must organize the controllable variables available to it (i.e., the marketing mix) so as to develop and maintain a satisfied group of buyers. (Robin 1977, pp. 137–38)

Robin concludes, "It should be noted that the antecedent condition C_1 represents an assumed organizational objective that is, as previously explained, a *normative judgment"* (Robin 1977, p. 138).

Is the positive/normative dichotomy unnecessary? Do normative statements play a role in scientific explanation? To evaluate these questions, we must refer to the meaning of *positive statements* versus *normative statements.* Recall that the positive/normative dichotomy provides categories based on whether the focus of the analysis is primarily descriptive or prescriptive. Positive marketing adopts the perspective of attempting to describe, explain, predict, and understand the marketing activities, processes, and phenomena that actually exist. This perspective examines *what is.* In contrast, normative marketing adopts the perspective of attempting to prescribe what marketing organizations and individuals ought to do or what kinds of marketing systems a society ought to have. That is, this perspective examines what *ought to be* and what organizations and individuals *ought to do.* Thus, one signal (but not the *only* one) of a normative statement is the existence of an *ought* or *should* or some similar term.

Returning to Robin's "explanation," are there any normative statements? In particular, is C_1 normative, as Robin indicates? Clearly, it is a *positive* statement, not a normative one. C_1 states that "long-run profit maximization is the primary objective of the organization."

If C_1 were normative, it would state, "Long-run profit maximization *ought to be* the primary objective of the organization." As a matter of fact, there are *no* normative statements in Robin's "explanation." Note that C_2 states that "competitors of approximately equal economic strength *exist,*" not that they "ought to exist." Further, C_3 and C_4 state that "the organization *has"* and that "products of the firm are"—both are positive statements.

There are no normative statements in Robin's "explanation" precisely because (as Robin himself points out) in scientific explanation "the explanandum [the statement to be explained] must be logically derivable from the explanans [the statements doing the explaining]." And any statement with an "ought" *cannot* be an antecedent condition in a logically valid scientific explanation. Thus, far from showing that the positive/normative dichotomy is *unnecessary,* Robin's "explanation" gives powerful justification for considering the dichotomy to be *necessary.* Robin's later analysis agreed:

> In my simple model, C_1 (antecedent condition No. 1) was given: "Long-run profit maximization is the primary objective of the organization." Professor Hunt states, "Clearly it (C_1) is a *positive* statement not a normative one." He further states that there are *no* normative statements in Robin's 'explanation.'" *I completely agree on both accounts.* (1978, p. 6; italics added)

1.7.5 Is the Positive/Normative Dichotomy Meaningless?

Robin also suggests that *the positive/normative dichotomy is "meaningless" because the information derived from a positive "study is of little interest unless it is given prescriptive overtones. . . . That is, the positive issues are barren except where they have prescriptive implications"* (1977, p. 136). We can begin our analysis of this comment by asking: To *whom* are positive studies "of little interest" unless there are prescriptive implications? Is it to the marketing manager? Are all positive studies "barren" unless they provide immediate guidance as to how the marketing manager can make better decisions? This seems to be the "meaningfulness" criterion that Robin is proposing.

No one would dispute that many of the positive studies in marketing have managerial implications. There is no doubt that the explanations and predictions from positive studies frequently serve as useful guides for developing normative decision rules and normative models. However, the view that all positive studies are "meaningless" unless they assist the marketing manager is exactly the kind of narrow perception of the scope of marketing that has long caused so much mischief in our discipline. The discipline of marketing does not exist solely and exclusively to serve the needs of the marketing manager, just as the discipline of psychology does not exist solely to serve the needs of the clinical psychologist. Research in marketing has many aims. Fortunately, the American Marketing Association (AMA) now specifically acknowledges that there are numerous "constituencies" for marketing research. The AMA Task Force on the Development of Marketing Thought identified five such groups: academicians (including students), managers, public policy members, special interest groups, and consumers (Monroe et al. 1988).

The prime directive for scholarly research is the same for marketing as for all sciences:

to seek knowledge. The knowledge must be intersubjectively certifiable and capable of describing, explaining, and predicting phenomena. At some times, the knowledge may assist marketing managers in making decisions. At other times, the knowledge may guide legislators in drafting laws to regulate marketing activities. At still other times, the knowledge may assist the general public in understanding the functions that marketing activities perform for society. Finally, the knowledge may simply assist marketing scholars in *knowing,* a not inconsequential objective.

1.7.6 Is the Positive/Normative Dichotomy Useless?

In a restatement of his position, Robin (1978, p. 6) agrees that "normative statements are not part of scientific explanatory models." Nevertheless, he still concludes that "*any attempt to classify marketing phenomena on the basis of a positive/normative dichotomy will lead either to confusion or a useless set of relationships.*" His conclusion is based in part on the observation that the positive and normative "parts of our world are so inseparably intermingled." Robin in this case seems to be echoing Etgar's observation that "Hunt's classification fails to pass a major test by which categorical schemes should [readers please note: this is a normative statement] be judged: namely, self-exclusivity—the principle that under a specific classificatory scheme, such as normative/positive, a given phenomenon should be classified as falling into one category only" (Etgar 1977, p. 146).

Is lack of self-exclusivity a fatal flaw? I think not. First of all, it is unnecessary to concede the *positive* issue of whether the positive/normative dichotomy is completely lacking in self-exclusivity. Many studies in marketing are either overwhelmingly positive (such as descriptive analyses of the channels of distribution for farm products) or overwhelmingly normative (such as models for the optimal allocation of media expenditures). Thus, it is clear that the classification in the model of many, if not most, marketing studies and phenomena is not in doubt; and this is all that is required for a taxonomic schema to be viable (Van Fraassen 1980, p. 16).

Second, it is unnecessary to concede the *normative* issue of whether lack of self-exclusivity would render the positive/normative dichotomy "useless." Though the exclusivity of classificational schemata is indeed desirable (see section 8.1.3), it is false that a lack of self-exclusivity is, by itself, a fatal flaw. Indeed, many useful classificational schemata lack self-exclusivity. For example, most universities evaluate their faculties on the dimensions of teaching, research, and service. Please note that these dimensions are closely related; some would even believe them to be "inseparably intermingled." Research activities often carry over into teaching and service activities. Likewise, teaching activities impact on research and service. Is this a "fatal flaw" of the teaching/research/service trichotomy, making it "useless"? As a second example, because normative economics is not independent of positive economics, should all economic thought be lumped into one side? Certainly, economists think not (Friedman 1953). So it is with the positive/normative dichotomy in marketing. The positive dimensions impact on the normative dimensions. The normative dimensions impact on the positive dimensions. The results are synergistic and thus useful to the discipline of marketing.

1.7.7 Is All of Marketing Thought Normative?

Hyman, Skipper, and Tansey (hereafter "HST") (1991) evaluate the positive/normative dichotomy as flawed because the meanings of "ought" and "should," (my rule of thumb for separating normative from positive) depend on their context. They then offer the following as their own rule of thumb for "normative": "*All normative sentences and only normative sentences offer a reason for action*" (p. 420; italics in original). Using their criterion, they conclude: "[A]lmost all marketing phenomena are normative phenomena, almost all marketing issues are normative issues, almost all marketing theories are normative theories, and almost all marketing answers are normative answers" (p. 420). They base their conclusion on their analyses of the concepts "ownership," "obligation," "rights," "values," and "needs." These terms, as well as "exchange," "price," "purchasing," "advertising," "promotion," "product," "power," and "conflict" (among many others), "are so blatantly normative that little argument is needed" (p. 420). Therefore, "marketing language is so saturated with value-laden terms and marketing theories are so thoroughly imbued with normative claims that no translation into positive language is conceivable" (p. 420). The implications for HST of finding the positive half of the positive/normative dichotomy to be an empty set in marketing are: "[M]ost of our empirical studies in marketing have been conducted on empirically untestable positions" and "the only way to make marketing positive is to start anew" (p. 421).

It is worth noting that HST's position—if true—would have implications beyond their claim that *marketing* is overwhelming normative. Consider the discipline of economics. It unquestionably focuses extensively (obsessively?) on price and its role in exchange relationships and clearing markets. But if price and exchange are irretrievably normative, as concluded by HST, then, no doubt, economics is overwhelming normative and no part of it is a positive science. Likewise, political science relies heavily on the concepts of power and conflict. Yet, these, too, according to HST, are normative. Thus, no positive research is possible in contemporary political science. How about social psychology, sociology, or anthropology? Clearly, according to simple extensions of the logic of HST, all these areas are overwhelmingly normative and no positive, empirical research is conducted in any of these areas. In short, according to HST's logic, all the social sciences are overwhelmingly normative and they all, like marketing, should "start anew." Have HST found the Achilles heel of the social sciences? Must we all "start anew"? Is the sky truly falling on the social sciences?

Although some may warmly applaud and others eagerly embrace the skeptical conclusions of HST (most for reasons other than HST's logic), I do not. Therefore, I will endeavor, following the analysis in Hunt (1991c), to show that their analysis is problematic and neither marketing nor other social sciences need discard all the research that has been conducted in the past few decades—we need not "start anew."

To examine HST's conclusion that all of marketing thought is normative, it will be helpful to work from an example. Consider the case of a marketing researcher running a marketing panel who reviews the purchase diary of Mary Smith, one of the consumers in the panel. After reviewing Mary's purchase diary for a particular day, the researcher notes:

"Mary Smith now owns one box of ten Hostess Twinkies after exchanging $5.00 with her local Safeway Supermarket." Is this statement by the marketing researcher positive or normative? Most marketing researchers and other social scientists would, no doubt, put this statement on the positive side of the positive/normative dichotomy. After all, the statement does not observe that the price of Twinkies was lower at United Supermarket and *prescribe* that Mary *should* have purchased her Twinkies there. It does not *judge* that it was *wrong* for Mary to buy the Twinkies at Safeway based on some criterion, such as a claim that Safeway treats its employees unfairly. It does not claim that Mary *ought* to have purchased the Twinkies at a supermarket evincing proper environmental concern. Nor does it *evaluate* Mary and find her to be a *bad* mother for allowing her children to eat such nutritionally empty products as Hostess Twinkies. The statement makes none of these normative claims. Rather, the statement claims that Mary in fact made a particular purchase at a particular store at a particular price and now owns a particular product. That is, the intent of the statement is to make the positive assertion that the evidence supports the fact that Mary made the purchase. However, HST would claim otherwise. On the basis that "ownership," "price," and "exchange" are "blatantly normative," all statements containing such terms are normative and, therefore, for HST are "forbidden" in positive marketing research. We need to explore how HST could reach such an idiosyncratic view.

What are the criteria for HST's conclusion that ownership, exchange, and price are irretrievably normative? They use their rule of thumb that "all normative sentences and only normative sentences offer a reason for action" in their evaluation of "ownership":

> *Ownership* is a normative term. Ownership can exist only within a normative framework—a web of promises and obligations. Only persons can own property, because only persons have property rights within a value system established by a set of laws. . . . Our proposed rule classifies sentences about ownership as normative, because mere ownership is a reason for acting a certain way (*not stealing,* for instance). (HST 1991, p. 420; italics added)

Thus, HST would contend that "Mary owns a box of Twinkies" is normative because "mere ownership is a reason for acting a certain way." To evaluate their reasoning we should first note that "Mary owns a box of Twinkies" is not, by itself, "a reason for acting a certain way." That is, the ownership of Twinkies does not provide a reason for not stealing—for thieves steal (Twinkies and other items) all the time. Rather, "Mary owns a box of Twinkies" must be conjoined with the values of the observer (it is right or wrong to steal) for the statement to be a reason for acting in a certain way (i.e., stealing or not stealing). In contrast, the normative statement "it is wrong to steal" offers, by *itself,* a reason for acting in a certain way.

The reason why "Mary owns a box of Twinkies" is not by itself a reason for acting is that it is (as previously discussed) a positive statement, not a normative one. The fact that societies set criteria for "ownership" does not imply that all individuals agree with the criteria, or that they agree to be bound by the criteria, or even that they are *aware* of

such criteria. However, if an individual does evaluate the criteria of ownership as *right,* such an individual has a reason for acting.

The situation will be even clearer if we examine a statement that HST themselves classify as positive: "By tonight it ought to be snowing in Chicago" (p. 418). By conjoining this statement with "I hate to fly when it snows," we have "a reason for acting." Thus, by HST's criterion, the *positive* "snowing statement" suddenly is transformed into a *normative* assertion. Indeed, it is difficult to imagine *any* positive assertion that could not become "a reason for acting" (on HST's logic) by someone, somewhere, somehow.

The point of the preceding analysis is not to emphasize that HST's criterion for normativity is even more ambiguous than my original "ought/should" rule of thumb (though it surely is). Rather, the point is to reemphasize HST's "context" insight. Recalling HST's (correct) conclusion that "ought" and "should" can occur in either positive or normative statements depending on context, we note that "own," "exchange," and "price" may likewise appear in either positive or normative assertions. Such terms are not "forbidden" to marketing science because they do not *automatically* make a statement normative. Thus, HST's idiosyncratic conclusion that almost all marketing thought is normative stems in part from the inconsistent application of their own "context" insight. I say "in part" because their highly suspect ontology also contributes to the confusion.

What is the ontology implied by HST's analysis—what, for them, *exists?* HST contend that ownership exists only within a "normative framework," a "web," a "value system," a "set of laws" (p. 420). HST are certainly correct that ownership, exchange, and price exist only within a societal context. Furthermore, the denotation of the terms "ownership," "exchange," and "price" is certainly different from "rock," "Twinkies," and "Mary." But why does this bother HST?

HST do not specifically state their ontology, but they gave clues to it in the draft of their paper that they (graciously) provided me and which was accepted for presentation. On page 9 of that draft they state: "Ownership cannot be a positive term because it cannot be measured by any imaginable tool of science." On the next page they further amplify their ontology by claiming that some concepts are normative because they "cannot be physically detected."

We now are in a position to better understand the second reason why HST reach their idiosyncratic conclusion that all marketing thought is normative. For HST, Hostess Twinkies exist because "Hostess Twinkies" denotes a physical entity that is observable and can be readily measured by simple counting. Likewise, Mary Smith exists because "Mary Smith" denotes a physical entity. But the assertion "Mary Smith owns a box of Twinkies after exchanging $5.00 with Safeway Supermarket" is normative because "ownership," "exchange," and "price" are not tangible, physical entities, but exist only within a societal system ("only persons own property"). It is easy now to see how HST can review the vocabulary of marketing (and, by implication, all the social sciences) and can find almost nothing on the positive half of the positive/normative dichotomy. They have discovered that the social sciences use many concepts that are intangible and unobservable, that is, distinctively *social*—and such concepts violate their physicalist ontology.

As the philosophy of science learned (or should have learned) with the demise of logical positivism, a physicalist ontology, especially for the *social sciences,* results in an impoverished vocabulary that is simply not capable of serving the needs of science or society. As argued in section 9.3, the ontology of scientific realism, with its claim that the long-term success of a scientific theory gives us reason to believe that something like the entities and structure of that theory actually exists, seems to suit better the needs of social science (and much natural science). Entities need not be tangible or observable to exist.

Hyman, Skipper, and Tansey (1991) quite rightly remind us that nothing in science is beyond critical examination. Lakatos (1978) was fundamentally misguided when he claimed that scientific progress required research programs to have a "hard core" that was beyond questioning, beyond critique. Nothing in marketing, nothing in the social sciences, nothing in the physical sciences is, or ought to be, immune from critical discussion. If the Three Dichotomies Model is a "truss in the conceptual structure of marketing" (HST 1991, p. 417), this is all the more reason to subject it periodically to critical examination.

Several conclusions are warranted. First, HST are on firm ground in pointing out that whether a sentence is positive or normative can often be determined only by carefully examining its context. Any rule of thumb, including both theirs and the one that I offered in my original article, can lead one astray. Moreover, they correctly conclude that purportedly positive statements may have a "hidden" normative component. This normative component may be by accident or design. Those who are skilled in rhetoric have long known that their normative views are often more persuasive when disguised as declarative, positive assertions.

Second, HST have not presented credible evidence that the positive/normative dichotomy is so ambiguous as to be useless. Normative statements can be unambiguously separated from positive statements by examining whether the statements, taken in context, make judgments, evaluations, commands, prescriptions, or imperatives. The positive/normative (is/ought) dichotomy has served well the objective of clear thinking for hundreds of years. It, likewise, serves marketing well.

Third, HST's idiosyncratic conclusion that marketing and other social sciences are overwhelmingly normative and should "start anew" (1) stems from the inconsistent application of their own insight that the meanings of words and sentences must be determined within their context, and (2) is grounded on an impoverished physicalist ontology. Such a physicalist ontology should be rejected on the grounds that it yields a vocabulary that would impede, if not thwart entirely, progress in social science.

Finally, the claim that the social sciences are entirely misguided, if not downright pernicious, has become commonplace over the past few decades. However, such cynical and nihilistic views usually come from advocates of deconstructionism, postmodernism, relativism, constructivism, and subjectivism. The positive/normative debate shows that one need not be mesmerized by any of these "isms" into reaching an extremely skeptical conclusion regarding social science. Although all philosophical roads do not lead to skepticism and nihilism, many clearly do—some by intent and others, as in this case, purely by accident.

1.8 THE THREE DICHOTOMIES MODEL AS A GENERAL TAXONOMICAL FRAMEWORK FOR MARKETING

All disciplines have taxonomical frameworks. These frameworks represent a loose consensus among the participants in a discipline concerning the discipline's fundamental nature. These frameworks—which may be explicit or implicit—customarily include the discipline's phenomena to be investigated and general approaches to such investigations. Among other things, the taxonomical framework of a discipline plays a central role in guiding the research efforts of scholars.

The Three Dichotomies Model of marketing seems to be a general framework that helps resolve some of the critical problems in marketing. The framework is (1) properly inclusive, (2) analytically useful, (3) pedagogically sound, and (4) conceptually robust. The model is *inclusive* and healing rather than *exclusive* and divisive. It succeeds in including within the scope of marketing a wide range of perceptions. Those who view marketing with a traditional *managerial* perspective have a "home" in the profit sector/micro/normative cell. Those who prefer a "broadened" perspective have the nonprofit half of the model. Those who wish to have more attention paid to the societal impact of marketing have the macro-positive and macro-normative cells. Finally, those who desire to focus their attention on the science of marketing have the micro-positive and macro-positive cells. As can be noted, the Three Dichotomies Model brings the various perspectives of marketing *together* rather than attempting to *exclude* people by using pejorative statements such as "What you are interested in is not *really* marketing." The model recognizes that marketers are a diverse group with different perspectives, different "homes," and different contributions to make.

The Three Dichotomies Model is *analytically useful.* It has been used to analyze the various approaches to the study of marketing: the functional, commodity, institutional, managerial, systems, and environmental approaches. The model has also been used to explore the "broadening of the concept of marketing" debate. Finally, it has been instrumental in addressing the "Is marketing a science?" controversy.

The Three Dichotomies Model is *pedagogically sound:* it is useful as a teaching device. Students can readily understand the model. They are familiar with the micro/macro dichotomy from economics, and the profit sector/nonprofit sector dichotomy is straightforward. Only the positive/normative dichotomy is outside the average business student's vocabulary and requires substantial elaboration. It is distressing that many beginning business students, both at the bachelor's and master's levels, perceive marketing to be exclusively advertising and personal selling. Salutary effects on students' views of marketing can be generated by demonstrating the broad scope of marketing with the aid of the model.

Finally, the Three Dichotomies Model is *conceptually robust.* The preceding sections have been devoted to analyzing and evaluating various comments and observations concerning specific aspects of the model. The framework has absorbed the blows with surprisingly little damage. The ultimate test of both its conceptual robustness and its desirability as a general taxonomical framework for marketing is *time;* the model's performance (over the decades since it was first introduced) warrants optimism.

1.9 PLAN OF MONOGRAPH

The plan or organization of a monograph is second in importance only to its content. Chapter 2 continues the introductory section by exploring several issues concerning the marketing discipline. First, it considers marketing as (1) a university discipline, (2) an applied discipline, (3) a professional discipline, and (4) a set of responsibilities. Next, it explores the controversy surrounding the issue of how marketing should be defined. Specifically, it reviews the definition approved by the AMA board in 2004 and how dissatisfaction with the 2004 definition led the AMA to approve the following definition in 2007: "Marketing is the activity, set of institutions, and processes for creating, communicating, delivering, and exchanging offerings that have value for customers, clients, partners, and society at large." The chapter concludes with a discussion of the controversy concerning how marketing research should be defined.

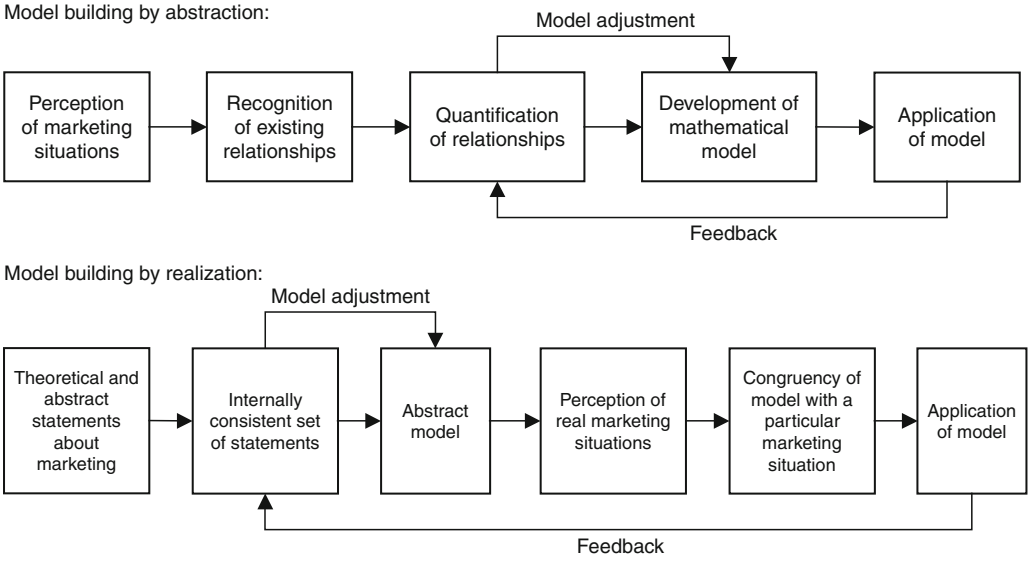
Part 2 of the monograph explores issues concerning the philosophical foundations of marketing theory. Chapter 3 explores the morphology of scientific explanation. That is, how does one go about *explaining* marketing phenomena? Chapter 4 discusses several issues in explanation, such as the relationships among explanation, prediction, retrodiction, and causation. Chapter 5 investigates the nature of laws in marketing and evaluates the four criteria for lawlike generalizations. Chapter 6 delineates the characteristics of the various kinds of laws. Chapter 7 begins by noting some misconceptions concerning the nature of theory. A perspective on theory is presented, and the three major criteria for theories are detailed. Chapter 8 examines the role of classification in theory development, contrasts positive theory with normative theory, explores whether deterministic theory in marketing is possible, and examines how "general theories" differ from other theories.

Part 3 of the monograph addresses several of the most prominent controversies that have been raised in the philosophy debates in marketing. Chapter 9 addresses the question: which philosophical foundation is most appropriate for grounding marketing research? Chapter 10 focuses on two questions: (1) Do the sciences differ from nonsciences in any fundamental ways? (2) Should researchers adopt qualitative methods because positivism is dead? Chapter 11 explores the question: what role, if any, should *truth* play in marketing research? Chapter 12 focuses on the question: can marketing research be objective?

Part 4 of the monograph develops arguments that a theory of competition developed by Robert M. Morgan and me, labeled resource-advantage (R-A) theory, provides the foundations for, that is, it is *toward,* a general theory of marketing. Chapter 13 begins the section by providing a brief overview of R-A theory, then discussing how the R-A research program was developed, and—because the foundational premises of any theory are central to understanding it—reviewing in detail the arguments for each premise of R-A theory. Chapter 14 develops arguments that constitute the first and second grounds for maintaining that resource-advantage theory provides the foundations for a general theory of marketing. Chapter 15 develops arguments that constitute the third ground.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Do the social sciences and marketing require a different methodology than the physical sciences? If yes, how would it differ? If no, why do so many social science researchers claim that their methodology must be different?
2. Lazer (1962) suggests that there are two approaches to model building in marketing—abstraction and realization (see illustration). Are these approaches two different *methodologies?* Do they belong in the context of discovery or in the context of justification? Which is more *deductive?* Which is more i*nductive?* Evaluate the usefulness of models. Which is superior for marketing? Why?



3. Differentiate between *marketing* research and *market* research. Can both result in possible publications for scholarly journals in marketing? Should they? Would marketing practitioners enjoy reading a journal that was exclusively devoted to results of *market* research studies? Would academicians? Is *marketing* research impractical?
4. Many of the articles in the *Journal of Marketing Research* (*JMR*) can be classified as very quantitatively sophisticated profit/micro/normative in content. At the same time, *JMR* is often criticized as being too scientific. Are the preceding two statements contradictory? Is the discipline called management science a *science?* Could it possibly be a science? Is marketing *management* a science? Is accounting a science? *Could* accounting be a science?
5. Table 1.1 purports to be a classificational schema that encompasses the entire scope of marketing. Examine several issues of the *Journal of Marketing* and the *Journal of Marketing Research.* Determine which articles fall into which categories. Can some fall into several categories simultaneously? Conduct the same procedure

for the chapters and major issues in a marketing textbook. Whenever an article, chapter, or issue cannot be classified satisfactorily, propose a modification of the schema to accommodate it.

6. What is the optimal combination of cells and topics in Table 1.1 for a basic course in marketing? A marketing management course? A marketing research course? MBA courses? Ph.D. courses? What were your criteria for optimality?
7. Why is it important to differentiate the *discovery* of theories and laws from the *justification* of theories and laws? Are the processes of discovery different in marketing than they are in the physical sciences? Should they be different?
8. Why is the "is marketing a science?" issue important? Does it have implications for the kind of research that is done in marketing? For how marketing is taught? For the standards for acceptance or rejection of manuscripts by the marketing journals? For marketing practitioners?
9. Why is the "broadening the concept of marketing" issue important? What are the implications of this issue for the areas in Question 8?
10. Grounded theory is described as a field research method by which theory is derived from data that are systematically gathered and analyzed throughout the entire research process (Glaser and Strauss 1967; Strauss and Corbin 1998). That is, the researcher "jointly collects, codes, and analyzes data" (Glaser and Strauss 1967, p. 45), which allows theory to emerge from the data. Theory generation involves an iterative process of generating conceptual categories based on the evidence, comparing those categories to additional data, and circling back to refine the conceptual categories based on comparisons. Glaser and Strauss (1967) advise the researcher to enter the field with a perspective, a focus, and a general question or a problem in mind, but "without any preconceived theory that dictates, prior to the research, 'relevancies' in concepts and hypotheses" (p. 33). However, researchers are advised to become "sufficiently theoretically sensitive" by building familiarity with existing theory. Familiarity with relevant literature before entering the field can serve several purposes, such as enhancing sensitivity to subtle nuances in the data, assisting in formulating questions that act as a starting point during initial interviews, and suggesting areas for productive theoretical sampling. During data analysis, literature can be used again as a secondary source of data for making comparisons with the emerging conceptual categories, to stimulate questions during analysis, and to confirm findings.
    a. Is grounded theory strictly inductive?
    b. What is the difference between being "theoretically sensitive" and approaching a problem with a "preconceived theory"?
    c. Consider the claim that "in doing this research I did not approach the problem with a preconceived theory, even though I admit to being theoretically sensitive." Given that researchers are also humans, is it possible that the claim is disingenuous? Is it possible that the claim is an elaborate self-deception?
    d. Has the grounded theory approach been fruitful? That is, has it resulted in significant and well-confirmed theories?

11. Bartels (1970) proposes that seven axioms comprise his metatheory:
    a. Theory proceeds from a concept of its subject and should be consistent with it.
    b. Theory is built upon basic concepts derived from the concept of the subject and from related disciplines.
    c. By subdivision of basic concepts, their range and qualities may be shown in intraconcept differences.
    d. Concepts in a dependent-independent relationship are the bases of explanation or prediction.
    e. Theory based on presumed relationships is valid to the degree that those relationships have generality.
    f. As theory bears the mark of the marketing theorist, individuality and diversity are normal characteristics of theory.
    g. All theories of a discipline, however diverse, should be embraceable, implicitly or explicitly, in a general theory, either by grouping or synthesis.

    Evaluate this metatheory. Is it a *logic* of discovery? Should it be evaluated entirely in the context of discovery? Which of the statements are normative, and which are positive?
12. Some marketers believe that marketing is an art, not a science. Others have observed that marketing people have been neither good artists nor good scientists. Finally, some have pointed out that truly great scientists have also been great artists, because genius in both science and art emanates from the taking of great care. Evaluate these positions. Be sure to define carefully what you mean by "art," "science," "artist," and "scientist." (Turn to the epigraph at the beginning of Chapter 6 for another point of view.)
13. Some writers believe that a necessary condition for scientific progress is that scientists must ask important questions. This position emphasizes the importance of the logic of discovery because no matter how good our process of validation, if the question is trivial, nothing can save it. Evaluate this position. How does one separate "important" questions from "unimportant" questions? Does current research in consumer behavior ask important questions? Does research in channels of distribution? Can the answers to "trivial" questions sometimes *become* important?
14. "If science is the body of knowledge obtained by methods based on observation, then painting is a science because it is based on observation." Evaluate.
15. Myers, Greyser, and Massy (1979) decry the lack of "problem-oriented" research in marketing. Examine the past two volumes of the *Journal of Marketing* and the *Journal of Marketing Research.* What percentage of the articles were "problem oriented"? What are the factors that influence the kinds of research that academicians will pursue? Which of these factors encourage and which discourage the pursuit of "problem-oriented" research?
16. "Studying the exchange process is the province of economics, not marketing. That [the exchange process] is the whole purpose of supply and demand analysis." Evaluate.

17. In "discovery-oriented" consumer research, Wells (1993) provides five guidelines for improving consumer research:
    (1) "Leave home." That is, move from brand purchase to more important marketing decisions.
    (2) "Forsake mythodology." That is, abandon the views that:
        a. Students are consumers
        b. Lab represents environment
        c. Statistical significance is substantive significance
        d. Correlation equals causation
        e. Limitations mentioned go away
    (3) "Reach out." That is, involve business, government, foundations, and other academic disciplines in research.
    (4) "Start small—stay real." That is, do not reach for abstract universal theories. Ask: "So what?" Be relevant.
    (5) "Research backward." That is, start with how research will be implemented by asking "Who would care if I found X?"

    Which of these lie in the context of discovery? Which lie more in the context of justification? Which of the five do you agree/disagree with? Why?
18. Peter and Olson (1983) titled their article using a metaphorical question: "Is Science Marketing?" They then invite readers to accept the metaphorical conclusion: science is marketing. Consider an alternative title: "Is Science Like Marketing in Many Respects?" Is the alternative title a metaphorical question? If yes, why? If not, why not? Would the alternative title have been as effective for the purposes of the authors? If yes, why? If no, why not?
19. Develop arguments in *favor* of the following position: "Although most writers, including Hunt and Menon (1995), place metaphors exclusively in the context of discovery, metaphors also belong in the context of justification." Now develop *counterarguments.* Which set of arguments/counterarguments is superior? Why?
20. The epigraph at the beginning of this chapter quotes the Greek philosopher Epictetus. What is the relevance of the quotation to the chapter? (Hint: this is the first chapter of the book.)

NOTES

1. As I discuss in Hunt (2001b), the overall philosophy underlying my work embraces (1) the importance of critical discussion in knowledge development, (2) the view that civility in critical discussion is a virtue, (3) a proscription against ad hominem discourse, (4) a proscription against sophistry, (5) a respect for reason and evidence, (6) the view that clarity in scholarship is a virtue, (7) the belief that critical pluralism is desirable, and (8) the view that, of all the "isms" in the philosophy of science, scientific realism seems to make the most sense.

2. These observations of Professor Kotler (1972a) were not included in his published paper.

3. Every generation of marketing students needs to be reminded that hostility to marketing activities and institutions is as ancient as marketing itself. Thus, Plato (1942, p. 271) has Socrates say, "In well-ordered states, [retail traders] are commonly those who are the weakest in bodily strength, and therefore of little use for any other purpose."

4. See: Mindak and Bybee (1971), Farley and Leavitt (1971), Zikmund and Stanton (1971), Carmen (1973), Robin (1974).

5. See: Lazer (1969), Dawson (1971), Aaker (1971), Kangun (1972), Webster (1974), Moyer (1972), Wish and Gamble (1971), Goble and Shaw (1975), Gist (1971), Lazer and Kelly (1973).

6. Hunt (1976a, 1976b) assumed that "causal structures" was implied by "uniformities and regularities." I now make "causal structures" explicit, to avoid ambiguity.

7. See Bergmann (1957), Hempel (1966), Kyburg (1968), Rudner (1966), Salmon (1963).

8. See Bergmann (1957, p. 164), Kyburg (1968, p. 5), Rudner (1966, p. 5), Salmon (1963, p. 10).

9. For an introduction to the use of metaphors in science, in general, and marketing strategy, in particular, see Hunt and Menon (1995).

2

ON THE MARKETING DISCIPLINE

> For here we are not afraid to follow truth wherever it may lead,
> nor to tolerate error so long as reason is left free to combat it.
> —*Thomas Jefferson on the founding of the University of Virginia*

Chapter 1 explored the nature and scope of marketing and the nature of science. It then developed the Three Dichotomies Model as a taxonomical framework to examine both marketing and science. This chapter continues the introductory material, first, by exploring the nature of the marketing *discipline.* The perspective developed here has come to be known as the "responsibilities framework" (Hunt 2007b). Second, it will examine the controversy concerning the definition of marketing. Third, using as an analytical tool the Three Dichotomies Model, it will explore the controversy concerning the definition of marketing *research.*

2.1 ON MARKETING AS . . .

What *kind* of discipline is marketing? Many writers claim that marketing is an applied discipline. Others opine that marketing is a professional discipline. Customarily, writers claiming that marketing is an applied or professional discipline make the further claim that marketing is *not* an academic discipline, and, therefore, marketing is misguided when it focuses on either basic research or research that is not (relatively immediately) useful to the marketing practitioner.

So what kind of discipline is marketing? One answer, and an answer that serves as a good starting point for analysis, would be: "Marketing is a discipline that is housed within colleges or schools of business, which are themselves housed within universities." Recalling Levitt's (1960) classic work on the importance of identifying the business of *firms,* the "university discipline" answer suggests that we need to explore first the "business" of universities. The business of this societal institution, I argue, strongly influences the business of our discipline. Our analysis here will follow closely the approach in Hunt (1992b, 2007b).

2.1.1 . . . A University Discipline

Universities, I suggest, are in the knowledge "business." Specifically, universities warehouse, retail, and manufacture knowledge. Universities store or "warehouse" knowledge

by means of libraries. They disseminate or "retail" knowledge through their teaching function. They produce or "manufacture" knowledge through research. Other institutions are also in the knowledge business, for example, public libraries, elementary and secondary schools, trade schools, corporate training programs, and corporate research and development departments. However, these institutions all differ significantly from universities.

Whereas public libraries focus on the needs of the general public, university libraries focus on the needs of scholars. Whereas trade schools provide skills-oriented training, highly context-specific knowledge, and certificate programs leading to craft occupations, universities provide "higher" education, (generally) context-free knowledge, and degree programs that either (a) constitute a general, liberal education with no specific occupational emphasis or (b) lead to employment in a professional occupation. Similarly, whereas corporate research is primarily oriented toward developing new products and services for the corporation (knowledge for profit's sake and the benefit—at least the proximate benefit—of the corporation), university research is primarily oriented toward basic research that contributes to our inventory of knowledge (knowledge for knowledge's sake and the benefit of society at large).

Surprisingly, many academicians are unaware that university faculty have assumed the role of producers of knowledge only since the early part of the nineteenth century. Although Western universities trace to the founding of institutions such as Oxford and the University of Paris in the thirteenth century, for their first 600 years, university professors were limited to *professing*. That is, they taught the knowledge that was produced by the ancient Greeks, was lost to Europe after the fall of the Western Roman Empire in the sixth century, and was rediscovered (compliments of the Arabs—thank you very much) in the thirteenth century. The professorial scholarship of the university "scholastics" in the Middle Ages did not build on, improve, or extend the ideas found in ancient manuscripts. Rather, by emphasizing hermeneutics, scholarship was restricted to interpreting the works of the ancient Greeks, particularly those of the canonical triumvirate: Aristotle, Ptolemy, and Galen.

The Scientific Revolution (roughly 1550 to 1700 A.D.) was a necessary precursor for the rise of the modern university ("modern" here being identified with the university's role in producing knowledge). Scientists (then called "natural philosophers") in the seventeenth century formed a new method of producing knowledge by conjoining the emphasis on critique, speculation, and reason they found in Plato's method of "critical discussion" with (a) the strong emphasis on careful observation and syllogistic logic that they saw in Aristotle, (b) the powerful tool of mathematics, as had been emphasized in the ancient works of Pythagoras, and (c) the use, where possible, of experimentation to arbitrate disputes, as was being forcefully argued by their contemporary, Galileo. Science and its method developed, it should be stressed, not because of the influence and support of universities, but despite their hostility and vigorous opposition. Universities in the seventeenth century were uniformly sectarian, and science threatened religion's authority.[1]

By demonstrating that new knowledge could be produced by method, rather than revealed by authority, revelation, or mystical experience, science made possible the "grand

compact" underlying the 1810 founding of the University of Berlin. The mission of this first modern university was to focus on graduate education (the Ph.D.) and research. It was to be "the workshop of free scientific research," according to its founders. To Berlin's faculty, the German state granted academic freedom (*akademische Freiheit*), that is, the privilege of conducting research free from the dictates of both church and state. *From* Berlin's faculty, the state demanded a commitment to the ideal of objective knowledge. That is, in exchange for faculty being independent from the pressures of political and religious groups, academic research at Berlin was to be unbiased as to religious and political points of view:

> The German professor . . . made it a rule to refrain from participation in politics for fear it would make an opportunist of him, which in the end would be bound to distort his disinterested pursuit of truth. If he kept his academic skirts clear of political bias, the state was more likely to respect the objectivity of his research. (Brubacher and Rudy 1976, p. 315)

For society, the objectivity of university research would guarantee its trustworthiness; university research could be *relied* on. Within the short span of a few decades, the University of Berlin became extraordinarily prominent. Gradually, the grand compact spread to other German universities, making them world leaders in education, particularly science education. It then spread to other European universities, and, in the latter half of the nineteenth century, it crossed the Atlantic.

At the beginning of the nineteenth century, American universities were patterned after Oxford and Cambridge. Accordingly, they were loose collections of colleges that (a) were sectarian, (b) offered only undergraduate and master's degrees, (c) focused on classical instruction in the humanities, and (d) lacked both academic freedom and a research mission. In the 1800s, scores of American educators traveled to Germany to earn the new, research-oriented Ph.D. degree.[2] They returned to become presidents of many American universities and to promote graduate education, research, and the values underlying the grand compact: academic freedom and objective knowledge. At the same time, the various state legislatures, many strongly encouraged by the Morrill Act of 1862, were chartering state universities. These distinctly American institutions were products of both European influences and the emerging American culture's emphasis on religious freedom, free speech, pragmatism, and egalitarianism. Thus, these universities were nonsectarian, were free or charged only a modest tuition, had a research mission, and had a broad range of undergraduate, graduate, and professional programs—including engineering and agriculture. (In contrast, engineering and agriculture were thought to be improper subjects for university education in Europe and were relegated to the so-called technical institutes—where many reside today.) Because of the powerful strain of pragmatism inherent in American culture, the state universities were to offer instruction and conduct research (as well as to provide service through "extension") in the practical subjects of engineering and agriculture. This paved the way in the next century for another practical subject, business, to be taught at the university level.

By 1900, the system of higher education in the United States was largely in place, and a pluralistic system it was. With over a thousand colleges and universities, public and private, sectarian and nonsectarian, competition was intense. Also by 1900, industrialization and mergers had led to the rise of large corporations, and, with them, a need for professional corporate managers. The old apprenticeship system for business education and training was woefully inadequate for these large, complex institutions. If engineering is appropriate for university education, why not business?

According to the U.S. Department of Education's National Center for Education Statistics, the world's first university-housed business school was established at the University of Pennsylvania in 1881 as the result of a $100,000 grant from a Mr. Joseph Wharton. It was followed by schools at the Universities of Chicago and California in 1898, at Dartmouth, Wisconsin, and New York University in 1900, and at Northwestern, Pittsburgh, and Harvard (the first exclusively graduate business school) in 1908. Growth in business education was slow, but steady, in the first six decades of the twentieth century. By 1965, business schools' 63,000 undergraduate degrees and 6,600 master's degrees represented 12.8 percent and 5.9 percent, respectively, of all such degrees granted in the United States. By 1995, approximately 20 percent and 23 percent of undergraduate and master's degrees, respectively, were awarded to business students. And, by 2005, the figures were approximately 21 percent and 25 percent, respectively.

Why the rapid growth in business education's market share since 1965? The precipitating event was the publication of the "Foundation Reports," authored by Gordon and Howell (1959) and Pierson (1959). These reports found business education, particularly at the undergraduate level, to be a veritable wasteland. Rather than being intellectually demanding, analytically rigorous, science-based, and professional/managerial, these reports found business courses, programs, and faculty to be intellectually shallow, "seat-of-the-pants," anecdotally based, and craft/vocational. Business schools' responses to the "Foundation Reports," as documented by Hugstad (1983), were to raise admissions standards and require (and integrate into business courses) more mathematics, statistics, computer science, economics, and behavioral science. These changes dramatically improved business education, making it more attractive to students and employers.

In short, the steep rise in business education's market share in the 1970s, 1980s, and 1990s resulted, first and foremost, from its producing a superior academic "product." In fairness, however, it must be admitted that the dramatic decline in the standards and quality of education provided by business schools' "competitors" in the late 1960s and 1970s also contributed to the market share gain. The attack on reason, civility, and tolerance in universities in the late 1960s is chronicled by Brubacher and Rudy:

> The most shortsighted limits to academic freedom in this period were posed by university students themselves. Some tried not only to "shut it down" by strikes, arson, and "trashing," but to shout it down as well. By stirring up noisy commotions they disrupted unpopular speakers on university platforms and unpopular professors in their classrooms. Worse yet they forced physical confrontations with the police when summoned to restore order to the campus, thus supplanting reason with force

> in the very citadel of reason. Sometimes motivated by theories of anarchism and nihilism they showed an anti-intellectualism that was anything but conducive to the open mind. (Brubacher and Rudy 1976, p. 328)

Although all areas in universities suffered when riotous students in the 1960s replaced reasoned discussion with violence and intimidation as the preferred method for effecting change, the liberal arts suffered most. For them, the 1960s were an "unmitigated disaster" (Bloom 1987, pp. 320, 357). In the name of "openness," "students' rights," "equality," and "relevance," liberal arts standards deteriorated, grade inflation spiraled out of control, the core curriculum was abolished, studies in pop culture replaced classical instruction, political activism replaced scholarship, and a form of dogmatic skepticism/nihilism replaced truth: "On the portal of the humanities is written in many ways and many tongues, 'there is no truth—at least here'" (Bloom 1987, p. 372).Thus, at the very same time that business schools were becoming more professional, analytical, scholarly, and ideas-oriented, that is, more "liberal" in the classic sense of the word, the liberal arts were embracing a dogmatic illiberalism. As business education was improving, liberal arts education was self-destructing. As business education became more attractive to students, liberal arts became less attractive.

The preceding discussion of the "business" of the university enables us to address: "What kind of business is the marketing discipline in?" We begin with an examination of marketing as an applied discipline.

2.1.2 . . . An Applied Discipline

When marketers claim that marketing is an applied discipline, and, therefore, academics should restrict themselves to applied research (instead of, as they sometimes mockingly put it, "pushing back the frontiers of knowledge"), what kind of research do they have in mind? Consider the following research question: How should the Jones Toy Company allocate its advertising budget among the various media to reach its primary target market, children under twelve years of age? Referred to as "consulting research" when done by academics, answering this kind of research question implies applying existing knowledge to the solution of a firm's specific corporate problem. If "applied" research means consulting research, should marketing academics restrict themselves to these kinds of projects?

There are three reasons, closely related, why marketing academics should not restrict themselves to doing just consulting research. First, because consulting research does not, except by accident, add to new knowledge (in that it just applies existing knowledge), the knowledge base of the discipline would stagnate at its current level. This would, most assuredly, not be in the best interests of marketing students and practitioners (present or future). Second, because marketing is housed within university schools (and colleges) of business, our discipline has an obligation, a duty, to adhere to all three elements of the university's core mission, that is, retailing, warehousing, and producing knowledge. Our responsibility to conduct original research that contributes to the corpus of marketing knowledge stems not—as our discipline's critics derisively contend—from the ego need

of "seeking the respect of our liberal arts colleagues." Our responsibility stems from our duty to respect, abide by, and support the university's core mission, a mission that is of utmost importance to our society.

As a third reason, recall that Day and Wensley (1983) proposed that the marketing discipline should adopt a more strategic orientation and urged the acceptance of a set of research priorities that emphasized marketing's role in the development of corporate, sustainable, competitive advantage. In his decade-later retrospective on marketing's lack of contribution to the "strategy dialogue," Day laments: "Within academic circles, the contribution of marketing . . . to the development, testing, and dissemination of strategy theories and concepts has been marginalized during the past decade" (1992, p. 328). Indeed, he maintains, "Academics outside of marketing pay little attention to marketing literature or theory." Moreover, he believes, "The prognosis for marketing—based on the present trend and past behavior of other disciplines—is not encouraging." The question is: *Why has marketing made so few original contributions to the "strategy dialogue" over the past decade?* Our answer to this question, drawing on the analysis in Hunt (1994b), provides further grounds for questioning the consulting oriented, "applied discipline" view.

As a result of being an author, a reviewer, a section editor, a proceedings editor, and a journal editor, I have read the reviews of well over 1,000 manuscripts for both marketing and nonmarketing journals (such reviews being written by both marketing and nonmarketing referees). These reviews reflect the norms of several disciplines' "gatekeepers." Consistent with nonmarketing reviewers and nonmarketing journals, marketing referees (quite appropriately) want to know the nature and extent of a manuscript's contribution to marketing's literature. However, quite inconsistent with nonmarketing reviewers and nonmarketing journals, marketing reviewers often react quite negatively when a manuscript offers a genuinely original contribution to knowledge. Criticisms such as "where is the precedent?" and "where is the authority?" are, in my experience, disproportionately prominent in reviews by marketing referees. Indeed, marketing authors have been known to cite nonmarketing researchers for authority even when, strictly speaking, the marketing author has made an original, nonmarketing contribution. Marketers making genuinely original contributions to knowledge often do so at their peril.

Why are original contributions to knowledge often punished by marketing journal referees? I suggest that this sorrowful situation has come about, at least in part, as a result of marketers' defining our discipline as an applied discipline. That is, the notion that marketing is an applied discipline implies for many journal reviewers that marketing's "job" is to take concepts, frameworks, and theories from other "more basic" disciplines and then apply them to marketing. Stated succinctly, the norm is "new to marketing, but not new elsewhere." With such a norm, the absence of original contributions to the strategy dialogue (or any other dialogue) is unsurprising. Also unsurprising is the lack of attention that our journals receive from nonmarketing academics. If original contributions to knowledge are systematically screened from our literature, why should others pay attention to our literature?

In summary, marketing should not be viewed as an applied discipline. That is, marketing should not be viewed as being *solely* interested in the kind of market research

questions as that presented in the Jones Toy advertising example. Viewing marketing as an applied discipline contributes to (1) stagnating marketing's knowledge base at its current level, (2) abrogating our discipline's obligation to adhere to all three elements of the university's core mission, that is, retailing, warehousing, and producing knowledge, and (3) marginalizing our discipline by other academic disciplines. In short, viewing marketing as an applied discipline is pernicious. Therefore, many writers contend that, because marketing is a profession, the marketing discipline is a professional discipline, and our responsibility, our only responsibility, is to produce knowledge that *can* be, has the *potential* to be, applied by marketing practitioners.

2.1.3 . . . A Professional Discipline

Myers, Massy, and Greyser (1980) strongly encourage the marketing discipline to focus, not on consulting research, but on "problem-oriented" research, for "if marketing knowledge over the long run is to be considered 'effective,' it should contribute something to improved decision making or other aspects of marketing management practice in the industrial sector" (p. 145). Such research, as discussed in section 1.1, would attempt to find general solutions to general classes of practitioner problems. In Myers's words:

> Marketing is a "professional discipline" and not an "academic discipline." Marketing academicians should recognize that the overall importance of research and knowledge development in this field, over the short-run or long-run, is to improve marketing practice and decision-making, and, in general, to advance the state of knowledge useful to the profession. (Myers, 1979, p. 62)

Peters (1980, p. 5) agrees: "Marketing scholars are beginning to view marketing as a professional discipline as contrasted to an academic discipline." Viewing the discipline this way, he argues, will encourage marketing faculty to do more applied research, consulting, textbook writing, continuing education teaching, and business community service. Giving these activities major credit in tenure and promotion would then help close the "marketing professor—practitioner gap" (p. 4). Likewise, Simon (1994, p. 29) holds that "marketing science is an applied discipline that should help to improve business practice." Similarly, Enis (1986, pp. 2, 3) contends: "We are in the business of providing rigorous, relevant, managerial education. . . . We should see ourselves as professional educators, in league with medical, legal, and engineering educators, preparing and renewing professional managers." Westing puts it this way:

> Our goal should be to try to make business more proficient and efficient—and this in not an unworthy goal. It is similar to the goals of engineering, law, medicine, pharmacy, agriculture, and education. We are all professional disciplines, rather than academic disciplines. . . . We should content ourselves with borrowing from basic disciplines and concentrate on applications which will enable business to do its job better and cheaper. (Westing 1977, p. 16)

> [Marketing is] not a discipline. Economics is the discipline of study in business. . . . So I would predict that if we continue down the road we are going, we would end up having lost the allegiance of our clients—the business people—without gaining the acceptance of our colleagues in the universities. . . . [We should] try to be good professional schools rather than try to be something we aren't and can't be, and that is academic scholars trying to push back the frontiers of knowledge. (Westing 1979, p. 53)

The "marketing is a professional discipline" argument appears to be: (1) there are two kinds of university disciplines, those that are professional and those that are academic; (2) academic disciplines, for example, physics, economics, psychology, and history, conduct basic research that contributes to knowledge; (3) professional disciplines, for example, law, medicine, and engineering, restrict themselves to borrowing knowledge from academic disciplines and applying it for the benefit of their practitioner clients; (4) marketing is a professional discipline, and, therefore, (5) marketing academics should restrict themselves to applying knowledge from academic disciplines such as economics, psychology, anthropology, sociology, mathematics, and statistics for the benefit of their clients, marketing practitioners.

The standard reply to the preceding argument goes something like this: "marketing academics should continue to conduct 'basic research' and not restrict themselves to 'problem-solving' research because the history of science tells us that, even though much research at the time it is conducted appears to be nonrelevant to solving practical problems, it ultimately becomes highly relevant."[3] Although there is merit to the "standard reply," I suggest it suffers because it tacitly accepts most of the premises of the "professional discipline" argument. Some of these premises are highly suspect, and others are demonstrably false.

First, it is simply false that academics in professional disciplines restrict themselves to "applying" knowledge from "academic disciplines." Legal scholars draw somewhat on areas such as philosophy, history, and the social sciences. Yet, it would be ludicrous to maintain that legal scholarship, as reflected in their journals, is accurately described as "applied" philosophy, history, and social science. Rather, legal scholars explore (primarily) the positive question of what kind of legal system *do* we have and the normative question of what kind of legal system *should* we have? Similarly, the engineering disciplines draw heavily on physics, chemistry, mathematics, and statistics. Yet, the engineering scholarship in their journals contains original contributions to engineering knowledge, not just "applied" physics, and the like. Likewise, medical research, as published in journals such as the *New England Journal of Medicine,* goes significantly beyond just being "applied" biochemistry, cell biology, and microbiology—it independently contributes to our knowledge of diseases, their cures, and so forth.

Second, just as marketing practitioners are clients of marketing scholars and their research, lawyers, physicians, and engineers are certainly clients of legal, medical, and engineering scholars. Yet, it is simply false that legal, medical, and engineering research efforts are exclusively (or even primarily) guided by, focused on, or for the benefit of, the interests—

especially the pecuniary interests—of lawyers, physicians, and engineers. To understand why this is the case requires an understanding of what constitutes a professional occupation.

The literature on the nature of professional occupations is enormous.[4] Having the same etymological root, just as professors profess, so do members of professions. What do members of professions claim when they *profess?* Foremost, they profess to have mastered an esoteric body of knowledge based on systematic theory that requires formal, advanced education and is useful in solving certain problems of their clients. By virtue of their professed superior knowledge, professionals can, if they choose to do so, take advantage of their clients. Consequently, the underlying values embodied in the organizational cultures of all professional associations—such values customarily formalized in codes of ethics—emphasize the responsibility of professionals to avoid conflicts of interest in servicing the genuine needs of their clients. Lacking professionals' knowledge, clients must be able to trust them. In exchange for status, authority, and autonomy (enforced by self-regulation), the social contract between society and the professions requires that professionals act in a fiduciary manner toward their clients. It is unsurprising, then, that the AMA's code of ethics indicates that the association is committed "to promoting the highest standard of professional norms and values for its members" (Laczniak and Murphy 2006, p. 174).

The preceding discussion enables us to better understand the nature of research in schools of law, medicine, and engineering. These schools are truly *professional* schools. Just as consumer goods' manufacturers view wholesalers and retailers as intermediate customers for their goods, schools of law, medicine, and engineering view practicing lawyers, physicians, and engineers as "intermediaries." They are intermediate clients for legal, medical, and engineering scholarship. The ultimate client for a truly professional discipline is always society and its needs. For law, it is society's need for a just legal system. For medicine, it is society's need for health care. For engineering, it is society's need for buildings, bridges, highways, and machines that are safe, functional, efficient, and economical. And for marketing? I suggest it is society's need for high-quality products and services that are reasonably priced, responsibly promoted, and conveniently available.

The "marketing is a professional discipline" argument got off to the wrong start with its initial premise that there are two kinds of university disciplines, professional and academic. It is true that (1) some disciplines in the academy are closely related to recognized professions, for example, law and medicine; (2) many disciplines are closely related to vocations aspiring to be professions, for example, marketing, management, finance, social work, and journalism; (3) other disciplines are less closely related to any specific occupation, for example, psychology and chemistry; and (4) still others are almost totally unrelated to any specific occupation, for example, English, history, sociology, and philosophy. Nevertheless, it is equally true that *all* university disciplines are "academic": we are *all* "members of the academy." As such, we are all responsible for all three elements of the university's core mission: retailing, warehousing, and producing knowledge. With regard to knowledge production, we are all responsible for upholding the "grand compact" of academic freedom for objective knowledge.

In 2000, the American Marketing Association launched its Professional Certified Marketer Program, which furthers the professionalization of marketing. Yet, it is still safe to

say that none of the various and remarkably heterogeneous occupations associated with marketing, that is, sales, advertising, brand/product management, marketing research, retail management, wholesale management, distribution management, and marketing management, has reached (or been accorded by society) the status of "profession."[5] Nonetheless, most marketers desire that all marketing practice become more *professional* (just as many of those who believe that no part of marketing is a science nonetheless often see benefits in marketing becoming more *scientific*). On similar grounds, we can conclude that marketing is, or should aspire to be, a professional discipline that is closely related to the occupations constituting marketing practice. However, the *implications of marketing being a professional discipline are almost exactly the opposite of what marketing's critics contend.*

First, as members of the academy, we have a responsibility to respect, uphold, and abide by the university's core mission, that is, retailing, warehousing, and producing knowledge. Second, we must uphold its "grand compact" with society, that is, in exchange for academic freedom we must strive for objective knowledge. Third, as a professional discipline we have a responsibility to keep in mind that society is the ultimate client of the knowledge we produce, and marketing practitioners are intermediate clients. More generally, as recommended by Wilkie and Moore (2003), marketing research on "what kind of marketing systems do we have?" and "what kind of systems should we have?" must be emphasized for us to discharge our responsibilities as members of a professional discipline in the university.

What, then, is a marketing system? Layton (2008, p. 219) provides a useful perspective: "A marketing system is a network of individuals, groups, and/or entities, linked directly or indirectly through sequential or shared participation in voluntary exchange, which jointly creates, assembles, transforms, and makes available assortments of products, services, experiences, and ideas, provided in response to customer demand." For him, marketing can be defined as "the study of (a) marketing systems; (b) the decision processes and choices made by the individuals, groups, and entities engaged in the voluntary exchange of service, and the co-creation of value within a marketing system; (c) the impact and consequences of marketing systems on society; and (d) the impact and consequences of society on marketing systems" (p. 221).

2.1.4 . . . A Set of Responsibilities

With marketing being viewed as a university discipline that aspires to be a professional discipline as a foundation for our analysis, marketing can be viewed from the perspective of "deontological philosophy."[6] In this view, marketing can be considered as a set of responsibilities, duties, or obligations. I start with our ultimate client, society.

To Society

To society, marketing academics owe the pursuit of original contributions to objective knowledge, that is, truth. Society, in exchange for the extraordinary privilege of

academic freedom, has the right to insist on objective, trustworthy knowledge. Is such knowledge possible? (Although "is" does not imply "ought," "ought" does imply "can.") In the 1960s and 1970s, the works of writers such as Hanson (1958), Kuhn (1962), and Feyerabend (1970) implied that, because of the "theory ladenness" of language and perception, objective knowledge was impossible. However, philosophy of science scholarship in the 1980s revealed that, as long as objectivity is not confused with OBJECTIVITY, that is, the objectivity of an infallible "god's-eye view," nor truth confused with TRUTH, that is, infallible truth, objective knowledge is both possible and desirable. Hanson, Kuhn, and Feyerabend (among scores of others) were simply wrong. In brief, because "precepts," as distinguished from "data," are in a very important sense both theory-free and substantially veridical, observations can be used to objectively test theories.[7] Indeed, rather than preventing objectivity, the "theory-informity" of observation enables science to be objective. When scientists and their communities are not objective, they lack the will, not the means. (We return to the subject of objectivity in Chapter 12.)

Also to society, we have a duty to turn out graduates who are technically competent to take their places in their chosen profession, marketing. Technical competence produces productive citizens, and it is the productivity of a society's citizens that determines its standard of living. But technical competence is not enough. We owe to society graduates who are ethically responsible. Our graduates must realize that they have responsibilities not only to themselves and their companies ("ethical egoism") but to other important societal stakeholders.

Finally, we owe society graduates whose education is such that, as citizens, they can identify, understand, reflectively evaluate, implement, and support the core cultural values of our country, as embodied most prominently in the principles underlying its founding documents. Thus, marketing graduates must be liberally educated, not just technically competent and socially responsible.

To Students

To our students, marketing academics owe the kind of education that will prepare them for entry, middle, and upper-level positions in marketing. Moreover, students should understand their society and marketing's role in that society. Therefore, as marketing professionals, they should be capable of recognizing their own responsibilities to society and responding to them. Our students' career responsibilities imply for us the duty to construct programs of instruction that emphasize both the "micro" and "macro" dimensions of marketing. Referring now to the Three Dichotomies Model, the scope of marketing programs should span all eight cells of the scope of marketing, not just the two micro-normative ones.

The kind of education just discussed and advocated places on us a continuing, derivative duty to learn. Staying abreast of the literature, both academic and trade, is obligatory. Translated to the specific courses we teach, the duty to learn implies a responsibility to revise. Sometimes "old yellow notes" contain timeless truths that bear

repeating semester after semester. Regretfully often, however, such notes contain only analyses of yesterday's hot topic, yesterday's faddish issue, or yesterday's solution to yesterday's problem.

Finally, we owe our students an obligation to listen. That is, our clients' expressed needs must serve as input for marketing programs and pedagogy. However, we also have a complementary duty: we must resist the temptation to obey. As professionals, just as physicians cannot allow patients to prescribe their own medicine, we—mindful of our fiduciary relationship with students—must also rely on our best professional judgment as to appropriate marketing programs, courses, and pedagogy.

To Disadvantaged Students: A Special Responsibility

The phrases "land of opportunity" and "American dream" have long been applied to the United States. Indeed, even with our nation's problems, of which there are many, each year millions of people of all races, creeds, and nationalities—many in desperation—seek a new start here. Why does the United States continue to be a nation of immigrants, instead of emigrants?

It is well known that, compared with the rest of the world, the United States has an extraordinarily fluid socioeconomic structure. Studies document not only the numerous anecdotes of "rags to riches," but also those of "riches to rags." For example, the Survey Research Center at the University of Michigan has, since 1968, been continuously tracking the fortunes of a panel, comprised of 5,000 American families and their descendants, that is representative of the total U.S. population (Duncan 1984). Its findings on economic mobility are striking. If we use a seven-year time period as a base, the findings imply that almost half of all families whose incomes put them in the bottom quintile at the beginning of the period will move up at least one quintile. Similarly, *more* than half of those families whose incomes place them in the top quintile in the beginning will move *down* at least one quintile. "Being on top"—unlike in rigidly structured societies—is no guarantee of "staying on top." Moreover, the results concerning intergenerational economic mobility are equally striking. More than half of all children born to families in the lowest quintile will advance at least one quintile above their parents and more than half of all children born to families in the highest quintile will drop at least one quintile (Hill et al. 1985). Neither economic success nor failure is determined by accident of birth. Indeed:

> Income mobility in the top income quintile was as great as it was at the bottom. . . . Family income mobility is pervasive at all income levels. In all, nearly one-quarter (23.1 percent) of the sample moved at least two quintile positions in either direction, about three-eighths (36.8 percent) moved at least one, and only two-fifths (40.1 percent) of the population remained in the same relative income position. . . . Only about two-thirds of the individuals living in families with cash incomes below the poverty line in a given year were still poor in the following year, and only one-third of the poor in a given year were poor for at least eight of the ten prior years. (Duncan 1984, pp. 14, 60)

What does income mobility have to do with business education and marketing? Plenty. It is axiomatic that one cannot ascend any ladder until one is on it. For a society to maximize the opportunity for every person to ascend the socioeconomic ladder, it must first enable people to get *on* the ladder. Business education, particularly undergraduate business education, I suggest, is an important and effective mechanism for many disadvantaged young people to "get on the ladder."

For those young people born to upper-income families, "getting on the ladder"—if they choose to do so—is relatively easy. As they grow up, they naturally acquire the social skills (among them, how to dress, sit, stand, gesture, talk, and groom themselves) commensurate with corporate America. Furthermore, through family and friends they have all the contacts in corporate America necessary for at least starting a prosperous career. Indeed, such fortunate youths often view universities as places where they can "find themselves."

Though undergraduate business programs service the entire spectrum of society, when compared with the liberal arts, a greater share of our students comes from the lower socioeconomic strata. In particular, undergraduate youths from "blueblood" families in the social register seldom major in business. Indeed, undergraduate education in business is not even permitted (needed?) at most prominent, elite, private universities (and a few public universities emulating their private cousins). In short, students from the less affluent portions of our society turn by the hundreds of thousands each year toward undergraduate business education as a vehicle for "getting on the socioeconomic ladder."[8] Thus, business education contributes significantly toward the ideal of equal opportunity for all and the "American dream."

To Practice

To marketing practice, marketing academics owe a continuing supply of technically competent, socially responsible graduates as new entrants to the marketing profession. Also, because "problem-oriented" (Myers, Massy, and Greyser 1980) research makes a legitimate and important kind of contribution to marketing knowledge, a significant portion of our knowledge production efforts should be of this genre. The results of such research should be communicated to marketing practitioners by appropriate means and should also find their way into our lectures, textbooks, and other instructional materials. Similar to our obligation to listen to students, we should seek the input of marketing practitioners about the kinds of problems that our problem-oriented research should address. Indeed, research that claims to be managerially relevant should be managerially *relevant.*

Doing research that educates the public about the social value of marketing activities and marketing institutions is also a responsibility we have to marketing practice. There has never been a book entitled *Does Finance Cost Too Much?* or *Does Management Cost Too Much?* or *Does Accounting Cost Too Much?* As we know, however, there was a famous study that was financed by the Twentieth Century Fund entitled *Does Distribution Cost Too Much?* (Stewart, Dewhurst, and Field 1939). This should tell us something about our discipline and our role in helping the general public understand it.

Even people who customarily evaluate issues in a thinking, logical, and rational manner seem incapable of approaching cognitively, logically, and rationally the subject of marketing, particularly the advertising component of marketing. People seem to put their minds "on hold" at the very mention of marketing. It is worth remembering that L.D.H. Weld (1882–1946), one of the founding fathers of the marketing discipline, was called before a special committee of the Minnesota legislature to explain why he taught what the legislature considered "dangerous doctrines" (Cowan 1960). As we know, he was only pointing out that marketing intermediaries have a positive role to play in the marketing of agricultural products and are not there to exploit anyone, let alone the farmer. We should also be mindful that only since the 1970s (and by very narrow margins in the U.S. Supreme Court) have the courts held that professional associations cannot forbid their members to advertise. For over 100 years, the American Bar Association and the American Medical Association were saying to us, their clients, "trust our members." At the same time, they were claiming that their members could not be trusted to engage in advertising because they would engage in advertising that was misleading and deceptive. Think of the incongruity here. If the American Bar Association and the American Medical Association did not themselves trust their members to be honest *communicators* about their services, why should society trust the members of these associations to be honest *providers* of said services?

The preceding notwithstanding, the marketing discipline should not play the role of professional apologist for products of shoddy quality, for genuinely misleading or deceptive advertising, for collusive pricing, or for coercive practices in distribution channels. To *good* marketing practice, we have a responsibility to research, expose, and publicize *bad* marketing practice. We should be at the forefront in researching questionable marketing practices, not standing at the sidelines.

To the Academy

To the academy as a societal institution, marketing academics have a responsibility for supporting and furthering its core mission of retailing, warehousing, and producing knowledge while upholding the university's implied contract with society, that is, objective knowledge for academic freedom. Therefore, our responsibilities to the academy entail developing the four cells comprising the positive, science "half" of marketing. As developed in Chapter 1, marketing science is the behavioral science that seeks to explain transactions, that is, exchange relationships. It is particularly interested in those exchange relationships involving buyers and sellers.

We also have a duty to adhere to the academy's core values. Paramount among these values are respect for reason, for evidence, and for reason applied to evidence. The fact that all knowledge-producing methods are fallible implies an intellectual openness, a civility toward alternative views. The fact that all genuinely rival, alternative views can be compared and evaluated by reason, evidence, and the application of reason to evidence implies the rejection of the relativistic thesis that, because of "incommensurability," all alternative views are equally good, equally bad, equally trustworthy, equally untrustworthy, equally true, equally false.[9]

2.1.5 Conclusion on the Nature of the Marketing Discipline

So, what kind of discipline is marketing? The responsibilities framework implies that marketing is a university discipline that aspires to be a professional discipline and that, accordingly, has responsibilities (a) to society, for providing objective knowledge and technically competent, socially responsible, liberally educated graduates, (b) to students, for providing an education that will enable them to get on and move up the socioeconomic ladder and prepare them for their roles as competent, responsible marketers and citizens, (c) to marketing practice, for providing a continuing supply of competent, responsible entrants to the marketing profession and for providing new knowledge about both the micro and macro dimensions of marketing, and (d) to the academy, for upholding its mission of retailing, warehousing, and producing knowledge, its contract with society of objective knowledge for academic freedom, and its core values of reason, evidence, openness, and civility.

2.2 THE DEFINING MARKETING CONTROVERSY

How to define marketing has proved to be a contentious issue for decades. As discussed in Chapter 1, in 1985 the American Marketing Association (AMA) adopted the following definition: "Marketing is the process of planning and executing the conception, pricing, promotion, and distribution of ideas, goods, and services to create exchanges that satisfy individual and organizational objectives." In 2004, the AMA adopted a revised definition: "Marketing is an organizational function and a set of processes for creating, communicating, and delivering value to customers and for managing customer relationships in ways that benefit the organization and its stakeholders." The 2004 definition proved to be quite controversial. Here, we briefly explore the extent to which the 2004 definition reflects the view that marketing practice should be viewed as a profession and the marketing discipline be considered a professional discipline. We then examine the replacement definition adopted by the AMA in 2007.[10]

At the outset, note that definitions are "rules of replacement" (Hempel 1970, p. 654). That is, a definition means that a word or group of words (the definiens) is proposed to be truth-functionally equivalent to the word being defined (the definiendum). *Good* definitions exhibit inclusivity, exclusivity, differentiability, clarity, communicability, consistency, and parsimony. That is, good definitions should (1) include all phenomena that should be "taken in," (2) exclude all phenomena that should be left out, (3) differentiate the definiendum from other (often closely related) terms, (4) clearly define the term, (5) communicate well the term's meaning to its intended audience, (6) be consistent with the meanings of other important terms, and (7) be no longer than necessary to accomplish criteria 1–6.

To many marketers, the 2004 AMA definition of marketing was sorely deficient. The 2004 definition lacked inclusivity because it seemed not to include the exchanges involved in marketing systems, such as channels of distribution. It also did not include mention of marketing institutions, such as retailers and wholesalers. As to exclusivity, the phrase

"delivering value to customers" appeared to exclude, inappropriately, the activities of all those organizations that do not have *customers,* for example, charities, the military, and political parties. As to clarity, the expression "creating, communicating, and delivering value" seemed unclear because, though organizations do create ideas, goods, and services, these market offerings have *value* only when participants in exchanges *perceive* them to have value. As to communicability, the phrase "managing customer relationships" in the 2004 definition appeared to communicate inappropriately the view that one kind of marketing strategy (i.e., relationship marketing strategy) is so important that it is a part of how the concept of marketing is *defined.*

If marketing practice is to be viewed as a profession, if marketing should be taught and researched in the manner of a professional discipline, and if the AMA is meant to be considered a professional association, then the 2004 definition is clearly deficient and was seen so by many marketers. Indeed, several special sessions at various AMA conferences were held to discuss the deficiencies of the 2004 AMA definition. These sessions led to a special section of the Fall 2007 *Journal of Public Policy and Marketing* (*JPP&M*) organized by Gregory Gundlach, which was devoted exclusively to issues in defining marketing.[11] In his contribution to the special section, Robert Lusch (2007, p. 267), who chaired the committee that recommended the 2004 definition, concluded that he did "not believe that the revised [2004] AMA definition is comprehensive enough" and, he maintained, "My sense is that if we get everything else 'right' but fail to develop a coherent and compelling body of thought regarding the aggregate marketing system, we will have failed society and ourselves as a profession." Responding to the controversy, the AMA appointed a committee to revisit the issue of how marketing should be defined.

2.2.1 On the 2007 Definition of Marketing

In early 2007, an American Marketing Association committee chaired by Donald Lehmann began reexamining the issue of how marketing should be defined.[12] Over a period of several months, the committee sought input on the definition issue from a wide range of marketing practitioners and academics, conducted two e-mail surveys of AMA members, and discussed numerous options and suggestions for revising the current definition. The final e-mail survey by the committee found that, of the 1,000+ members responding, the overwhelming majority favored a version of the committee's revised definition over the 2004 definition. After extensive deliberation, the committee unanimously recommended the following definition to the AMA Board of Directors: "Marketing is the activity, set of institutions, and processes for creating, communicating, delivering, and exchanging offerings that have value for customers, clients, partners, and society at large." The AMA Board adopted the committee's recommendation in the fall of 2007.

The 2007 definition is a positive step toward viewing marketing as a profession: it can contribute to the goal of practicing, teaching, and researching marketing in a professional manner. First, consider the phrase "marketing is the activity." This phrase, which substitutes for "marketing is the organizational function," recognizes that marketing is an action word. Specifically, "marketing" is a gerund, a verbal form that ends in *–ing* that is

used as a noun, but conveys the meaning of the verb, "to market." That is, marketing is something that organizations as well as individuals (e.g., entrepreneurs and consumers) engage in or *do*. Because the definition is put forth by the AMA, a professional association (and consistent with the responsibilities framework), it should be stressed that the definition implies that the activity of marketing is to be conducted ethically and responsibly. It is also important to stress that, with the deletion of "marketing is the organizational function," marketing is not something that is done just by those in organizations' formal marketing departments. Rather, modern marketing practice and theory views marketing, among other things, as an orientation that is recommended as a guiding philosophy for the entire organization and its employees.

Second, consider the phrase "set of institutions and processes." This phrase uses "institutions" in two ways: (1) "institutions" refers to types of organizations that engage in marketing, for example, manufacturers, wholesalers, retailers, advertising agencies, distributors, and marketing research firms; (2) "institutions" refers to the formal and informal norms that guide, inform, and regulate ethical, responsible, and legal marketing. For example, "treat suppliers fairly," "do not engage in deceptive advertising," and "do not, on penalty of the law, conspire to fix prices" are all societal institutions. The phrase "institutions and processes" also implies that the marketing systems in a society (e.g., channels of distribution, networks, and supply chains) are central to the practice and study of marketing. Understanding, developing, and maintaining marketing systems are important tasks for efficient/effective marketing practice; understanding, analyzing, and theorizing about marketing systems are important—yet now often neglected (Wilkie and Moore 2003)—research areas for marketing academic research.

Third, consider the phrase "for creating, . . . offerings." This phrase stresses that it is market *offerings* that are being exchanged, not just tangible goods (Vargo and Lusch 2004). Drawing on resource-advantage theory, "a market offering is a distinct entity that (1) is composed of a bundle of attributes, which (2) may be tangible or intangible, objective or subjective, and which (3) may be viewed by some buyer(s) as a want satisfier" (Hunt 2000b, p. 54). Therefore, the phrase "for creating, communicating, delivering, and exchanging offerings" delineates four central components of marketing activity: (1) "Creating" means developing new (both radically new and incrementally new) market offerings, often as a result of marketing research that explores consumers' needs, wants, tastes, and preferences. (2) "Communicating" implies the use of advertising, personal selling, sales promotion, and other vehicles to inform potential customers and clients about the availability and attributes of market offerings. (3) "Delivering" means the process of moving market offerings and their ownership from production to consumption, a process that often involves using intermediaries. (4) "Exchanging" market offerings captures the historical focus of the purpose of marketing and includes discrete, individual, or "transactional" exchanges and long-term, multiple, or "relational" exchanges. It is important to stress that *exchanging* implies that the parties find mutually agreed-upon terms of exchange, including the prices of each of the components of the market offerings (e.g., the prices of the tangible goods, warranties, and delivery).

Fourth, the phrase "offerings that have value for customers, clients, partners" identifies

three key constituencies or stakeholders of marketing. (1) In the case of for-profit organizations, the constituency of "customers" is paramount. Indeed, it is customers' perceptions of rivals' market offerings that determine their relative value. As R-A theory puts it, for determining relative value, "consumer perceptions are dispositive" (Hunt 2000b, p. 54). (2) In the case of nonprofit institutions (e.g., United Way and the Girl Scouts), though these organizations engage in marketing, they do not see themselves as having "customers." Rather, they normally see themselves as having *clients* (or members and donors) for whom services are provided (and from whom donations are solicited). Again, it is nonprofit organizations' clients whose perceptions determine the relative value of the organizations' market offerings. (3) The inclusion of "partners" recognizes that the importance of collaborative networks and alliances in modern marketing practice. All partners in such arrangements must perceive the value to be obtained from participating in the *partnerships.*

Fifth, the phrase "offerings that have value for . . . society at large" acknowledges that, as discussed in the responsibilities framework, society is always the ultimate client for a truly professional discipline. Recall that the framework maintains that marketing must be responsive to society's need for high-quality products and services that are reasonably priced, responsibly promoted, and conveniently available. The definition, therefore, acknowledges that the ethical and responsible practice of marketing benefits society—and the normal, everyday practice of marketing, I argue, is ethical and responsible. That is, creating market offerings that have value to customers and clients benefits society. Communications about, and the delivery of, market offerings to customers and clients benefits society. Exchanging market offerings benefits society. Therefore, *marketing* benefits society.

But the practice of marketing benefits society in a way that is seldom recognized within the marketing academic community: marketing benefits society because it contributes to increases in productivity and economic growth. Over the past fifty years there has been a revolutionary change in economics about the question of what causes economic growth. It is now (almost) universally acknowledged that the primary cause of economic growth is firm-produced, competition-prompted innovations that increase productivity (i.e., increases in efficiency and effectiveness) (Grossman and Helpman 1994; Romer 1994; Solow 1994).[13] Therefore, the normal, everyday practice of marketing—creating, communicating, delivering, and exchanging market offerings that have value to customers—promotes firm-level innovations that, in turn, increase firm-level productivity that, again in turn, increase industry-level productivity that, also in turn, provide the increases in societal-level productivity that produce economic growth. Therefore, marketing benefits society by contributing to economic growth. Marketing practitioners do not start out with the objective of contributing to economic growth—it just works out that way.

In conclusion, the 2007 AMA-adopted definition of marketing *appropriately:* (1) acknowledges (a) the activity of marketing, (b) the institutions of marketing, and (c) the processes in marketing, (2) recognizes the role of marketing in (a) creating, (b) communicating, (c) distributing, and (d) exchanging market offerings, and (3) points out how the normal, everyday, ethical, and responsible practice of marketing benefits (a) customers,

(b) clients, (c) partners, and (d) society. As such, the definition has much to recommend it, especially for any marketing practitioner or academic who views marketing practice as a profession and marketing academe as a professional discipline.

2.3 THE DEFINING MARKETING RESEARCH CONTROVERSY

What is the nature of marketing research? Historically, the American Marketing Association defined marketing research as the "systematic gathering, recording, and analyzing of data about problems relating to the marketing of goods and services." In 1987 the Board of Directors of the AMA approved a revised definition, which was reaffirmed (after correcting five grammatical/punctuation errors) by the Board in 2004. This definition is:

> Marketing research is the function that links the consumer, customer, and public to the marketer through information—information used to identify and define marketing opportunities and problems; generate, refine, and evaluate marketing actions; monitor marketing performance; and improve understanding of marketing as a process. Marketing research specifies the information required to address these issues, designs the method for collecting the information, manages and implements the data collection process, analyzes the results, and communicates the findings and their implications.

The purpose of this section is to explore the *inclusivity* of the AMA definition of marketing research. That is, to what extent does the definition include all the activities and phenomena that marketers would *want* to be labeled "marketing research"? In doing so, we shall generate six prototypical research questions in marketing, examine the characteristics of these research questions, and inquire whether research conducted about these questions would be properly called "marketing research" under the AMA's definition. It is hoped that this procedure will shed light on whether the definition is properly inclusive.

2.3.1 Research Questions in Marketing

Table 2.1 displays six prototypical research questions that researchers commonly explore when they contend they are doing "marketing research." Although each question examines the same substantive area (advertising), the kinds of research projects designed to explore these questions will vary greatly. The table categorizes each research question according to the Three Dichotomies Model and (1) identifies whether such research is primarily conducted by practitioners or academicians, (2) indicates whether such research would be publishable, (3) states the proximate (immediate) purpose or objective of the research, (4) shows the ultimate potential value or consequences (intended or unintended) of the research, and (5) indicates whether the research would be considered "marketing research" under the AMA definition.

Table 2.1

Prototypical Research Questions in Marketing

Research question	Category[a]	Primary researchers[b]	Publication of knowledge[c]	Proximate purpose[d]	Ultimate potential value	Consistency with AMA definition
1. How should the Jones Toy Co. allocate its advertising budget among the various media to reach its primary target market, children under twelve years of age?	Profit/micro/ normative	Practitioners	No	Better decisions for a firm	Firm productivity Societal productivity	Yes
2. How should firms *in general* allocate their advertising budgets among the various media in an optimal fashion?	Profit/micro/ normative	Academicians Consulting firms	Yes	Better decisions for firms in general	Firm productivity Societal productivity	No
3. To what extent does television advertising in general shape children's beliefs about products and consumption?	Profit/micro/ positive or Profit/macro/ positive	Academicians	Yes	K.f.K.S.[e]	Firm productivity Societal productivity Better public policy Better informed citizenry	No
4. To what extent is television's shaping of children's beliefs about products and consumption injurious to society?	Profit/macro/ normative	Academicians	Yes	K.f.K.S.[e]	Socially responsible firms Better public policy Better informed citizenry	No
5. To what extent should the federal government restrict or regulate the amount or content of advertising directed at children?	Profit/macro/ normative	Academicians	Yes	Better public policy	Better public policy	No
6. What are the best research methods to explore questions one through five?	Not applicable	Academicians	Yes	Better research	Firm productivity Societal productivity Socially responsible firms Better public policy Better informed citizenry	No

Source: Adapted from Hunt (1987). Reprinted by permission of the American Marketing Association.

[a]Using the "Three Dichotomies Model."

[b]Emphasis on "primary." That is, it is recognized that on occasion practitioners, consulting firms, and academicians conduct all kinds of research.

[c]The immediate objective of the inquiry.

[d]In scholarly journals such as the *Journal of Marketing,* the *Journal of Marketing Research,* and the *Journal of the Academy of Marketing Science.*

[e]"Knowledge for Knowledge's Sake."

Research Question 1

The first research question was used as an example in section 2.1.2. It asks: "How should the Jones Toy Co. allocate its advertising budget among the various media to reach its primary target market, children under twelve years of age?" A research project addressing this question would be profit/micro/normative, because it takes the perspective of an individual firm and attempts to provide specific guidance for its marketing problem. Academicians in their consulting activities sometimes develop research projects to answer this kind of question. However, the overwhelming majority of these projects are done by practitioners, be they "in-house" marketing researchers or those in marketing research agencies. This kind of project is labeled "market research" in section 1.1, called "problem-solving" research by Myers, Massy, and Greyser (1980), and referred to as "applied" research by numerous others.

The results of a research project directed at Question 1 would in general not be publishable in journals such as the *Journal of Marketing* (*JM*) or the *Journal of Marketing Research* (*JMR*). These journals have adopted the value system of scholarly journals, which requires manuscripts to make some new contribution to marketing knowledge that is *generalizable* to some extent. Therefore, research projects that simply *apply* existing marketing knowledge to the solution of a firm's problem would be unacceptable.

In researching Question 1, the immediate objective of the researcher is to help the manager make a better decision, thereby increasing the firm's productivity. That is, better decisions increase the firm's efficiency and/or effectiveness, and, as a consequence, increase productivity in the sense of resource-advantage theory (Hunt 2000c; Hunt and Morgan 1995, 1996, 1997). However, the consequences do not necessarily stop there. Society also benefits—as discussed in section 2.2.1—because increases in individual firm productivity can lead to greater efficiency/effectiveness for society as a whole.

A research project designed to explore Question 1 would obviously fit comfortably within the AMA definition. This is the type of research that is routinely and regularly conducted by corporate marketing research departments and marketing research agencies. What might surprise some people is the fact that this is precisely the kind of marketing research project that is most often conducted by marketing academicians.

There are no accurate figures on the total number of marketing professors, even in the United States, let alone the world. Nevertheless, given that there are approximately 1,300 four-year institutions offering bachelor's degrees in business in the United States and about 700 such institutions offering a major in marketing, an estimate of 5,000 marketing academicians in four-year institutions would not seem unreasonable (*Peterson's Annual Guide to Four-Year Colleges 2000*). Again, although no "hard" numbers are available, probably 90 percent of the 5,000 marketing academicians do almost *exclusively* these kinds of "consulting" research projects. Many marketers, both practitioners and academicians, seem to believe that the remaining 10 percent should *also* focus exclusively on consulting research. Put another way, to what extent should *any* marketing academician explore research questions similar to Questions 2 through 6, and to what extent should such projects be considered "marketing research"?

Research Question 2

The second research question asks: "How should firms in general allocate their advertising budgets among the various media in an optimal fashion?" Like research Question 1, Question 2 is also profit/micro/normative. However, Question 2 calls for research that attempts to generate a procedure or model to solve a particular class of marketing problem. The procedure or method would presumably be applicable across many firms in many different contexts. Such context-free research is conducted primarily by academicians and consulting firms. To the extent that practitioners conduct these kinds of projects, the results of such studies are generally held to be proprietary in nature and not disseminated to the larger marketing community. Consulting firms also tend to keep such studies proprietary.

Unlike research projects directed toward Question 1 that focus on a particular firm, projects directed at Question 2 attempt to improve the decision making of firms *in general* in a particular decision area. Like Question 1, the ultimate consequences are higher levels of productivity for both firms and society. Given that the procedure or model developed by the researcher makes a significant "enough" contribution to marketing knowledge and is perceived to be generalizable across contexts, the results of such a project would be potentially publishable in journals such as *JM, JMR,* and the *Journal of the Academy of Marketing Science* (*JAMS*).

Research projects directed at addressing questions such as Question 2 are what Myers, Massy, and Greyser (1980) call "problem-oriented" research. Consistent with the views of McAlister (2005) and Webster (2005) and the objectives of the Marketing Science Institute, they recommend that marketing academicians focus more of their attention on these kinds of research projects. On the other hand, writers such as Anderson (1983), Arndt (1985), Hirschman (1987), Holbrook (2005), and many others (especially in the consumer behavior area of marketing) believe that academic marketing researchers spend too much of their time working on "problem-oriented" research and that too much journal space is devoted to reporting the results of such projects.[14] Curiously, the proposed AMA definition of marketing research would seem to exclude research projects directed at these kinds of questions from being considered "marketing" research. This anomaly would appear to be either an error of interpretation on my part or a gross oversight on the part of the AMA Board. (Surely, the Board did not want to exclude "problem-oriented" research.)

Research Question 3

The third research question asks: "To what extent does television advertising in general shape children's beliefs about products and consumption?" Research projects directed at this question would be either profit/micro/positive or profit/macro/positive, depending on the nature of the specific research design. Such projects are almost exclusively the province of academicians and would be considered "basic" marketing research by Myers, Massy, and Greyser (1980) or "pure" consumer research by Holbrook (1986). The results of such research projects would be potentially acceptable for publication in *JM, JMR,*

JAMS, the *Journal of Macromarketing,* the *Journal of Public Policy and Marketing,* and the *Journal of Consumer Research.*

The proximate purpose of such "basic" research projects is "knowledge for knowledge's sake." Ultimately, the results of such projects might be useful in guiding decision makers in their efforts to determine the "best" solutions to research questions like 1 and 2, thus impacting on firm and societal productivity. Likewise, such research efforts might be useful to government officials in their efforts to develop better public policy. (Please note that better public policy may also be in the best interests of firms.) Finally, the results of such research projects may simply result in a better informed citizenry.

Marketing practitioners routinely decry the emphasis on basic research projects (like number 3) by marketing academicians, and this negative attitude toward such research may be responsible for the apparent exclusion of these research projects from the official AMA definition. Many marketing academicians share practitioners' disdain for any research that does not have a predictable, observable, relatively direct benefit to marketing management (Enis 1986; Parasuraman 1982; Peters 1980; Westing 1979). For example, Myers, Massy, and Greyser (1980) propose that "although much basic research in marketing is generated for 'its own sake,' the Commission's view was that if marketing knowledge over the long run is to be considered 'effective,' it should contribute something to improved decision making or other aspects of marketing management practice in the industrial sector" (p. 145). Similarly, Parasuraman contends that "the *raison d'etre* for any marketing theory is its potential application in marketing practice" (1982, p. 78).

In contrast, consistent with the view articulated in section 1.7.5, there have always been academicians and practitioners advocating the legitimacy and desirability of basic research in marketing. The *Journal of Marketing* in its first few decades published *primarily* basic research. In fact, the very first article in the very first issue of *JM* examined whether the interests of the consumer were being well served by the Agricultural Adjustment Administration (Anderson 1936). Decades ago, Levy called for splitting the "basic" side of marketing from its "applied" side, referring to the former as "marcology" (1976). Similarly, Anderson (1983, p. 27) has called for more basic research, pointing out that "it is clear that marketing must be more concerned with the pursuit of knowledge as knowledge."

Most interestingly, when the AMA Board adopted a definition of marketing research that excluded basic research efforts, an AMA committee was specifically recognizing the legitimacy of such research. The special task force on the development of marketing thought noted that research in marketing has many "clients" and identified the following five "audiences of marketing knowledge" (Monroe et al. 1986, p. 8):

1. managers of enterprises ("practitioners")
2. educators/teachers, scholars, and students
3. public policymakers
4. special interest groups (includes hostile groups, such as consumer interest groups, as well as supportive groups)
5. consumers (all of us)

As with many large institutions, the "left hand" seems oblivious or indifferent to the "right."

Research Question 4

The fourth research question asks: "To what extent is television's shaping of children's beliefs about products and consumption injurious to society?" Research projects directed at this question would be profit/macro/normative, conducted almost entirely by academicians, and publishable in journals such as *JM, JAMS,* the *Journal of Macromarketing,* and the *Journal of Public Policy & Marketing.*

Research projects directed at answering this question are pursued "for the sake of knowledge." Some commentators contend (or their works tend to imply) that the knowledge generated by such a project should not influence marketing management decisions (Friedman 1970; Levitt 1958). Others believe that such information should be used by "socially responsible" managers (Gray 1968; Morell 1956). In any respect, such knowledge might result in better public policy and, of course, a better informed citizenry. Although "better public policy" would seem to be in the best interests of *all* marketing practitioners, and socially responsible decisions are definitely an objective of at least *some* (Wood, Chonko, and Hunt 1986), this kind of research project would, again, not be "marketing" research under the new definition.

Research Question 5

The fifth research question asks: "To what extent should the federal government restrict or regulate the amount or content of advertising directed at children?" Efforts directed at answering this question would be categorized as profit/macro/normative, conducted almost exclusively by academicians, and potentially publishable in *JM, JAMS,* the *Journal of Macromarketing,* and the *Journal of Public Policy and Marketing.*

The proximate purpose of research conducted here would be the same as its ultimate value: better public policy. Please note that such research efforts could be significantly informed by the results of projects designed to answer Questions 3 and 4. Also, note that this kind of research would not be, officially, "marketing research" according to the definition.

Research Question 6

The sixth research question asks: "What are the best research methods to explore Questions 1 through 5?" Research directed at this question cannot be classified within the Three Dichotomies Model because the model focuses on substantive rather than methodological issues. Research efforts dealing with methodological issues on a fundamental or philosophical level (such as scientific realism vs. relativism) would potentially be publishable in the *JM* and *JAMS.* Similarly, research efforts on research techniques (such as factor analysis, multidimensional scaling, structural equation modeling, etc.) would potentially be publish-

able in *JMR.* Historically, the *Journal of Marketing Research* has devoted a high proportion of its pages to the development of better marketing research techniques. The emphasis on what is considered by many to be "esoteric" research techniques has been criticized by practitioners and academicians. For example, Arndt (1985) claims that our discipline suffers from "instrumentitis" as a result of undue emphasis on research techniques.

The proximate purpose of methodological research efforts is better research on *all* marketing research problems. To the extent that better methods are used in the conduct of marketing research, all the clients of marketing knowledge are better served. Nevertheless, research on methodological issues would appear not to be "marketing" research as per the definition.

2.3.2 Conclusion on the Nature of Marketing Research

Is the AMA definition of marketing research *properly* inclusive? Quite obviously, the definition specifically addresses only one of the preceding prototypical marketing research questions, that is, research directed at solving a specific firm's marketing problem. Research directed at providing new knowledge for solving general *classes* of marketing management problems, or for providing *basic* knowledge that might be *ultimately* useful in solving marketing management problems, or for informing public-policy decisions, or for addressing the interests of society in having a well-informed citizenry do not seem to find a "home" within the new definition.

The preceding notwithstanding, there is an argument that the AMA definition is in fact properly inclusive. It can be argued that the definition was never meant by the AMA Board of Directors to be a good definition of marketing research in *all* its aspects. Rather, the purpose of the definition was (perhaps for them) to articulate for students and the public at large what *practitioner* marketing researchers actually do (or ought to do) in corporate and agency research organizations. This is obviously the case when words like "the function" are used in the definition. Recall that good definitions exhibit not just inclusivity, but communicability as well. The definition, it may be argued, communicates well with its intended audience.

Rather than changing the *definiens* to make it include a broader array of research issues under the rubric of marketing research, it would be simpler and just as appropriate to change the *definiendum.* That is, rather than change the *definition,* one could delimit the construct being *defined.* We could clearly state that we are defining marketing research of the "problem-solving" or "applied" variety. Changing the first part of the first sentence in the definition to read: "Marketing research is the function or activity *within the firm* that links the consumer . . ." would succinctly accomplish this objective.

Definitional issues aside, our discussion explicitly and implicitly raises numerous fundamental questions about research in the marketing academic discipline. To what extent should marketing academicians focus exclusively on consulting research (should the 10 percent join the 90 percent)? To what extent should more marketing academicians be encouraged to conduct scholarly research (should the 90 percent be encouraged to join the 10 percent)? What are the institutional mechanisms that encourage/discourage research

of a consulting/scholarly nature? Should specific institutional mechanisms be developed to encourage/discourage more research of a consulting/scholarly nature?

The view here is that marketing academicians should not do just consulting research—the 10 percent should not join the 90 percent. Rather, we should encourage more scholarly research—the 90 percent should be encouraged to become more professionally active in scholarly activities. There are numerous institutional mechanisms that tend to encourage consulting research and discourage scholarly research. Furthermore, changes in these institutional mechanisms would be desirable from the perspective of both marketing practitioners and academicians alike. The myopic view of the American Marketing Association's definition of marketing research represents a small, but potentially significant, institutional mechanism discouraging scholarly marketing research. As discussed in section 2.2.1, the AMA's 2007 definition of marketing recognizes that "marketing" does not equate with "marketing management." Likewise, the AMA's definition of "marketing research" should not equate with "the marketing research conducted by a firm's marketing research department" or "the marketing research conducted by a marketing research firm." It should not, but it does.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Westing (in Ferrell, Brown, and Lamb 1979) claims that marketing academia is "moving farther and farther away from our clients who are the businessmen of the country." What is a "client"? Is marketing academia "moving farther away"? Westing claims that marketing is a professional discipline like medicine. Are the "clients" of medical schools the practicing physicians or people who are sick? Does marketing have any *other* clients? Who are they, and how would these clients influence the nature of the market discipline? Is society in general a client?
2. Westing (in Ferrell, Brown, and Lamb 1979) claims that "we are not a discipline. Economics is the discipline of study in business." Further, he observes that economics is the "mother science of marketing." He then proposes that the application of economics to marketing has been largely unsuccessful because "economics has never paid much attention to demand creation" and the theory of pricing was not "much of a contribution to the field." Evaluate the consistency of these views. What does the concept "mother science" imply?
3. Many writers claim that "marketing is a professional discipline, not an academic discipline." Others refer to marketing as an "applied" rather than a "basic" discipline. Are these positive or normative statements? Differentiate among these kinds of disciplines. What are the consequences for marketing teaching and research if it is a professional or academic or applied or basic discipline? What is the nature of the marketing discipline? What should it be?
4. A colleague once observed:

   > Economics, not marketing, is the professional discipline because economics is much more of a profession than marketing. After all, there are far more

> professional economists than professional marketers, and economics has far more professional status.

Evaluate.

5. Peters (in Ferrell, Brown, and Lamb 1979) suggests that the "fast-track research schools are influencing the other types of schools so that everyone now, or almost everyone, is setting up 'publish or perish' requirements." Do you agree? Estimate the total number of colleges and universities that teach marketing. What percentage of faculty do you think really "publish or perish"? Estimate the number of faculty that teach marketing. Now estimate the percentage of faculty who have published one or more journal articles in *JM, JAMS, JMR,* or *JCR* in the past three years. Does this suggest that there must be a whole lot of perishing going on?
6. Peters (in Ferrell, Brown, and Lamb 1979) indicates that his school gives "credit for unpublished, applied research studies if they truly make a contribution to the applied area of the discipline." What is an "applied research study"? If such a study is unpublished, how could one decide whether it makes a contribution or not? What does "make a contribution" mean?
7. Myers, Greyser, and Massy (1979) believe that:

   > The objectives of knowledge generation in our field should be to improve marketing management practice. Thus, even basic research if it is to be considered "effective" should, over the long run, contribute something to improved decision-making or other aspects of management practice. (1979, p. 21)

   Evaluate. Before research is undertaken, what clues are there that a piece of research would "over the long run" contribute to management practice? How long is the "long run"?
8. Myers, Greyser, and Massy (1979) conclude:

   > What the commission in effect rediscovered in the management science/model-building area was a reaffirmation of what many model-builders themselves have long believed—comparatively few firms or practicing management people seem to be using their models. (1979, p. 22)

   Why is this the case? Is it reasonable to expect line managers to use sophisticated marketing models extensively?
9. Accounting practitioners have historically been very generous in supporting academic accounting departments. Marketing practitioners provide very little support to academic marketing departments. Why is this the case? What steps could be undertaken by marketing academia to encourage marketing practitioners to be more supportive?

10. Differentiate between the study of consumer behavior and the study of buyer behavior. Is consumer behavior a subset of marketing? Is buyer behavior? Ought they to be? What were your criteria for inclusion/exclusion?
11. All of academia (not just marketing and business schools) seems to always be under siege from taxpayers, legislators, and even university officials to be *more productive* and *more relevant*. Apparently, many groups are no longer are willing to accept semester credit hours taught, articles and books published, and community service rendered as adequate indicators of productivity.
    a. Develop a set of productivity measures that would satisfy the critics of academe.
    b. To what extent are your measures consistent or inconsistent with traditional measures?
    c. Which stakeholders of universities would be better served by your measures? Which ones worse served? Why?
12. The AMA "task force" (Monroe et al. 1988) identifies five "constituencies of marketing knowledge":
    (1) academics and students
    (2) practitioners
    (3) policymakers
    (4) special interest groups
    (5) customers/consumers

    This chapter discusses four "clients" of marketing:
    (1) society
    (2) students
    (3) practice
    (4) academy

    Do the "constituencies of marketing knowledge" differ from the "clients of the marketing discipline"? Defend the view that Monroe et al. (1988) and the book are consistent. Now attack this view. What is your philosophy on this issue? Why?
13. As noted in endnote 14, Gaski (2007, p. 129) concludes that "attending to the prescriptive duty on the teaching side, ironically, may actually deliver more societal value than any macro or 'marketing-and-society' curriculum in its own right." What does Gaski mean here? Do you agree or disagree? Why? (Hint: see section 2.2.1.)

NOTES

1. For example, as late as 1874, the Catholic University in Ireland refused to allow science to be taught (Gieryn 1983).

2. Oxford and Cambridge at the time still admitted only Anglicans and were not yet research oriented. Oxford granted its first Ph.D. in 1917, fifty-six years after Yale granted the first Ph.D. in the United States. Between 1815 and the outbreak of World War I, over 10,000 American students earned degrees at German universities, half of them at the University of Berlin (Brubacher and Rudy 1976, p. 175).

3. This seems to be the argument of Myers, Massy, and Greyser (1980, p. x), when, among other things, they discuss scholarly research as "basic fuel."

4. For an introduction, see Lynn (1965), Vollmer and Mills (1966), and Moore (1970). For an excellent summary related to management as a profession, see Osigweh (1986).

5. It is worth noting that medicine and law have a single, dominant occupation associated with them, and even accounting has only two: the CPA and the corporate accountant. The radical heterogeneity of marketing occupations poses a unique difficulty for those wanting "marketing" *per se* to be a profession. Indeed, the "marketing manager" as customarily described in marketing textbooks, constitutes a distinct minority as to numbers of people engaged in marketing occupations.

6. All moral philosophies can be categorized as either deontological (which focus on the inherent rightness/wrongness of behaviors) or teleological (which focus on the amount of good or bad embodied in the consequences of behaviors). See section 13.3.3 and Hunt and Vitell (1986, 1993, 2005, 2006) for discussions of a theory of ethics embracing both deontological and teleological philosophies.

7. See Chapter 12 for an extensive analysis of objectivity.

8. Engineering is, of course, the other popular major for getting on the socioeconomic ladder. MBA programs also have this role, but there is less evidence that MBA programs *disproportionately* serve those from lower socioeconomic strata.

9. If two genuinely rival, alternative views—for example, two theories or paradigms—are "incommensurable," then, by definition, their relative merits cannot be evaluated. Consequently, each view must be considered equally good, equally bad, equally trustworthy, equally untrustworthy, equally true, or equally false, because if a judgment can be made that one alternative view is superior to its rival, then the two views are commensurable, not incommensurable.

10. The material in this section draws extensively from Hunt (2007b).

11. The articles in the special section of *JPP&M* in 2007 provide a very useful discussion of definitions in general, the role of the AMA in defining marketing, and various proposed definitions. See Gundlach (2007) for a good overview of the debate about defining marketing. See the appendix in Ringold and Weitz (2007) for a good summary of definitions of marketing from 1920 through 2007.

12. Committee members were Wayne McCullough (Daimler-Chrysler), Jimmy Peltier (University of Wisconsin–Whitewater), Ric Sweeney (University of Cincinnati), Joan Treistman (M/A/R/C Research), William Wilkie (University of Notre Dame), Becky Youngberg (American Marketing Association), and George Zinkhan (University of Georgia). I was also on the committee.

13. For an overview of the revolutionary changes taking place in growth economics, see Hunt (2000c, pp. 179–204, 2007a).

14. Gaski's (2007) commentary provides an insightful "reconciliation" (p. 128) of what he regards as the "overdrawn differences within the marketing academic profession" (p. 126) concerning the alleged value of managerially relevant versus macromarketing teaching and research. He concludes: Through marketing transactions, capitalist economics has done much good for the world and its people, demonstrably delivering a level of widespread prosperity and liberty unmatched by rival systems. . . . Given the self-evident economic results and societal benefits resulting from free-market capitalism, the supply of business education is a noble endeavor. . . . The research function of business schools provides adequate opportunity for both managerial relevance and a macromarketing or public policy inclination, but attending to the prescriptive duty on the teaching side, ironically, may actually deliver more societal value than any macro or 'marketing-and-society' curriculum component in its own right. (Gaski 2007, p. 129)

PART 2

THE FOUNDATIONS OF MARKETING THEORY

This section develops the philosophy of science foundations of marketing theory. Chapter 3 explores the morphology of scientific explanation. That is, how does one go about *explaining* marketing phenomena? Various explanatory models are developed, including the deductive-nomological, deductive-statistical, inductive-statistical, pattern, and functionalist models. Chapter 4 discusses some issues in explanation, such as the relationships among explanation, prediction, retrodiction, and causation. Several marketing explanations, involving areas such as the product life cycle, consumer behavior, price discrimination, and the wheel of retailing, are then analyzed. Chapter 5 investigates the nature of laws in marketing and evaluates the four criteria for lawlike generalizations—generalized conditionals, empirical content, nomic necessity, and systematic integration.

Chapter 6 delineates the characteristics of the various kinds of laws: equilibrium laws, laws of atemporal coexistence, laws of succession, process laws, axioms, fundamental laws, derivative laws, bridge laws, statistical laws, and universal laws. Chapter 7 begins by noting some misconceptions concerning the nature of theory. A perspective on theory is presented, and the three major criteria for theories are detailed. These criteria reveal that theories must (1) contain systematically related sets of statements, (2) contain some lawlike generalizations, and (3) be empirically testable. Chapter 8 examines the role of classification in theory development, contrasts positive theory with normative theory, explores whether deterministic theory in marketing is possible, and examines how "general theories" differ from other theories.

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3

ON THE MORPHOLOGY OF EXPLANATION

> Science has not the monopoly of truth but only the monopoly of the means for checking truth and enhancing it.
>
> —*Mario Bunge*

The term *explanation* plays a prominent role in all kinds of scientific inquiry. Although the observation, description, and classification of phenomena are important in science, the *explanation* of phenomena remains the *sine qua non* of science; without explanation, there is no science. As will be shown later, the systematic explanation of phenomena is a logical requirement for the scientific *understanding* of phenomena.

Ernest Nagel (1961, p. 15) suggests that "the distinctive aim of the scientific enterprise is to provide systematic and responsibly supported explanations." However, terms such as *systematic, responsibly supported,* and *explanation* are remarkably compressed concepts, and much of this monograph will be devoted to unpacking them. The objective of this chapter is to unpack the concept of explanation. That is, how does one go about explaining the occurrence of some phenomenon? What general characteristics do all satisfactory explanations have? How can various explanations be evaluated? Are some explanations better than others? Before turning to these questions, the next section will explore the purpose of explanations in marketing.

3.1 EXPLANATIONS IN MARKETING

Carl Hempel (1965a, p. 334) suggests that scientific explanations should be viewed as scientific answers to "why" questions. Why did phenomenon X occur? Phenomenon X occurred because. . . . Thus, marketers might want to know: Why have the sales of product X been decreasing rapidly? Why do newspapers charge lower rates to local advertisers than to national advertisers? Why did budget motels enter the lodging industry in the 1970s? Why do people purchase particular brands of detergents? Possible explanations for these "why" questions might involve, in turn, the product life cycle, price sensitivity, the "wheel of retailing," and a consumer behavior model. These specific marketing explanations will be evaluated in detail in the next chapter. At this point, we need only

emphasize that marketing research is vitally concerned with explaining marketing phenomena and that answers to "why" questions usually serve as precursors to "what will happen if" questions. For example, if one can explain why people buy particular detergents, one can predict "what will happen if I produce a particular brand of detergent."

Explaining phenomena requires explanation *models.* A model is any structure that purports to *represent* something else (Rigby 1965, p. 109). Aeronautical engineers use miniature models of airplanes to represent full-sized airplanes in wind-tunnel tests. A road map is a model of a highway system. *Mathematical* models use mathematical symbols to represent certain characteristics of phenomena. *Verbal* models use words to represent phenomena. Thus, a poem is a kind of verbal model. A photograph or statue is an *iconic* model that *looks like* what it is supposed to represent. Most marketing models are *verbal* models, although mathematical and statistical models are also common. All theories are models because (as Chapter 7 will reveal) all theories purport to represent some aspects of real-world phenomena. However, the converse is not true: all models are not theories, in that many models will not have all the requisites of theoretical constructions. Therefore, we ask: What is an *explanatory* model? Our answer is: *An explanatory model is any generalized procedure or structure that purports to represent how phenomena are scientifically explained.*

Subsequent sections of this chapter will analyze six different kinds of explanatory models: deductive-nomological explanations, deductive-statistical explanations, inductive-statistical explanations, statistical relevance explanations, pattern explanations, and functionalist explanations. These various kinds of explanatory models are structurally dissimilar. That is, they employ fundamentally different kinds of logic and evidence to explain phenomena. Before analyzing and comparing them, we need to develop some normative criteria for evaluating the adequacy of purportedly explanatory structures.

3.2 CRITERIA FOR EVALUATING EXPLANATORY MODELS

Generally, most philosophers of science agree that to seek an answer as to why a phenomenon occurred is to show at least that, given some antecedent conditions, the phenomenon was somehow *expected to occur.* Thus, any explanation of the decreased sales of a product must show that, given certain conditions, one would have *expected* the decreased sales. If one seeks to explain the growth of budget motels, one must show that, given certain other phenomena, the growth of budget motels could be *expected.* So the first criterion is that any model that purports to be explanatory must somehow show that the phenomenon to be explained was expected. Three other normative criteria seem equally appropriate for assessing the explanatory adequacy of a model. Explanatory models should be *pragmatic, intersubjectively certifiable,* and have *empirical content.*[1]

Pragmatism, the second criterion, can best be illustrated with an example. Suppose a particular analysis logically *precluded* Newton's laws of motion as qualifying as explanatory. Because Newton's laws are prime examples of what would almost universally be considered explanations in the sciences, simple pragmatism would suggest that the analysis

was too restrictive and that we should go back to the drawing board. Thus, *pragmatism* dictates that models incorporating devices structurally similar to Newton's laws should be considered explanatory. More generally, models of explanatory structures should be in accord with scientific practice.

The third criterion provides that explanations, like all scientific knowledge, must be objective in the sense of being *intersubjectively certifiable.* That is, different investigators (hence, *intersubject*) with different opinions, attitudes, and beliefs must be able to check the logic and make observations or conduct experiments to determine the truth content of the purported explanation. The intersubjectively certifiable criterion implies that *explanatory structures must be testable.* For example, one criticism of cognitive dissonance theory in social psychology is that the proponents of dissonance theory generally find results favorable to the theory; whereas nonbelievers in dissonance theory generally find scant evidence to support the theory. That is, cognitive dissonance researchers have had problems in attempting to intersubjectively certify dissonance theory.

The fourth criterion requires explanations to have *empirical content.* Not only must an explanation be testable, it must be *empirically* testable. This rules out so-called purely analytic explanations, where statements are true, not by recourse to empirical (real-world) phenomena, but just because of the way terms are defined. There is something uncomfortable about the claim that one can explain the high market share of a brand by pointing out that more people buy it than any other brand. Such explanations would be purely analytic: true by definition alone. Explanations using extraempirical statements are also ruled out by the empirical content criterion. As Lambert and Brittan (1970, p. 26) point out, "Appeals to God's will, for instance, although satisfying to many people, are not generally held to be explanatory; that the Lisbon earthquake occurred because God willed it is not really an assertion open to scientific investigation."

The preceding criteria do not exhaust the possibilities. However, they do appear to be a minimal set of desirable attributes for scientific explanations. Given these criteria, we can begin exploring the structure of various explanatory models.

3.3 DEDUCTIVE-NOMOLOGICAL EXPLANATION

The classical model of explanation is the deductive-nomological (D-N) model. Suggested by Hempel (1965a, p. 335), the terms literally mean "to deduce from laws." The D-N model, referred to as hypothetic-deductive (Kaplan 1964, p. 10; Brodbeck 1968, p. 385), covering law (Dieks 1980), and, simply, deductive (Kaplan 1964, p. 336), is classical in the sense that early philosophers such as Hume (1739/1911) and Kant (1783/1968) implied the D-N model in their writings when referring to explaining phenomena. For example, in discussing the nature of science, Kant (1783/1968, p. 18) refers to "the laws from which reason explains the facts." Rigorous explication of the D-N model has been developed by Stallo (1882/1960), Campbell (1952), Cohen and Nagel (1934), and Hempel (1965a).

The deductive-nomological model of explanation has the following structure:

$$\begin{array}{l} C_1, C_2, \ldots C_k \\ L_1, L_2, \ldots L_r \end{array} \Big\} \text{ Explanans } S$$

$$\overline{\qquad E \qquad} \Big\} \text{ Explanandum}$$

The C_1,C_2, . . . C_k, refer to the characteristics or facts of the particular situation, and the L_1, L_2, . . . L_r refer to certain laws of strictly universal form. These laws state that *every* time some particular set of phenomena (C_1, C_2, . . . C_k) occurs, then some other phenomenon (E) will also occur. Together, the characteristics and the laws jointly make up the explanans. The explanans deductively implies (this is the meaning of the solid horizontal line) the explanandum *E,* which represents the phenomenon to be explained. The D-N model suggests that to explain a phenomenon is to deductively subsume the phenomenon under a set of laws and, therefore, to show that the phenomenon scientifically could be *expected to occur* (Cohen and Nagel 1934, p. 397). Note that in the D-N model, *if* the explanans is true, then the explanandum *must* be true because the laws are of strictly universal form. That is, the laws state that *every* time characteristics C_1, C_2, . . . C_k occur, then E must occur.

A simple example of D-N explanation should make the model clearer. Why does a cube of ice with dimensions 1 foot × 1 foot × 1 foot float in water? The characteristic facts are that the cube of ice weighs approximately 56.2 pounds and that a cubic foot of water weighs approximately 62.4 pounds. Archimedes' principle states that a body in a fluid will displace an amount of that fluid equal to the weight of the body and that the fluid will exert an upward force on the body equal to the weight of the displaced fluid. Also, Newton's first law of motion states (essentially) that every time the result of all the forces acting on a body equals zero, the body will remain at rest. The characteristics (the weight of ice versus the weight of an equal volume of water) in conjunction with the principle/law (Archimedes' and Newton's) logically imply (predict) that the cube of ice will float in the water.

Many explanations of why ice floats in water will mention Archimedes' principle but neglect to explicitly state Newton's law. A logician would refer to such explanations as *enthymemes* (or elliptical explanations [Hempel 1965a, p. 414]) because one of the laws necessary to deduce the explanandum was suppressed or skipped over. Enthymemes are common in scientific discourse.

The D-N model of explanation certainly satisfies the four criteria in section 3.2 for evaluating purportedly explanatory structures. The classical explanations of the sciences are unquestionably consistent with it. If the terms have empirical content (refer to real-world phenomena), then the model assures us that the explanation will be testable (at least testable in principle), which means that the explanation will be intersubjectively certifiable. The beauty of the D-N structure is that the explanandum is a logical consequence of the explanans, which greatly simplifies empirical testing. To the extent that the explanandum is verified by empirical observation, evidence is provided that reality is actually isomorphic (structurally similar in essential respects) with the proposed explanans. Thus, *scientific understanding* of the real world is increased. Some philosophers of science accept only

the D-N model as satisfactorily explanatory (Donagan 1966, p. 132). This tunnel view of explanation is too restrictive, as our analysis of statistical explanations will show.

3.4 STATISTICAL EXPLANATION

Statistical explanation differs markedly from deductive-nomological explanation. Fundamentally, whereas D-N explanations employ only laws of strictly universal form, all statistical explanations contain at least one law in the probabilistic or statistical form:

$$P(G, F) = r$$

The law states that "the probability of *G,* given *F,* is *r*." An interpretation of this law suggests that in the long run the expected proportion of those cases of *F* that are also *G* is *r.*

We say an *interpretation* of the statistical law because there are basically three different theories of probability: mathematical probability, relative frequency, and subjective probability (Kaplan 1964, p. 225). Some familiarity with these three theories is necessary to analyze the basic nature of statistical explanations.

3.4.1 Theories of Probability

Roughly speaking, mathematical probability is an *a priori* notion, in which probabilities can be assigned to events without observation of the frequencies with which those events actually occur. To mathematical probability advocates, the probability of an event is determined simply by dividing the number of "favorable" cases of an event by the total number of all alternatives, provided that all cases are equally likely (equipossible). Therefore, the probability of throwing an "ace" with a "fair die" is 1/6 because there are six equally likely alternatives, of which an "ace" is one. The major problem with mathematical probability is that not all statistical situations are *a priori* deterministic. Thus, enter the relative frequency advocates.

The relative frequency approach also divides events into favorable and unfavorable cases, but it does so on the basis of actual observation of historical data rather than *a priori* judgment. Here the desirability or necessity of equipossible events vanishes. What is the probability that Jones Steel Company will get the next order from Smith? Because Smith historically has given Jones 20 percent of all their steel orders, the probability is 0.2. Unfortunately, there often is no sound basis for assuming that past frequencies will continue into the future, as there is for mathematical probability, and, also, many events *have* no historical frequencies. What is the probability of total nuclear war next year? What is the probability that Joseph Parker will get a Ph.D.? Now enter the subjectivists.

The subjectivists claim that if the statement "Nuclear war is more likely during crisis periods than during detente periods" has meaning, then ascribing probability numbers to phrases such as "more likely" might prove a useful enterprise. For example, a group of scientists in the 1970s estimated a probability of 0.5 that total nuclear war would break

out before the year 2000. Obviously, a probability of that kind could not be estimated from either historical frequency or mathematical *a priori* reasoning. (Equally obvious is the fact that the phenomenon did not occur.) In the nuclear war example, the term *probability* stands for the subjective degree of belief or certitude concerning the occurrence of an event, rather than its historical frequency. Although initially viewed with horror by traditional statisticians, by the 1960s, the so-called Bayesian approach to probability—named after the English clergyman Thomas Bayes (1702–1761)—had gained widespread respectability because of its admitted usefulness (Schlaifer 1959).

The preceding discussion of the theories of probability is meant to be suggestive rather than exhaustive. Each theory is useful, given a particular kind of problem. However, the notion of statistical explanation that follows presumes a mathematical or relative frequency interpretation of probability.

3.4.2 Statistical Explanation and the Social Sciences

Gustav Bergmann (1957) points out that statistical laws and statistical explanations gain in importance when our knowledge is imperfect because we do not know all of the variables that influence the phenomenon to be explained. Although the terms *perfect* and *imperfect* may have unfortunate connotations (because "perfect" suggests that knowledge-with-certainty is possible), the basic notion seems reasonable. May Brodbeck amplifies the view by succinctly stating the reasons why statistical explanation has such importance in the social sciences.

> Without some abstraction or selection from all the possibilities the world presents[,] there can be no science at all. By their very nature scientific laws describe only certain features of the kinds of things or events they hold to be connected. How much can safely be ignored depends upon the way things are. . . . To say, in consequence, that abstraction is all very well for the physical sciences but will not do for the study of man and society is the counsel of desperation; that is, no solution at all. The social scientist, striving to merit the honorific half of that title, settles for something less than perfection. . . . The use of the statistical concept [in the physical sciences] marks our ignorance of all the influencing factors, a failure in either completeness or closure or, usually, both. Similarly, the social scientist, deliberately selecting for study fewer factors than actually influence the behavior in which he is interested, shifts his goal from predicting individual events or behaviors to predicting a random variable, that is, to predicting the frequency with which this kind of behavior occurs in a large group of individuals possessing the circumscribed number of factors. This is the price. The reward, of course, is that instead of helplessly gazing in dumb wonder at the infinite complexity of man and society, he has knowledge, imperfect rather than perfect, to be sure, but knowledge not to be scorned nonetheless, of a probability distribution rather than of individual events. After all, while we might much prefer to know the exact conditions under which cancer develops in a particular person, it is far from valueless to know the factors which are statistically correlated to the frequency of its occurrence. (Brodbeck 1968, pp. 293–94)

It is important to differentiate clearly between the assertion that some explanations are statistical in form and the assertion that, because no scientific explanation is *known* to be true, all scientific explanations are only more or less *probable.* This would be confounding the nature of the structure of explanations with the quantity and quality of the evidence in support of those explanations. As Hempel points out, "The distinction between lawlike statements of strictly universal form and those of probabilistic form pertains, not to the evidential support of the explanation in question, but to the claims made by them: roughly speaking, the former attribute (truly or falsely) a certain characteristic to all members of a certain class; the latter to a specified proportion of its members" (1965a, p. 379).

A similar confusion sometimes surrounds the terms *deduction* and *induction* as they relate to statistical explanation. As we shall see in the following section, not all statistical explanations are inherently inductive (i.e., some are deductive). Likewise, not all inductive processes are inherently statistical.

3.4.3 Deductive-Statistical Explanation

In the deductive-statistical model (D-S), the explanandum E is deduced from the explanans in exactly the same fashion as in the D-N model; that is, the phenomenon to be explained is a logical consequence of the explanans. Therefore, if the explanans is true, then the explanandum must be true. If the explanandum is false, then the explanans is false. However, the explanandum may be true and the explanans false. The reader is advised to review the preceding three statements as many times as is necessary to see that they are logically true.

The D-S explanatory model can best be illustrated by an example. Assume the following definitions and statistical laws:

C_1 = Purchasing agent J sees no difference between the offerings of suppliers X and Y.
C_2 = Purchasing agent J desires to split his orders between suppliers X and Y approximately evenly.
C_3 = Purchasing agent J does not want his suppliers to become complacent.
C_4 = Purchasing agent J decides to flip a fair coin to decide which supplier gets each order.
C_5 = Successive tossings of the coin are "independent."
O_k = Supplier k gets an order.
O_k* = Supplier k gets two orders in succession.
SL_1 = $P(O_k, C_1 \cdot C_2 \cdot C_3 \cdot C_4) = 0.5$. The probability of k getting any order is a statistical law with probability 0.5, given C_1,C_2, C_3, and C_4.
SL_2 = $P(m + n) = P(m) \times P(n)$. This is a statistical law that if two events, m and n, are independent, then the probability of both events occurring $(m + n)$ equals the probability of m times the probability of n.

The schema for a deductive-statistical explanation can now be formed:

C_1, C_2, C_3, C_4, C_5 } Explanans S
SL_1, SL_2

$P(O_k^*, S) = 0.25$ } Explanandum E

The D-S model states that, given S (the characteristics C_1, C_2, C_3, C_4, and C_5 and the two statistical laws SL_1 and SL_2), the probability that supplier k will get two orders in succession is precisely 0.25 (that is, 0.5×0.5). Just as in the D-N model, the explanandum is a deductive, logical consequence of the explanans.

Note that the explanandum in the example is a statement in statistical form. This is important and no accident. *The D-S model can explain only other statistical laws; the only statements that are deductive, logical consequences of statistical laws are other statistical laws.* Section 3.4.4 will demonstrate that *all other* kinds of explananda are inductively inferred from the explanans and not strict logical consequences of the explanans.

The D-S model is reasonably consistent with our normative criteria for explanations in section 3.2. If the terms in the explanandum have empirical counterparts, the D-S model is intersubjectively confirmable by empirical testing. That is, we can observe real-world phenomena to determine whether the explanandum actually occurs. Many of the classical explanations in genetics are fundamentally of the D-S variety. Likewise, the decomposition of radioactive materials in physics (the "half-life" statistical laws) can be used to explain other statistical laws. Nevertheless, most statistical explanations in the social sciences are not D-S in nature. Most statistical explanations are *inductive,* not *deductive.*

3.4.4 Inductive-Statistical Explanation

Unlike with the D-S and D-N models, with the inductive-statistical (I-S) model, the phenomenon to be explained is *not* a logical consequence of (is not necessarily implied by) the explanans. In the I-S model the explanans only confers a certain likelihood that the phenomenon will occur.

To put I-S into schematic form, we need to add two items to the example in section 3.4.3:

O_k^{**} = Supplier k gets at least one order in the next ten orders.
$SL_3 =$ $P(O_k^{**}, C_1 \cdot C_2 \cdot C_3 \cdot C_4 \cdot C_5 \cdot SL_1 \cdot SL_2) = 1 - (0.5)^{10} = 0.999\ldots$ The statistical law that the probability of supplier k getting at least one order in the next ten orders is extremely high, given $C_1 \ldots C_5$ and SL_1 and SL_2.

Following Hempel's (1965a, p. 383) procedure, the schema for the inductive-statistical explanation can now be formed:

C_1, C_2, C_3, C_4, C_5 } Explanans S
$SL_1, SL_2, SL_3,$
[it is very likely that]
O_k^{**} Explanandum E

Given that the circumstances ($C_1 \ldots C_6$) and the appropriate statistical laws (SL_1, SL_2, SL_3) are true, O_k** is *very likely* to occur. Instead of a single line indicating that O_k** is deductively subsumed under the explanans, we show a double line indicating that the explanans only confers *inductive* support to O_k**. The conclusion E is not a logical consequence of the premises S in the sense that it is possible for E not to occur and the premises still be true. That is, the negation of E does not logically imply the negation of S.

An Evaluation of the I-S Explanatory Model

Evaluating the I-S model via the criteria of section 3.2 reveals substantial conformity. For example, "Why were 51 percent of all babies born last year in General Hospital male?" This phenomenon would classically be explained by the use of an I-S model referring to certain statistical laws in genetics. The model also makes predictions of a sort, thus making the model testable and, hence, intersubjectively confirmable. Finally, the explanans would lead one to *expect* the explanandum to occur.

Three problems sharply demarcate the fundamental differences between the D-N and I-S explanatory models. First, consider the consequences of testing a particular D-N model.

Test 1	*Test 2*
D-N model K implies A.	D-N model K implies B.
A is false.	B is true.
D-N model K is false.	D-N model K is supported.

Test 3
D-N model K implies B_1, B_2, B_3, B_4.
B_1, B_2, B_3, B_4 are true.

D-N model K is strongly supported.

Test 1 shows that *if* D-N model K does imply A (i.e., if our logic is correct) and if A is in fact false (i.e., if our measurements are accurate), then all or some part of model K is false. The D-N model is *falsifiable* in a very strong sense because the D-N model incorporates laws of strictly universal form. The laws in the D-N model state that every time certain circumstances prevail, then some phenomenon will occur. Therefore, if we observe the required circumstances, and if phenomenon A does *not* occur, there exist only three possibilities: (1) there has been an error in logic (model K does not really imply that A will occur); (2) there has been an error in measurement (the characteristics $C_1 \ldots C_k$ were not present, or phenomenon A occurred but the measurements did not detect it); or (3) all or part of model K is false. So, the researcher must check the logic, check the measurements, and then draw the appropriate conclusions.

Now examine Tests 2 and 3 of the D-N model. Note that it is never appropriate to conclude that a particular D-N explanation is true. Rather, repeated empirical testing can

only inductively confer more or less empirical *support* or corroboration that a particular D-N explanation is true.

Now examine the consequences of testing a particular I-S model:

Test 1
I-S model *J* suggests *A*.
A is false.

I-S model *J* is not supported.

Test 2
I-S model *J* suggests *B*.
B is true.

I-S model *J* is supported.

Test 3
I-S model *J* suggests B_1, B_2, B_3, B_4.
B_1, B_2, B_3, B_4 are true.

I-S model *J* is strongly supported.

I-S model *J* suggests (makes more or less very likely) that if certain circumstances occur, then phenomenon *A* will occur. Empirical Test 1 shows that, in fact, *A* does *not* occur; that is, *A* is false. Once again, just as with the D-N model, the researcher should check both the logic and the measurements. However, in contrast with the D-N model, if the logic and measurements are found to be correct, the researcher cannot claim that I-S explanation *J* is false. Rather, because of the statistical nature of the laws in model *J,* the researcher can only conclude that the model is *not* supported. An example may serve to clarify the preceding observation.

Consider the I-S example previously discussed in this section that had the statistical law $SL_3 = P(O_k{**}, C_1 \ldots)$. The law suggests that $O_k{**}$ (getting at least one order in the next ten orders) is very likely to occur. Suppose we observe that $O_k{**}$ does *not* occur; *X* fails to get a single order ten times in succession. The apologist for the I-S model might say, "Isn't that just my luck? Every time *I* test a model, the observation winds up in the tail of the distribution!" Or the researcher might ascribe the poor results to "noisy data." The preceding implies a fundamental difference between D-N and I-S explanations: I-S *models are fundamentally not falsifiable.* Further, if falsifiability is a reasonable normative criterion for explanatory models, then I-S models cannot be considered *explanatory.* This is exactly the conclusion of those holding what is here labeled the "strict deductivist" position.

Is there another sense of the word *falsifiable* that might admit I-S explanations? Yes, there is. Consider the following test:

Test 4
I-S model *J* suggests A_1, A_2, A_3, A_4.
A_1, A_2, A_3, A_4 are all false.

It is very likely that I-S model *J* is false.

Test 4 shows that a whole series of observations has failed to support I-S model *J.* This might be considered very strong evidence that model *J* is probably false and could be

called the *weak falsifiability criterion.* In the previous example, if *X* loses forty orders in a row, this is strong evidence that the underlying statistical law in model *J* is false.

If strict falsifiability is demanded, then I-S models cannot be deemed satisfactory explanations. If a weak falsifiability criterion suffices, then I-S models should be considered acceptable explanations. Is the weak falsifiability criterion justified? I believe the answer is yes. Even Hempel (1965a), who at one time was a strict deductivist, finally moved toward the acceptance of statistical explanations as explanatory. If weak falsifiability were rejected, then almost all explanations in marketing and other social sciences would also have to be rejected. As will be shown in Chapter 6, almost all the laws (hence, all the explanations) in the social sciences are basically statistical in nature.

The second problem of the I-S model concerns how to ascertain the amount of inductive support that the premises confer upon the conclusion; for example, in Test 4, precisely what do we mean by the phrase "it is very likely that"? What is needed is a system of *inductive logic* to quantify the amount of inductive support, and this has been an intractable problem for centuries. Carnap's (1962) system of inductive logic suggests that for simple I-S models with only one statistical law, the likelihood of (degree of inductive support for) the occurrence of an event is the statistical probability of the event. Therefore, because SL_1 and SL_2 combine to form SL_3 in the previous example, the structure would be:

$$\frac{\begin{array}{c}C_1, C_2, C_3, C_4, C_5 \\ SL_3\end{array}}{O_k{**}} [r = 0.999 \ldots]$$

Unfortunately, Carnap's system has not been extended to more complicated statistical systems with multiple statistical laws. *The degree of inductive support that a series of statistical laws confers upon the occurrence of an event remains unresolved except where all the laws can be compressed into a single law.*

The third problem with the I-S model concerns its ambiguity (Hempel 1965a, p. 394). Consider the following I-S argument:

C_1 = Jones is a Scottish-American.
C_2 = Jones drinks alcoholic beverages.
SL_1 = 90 percent of all Scottish-American drinkers drink Scotch.

[very likely]

Jones drinks Scotch.

However, suppose the following premises are also true:

C_3 = Jones is a Southerner.
SL_2 = 90 percent of all Southerners who drink do *not* drink Scotch.

[very likely]

Jones does not drink Scotch.

The preceding two arguments (whose premises we assume to be true only for expository purposes) yield contradictory conclusions. It is psychologically uncomfortable to note that *ex post* we can always find a model consistent with the observation. If Jones is observed drinking Scotch, the I-S model builder says, "Aha, he drinks Scotch because he is a Scottish-American." And if Jones is observed drinking bourbon, the I-S model builder says, "Indeed, as I always said, very few Southerners drink Scotch."

Carnap (1950, p. 211) suggests resolving the ambiguity problem with *the requirement of total evidence:* "In the application of inductive logic to a given knowledge situation, the total evidence available must be taken as a basis for determining the degree of confirmation." Similarly, Hempel (1968) proposes *the requirement of maximal specificity:* "In formulating or appraising an I-S explanation, we should take into account all the information . . . which is of potential explanatory relevance to the explanandum event" (1968, p. 118). (See Cooke [1981] for an evaluation of this criterion.) The criterion implies that an acceptable statistical explanation of why Jones drinks Scotch must be based on a statistical law using as a reference class the most narrowly defined class that our total evidence suggests as relevant. In this case we need a statistical law concerning the proportion of Scottish-American Southerners who drink Scotch. Using any other reference class may yield poor results.

3.5 ARE LOGICAL EMPIRICIST MODELS OF EXPLANATION ADEQUATE?

The D-N, D-S, and I-S models of explanation are all associated with the logical empiricist approach to science, and, as Frederick Suppe (1977a) has correctly observed, the logical empiricist treatment of science has been subject to significant and sustained attack. In particular, the "received view" of the logical empiricists concerning the nature of scientific theories and explanations has been assaulted. Indeed, Suppe (1977a, p. 619) concludes that "the last vestiges of positivistic philosophy of science are disappearing from the philosophical landscape." Therefore, he proposed, "Positivism truly belongs to the history of the philosophy of science, and its influence is that of a movement historically important in shaping the landscape of a much-changed philosophy of science" (Suppe 1977a, p. 632). The purpose here is to review and evaluate the attacks on the logical empiricist models of explanation. Both the deductive-nomological model and the inductive-statistical model will be examined.

3.5.1 Is the D-N Model Dead?

Attacks on the D-N model have generally been prefaced by showing that some "explanation" may "fit" the D-N model and yet be regarded as unsatisfactory by most people. A well-known illustration is the "flagpole" explanation proposed by Bromberger (1966) and discussed by Suppe:

> Using geometric optics, we can form a law of coexistence which correlates the height of a flagpole, the angle of the sun to the horizon, and the length of the shadow cast by the flagpole. Using this law and initial conditions about the height of the flagpole

> and the sun's angle, we can explain the length of the shadow in accordance with the D-N model. However, if we take our initial conditions as being the length of the shadow and the sun's angle, using the law the D-N model allows us to (causally!) explain the height of the flagpole. But only the former case is a genuine explanation, the latter being spurious; since the D-N model sanctions both as genuine, the D-N model is defective. (Suppe 1977a, p. 621)

Bromberger (1966) concludes that only explanations that employ causal laws can be satisfactory. Similarly, Jobe (1976) uses examples such as Ohm's law and concludes that satisfactory D-N explanations must use "genuine laws of nature." Finally, Brody (1972, p. 20) uses the following illustrations (which are adapted from Aristotle):

A. a. The planets do not twinkle.
   b. All objects which do not twinkle are near the Earth.
   c. Therefore, the planets are near the Earth.
B. a. The planets are near the Earth.
   b. All objects near the Earth do not twinkle.
   c. Therefore, the planets do not twinkle.

Brody proposes that both (A) and (B) are "acceptable" D-N explanations but that only (B) *should* be considered as explanatory. He concludes that "a deductive-nomological explanation of a particular event is a satisfactory explanation of that event when, besides meeting Hempel's requirements, its explanans essentially contains a description of the cause of the event described in the explanandum" (Brody and Cunningham1968, p. 23).

The preceding attacks are persuasive. To suggest that "lack of twinkling" *explains* the distance of the planets from the Earth clearly will not do, just as the length of women's hemlines will not *explain* stock market purchases. In other words, as will be shown in section 4.1, predictive adequacy is not sufficient for explanatory adequacy.

The position here is that any *satisfactory* explanation must contain in its explanans some mechanisms or laws or lawlike generalizations that are *purportedly* causal (see section 4.2 for more on causality). This position coincides with the views of advocates of *scientific realism,* a key part of the underlying, philosophical foundation of this monograph. For example, the realists Keat and Urry (1975, p. 13) point out that the D-N model fails "to distinguish between providing the grounds for expecting an event will occur, and explaining *why* it will occur." Keat and Urry note that logical empiricism and scientific realism share substantial common ground:

> Both share a general conception of science as an objective, rational enquiry which aims at true explanatory and predictive knowledge of an external reality. . . . First, the idea that scientific theories must be objectively assessed by reference to empirical evidence. This evidence is such that all scientists who are competent, honest, and lacking in perceptual deficiencies can agree upon it, though not necessarily with total certainty. Second, there is the idea that there are "objects," in the broad-

> est sense of the term, which exist independently of our beliefs and theories about them. . . . This means a rejection of the view that scientific theories determine that reality, rather than make genuine discoveries about it. . . . Both hold that there are general standards of scientificity, of what counts as an adequate explanation, of what it is that we must try to achieve by scientific theories, of the manner in which empirical evidence should be used to assess their truth or falsity, and so on. Whilst disagreeing about what these standards are, both believe that they exist. . . . They are external and universal standards, independent of particular, substantive theories and explanations, and applicable to all periods in the historical development of science. (Keat and Urry 1975, p. 44)

Curiously, Hempel himself, in his early work on the D-N model, ascribed a role for causal mechanisms (1965c, p. 250). However, his later works dropped the requirement (1965a, pp. 351–54). Dropping any reference to causal mechanisms from the D-N model resulted from the desire of the logical positivists and their successors, the logical empiricists, to expunge all metaphysical concepts from their analyses. And, because causality can never be *conclusively* verified, it was considered too metaphysical. What the logical empiricists never completely realized (or never could bring themselves to admit) is that if the *conclusive verification* criterion were applied to all concepts, laws, and theories in science, the domain of scientific inquiry would be dangerously close to an empty set. The concept *certainly* belongs in theology, not science.

Is the D-N model dead? Not at all. The basic structure of the model still stands. The attack discussed in this section simply reveals that the model is not restrictive enough. The D-N model should be modified to require the specific inclusion of purportedly causal mechanisms, entities with causal powers, or causal laws in the explanans.

3.5.2 Is the I-S Model Dead?

As Suppe (1977a) points out, leading the charge against the I-S model have been Salmon (1971), Jeffrey (1966), and Greeno (1966). Recall that both the D-N and I-S models of explanations are, fundamentally, *arguments.* That is, premises (the explanans) and a conclusion (the explanandum) are joined by a "therefore." With the D-N model, the *premises* deductively imply or entail the conclusion. With the I-S model the premises suggest the conclusion with "high probability." Salmon, Jeffrey, and Greeno deny that explanations are arguments, leading Suppe to conclude that "one will have to give up the requirement that statistical explanations are 'correct' arguments. . . . The important point is that Hempel's I-S model is defective, hence by implication so is his D-N model" (Suppe 1977a, p. 623).

If explanations are not arguments, what are they? Salmon (1971) proposes the "statistical relevance" (S-R) model of explanations. By this account, an explanation "is an assembly of facts statistically relevant to the explanandum regardless of the degree of probability that results" (Suppe 1977a, p. 623). Note that the S-R model violates the "high probability" requirement of the I-S model.

Writers cite numerous examples of satisfactory explanations that do not accord a high probability to the explanandum. Three examples can illustrate the procedure: (1) Half-life laws can be used to explain the emission of an electron from a radioactive substance where the probability of the electron emission at the time is very low. (2) The fact that a person gets lung cancer is often explained by noting that the person smokes two packs of cigarettes per day, even though not all cigarette smokers contract cancer. (3) Finally, an injection of penicillin may legitimately be thought to explain why a patient gets a rash even though only a few patients have this reaction. These kinds of examples have led writers to propose that "it is not the high probability conferred on the explanandum which makes an account explanatory, but rather the specification of a so far unnoticed factor which changes this probability to a value which differs from the value ascribed to it on the basis of the information that was available before the explanation had been given" (Kruger 1976, p. 131).

However, the S-R model does not propose that *any* assembly of facts will qualify as explanatory, only an assembly of those facts that are *relevant;* and most analyses of the model have been directed at separating the "relevant" from the "irrelevant." Criteria such as "homogeneous reference classes" and "screening-off relations" have been proposed to help solve the problem of separating "causal" relevance relations from "merely" statistical relevance relations. We shall not evaluate these issues here. Interested readers should consult Shrader (1977) and Meixner (1979) for reviews. The purpose here is to evaluate the following line of reasoning implied by advocates of the S-R model:

1. S-R explanations are adequate.
2. S-R explanations are *not* arguments.
3. I-S explanations are arguments.
4. Therefore, I-S explanations and, by implication, D-N explanations are defective.

To evaluate whether S-R explanations are adequate, consider the S-R explanation "cigarette smoking causes lung cancer in a person." Suppose scientists someday discover a "cancer gene" such that 50 percent of all the people who have this gene will ultimately contract cancer. Suppose further research yields the finding that "98 percent of all people who smoke two packs of cigarettes daily *and* have the cancer gene will ultimately develop lung cancer." Note that the "98 percent" statistical generalization enables one to *explain* the incidence of lung cancer by way of an I-S explanation, because 98 percent would provide "high probability."

The preceding example enables us to draw several conclusions. First, *if* the original cigarette S-R explanation is "adequate," then the "98 percent" I-S explanation *must* be adequate, because the 98 percent I-S explanation contains all the information of the S-R explanation and *more.* Further, if adequacy is considered a relative concept, then no one would dispute that the 98 percent I-S explanation is *better than* the S-R explanation. (See Watkins [1984, pp. 239–401] for more.)

Second, the fact that the 98 percent I-S explanation is *better* than the S-R explanation suggests that the goal of all scientists employing S-R explanations should be to upgrade

these explanations to I-S types. Third, if the goal of scientists should be to upgrade S-R explanations (which are not arguments) to I-S types (which are all arguments), one cannot claim that I-S explanations are "defective" because they are arguments. *If anything, S-R explanations are defective because they are not correct arguments.*

Are S-R explanations adequate? If "adequate" is construed as "useful," then the answer must be yes. It is certainly useful to know that although only a small percentage of smokers will contract lung cancer, heavy smokers are five to ten times more likely to develop lung cancer than are nonsmokers. However, as a goal for developing explanations of phenomena, S-R explanations are *inadequate* because they should be viewed as intermediate explanations directed toward the development of I-S explanations or D-N explanations. The original "cigarette" S-R explanation was "adequate" only because it was the best that science could provide at the time. This is as it should be, because all explanations must be evaluated given our knowledge base at a point in time (Gardenfors 1980).

In an analysis similar to the one here, Strevens (2000) evaluates the view that the size of a probability makes no difference to the quality of a probabilistic explanation and Salmon's (1984, p. 388) statement that any view to the contrary is an "anachronistic carryover" from Laplacean determinism. Using explanatory practices in statistical mechanics, Strevens (2000, p. 367) concludes that, "although low probabilities may have some explanatory power, the explanatory power of high probabilities is much greater." Indeed, though "hard core" critics of logical empiricism continue to cite Salmon's work as evidence of the inadequacies of the D-N and I-S models, Salmon's most recent work has, essentially, abandoned his S-R model. He has adopted—as does this work—a realist view of scientific explanation. Salmon writes:

> It seemed obvious at the time [of writing the 1971 book] that statistical relevance relations had some sort of explanatory power in and of themselves. As I have said repeatedly throughout this [1984] book, that view seems utterly mistaken. . . . Their fundamental import lies in the fact . . . that they constitute evidence for causal relations. (Salmon 1984, pp. 191–92)

In conclusion, the logical empiricist models of explanation remain the most viable models available for explaining phenomena. This is not to say that there are no unresolved issues with respect to the models. The D-N model should be made *more* restrictive by requiring causal mechanisms, entities with causal powers, or causal laws in the explanans. The I-S model should be made *less* restrictive by relaxing the "high-probability" requirement and recognizing that sometimes the best that science can provide at a point in time is an "explanation" of the S-R variety. Nevertheless, these modifications neither suggest that the basic logical empiricist models of explanation are "fundamentally defective" nor that the S-R model should be the goal of science, as Salmon himself now admits.

3.6 THE PATTERN MODEL

The pattern model (P-M) purportedly constitutes a fifth distinctive kind of explanation (in addition to the D-N, D-S, I-S, and S-R models). Abraham Kaplan (1964), an advocate of

the pattern model, views P-M explanations as extremely important in the methodology of the behavioral sciences. Kaplan defines and discusses the pattern model:

> Very roughly, [in the pattern model] we know the reason for something when we can fit it into a known pattern . . . something is explained when it is so related to a set of other elements that together they constitute a unified system. We understand something by identifying it as a specific part in an organized whole . . . in the pattern model we explain by instituting or discovering relations. . . . These relations may be of various different sorts: causal, purposive, mathematical, and perhaps other basic types, as well as various combinations and derivatives of these. The particular relations that hold constitute a pattern, and an element is explained by being shown to occupy the place that it does occupy in the pattern. . . .
>
> The perception that everything is just where it should be to complete the pattern is what gives us the intellectual satisfaction, the sense of closure, all the more satisfying because it was preceded by the tensions of ambiguity. (Kaplan 1964, pp. 332–35)

Is the P-M explanatory? Does it meet the normative criteria set forth in section 3.2? Certainly, many so-called explanations in the behavioral sciences and marketing simply show how the phenomenon fits into a distinctive pattern; thus, pragmatism favors the pattern model. Also, many of the concepts employed in pattern explanations have empirical content; that is, their terms have empirical referents. As we shall see, however, the criterion of *intersubjective confirmability* poses problems for P-M explanations. The potential for the intersubjective confirmability of pattern models can be best explored by analyzing an example of a P-M explanation by Kaplan:

> According to the pattern model, then, something is explained when it is so related to a set of other elements that together they constitute a unified system. We understand something by identifying it as a specific part in an organized whole. There is a figure consisting of a long vertical straight line with a short one branching upwards from it near the top, and a short curved line joining it on the same side near the bottom; the figure is meaningless until it is explained as representing a soldier with fixed bayonet, accompanied by his dog, disappearing around the corner of a building (the curved line is the dog's tail). We understand the figure by being brought to see the whole picture, of which what is to be explained is only a part. (Kaplan 1964, p. 333)

Is the preceding P-M explanation intersubjectively certifiable? To analyze it, let's designate the soldier-with-dog P-M explanation as *J*. Consider now a second P-M explanation, *K,* for the figure in Kaplan's example. Pattern model *K* proposes that the figure actually is a dead tree with small branches at the top and the bottom. Which explanation is correct? If two subjects perceive different patterns that encompass the same phenomenon, what objective criteria can be used to confirm one pattern over the other? Kaplan (1964) mentions "intellectual satisfaction" as a criterion. But intellectual satisfaction is an irretrievably individual phenomenon. Model *K* may be more intellectually satisfying

to me, whereas model *J* may be more intellectually satisfying to you. A criterion such as familiarity suffers from the same weakness. A familiar pattern to a Western European might be totally unfamiliar to an East Asian.

The intersubjective confirmability criterion for the D-N, D-S, and I-S models is overcome via empirical testing. All of these models employ laws that make predictions that are susceptible to testing. Choosing between rival explanatory constructions can be accomplished by examining multiple tests of the models where the rival constructions predict different outcomes. At present, there are no similar "tests" for pattern models. *Therefore, the pattern model fails the criterion of intersubjective confirmability and should not be considered as having explanatory power.*

It can be shown that all of the examples discussed by Kaplan either (1) fail the intersubjective confirmability criterion or (2) are actually D-N explanations in disguise. Kaplan discusses a P-M to explain thunder: "[A] bolt of lightning heats the air through which it passes, which then expands, disturbing the air around it and thus setting up sound waves" (1964, p. 334). This particular pattern model has explanatory power *only* because it presumes some lawlike relationships: (1) lightning heats air; (2) heated air expands; (3) expanding air will disturb surrounding air; and (4) disturbed air creates sound waves. That D-N explanations will have "patterns" is undoubtedly true, but that patterns *alone* have explanatory power is an entirely different assertion.

The true value of pattern models may lie not in the context of justification (with the notion of explanation), but rather in the context of discovery. Because explanatory schemata frequently have distinctive patterns and because these patterns may be consistent across different kinds of phenomena, the theorist searching for tentative explanations might start by first looking for a familiar pattern. Kaplan (1964, p. 332) himself admits that "the pattern model may more easily fit explanations in early states of inquiry."

3.7 FUNCTIONALIST EXPLANATION

No one familiar with the social sciences can avoid being exposed to functionalism, and the advocates of functionalism as a distinct methodology are legion (Malinowski 1944; Merton 1938; Parsons 1949; Radcliffe-Brown 1952; Stinchcombe 1968). Some students of marketing, notably the late Wroe Alderson (1957, 1965), have also taken up the functionalist banner. Is functionalist explanation fundamentally different from other forms of explanation? First, we should note that functionalism belongs in the general class of philosophical inquiry known as *teleology* (literally, the study of purposes). No one would deny that much animal and human behavior is purposive, leading analysts such as Taylor (1967), Grene (1976), and Wright (1977) to conclude that the explanation of human behavior may be irreducibly teleological. Others, such as Utz (1977) and Clark (1979), propose that all purportedly teleological explanations can be recast as D-N or I-S explanations (as in section 3.7.2). The most prominent of the teleologically oriented writers have been the functionalists.

Despite the popularity of functionalism, there are major logical difficulties with functionalism and functional explanation. First, despite all the writings on functionalist methodology, the

meanings of the terms *function, functional,* and *functional explanation* lack both specificity and universal consensus, even among advocates of functionalism. Before analyzing the basic logic of functional explanations, we must first explore the different usages of the term *function.*

3.7.1 Uses of the Terms *Function* and *Functional Explanation*

Ernest Nagel (1961, p. 522) suggests that functionalists use the term *function* in at least six different ways. These different usages alone account for substantial confusion in functionalist literature, and four of them seem particularly appropriate for our analysis.

First, the term *function* is sometimes used to simply signify the dependence or interdependence between variables; that is, *X* is a function of *Y.* For example, "the incidence of purchase of major-brand gasoline is a function of the generalized self-confidence of the subjects." However, such lawlike locutions are precisely the kinds of statements that are found in nonfunctionalist approaches. Hence, if the entire functionalist procedure were so construed, then functionalism could not be considered a distinctive mode of inquiry. That is, functionalist explanations would not be different from other kinds of explanations.

Second, biologists and others use the term *function* to refer to certain organic processes ("vital functions") such as reproduction and respiration that are considered indispensable for the continued life of the organism or the maintenance of the species. Similarly, in anthropology, Malinowski asserts that "in every type of civilization, every custom, material object, idea, and belief fulfills some vital function" (1936, p. 132). Thus, functionalists sometimes use the term *function* as synonymous with "indispensable role."

Third, the term *function* sometimes signifies a generally recognized use or utility of a thing. "The function of a salesperson's call report is to transmit intelligence," or "the function of advertising is to create sales." However, if all uses of the term *function* were confined to relatively simple assertions about the intended use of certain phenomena, then functional explanations would be weak, if not impotent.

Fourth, and finally, the term *function* often signifies the contribution that an item makes or can make toward the maintenance of some stated characteristic or condition in a given system to which the item is assumed to belong. *Thus, functional analysis seeks to understand a behavior pattern or a sociocultural institution by determining the role it plays in keeping the given system in proper working order or maintaining it as a going concern* (Hempel 1959, p. 277). If functionalism is to lay claim to being a distinct method of inquiry, it will do so on the basis of this final interpretation of function.

The fourth view of function and functional explanation (which we shall adopt and analyze) seems reasonably consistent with Wroe Alderson:

> Functionalism is that approach to science which begins by identifying some system of action, and then tries to determine how and why it works as it does. Functionalism stresses the whole system and undertakes to interpret the parts in terms of how they serve the system. Some writers who are actually advocates of functionalism prefer to speak of the holistic approach because of emphasis on the system as a whole. (1957, p. 16)

Similarly, Radcliffe-Brown discusses his version of functionalism:

> [In] social life, if we examine such a community as an African or Australian tribe, we can recognize the existence of a social structure. Individual human beings, the essential units in this instance, are connected by a definite set of social relations into an integrated whole. The continuity of the social structure, like that of an organic structure, is not destroyed by changes in the units. Individuals may leave the society, by death or otherwise; others may enter it. The continuity of structure is maintained by the process of social life, which consists of the activities and interactions of the individual human beings and of the organized groups into which they are united. The social life of the community is here defined as the *functioning* of the social structure. The *function* of any recurrent activity, such as the punishment of a crime, or a funeral ceremony, is the part it plays in the social life as a whole and therefore the contribution it makes to the maintenance of the structural continuity. (1952, p. 179)

Last, Malinowski presents his view of functionalism:

> [Functionalism] aims at the explanation of anthropological facts at all levels of development by their function, by the part which they play within the integral system of culture, by the manner in which they are related to each other within the system, and by the manner in which this system is related to the physical surroundings. . . . The functional view of culture insists therefore upon the principle that in every type of civilization, every custom, material object, idea, and belief fulfills some vital function, has some task to accomplish, represents an indispensable part within a working whole. (1936, p. 132)

Although we shall adopt the fourth interpretation of the term *function* for analytical purposes, a caveat is warranted. A proper evaluation of any author's functionalist theoretical construction requires the reader to consider carefully how the author uses the term *function.* Not only do different authors use the term differently, but also individual authors (perhaps unknowingly) slip back and forth in their usage of the term. This can create considerable problems for anyone attempting to analyze functionalist explanations.

3.7.2 Preliminary Problems of Functional Explanation

Before we attempt a formal analysis of functional explanation, two minor issues need resolving.[2] One logical requirement for *causal* explanations (see Chapter 4 for a discussion of causality) is temporal sequentiality: If *A* is supposed to cause *B,* then *A* must occur before *B* in time. Functionalist explanations, like all teleological explanations, make liberal use of the concepts "goals" and "purposes." Because goals and purposes refer to *future* events, does this not ascribe causal efficacy to future events? That is, does this not mean that future phenomena can cause present phenomena? For example, can the

goal of an increased market share (a future event) *cause* a firm to increase advertising effort and have explanatory power? Is this not contradictory? The reader will note that a simple resolution to this apparent contradiction lies in the manner of phrasing. Future events do not cause or explain present actions; the *desire* for a future event may cause or explain present actions. Here, the desire temporally precedes the behavior that one seeks to explain, and the apparent contradiction dissolves.

A similar teleological problem confronts the user of explanations of this sort: "The chameleon has the ability to change its skin color in order to blend in with its varying backgrounds, thus protecting it from natural enemies." The phrase "in order to" signifies a teleological emphasis in the explanation. However, using Darwinian theory, such covertly teleological explanations can be entirely avoided. A skeletal outline of such an explanation might include statements along these lines: (1) An early mutant lizard had the ability to change colors. (2) This ability increased the likelihood of its survival and the survival of those of its progeny that also carried the mutant gene. (3) Over time, the proportion of the species carrying the mutant gene rose due to "survival of the fittest." Note that no reference to purposive or teleological factors is required. Most such covertly teleological explanations can be recast in other, nonteleological, forms. Similarly, some purportedly functionalist explanations can also be recast. These two minor problems resolved, we are now in a position to explore the formal logic of functional explanation.

3.7.3 The Logic of Functional Explanation

The best way to evaluate the logic of functional explanation is to (1) present a classical functional explanation in the fourth sense of the term *function,* (2) dissect the explanation so as to lay bare its logical structure, and (3) evaluate that structure. Malinowski's well-known explanation of the function of mourning in primitive cultures provides just such a typical illustration:

> The ritual despair, the obsequies, the acts of mourning, express the emotion of the bereaved and the loss of the whole group. They endorse and they duplicate the natural feelings of the survivors; they create a social event out of a natural fact. Yet, though in the acts of mourning, in the mimic despair of wailing, in the treatment of the corpse and in its disposal, nothing ulterior is achieved, these acts fulfill an important function and possess a considerable value for primitive culture.
>
> What is this function? The death of a man or a woman in a primitive group, consisting of a limited number of individuals, is an event of no mean importance. The nearest relatives and friends are disturbed to the depth of their emotional life. A small community bereft of a member, especially if he be important, is severely mutilated. The whole event breaks the normal course of life and shakes the moral foundations of society. The strong tendency on which we have insisted in the above description: to give way to fear and horror, to abandon the corpse, to run away from the village, to destroy all the belongings of the dead one—all these impulses exist, and if given way to would be extremely dangerous, disintegrating the group, destroy-

> ing the material foundations of primitive culture. Death in a primitive society is, therefore, much more than the removal of a member. By setting in motion one part of the deep forces of the instinct of self-motivation, it threatens the very cohesion and solidarity of the group, and upon this depends the organization of that society, its tradition, and finally the whole culture. For if primitive man yielded always to the disintegrating impulses of his reaction to death, the continuity of tradition and the existence of material civilization would be made impossible. (Malinowski 1954, p. 52)

What is the basic structure of the preceding functional explanation? Basically, the structure is as follows:

Functional Model 1

1. At some time *t,* a system *s* (a primitive society) is in state *k* (proper working order).
2. The class of systems *S* (primitive societies) of which *s* is a member must have condition *n* (group cohesiveness and solidarity) in order to maintain state *k* (proper working order).
3. Phenomenon *j* (death of a member) has negative effects on condition *n* (group cohesiveness and solidarity).
4. If characteristic *c* (mourning and other acts of bereavement) were present in system *s* at time *t,* then *c* would counter the effect of *j* and condition *n* would be satisfied.
5. Therefore, statements 1 through 4 explain why characteristic *c* is present in system *s* at time *t.*

Several observations on the morphology (structure) of Functional Model 1 (FM1) are apparent. First, FM1 shows that functional explanations belong to a class of explanations called *homeostatic* or *equilibrating.* FM1 suggests that there are certain preferred states in the system (e.g., survival) and that if the existence of these preferred states is threatened (e.g., by a death), the system will adopt certain mechanisms to return to these preferred states, Thus, FM1 is an equilibrating model of explanation.

Second, the FM1 explanatory structure incorporates certain laws or lawlike statements. Note the verb "must have" in statement 2, the phrase "has negative effects on" in statement 3, and the verb "would counter" in statement 4. All of these statements can be construed as having essentially lawlike form (see Chapter 5). Statement 5, the explanandum, is thus derived from statements 1 through 4 (the explanans) in precisely the same way as in the D-N, D-S, and I-S explanatory models. *Therefore, FM1 is neither a fundamentally different kind of explanation nor a different methodology. To the extent that it is an explanation at all (see next paragraph), FM1 is a special case of deductive-nomological or statistical explanation where certain of the lawlike statements involve homeostatic mechanisms.*

Third, as Hempel (1959) has observed, the explanandum (statement 5) in FM1 is not

a logical consequence of the explanans (statements 1 through 4). Statement 4 essentially provides that *if c* were present, then condition *n* would be satisfied; that is, *c* is sufficient for *n.* However, what is required is a statement of the following variety: *if n* is to be satisfied, then *c* must be present; that is, *c* must be necessary or indispensable for *n.* Logicians would refer to this logical error as affirming the consequent (Salmon 1963, p. 27). This fallacy can be illustrated by the following incorrect syllogism:

All people are mortal.
Fido is mortal.
Fido is a person.

Note that the preceding syllogism and FM1 have the same structure. Therefore, the host of functional explanations that have the basic structure of FM1 are logically false—the premises do not imply the conclusion.

Because there are almost no circumstances where a characteristic *c* is functionally indispensable for condition *n,* can a form of functional explanation by salvaged? Is it possible to reconstruct a functional model that both captures the essence of functionalism and, at the same time, is logically correct? Functional Model 2 (FM2) attempts to do just that. Note that although statements 1 through 3 are identical to those of FM1, statements 4 and 5 are different.

Functional Model 2

1. At some time *t,* a system *s* (a primitive society) is in state *k* (proper working order).
2. The class of systems *S* (primitive societies) of which *s* is a member must have condition *n* (group cohesiveness and solidarity) in order to maintain state *k* (proper working order).
3. Phenomenon *j* (death of a member) has negative effects on condition *n* (group cohesiveness and solidarity).
4. Set *C* is the set of all sufficient items that, if any one were present in the system *s,* would counter phenomenon *j* and maintain condition *n* and thus system state *k.*
5. Therefore, statements 1 through 4 explain why some item in set *C* will be in system *s* at time *t.*

A functional explanation such as FM2 can now predict only that some item in the set *C* will occur. When sociologists Merton (1968, p. 106) and Parsons (1949, p. 58) discuss *functional equivalents,* they are in our terms exploring the nature of set *C.* Thus salvaged, constructing sound functionalist explanations, though such constructions constitute valuable contributions to science, is no small task. The job of identifying the complete set of functional alternatives and then tying the set into functionalist lawlike generalizations is seldom achieved. All too often, functional explanations of phenomena degenerate into one or more of the following: (1) *ad hoc ex post* rationalizations of why some phenomenon

has occurred, (2) pseudo-explanations that are empirically empty, (3) logically fallacious explanations, or (4) hopelessly circular explanations, such as:

1. Why does *X* do *Y?*
2. Because *X* has goal *J,* and
3. *Y* leads to the satisfaction of goal *J.*
4. Therefore, *X* does *Y.*
5. How do you know *X* has goal *J?*
6. Because *X* does *Y!*

These observations have led many analysts of functionalism to believe that the major importance of functionalism lies not in the context of justification but in the context of discovery (Kaplan 1964, p. 365; Rudner 1966, p. 109; Hempel 1959).

3.7.4 Functionalism in the Context of Discovery

The claim that functionalism has a unique *methodology* implies that the *logical apparatus* for confirming or falsifying functional explanations, theories, and laws is distinct from the logical apparatus used in other branches of scientific inquiry. As discussed in Chapter 1, the methodology (as contrasted with the techniques) of a discipline concerns the very bases or criteria on which to test the truth content of the discipline's claims about knowledge. On the other hand, the assertion that the importance of functionalism for the social sciences lies primarily in the context of discovery implies that functionalism may have heuristic value to scholars in searching for fundamental relationships among social phenomena. Presumably, Robert Bartels was suggesting that marketers adopt a functionalist "set" in his "theory of social initiative."

> Different societies attain similar ends (in relative measure) by different means. The level of technology, the values of the group or nation, even the relative importance attached to economic, intellectual, religious, or leisure activity are factors which must be considered in interpreting the marketing process and institution of a people. *Ecological orientation, in other words, is the starting point in marketing analysis.* (Bartels 1968, p. 32, italics added)

Under what circumstances might adopting a functionalist perspective be desirable for a researcher? Arthur Stinchcombe (1968, p. 80) has proposed these criteria, "Whenever we find *uniformity of the consequences* of action but *great variety of the behavior causing those consequences,* a functional explanation in which the consequences serve as causes is suggested." He (1968, p. 82) has further proposed several situations in which the researcher should consider functional explanations: (1) If, when subjects experience increased difficulty in achieving their goals, they increase their activity, functional explanations are indicated. (2) If a variety of explanations or purposes, or inadequate and

inconsistent purposes, are offered by people behaving to explain their behavior, a functional explanation is indicated. (3) If it is known that some causal process is operating that selects patterns of behavior according to their consequences, a functional explanation is indicated. That is, when we know that processes are selecting out certain functional behavior, it is strategic to look for those functions in any bit of behavior that we find in that selective context.

In the context of discovery, the value of functionalism is primarily an empirical question: Is it likely that adopting a particular perspective or mode of exploration will lead to the discovery of new knowledge? The study of scientific discovery, needless to say, is extraordinarily important for understanding science. If we are to evaluate the scientific worth of functionalism on the basis of the quantity and quality of scientific knowledge that a functionalist "set" generates, then the jury on functionalism is still out in both marketing and the other social sciences. As Mahner and Bunge put it:

> To conclude, social functionalism can be barren or fruitful. It will be barren if it only restates Doctor Pangloss's thesis that we live in the best of all possible worlds: that in which everyone maximizes his or her expected utilities. But it will be fruitful if it analyzes social systems and attempts to discover what makes them tick—that is, their mechanisms. (Mahner and Bunge 2001, p. 90)

3.8 SUMMARY AND CONCLUSIONS

Explanations play a crucial role in scientific inquiry. A major task of science is to explain the phenomena that constitute its basic subject matter. In general, explanations are scientific answers to *why* questions. Any proposed explanation of a phenomenon must at least (1) show that somehow the phenomenon was expected to occur, (2) be intersubjectively certifiable, and (3) have empirical content. Generalized procedures or structures that purport to show how phenomena can be explained are called explanatory models. Of the six purportedly explanatory models that have been examined, only the deductive-nomological (D-N), deductive-statistical (D-S), and inductive-statistical (I-S) models meet the criteria for satisfactory explanations. All three kinds of explanations, it should be noted, should contain causal mechanisms, entities with causal powers, or causal laws in their explanans. The pattern model (P-M) fails the intersubjectively certifiable criterion, and functionalist explanations, to the extent that they are satisfactory explanations at all, are simply special cases of deductive-nomological explanations or statistical explanations. The greatest value of functionalism probably lies in the context of discovery rather than justification.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Zaltman, Pinson, and Angelmar (1973)—drawing upon Harvey (1969)—suggest two alternative routes to scientific explanation—one by Bacon and one by Harvey. The "Baconian" route has the following order: (1) perceptual ex-

periences; (2) unordered facts; (3) definition, classification, measurement; (4) ordered facts; (5) inductive generalizations; (6) laws and theory construction; and (7) explanation.

The "Harvey" route has this order: (1) perceptual experiences; (2) image of real-world structure; (3) *a priori* model; (4) hypotheses; (5) experimental design; (6) data; (7) verification procedures; (8) laws and theory construction; and (9) explanation. Both (7) and (8) feed back into (2).

Are these two different *models* of explanation? What are the essential differences between these models? Do the models fall within the scope of the logic of justification or the logic of discovery? Which model is superior? Why?

2. Find three other definitions of the term *model in* the marketing literature. How do the perspectives of these definitions differ from the perspective presented here? Would the other definitions allow someone to distinguish among models and laws, theories, explanations, and hypotheses? Would a road map be a *model* using these other definitions? If not, would you rather choose to declare (1) that a road map is not a model or (2) that the definition of the term *model* is inadequate? Evaluate the usefulness of the varying perspectives on models.
3. How does an explanation of some phenomenon differ from an explanatory model?
4. What phenomena does marketing science seek to explain? Assess the current "state of the art" of our ability to explain each phenomenon you identify. Which phenomena do we seem to be making the most progress in explaining? Which phenomena the least? Why?
5. Find an explanation of some phenomenon in the marketing literature. Evaluate the nature, adequacy, and usefulness of the explanation.
6. Succinctly summarize the nature of the D-N, D-S, and I-S models of explanation. What are the essential differences among these models? Does *induction* play any role at all in the D-N and D-S models?
7. The notion of *Verstehen* suggests that the only way to understand and explain some human process is to be a *participant* in that process (Abel 1948). For example, only women could (or should) teach women's studies courses. Only marketing practitioners could possibly understand marketing phenomena. Would such a procedure and such knowledge pass the *intersubjectively certifiable* criterion?
8. Are explanations in marketing a *means* or an *end,* or both? That is, *why* are we interested in "why" questions? Would your answer depend on who you are, that is, a practitioner, student, or academician?
9. From the works of Wroe Alderson (1957, 1965), select what purports to be a *functionalist* explanation of some marketing phenomenon. Evaluate the adequacy of this explanation.
10. How does requiring explanations to be *falsifiable* differ from requiring them to be *confirmable?* Why are confirmability and falsifiability important?

11. In analyzing the work of Wroe Alderson, Hostiuck and Kurtz state:

> Functionalism, for example, is mostly an analytical-conceptual schema. But, there is increasing evidence that it may also qualify as a theoretical structure. It is a "systematically related set of statements" that certainly includes "some lawlike generalizations." The aspects of "empirical testing" may admittedly have to wait for further developments in qualitative analysis. (Hostiuck and Kurtz 1973, p. 150)

Evaluate their conclusion.

12. It is often suggested that to explain a phenomenon is to make the "unfamiliar become familiar." Is *making familiar* a necessary condition for explanation? Is it sufficient? Is it desirable?
13. Louch (1979, p. 284) proposes that "when we say that a man . . . kills his father because he has been cut out of the will . . . we are offering [an explanation of a case that does] not require the support of general or theoretical statements." How satisfactory is this explanation, given the fact that only an infinitesimal fraction of all children who are cut out of their parents' wills subsequently kill their parents? What were your implicit or explicit criteria for "satisfactory"?

NOTES

1. Lambert and Brittan (1970, p. 26) have an excellent discussion of normative criteria for evaluating explanations.

2. The analysis that follows in this section draws to varying degrees on the writings of Ernest Nagel (1961, pp. 520–34), Richard Rudner (1966, pp. 84–111), and Carl Hempel (1959).

4

EXPLANATION: ISSUES AND ASPECTS

> Whenever we propose a solution to a problem we ought to try as hard as we can to overthrow our solution rather than defend it. Few of us, unfortunately, practice this precept; but other people, fortunately, will supply the criticism for us if we fail to supply it ourselves.
>
> —*Karl R. Popper*

The purpose of this chapter is to explore certain issues in explanation and to evaluate several marketing explanations. The issues include the interrelationships among prediction, explanation, and scientific understanding. The nature of causal explanations will next be explored, followed by an investigation of the ways in which explanations are incomplete. The fundamental explananda of marketing science are next proposed. The chapter concludes with a formal analysis of four different explanations in marketing: a product life cycle explanation, a consumer behavior explanation, a price discrimination explanation, and a wheel of retailing explanation.

4.1 EXPLANATION, PREDICTION, AND RETRODICTION

Does having an acceptable explanation for a phenomenon imply that we could have predicted it? That is, does being able to explain market share imply that we could have predicted market share? Conversely, does being able to predict market share imply that we can explain it? The issue has relevance to marketing because many marketing theorists apparently believe that explanation and prediction are not systematically interrelated. Robert Bartels (1970, p. 9) suggests, "Explanation, however, rather than prediction is generally the objective of theory in the social and behavioral sciences." Likewise, Luck, Wales, and Taylor (1970, p. 4) believe that explanation does not imply prediction. However, Green, Tull, and Albaum (1988, p. 4) maintain that "the functions of marketing research include *description* and *explanation* (which are necessary for *understanding*), *prediction,* and *evaluation* . . . [which are] necessary for effective (1) *planning* of future marketing activity, (2) *control* of marketing operations in the present, and (3) *evaluation* of marketing results."

Hempel (1965a, p. 367) refers to the issue at hand as the thesis of structural identity

or structural symmetry: "(1) Every adequate explanation is potentially a prediction, and; (2) every adequate prediction is potentially an explanation."

Let's explore each half of the thesis, in turn. (See also Hunt 2003a, pp. 74–76.)

4.1.1 Explanations as Potential Predictions

To writers such as May Brodbeck and Karl Popper, the thesis of structural identity seems to be not an issue but a fact. First, Brodbeck:

> Prediction has the same logical form as explanation. In predicting something as yet unknown, we deductively infer it from particular facts and laws that are already known. This deductive tautological connection among statements also shows why observations confirm or refute hypotheses. If a prediction inferred from a set of premises turns out to be true, then the generalization is further confirmed. If it turns out to be false, then we know that either the generalization or the individual fact used in making the prediction *must* be false. Because we are less likely to be mistaken about individual facts, in most cases the failure of a prediction means that the generalization is thereby refuted.
>
> It makes no difference whether the premises are statistical or deterministic, as nonstatistical generalizations are called. If they are deterministic, we may predict an individual event; if they are statistical, only statements about classes of events may be either explained or predicted. (Brodbeck 1968, pp. 9–10)

Popper (1960, p. 133) comes to similar conclusions concerning the basic way in which explanations, predictions, and testing are interrelated. He suggests that explanations, predictions, and testing do not differ in logical structure. Rather, the differences are related to what is considered *known* or *given* and what is considered *unknown* or to be *uncovered.* With *explanation,* certain phenomena are known, and what is to be uncovered are the laws and theories that can explain the phenomena. For example, if we know that many consumers are brand loyal, our task in marketing may be to *explain* their brand loyalty by uncovering certain laws and theories. With *prediction,* certain laws and theories are known, and we wish to apply our scientific knowledge by predicting certain phenomena. For example, if certain laws and theories concerning brand loyalty are known, marketing practitioners may apply these constructions to predict the characteristics of consumers who might be brand loyal to their particular products. Finally, with *testing,* certain laws and theories are proposed, and we compare the actual phenomena with the phenomena that the laws and theories predicted would occur. To the extent that the laws and theories predict correctly or incorrectly (i.e., the test results are positive or negative), we have corroborative or noncorroborative evidence that the real world is actually constructed as our laws and theories would suggest. (See sections 11.2.1 and 12.6.)

To Brodbeck and Popper, every explanation implies an *ex post facto* prediction. Thus, the explanation-implies-prediction argument is:

The E-P Argument

a. The explanation of phenomenon X at time t_n implies that
b. *if* person A had been present at time t_{n-2}, and
c. *if* A had known circumstances $C_1, C_2, C_3, \ldots C_k$, and
d. *if* A had applied laws $L_1, L_2, L_3, \ldots L_j$,
e. *then* A could have predicted that X would occur at time t_{n-1}.
f. Therefore, all explanations are potentially predictive.

Careful examination of the premises (statements [a] through [d]) of the E-P argument shows them to be true. All of the models that do explain phenomena—the D-N, D-S, and I-S models—are consistent with the premises. Models that do *not* explain phenomena, such as the pattern model, are *not* consistent with the premises of the E-P argument. Finally, the conclusions (statements [e] and [f]) of the E-P argument do seem to be logically implied by the premises. Therefore, we must conclude that the E-P argument is true: every adequate explanation is potentially a prediction. That is, if we could not have predicted the occurrence of a phenomenon, we cannot now satisfactorily explain it.

The preceding discussion provides insight into a common circumstance in the social sciences and marketing. Someone creates a model or theory. A critic evaluates the model and concludes that it is not empirically testable because the model has no predictive capacity. The defender of the model then claims, "The fact that my model makes no predictions is irrelevant because my purpose is to *explain* the phenomenon, not predict it!" In light of this analysis, the defender's defense becomes vacuous because *all adequate explanations must have predictive capacity.*

Dubin's Power Paradox

Dubin (1969) struggles at length with his "power paradox," and though he refers to *understanding* rather than *explanation,* the issue is similar to the thesis of structural identity. He (1969, p. 17) states the power paradox in terms of a question: "Why is it that we can create models of social behavior that are powerful in contributing to understanding, without providing, at the same time, precision in prediction?" Because Dubin uses the term *model* as synonymous with our term *theory,* we can reconstruct the premises and conclusions of Dubin's paradox for analysis:

a. Many social science theories do not provide precision in prediction.
b. Many of these same theories contribute powerfully to understanding.
c. The preceding seems paradoxical.

Many marketing theorists believe that their theories and models contribute to the understanding of marketing phenomena, while, at the same time, admitting the lack of predictive power of their theories. Bettman and Jones (1972) evaluated several models of consumer behavior, including those proposed by Farley, Ring, and Nicosia. After ob-

serving the lack of predictive power of many of these models, they conclude, "The main use of these models may lie in attempting to understand behavior rather than predict it" (1972, p. 556). Can we understand behavior without being able to predict it? Perhaps an analysis of Dubin's power paradox will shed light on the issue.

Because paradoxes are created by humans, not nature, we should explore whether the premises ([a] and [b]) of the paradox are true. Even a casual observer of the social sciences would conclude that (a) is true; many social science and marketing models and theories do not predict. However, is (b) true? Do these nonpredictive models contribute powerfully to understanding? The answer depends on the meaning of the term *understanding.* Bunge (1967b, p. 31) suggests that two usages are common, (1) *intuitive understanding* and (2) *scientific understanding.* Intuitive understanding of a phenomenon suggests that we are *psychologically comfortable* or *familiar* with the phenomenon. Scientific understanding of a phenomenon implies, at least, that we can *scientifically explain* the phenomenon. Now, intuitive understanding does not imply scientific understanding; many people are psychologically comfortable with a rainbow but cannot explain how rainbows occur. Also, intuitive understanding is inescapably individual in kind. Phenomena that may be psychologically comfortable to one person (e.g., thunder) may be psychologically uncomfortable to another person. Therefore, just as we found with the pattern model (section 3.6), intuitive understanding fails the criterion of being intersubjectively confirmable and cannot be considered as a part of scientific knowledge.

Clearly, Dubin does not mean *understanding* in the intuitive sense, but rather, in the scientific sense. (Dubin's work, after all, is on the philosophy of social science.) However, because scientific understanding (in order to be intersubjectively confirmable in the context of section 3.2) implies explanatory power, and because explanatory power implies being potentially predictive, *the power paradox disappears. Premise* (b) *is false; marketing and other social science theories that do not predict do not make powerful contributions to (scientific) understanding.*

Five Levels of Prediction

What does it mean, however, to claim that a theory "does not predict" or that it "lacks precision" in prediction? Consider, for example, the following five levels of prediction:

1. X and Y may or may not be related.
2. X and Y are related.
3. X and Y are positively (or negatively) related.
4. X and Y are related at $r = k \pm e$.
5. X and Y are related, in the presence of $M, N, \ldots$, at $r = j \pm e$.

Clearly, theories that predict at level (5) have greater precision than those at level (4), and so forth. That is, the five levels (of which there obviously can be others) are ordinally ranked. Equally clearly, most social science researchers would be pleased with level (5) and view level (1) with disdain. However, what about levels (2) through (4)?

Meehl (1990, p. 204) argues that levels (2) and (3) are unacceptable because "everything correlates to some extent with everything else." Similarly, Bass (1995) maintains that (1) and (3) are unacceptable. The view here is that, though levels (4) and (5) are worthy goals, level (3), but not (2), has sufficient precision to warrant a claim of increasing scientific understanding. That is, the belief that "the world is correlated +0.2" (as many scholars profess) has sufficient merit to render all level (2) predictions as unsatisfactory, but research that finds that "*X* and *Y* are correlated –0.2" warrants a claim of increasing scientific understanding.

4.1.2 Predictions as Potential Explanations

The second half of the thesis of structural identity asserts that every adequate prediction is potentially an explanation. The case favoring the second half of the structural identity thesis usually rests on this kind of argument:

The P-E Argument

a. The prediction that phenomenon *X* will occur at time t_{n+1} means that
b. *if* person *A* observes certain circumstances C_1, C_2, C_3, . . . C_k at time t_n, and
c. *if A* applies laws L_1, L_2, L_3, . . . L_j at time t_n,
d. *then A* can predict that *X* will occur at time t_{n+1}.
e. Furthermore, if *A* waits until time t_{n+1}, and
f. *if A* observes that phenomenon *X* occurs,
g. *then A* can explain phenomenon *X* by reference to statements (b) and (c).
h. Therefore, all adequate predictions are potential explanations.

Many people who readily accept the explanation-implies-prediction argument totally reject the prediction-implies-explanation argument. Critics claim that they can predict the business cycle by measuring the length of hemlines on women's skirts, but no reasonable person would argue that women's hemlines *explain* business cycles. Similarly, one can accurately predict the birthrate in Oslo, Norway, by observing the temperatures of the sidewalks in Madison, Wisconsin, but reasonable people would not claim to *explain* the former by the latter.[1]

The critic's claim can be supported and the P-E argument broken by attacking statement (c), "if *A* applies laws L_1, L_2, L_3, . . . L_j at time t_n." Are there statements that can be used to make accurate predictions but that are not *laws*? Because the subject of "laws" is the topic for Chapter 5, the treatment here will be extremely brief. However, consider the following statement: "All desks in room 201 that have the initials S.D.H. also have the initials S.E." The statement has universal form (all *X* are *Y*) and can generate a prediction of a sort; that is, if you go to room 201, and if you find any desk with S.D.H. on it, you will also find S.E. on it. Nevertheless, such statements are not properly considered *laws*, but rather, are *accidental generalizations.* Therefore, because accidental generalizations have predictive capacity, and because explanations must contain laws, and because ac-

cidental generalizations are not laws, *the P-E argument is false.* Contrary to the thesis of structural identity, *all adequate (accurate) predictions are not potential explanations.*

The conclusion that the P-E argument, the second subthesis of structural symmetry, is false is consistent with scientific realism—the philosophy underlying this monograph—but contrary to logical empiricism. For example, Hempel (1965a) reviews criticisms similar to those developed here, and, though acknowledging their force, states simply that the P-E argument must "be regarded here as an open question" (p. 376). The reason why Hempel and other logical empiricists could not deny the second subthesis in the symmetry argument was that they were firmly committed to the logical positivist view that the concepts "cause" and "causal" were metaphysical and superfluous to science, that is, they were firmly committed to the Humean view of causality. For example, Brodbeck (1962, p. 250) defended the thesis of structural symmetry by noting that critics had adopted the "causal idiom" and that the truth-content of "statements like '*C* is the cause of *E*' is problematic."

4.1.3 Are Explanations and Predictions Potential Retrodictions?

Although the terms *explanation* and *prediction* are familiar words in common English, the term *retrodiction* belongs strictly to the scientific vocabulary. The term *retrodiction,* as used by Ryle (1949, p. 124), implies making inferences about the past on the basis of present observations. Hempel (1965d, p. 173) and Hanson (1963, p. 193) use the term *postdiction* to imply the same procedure.

Note the significant difference between retrodiction and both explanation and prediction. With explanation, the phenomena that do the *explaining* occur in time before the phenomenon to be explained. Likewise, with prediction, the phenomena that do the *predicting* antecede the phenomenon to be predicted. In contrast, with retrodiction, the phenomena that accomplish the *retrodicting* occur *after* the phenomenon to be retrodicted.

An example of retrodiction shows how it differs from both explanation and prediction. Carbon-14 dating of objects classically illustrates the retrodiction of when an animal died. By measuring the amount of carbon 14 contained in the bone structure of a deceased animal and by employing certain (statistical) laws concerning radiocarbon decay, the date when the animal died can be *retrodicted* within an accuracy of ±50 years (Blanchard et al. 1972, p. 315). Thus, retrodiction has this formal structure:

a. Certain circumstances $C_1, C_2, C_3, \ldots C_k$ are observed (e.g., the amount of carbon 14 in the bones of a deceased animal), and
b. certain laws $L_1, L_2, L_3, \ldots L_j$ (e.g., laws governing radiocarbon decay) are then applied to
c. *retrodict* the occurrence of some past phenomenon (e.g., date of death) that anteceded $C_1, C_2, C_3, \ldots C_k$.

Must we be able to retrodict in order adequately to explain or predict? Hanson (1963, p. 193) argues affirmatively: "Every prediction, if inferentially respectable, must possess

a corresponding postdiction [retrodiction]." Unfortunately, Hanson uses as his model for explanatory adequacy certain models such as Newtonian mechanics that are deterministic in a very strong sense. That is, the relationships in the models are reversible with respect to their time variables. Hanson confuses necessary conditions with sufficient conditions. Many very useful explanations need only show that condition $C_1, C_2, C_3, \ldots C_k$ are *sufficient* to predict *X*. Retrodiction requires that conditions $C_1, C_2, C_3, \ldots C_k$ were *necessary* for *X* to occur; that is, *X* could not have occurred without $C_1, C_2, C_3, \ldots C_k$ also occurring. It is one thing to be able to predict that certain circumstances will be sufficient to ensure the early demise of a new product. It is an entirely different thing to be able to take a dead product and retrodict the circumstances that killed it. Although retrodiction would be a desirable characteristic of any model, neither explanatory nor predictive adequacy implies the ability to retrodict.

4.2 CAUSAL EXPLANATIONS

Are all explanations *causal?* Can an explanation be adequate and yet noncausal? The paradox of causation: although the term *cause* is widely used in both everyday language and scientific language, for centuries the notion of *causality* has steadfastly resisted definitive explication.

In everyday language we ask, "Why did the window break?" And we are satisfied with the explanation: "*Because* little Jimmy threw a ball into it!" Also, "What caused the house to burn down?" "Little Jimmy caused it by playing with matches." Likewise, physical scientists remain comfortable with assertions such as, "The force of gravity *causes* the missile to fall to the earth" and "The rays of the sun *cause* the ice to melt." Marketers seem comfortable with statements such as, "Lack of promotion caused the product to fail" and "High prices caused the recent sales decline." However comfortable both laymen and scientists are with the terms *cause* and *causal explanation,* the next section shows how methodologically troublesome the notion of causality has been.

4.2.1 The Notion of Causality

Exactly what is meant by the assertion "*X* causes *Y*," and precisely what kinds of evidence can be gathered to support the assertion? In particular, the fundamental problem of causation is: *What evidence can empirically or logically separate the assertion* "X *causes* Y" *from the assertion* "X *and* Y *occur regularly in the same pattern*"? Perhaps the best place to start would be with a review of some historical perspectives on causation.

Most of the perspectives on causation invoke two common themes. To illustrate the first theme, in 1840 William Whewell (1840/1968, p. 67) suggested that "by cause we mean some quality, power, or efficacy by which a state of things produces a second state." He then espoused his version of the law of universal causation:

> We assert that "Every event must have a cause": and this proposition we know to be true not only probably, and generally, and as far as we can see; but we cannot

> suppose it to be false in any single instance. We are as certain of it as of the truths of arithmetic or geometry. We cannot doubt that it must apply to all events past and future, in every part of the universe, just as truly as to those occurrences which we have ourselves observed. (Whewell 1840/1968, p. 67)

Although not subscribing to the law of universal causation, modern-day adherents to scientific realism believe that causation must involve the powers and capacities of things. As discussed in Chapter 1, scientific realism holds that "the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, p. 26). The realists Harré and Madden discuss causal powers:

> The proper analysis of the ascription of a power to a thing or material (and, with some qualifications, also to a person) is this:
>
> "*X* has the power to *A*" means "*X* will (or can) do *A,* in the appropriate conditions, *in virtue of its intrinsic nature.*"
>
> In ascribing powers to people, "can" must be substituted for "will." Whether he will or not has to be explained by considerations other than the extrinsic conditions for action. It is the reference to the nature of the potent thing that marks the difference between the ascription of powers and of mere dispositions. (Harré and Madden 1975, p. 86)

Harré and Madden then maintain that their analysis leads to two important points:

> 1. To ascribe a power to a thing or material is to say something specific about what it *will* or *can do,* but it is not to assert any specific hypotheses about the nature of that thing. To ascribe a power to a thing asserts only that it can do what it does in virtue of its nature, whatever that is. It leaves open the question of the exact specification of the nature or constitution in virtue of which the thing, person, or material has the power. It leaves it open to be discovered by later empirical investigation, should that prove to be possible.
> 2. But to ascribe a power is to say that what the thing or material does or can do is to be understood as brought about not just by the stimuli to which it may be subject or the conditions which it finds itself in, i.e., by extrinsic conditions, but in some measure by the nature or constitution of that thing or material, i.e., by intrinsic conditions. (In a sense the ascription of a power is a schema for an explanation of the manifestation of the power.) (Harré and Madden 1975, p. 87)

William Stanley Jevons (1835–1882) illustrates a second theme: "A cause is defined as the necessary or invariable antecedent of an event, so that when the cause exists, the effect will also exist or soon follow" (1968, p. 140). A modern version of this kind of

analysis is the "INUS condition" proposed by Mackie (1965). INUS stands for "insufficient-necessary-unnecessary-sufficient." He (1965, p. 257) proposes that "*A* is an INUS [causal] condition of a result *P* if and only if, for some *X* and some *Y,* (*AX* or *Y*) is a necessary and sufficient condition of *P,* but *A* is not a sufficient condition of *P* and *X* is not a sufficient condition of *P.*" That is, a cause may be an insufficient but necessary part of a condition that is itself unnecessary but sufficient for the result. As Sosa (1975, p. 4) has pointed out, an INUS condition differs little from a condition that is, *ceteris paribus,* sufficient. Bagozzi proposes that many of the causal relations proposed by marketers are INUS conditions and offers the following example:

> As an example, let us examine the claim sometimes made by marketers that brand image (measured by the brand name) affects the perception of quality. When marketers make this claim they are not saying that the brand image is a necessary cause or condition for the attribution of quality. One may judge a product as high or low in quality without knowing the brand. Similarly, marketers are not claiming that the brand image is sufficient for the perception of quality since one must at least attend to, be aware of, and evaluate the brand name before such an attribution can be made. Rather, the brand image may be regarded as an INUS condition in that it is an insufficient but necessary part of a condition that is itself unnecessary but sufficient for the result. Many of the causal relations investigated by marketers are of this sort. (Bagozzi 1980, pp. 17–18)

Therefore, by "*X* causes *Y*" writers usually mean that (1) "*X* has the power to produce *Y*," or (2) "*X* is an invariable antecedent of *Y*," or (3) "*X* is *necessary* for *Y*," or (4) "*X* is *sufficient* for *Y*." But how can we *know* that "*X* has the power to produce *Y*" or any of the other conceptualizations of cause? What are the kinds of evidence or criteria? So far, we have synonyms for *cause* but no objective criteria. In short, what are the necessary and sufficient conditions to enable one to label a relationship *causal?* Many philosophers of science have attempted to generate the necessary and sufficient conditions for causality, including J.S. Mill with his canons of induction.

The logical positivists and logical empiricists believed that "cause" was a metaphysical concept that could (and should) be avoided. Thus, following the position of David Hume, Kyburg (1968, p. 236) questions whether the concept of "cause" is "of scientific interest," and Brodbeck (1968, p. 672) contends that "as we learn more about the laws of temporal processes, the notion of cause tends to be eliminated." Similarly, Dubin states:

> Empirically relevant theory in the social sciences is built upon an acceptance of the notion of relationship rather than the notion of causality. . . . The *operations* by which we test [a] relationship between theoretically predicted values and empirical values differ in *absolutely no respect* whether we label the relationships among units of a model as *laws of interaction* or as *causal laws.* . . . The temptation is strong to interpret sequential laws of interaction as though they were causal in structure. This gratuitous assumption of causality adds nothing to social science, however much it satisfies psychological needs. (Dubin 1969, pp. 91, 94, 106)

Cause and the Philosophers' Fallacy of High Redefinition

So the term *causation* is firmly ensconced in common English and the technical languages of both the social and physical sciences. At the same time, sufficient conditions to apply the term (at least with great confidence) do not exist. Therefore, Dubin and other "Humeans" suggest throwing the term out. To do so, I argue, would be to commit the "philosophers' fallacy of high redefinition." First used by Edwards (1951) in his analysis of Bertrand Russell's doubts on induction and used later by Harré (1986) and me (Hunt 1990b), this is the fallacy of redefining a concept (e.g., scientific knowledge, truth, falsity, objectivity, causality, or progress) in such a manner (e.g., must be "known with certainty" or "known with probability *p*") that the concept cannot be realized, and then lapsing into relativism, nihilism, or skepticism. Instances of the philosophers' fallacy abound in marketing. For example, Anderson (1986, p. 163) claims that theories cannot be falsified because their falsity "can never be known with certainty." Furthermore, "science is a process without a goal" because "there is no *guarantee* that it progresses toward anything—least of all toward 'truth'" (Anderson 1983, p. 22, emphasis added). Popper's theory for relativists is faulty because "it is impossible to *conclusively* refute a theory" (Anderson 1983, p. 21; emphasis added). Also, because science cannot conclusively show that it produces "objective absolute meanings," then "all meanings [in science] . . . are subjectively determined," and, therefore, "science is subjective" (Peter and Olson 1983, pp. 120–21). Although proponents of such views put them forth as supposedly implying relativism, subjectivism, and irrationalism in science, scientific realism views such innocuous claims as implying nothing other than that science is fallible.

4.2.2 Evidence for Causation

Much better than throwing "causality" out or lapsing into relativism would be an attempt to set forth certain *necessary* or *minimal* conditions for inferring causality. This procedure could at least point out some patently noncausal explanations. *Pragmatically speaking, we may refer to causal explanations as those explanations that employ nonspurious, theoretically supported, sequential laws in their explanans.* Using the preceding definition as a guide, we can identify four criteria for classifying an explanation as causal.

1. *Temporal sequentiality.* If changes in factor *A* are to be used to explain causally factor *B,* then the occurrence of the changes in *A* must precede in time the occurrence of changes in *B.* That is, *A* and *B* must be related by a *law of successon.* Not all laws are laws of succession (see Chapter 6). Many laws, such as Boyle's law of gases, are *laws of atemporal coexistence;* they show a relationship that must be realized and contain no time variable.

The notion of temporal sequentiality suggests that if the introduction of additional salespeople is to be considered a *cause* of increased sales, then the salespeople must be added before the observed increase in sales. Similarly, if a new volume discount policy is to be considered a *cause* of better channel relations, then the discount policy must pre-

cede in time the improved channel relations. However, the direction of the sequentiality of phenomena is not always intuitively obvious. Though it was presumed for years that attitude changes preceded behavior changes, research shows that behavior changes often precede attitude changes (Fishbein and Ajzen 1972; Ray 1973).

2. *Associative variation.* If factor *A* is a cause of factor *B,* then changes in the level or presence of factor *A* must be systematically associated with changes in the level or presence of factor *B.* Although it is true that "correlation does not imply causation," the observation that two factors are systematically associated (correlation being a measure of the degree of this association) is evidence *in support of* causation. Conversely, the *absence* of association is very strong evidence that the two factors are *not* causally related. If market shares are caused by advertising, then we could expect to find differences in market shares systematically associated with differences in the quantity or quality of advertising.

3. *Nonspurious association.* If *A* causes *B,* then there must be no factor *Z* that, if introduced into the explanation, would make the systematic association between *A* and *B* vanish. If one diligently explores for other factors, that is, other possible causes, that might possibly have resulted in changes in *B* and can find no such factors, then this evidence supports the assertion that the association between *A* and *B* is a true causal relationship rather than a spurious one.

The nonspurious criterion emphasizes the tremendous scientific value of experimental research designs, when compared with nonexperimental designs. Although definitions of "experimental design" differ, most experimental designs either systematically exclude from the research setting, or carefully monitor and control, factors other than the independent variable (the one purportedly doing the *causing*) that might influence the dependent variable (the one being *caused*). To discount the possibility that other factors cause changes in *B* becomes much more difficult in *nonexperimental* designs. Causal imputations in marketing are particularly likely to fall short on the nonspuriousness criterion because most research in marketing has traditionally relied upon nonexperimental designs—and for good reason.

Unfortunately, many important research questions in marketing simply do not lend themselves to an experimental manipulation of the independent variables (and, hence, to stringent tests for nonspuriousness). Indeed, a good case can be made that most of the truly important research questions in marketing do not lend themselves to formal experiments, to mathematical modeling, or even to the analysis of archival or scanner data. It appears that most of the truly important research questions in marketing—especially strategic, in contrast to tactical, issues—require survey data or a qualitative methodology. Therefore, it is extremely unfortunate that the norms employed by many of the editors and reviewers of marketing's major journals continue to discriminate systematically against studies based on survey data and qualitative methodologies. The excessively high publication hurdles that researchers face when using survey and qualitative methodologies have increasingly pushed marketing researchers into research designs that, by their very nature, cannot address some of the most important research questions in marketing. Consequently, the discipline suffers.

4. *Theoretical support.* The fourth criterion suggests that well-conformed theories can be used to support the assertion that *A* causes *B.* That is, if *A* causes *B* is consistent with theory *X,* and if theory *X* has been successfully used to explain other phenomena, then theory *X* provides theoretical support for the assertion that *A* causes *B.*

Suppose someone says, "The length of women's skirts *causes* our market share to rise or fall." Suppose further that an examination of the evidence reveals (1) the general rising of women's skirts has preceded the rising of market shares, (2) the correlation between the two factors has been very strong, and (3) no third factor can be found that makes the correlation vanish. That is, the evidence accords with the temporal sequentiality, associative variation, and nonspuriousness criteria. Clearly, most people would *still* view as ridiculous the claim that "the length of women's skirts *causes* market share." The causal claim in question is ridiculous precisely because it has no theoretical support; that is, it does not fit into all the other things we know about factors related to, associated with, the lengths of skirts and market share.

The criterion of theoretical support must not be pushed too far. Poor Galileo paid a heavy price because his notions concerning the movements of celestial bodies did not fit with the theological belief that the Earth was the center of the universe. Also, recall that Einstein's theory of relativity did not fit with Newtonian mechanics. Nevertheless, the burden of proof must lie with the proposer of new, nonfitting truth-claims.

Many marketing researchers advocate the so-called *Granger conditions* as a means of testing for causal relationships. Jacobson and Nicosia (1981), using the techniques of Granger (1969), Sims (1972), and Pierce and Haugh (1977), explored for causal relationships between advertising and various measures of aggregate demand. Bass and Pilon (1980) used similar techniques to evaluate their time-series model of market share behavior. Granger's definition of causality suggests that a variable *X* is causally related to *Y* if we are better able to predict *Y* by using all of the available variables, including *X,* than by using the same set of variables without *X.* Using this definition, Granger, Sims, and Pierce and Haugh propose a wide range of specific tests. These tests are cross-correlational in nature and use time-series data. All of the tests use as evidence of causality what is referred to here as "temporal sequentiality" and "associative variation."

Other marketing researchers are vigorously pursuing the structural equation approach to causal modeling. Originally conceptualized by Bock and Borgman (1966) and later developed by Joreskog (1968, 1973), structural equation modeling (SEM) uses, among others, the maximum likelihood method for estimating parameters. Bagozzi (1980) introduced the approach in marketing and used it to explore for causal relationships between performance and satisfaction among industrial salespeople. Bentler (1990), Bollen (1989), Fornell (1983), Rigdon (1995), and Rigdon and Ferguson (1991), among others, develop the approach. Major advantages of SEM include the ability to control for measurement error, an enhanced ability to test the effects of experimental manipulations, the ability to test complex theoretical structures, the ability to link micro and macro perspectives, and more powerful ways to assess measure reliability and validity (MacKenzie 2001).

In conclusion, the use of the concepts *cause* and *causation* remain and should remain in marketing. Indeed, the search for true causal relationships is central to the mission of marketing science. However, we must never delude ourselves into believing that we can ever

know any causal relationship with certainty. Purportedly causal relationships are always only more or less probable, and we should always diligently explore the possibility that the relationships are actually spurious. The very essence of science is that all statements are tentative; all are subject to change and revision on the basis of future evidence.

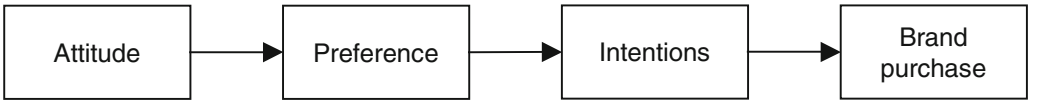
4.3 EXPLANATORY INCOMPLETENESS, EXPLANATION CHAINS, AND INFINITE REGRESS

All explanations are incomplete in a fundamental way: something is always left unexplained. Phenomenon K (e.g., the path of a celestial body) is explained by subsuming it under laws L_1 and L_2 (Newtonian mechanics). But the critic rightly complains that L_1 and L_2 are unexplained. The theorist responds that L_1 and L_2 can be explained by subsumption under L_3 and L_4 (Einstein's Special Theory of Relativity), and the critic demands the explanation chain of L_3 and L_4. The theorist stops the development of the *explanation chain* at the most basic lawlike statements known at the time (Einstein's general theory of relativity), prompting the critic to note that the explanation is still incomplete.

Since all explanatory structures involve potentially infinite regresses of the preceding variety, explanations are incomplete in this sense. Thus, the critic's point must be admitted. Fortunately, the problem poses no insurmountable conceptual barrier when placed in proper perspective. Surely, no one would seriously propose that in order to explain *anything* we must explain *everything.* Such nihilism would place ludicrous requirements on scientific explanations in light of the admitted usefulness of explanations that involve potentially infinite regresses. In addition, the admission that the most basic laws underlying explanations are left unexplained must be clearly differentiated from the assertion that the basic laws are *unsupported.* Even though the basic laws at the end of the explanation chain may be unexplained by other laws, there may well exist tremendous empirical support for the veracity of the laws.

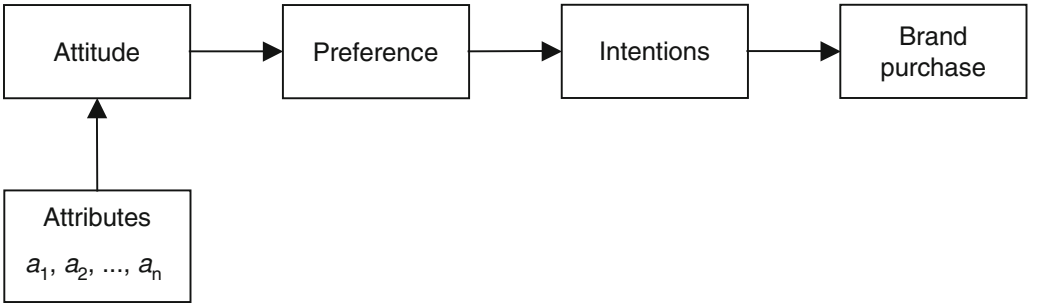
4.3.1 Marketing Explanation Chains

Explanation chains abound in marketing. For example, many studies in consumer behavior, at least implicitly, employ variants of the A-P-I-B chain:



The chain implies that attitude can be used to explain preference and preference can be used to explain intentions and, thus, intentions can be used to explain brand purchase. The chain can be stated even more simply by starting at the other end. Consumers are more likely to purchase brands they intend to buy than brands they do not intend to buy; consumers are more likely to intend to buy brands they prefer than brands they do not prefer; finally, consumers are more likely to prefer brands toward which they have a favorable attitude than brands toward which they do not have a favorable attitude.

Bunge (1967b, p. 29) has suggested that explanation chains be evaluated according to their explanatory depth. Some explanation chains remain at a shallow, superficial, or trivial level, even though they involve several stages. Other explanations seem deep, relying on profound or fundamental lawlike statements. Returning to the A-P-I-B chain, there is something intellectually disquieting about its depth. The chain seems to regress no further than a seemingly superficial level. Now consider the "multiattribute" chain:



The multiattribute chain goes one step further by showing the attributes that form attitudes. Therefore, it is deeper and more satisfactory than A-P-I-B.

How deep must an explanatory chain go to reach respectability? Even chains with the depth of A-P-I-B have found usefulness in short-range predictions (explanations) of the purchase of consumer durables (Katona 1960). Unfortunately, the A-P-I-B chain is usually of limited usefulness to practitioners because it is not deep enough to provide most marketers with guidance as to *how* and *where* to *influence* the chain. Marketers contend, "Don't tell me that people who have a favorable attitude toward my brand are likely to purchase it; tell me what factors that I control can influence attitude!" Therefore, from the marketing practitioners' perspective, the respectability of an explanatory chain is directly proportional to the guidance it gives marketers in their attempts to influence the chain. In contrast to the A-P-I-B chain, the multiattribute chain is useful to practitioners attempting to influence the chain because it specifies attributes that can be influenced.

From the marketing academician's perspective, the issue of how deep an explanatory chain must be for *scientific respectability* is also ambiguous. Such academic norms are essentially set by peer group consensus. These norms are strongly influenced by the editors and reviewing staffs of the major professional journals. Consequently, different journals have (widely) different norms.

4.4 OTHER FORMS OF EXPLANATORY INCOMPLETENESS

Besides the problem of the infinite regress of explanation chains, numerous other ways in which explanations may be incomplete have been suggested. Three in particular seem worthy of note: enthymemes, partial explanations, and what Hempel (1965a, p. 424) refers to as explanation sketches.

4.4.1 Enthymemes

Many explanations are incomplete in that they are elliptically formulated; the logician refers to these as *enthymemes* (Lambert and Brittan 1970, p. 29). To be elliptically incomplete implies that certain necessary statements or laws are skipped over or suppressed in the explanation. The person offering the explanation presumably *assumes* that the reader will either consciously or unconsciously fill in the missing statements. Most narrative explanations are elliptically formulated because both writer and reader would perceive fully articulated explanations as tedious and unnecessary. This form of incompleteness is usually harmless enough, provided the writer is certain that the reader can fill in the explanation with the appropriate statements. Refer to the structural analysis of the consumer behavior explanation later in this chapter for an illustration of an enthymeme (see section 4.7.1).

4.4.2 Partial Explanations

Often a proposed explanation is *partial* in the sense that the explanation does not explain why *k* occurred, but only that some phenomenon of type *G* occurred and that *k* is a subclass of *G.* For example, using the construct "generalized self-confidence," it might be possible to explain why a person purchased a "major brand" of gasoline but not which *specific* brand. Recall that FM2 in section 3.7.3 was a partial explanation and, in this sense, incomplete. Naturally, we would prefer all explanations to be complete, but the importance of partial explanations should not be minimized.

4.4.3 Explanation Sketches

The last form of explanatory incompleteness is suggested by the term *explanation sketch.* Here, the proposed explanation neither is *elliptically* formulated nor is it *partial* in the sense of the previous section. An explanation sketch implies that only a general outline of the explanation is offered. ("Here are some variables that might be related to brand preference.") Explanation sketches need substantial elaboration and development before they can qualify as complete explanations.

Explanation sketches probably belong more in the context of discovery than in the context of justification. They are meant to suggest fruitful areas of inquiry for researchers exploring the phenomena in question. Most explanations in both marketing and other social sciences may come closer to being explanation sketches than to being fully articulated explanations.

4.5 THE FUNDAMENTAL EXPLANANDA OF MARKETING

If the distinctive aim of science is to explain phenomena, what phenomena does marketing theory attempt to explain? That is, what are the fundamental explananda of marketing science? Alternatively, what are the fundamental "dependent variables" of marketing

science? Consistent with the perspective of most marketing theorists (Alderson 1965; Bagozzi 1974; Kotler and Levy 1969a), this writer has proposed that the basic subject matter of marketing is the exchange relationship or transaction. The discipline of marketing has its normative or "applied" side, which is not science. The positive or "basic" side houses the science of marketing. *Therefore, marketing science is the behavioral science that seeks to explain exchange relationships.* Given this perspective of marketing science and adopting the customary (albeit somewhat arbitrary) convention of designating one party to the exchange as the "buyer" and one party as the "seller," the fundamental explananda of marketing can be logically derived. As argued for in Hunt (1983a), the four sets of fundamental explananda (FE) of marketing science are:

FE1. The behaviors of buyers directed at consummating both individual exchanges (i.e., transactional marketing) and ongoing exchanges (i.e., relationship marketing).
FE2. The behaviors of sellers directed at consummating both individual and ongoing exchanges.
FE3. The institutional framework directed at consummating and/or facilitating both individual and ongoing exchanges.
FE4. The consequences for society of the behaviors of buyers, the behaviors of sellers, and the institutional framework directed at consummating and/or facilitating both individual and ongoing exchanges.

The first set of fundamental explananda indicates that marketing science seeks to answer this question: *Why do which buyers purchase what they do, where they do, when they do, and how they do?* The "which buyers" seeks to explain why certain buyers enter into particular exchange relationships and others do not. The "what" indicates that different buyers purchase different product/service mixes. The "where" is the institutional/locational choice of buyers. That is, why do some buyers purchase at discount department stores and others at full-service department stores, and why do some buyers purchase in neighborhood stores and others in shopping centers? The "when" refers to the timing decisions of buyers. Why do buyers purchase differently at different stages in the family life cycle? Finally, the "how" refers to the processes that consumers use in making their purchasing decisions. That is, what are the identifiable stages in consumer decision making? The "how" also refers to any organizational systems that buyers develop to accomplish the purchasing task, for example, the sharing of buying responsibilities among various members of the household.

The second set of fundamental explananda of marketing concerns the behaviors of sellers. As Lutz (1979, p. 5) has pointed out, "It has been extremely unfortunate that the vast bulk of theory-based behavioral research in marketing has been on consumer behavior." He then concludes that "if we truly believe that exchange is the fundamental building block of marketing, then we have virtually ignored (in a scientific sense) the behavior of the party selling to the consumer." The guiding question is, *Why do which sellers produce, price, promote, and distribute what they do, where they do, when they do, and how they do?* The "which" points out that not all sellers participate in all exchanges. The "what"

seeks explanations for the kinds of products produced, prices charged, promotions used, and distributors employed. The "when" seeks explanations for the timing of the behaviors of sellers. The "where" refers to the locations chosen by sellers to do business. The "how" refers to the processes involved and to the organizational frameworks developed by sellers when they engage in exchange relationships.

The third set of fundamental explananda suggests that marketing science seeks answers to these questions: *Why do which kinds of institutions develop to engage in what kinds of functions or activities to consummate and/or facilitate exchanges, when will these institutions develop, where will they develop, and how will they develop?* The "which" points out that not all kinds of institutions participate in the consummation and/or facilitation of all kinds of exchanges, and seeks to identify the kinds of institutions and "what" specific kinds of activities (functions) will be performed by each. The "when" refers to the evolution or changing of the kinds of institutions over time and "where" these changes will take place. The "how" refers to the processes that bring about these institutional changes.

As used here, the term *institution* refers to the entities that produce goods and services (such as manufacturers), to the intermediaries that either take title to goods or negotiate purchases or sales (such as wholesalers and retailers), to purely *facilitating* agencies (such as those that are solely engaged in transportation, warehousing, advertising, or marketing research), and, as emphasized by Arndt (1981b, p. 37), to the sets of norms, conditions, and rules for transactions and other interactions among the entities. Note that the study of *marketing systems,* therefore, can be considered the study of collections of interacting, marketing institutional entities, and the norms that guide them. In short, the third set of explananda seeks to explain the nature and development of all kinds of marketing systems.

The fourth set of fundamental explananda concerns the consequences of marketing for society. The guiding question is, Why do which kinds of behaviors of buyers, behaviors of sellers, and institutions have what kinds of consequences for society, when they do, where they do, and how they do? The "which" directs the theorists to focus on specific kinds of behaviors and/or institutions and explain "what" kinds of consequences these behaviors or institutions will have for society. Again, the "when" refers to the timing of the consequences and the "where" focuses on those on whom the consequences will fall. For example, will the consequences fall disproportionately on the disadvantaged members of society? Finally, the "how" focuses on the processes and mechanisms by which various parts of society are impacted by marketing activities. The study of the kinds of consequences discussed here is generally subsumed under the term *macromarketing*. The preceding four sets of explananda are proposed to be fundamental in the sense that every phenomenon that marketing science seeks to explain can ultimately be reduced to a phenomenon residing in one of the four sets.

4.6 A PRODUCT LIFE CYCLE EXPLANATION

As samples of "why" questions in marketing, section 3.1 asked (1) why the sales of product *X* have been decreasing rapidly; (2) why consumers purchase particular brands

of detergents; (3) why budget motels entered the hotel/motel industry in the 1970s; and (4) why newspapers charge lower rates to local advertisers than to national advertisers. This section and the three subsequent sections will offer typical marketing explanations of these phenomena and then systematically explore the underlying structure of the explanations.

Why are the sales of product X *decreasing rapidly?* A marketing explanation of this phenomenon might include reference to the product life cycle (PLC) concept. The product life cycle suggests that products go through four stages: introduction, growth, maturity, and decline. During the introductory stage, sales increase very slowly and profits are usually negative. During the growth stage, the product catches on and both sales and profits increase rapidly. At the maturity stage, sales begin to level off and profits start a gradual decline. Finally, both sales and profits decrease precipitously during the decline stage. *The decrease in sales of product* X *can be explained by noting that* X *is in the decline stage of its life cycle where rapidly decreasing sales and profits are to be expected.*

Product life cycle explanations of the preceding variety frequently turn out to be vacuous. The crucial part of the explanans is the statement "*X* is in the decline stage," because this statement carries the burden of explaining the decreasing sales. Yet, how do we know that "*X* is in the decline stage"? Kotler (1972c, p. 436) has observed that "the stages, if stages there are [of the product life cycle], are too variable in length to permit a prediction of when the next one will occur." Since the lengths of the stages are too variable, the primary factor determining the stage of the life cycle is sales. But the explanans then turns into a *tautology* or an *analytic* explanation because if the level of sales determines the stage of the life cycle, then the stage in the life cycle cannot be used to explain the level of sales. Unless and until the product life cycle can be refined to the point where the stages can be identified independent of the sales variable, the life cycle concept will remain impotent and void of explanatory power.

Recognizing the essentially tautologous nature of the product life cycle, Tellis and Crawford (1981) have developed a substitute notion, the product evolution cycle (PEC). Drawing upon the theory of biological evolution, they propose that sales are an evolutionary function of three motivating forces: market factors, managerial effectiveness, and government mediation. Unlike the PLC, the PEC does not assume that sales is a function of time. Rather, the evolution of sales proceeds within the dimension of time. The evolution proceeds in the direction of greater efficiency, greater complexity, and greater diversity. The PEC would appear to resolve some of the troublesome tautological problems of the PLC.

4.7 A CONSUMER BEHAVIOR EXPLANATION

The next explanation attempts to answer the question *Why do consumers purchase particular brands of detergents?* Engel, Kollat, and Blackwell (1973) present a model of habitual decision-process behavior, and, in a section entitled "Using the Model to Explain Consumer Behavior," they provide an explanation for detergent purchasing:

Laundry detergents generally are purchased on the basis of habit rather than extended problem solving. A problem is recognized when the housewife runs out of detergent, and her decision usually calls for purchase of a preferred brand on her next visit to the grocery store. There is no need to engage in conscious weighing of alternatives or external search for information. The situation changes, of course, when a significant new product comes on the market, but innovations of that magnitude are a rarity in this industry.

Survey results showed that women evaluate a detergent on the following bases: (1) cleaning ability (96 percent), (2) low suds (54 percent), (3) safety to colors (48 percent), (4) whitening and brightening ability (44 percent), (5) price (31 percent), and (6) fresh smell (20 percent). In addition to these evaluative criteria, 86 percent favored a powdered form and 60 percent preferred to use warm water. Several major brands were found to rate highest on these criteria, with Tide being the dominant favorite. These ratings of brand attitude closely paralleled market shares. The result is strong loyalty toward one or two preferred brands and only relatively small incidence of permanent brand switching. If the housewife does switch, it tends to be a temporary action to take advantage of a price reduction.

Those who were interviewed, for the most part, evidenced satisfaction with their present alternative. Postdecision evaluation, therefore, seldom takes place. (Engel, Kollat, and Blackwell 1973, pp. 66–67)

4.7.1 A Reconstruction of the Explanation

The explanation provided by Engel, Kollat, and Blackwell uses a narrative format that is pedagogically appropriate for a textbook, but makes evaluation difficult. The following reconstruction (1) captures the essence of the explanation, (2) shows specifically how the explanation relates to the model, and (3) lends itself more readily to structural analysis and evaluation.

a. Most people do not engage in conscious weighing of alternatives or external search for information when purchasing detergents.
b. Therefore, for most people, and except for unusual circumstances, the purchase of detergents can be classified as habitual in nature.
c. Therefore, the habitual decision-process behavior model can be applied to the purchasing of detergents.
d. The *stimulus* occurs when the housewife runs out of detergent.
e. The *evaluative criteria* for detergents (surveys show) are in rank order of most often mentioned to least often mentioned: (1) cleaning ability, (2) low suds, (3) . . .
f. Additional evaluative criteria suggest that 86 percent of women prefer detergent in powder form, and 60 percent prefer to use warm water (survey results).
g. The model suggests that the components of *attitude* are the evaluative criteria. That is, people are likely to *prefer* brands that they *rate highly* on specific attributes considered *important* in that generic product.

h. The model suggests that people are more likely to purchase brands they prefer (favorable attitude) than brands they do not prefer.
i. Therefore, the model suggests that people are likely to purchase brands rated highly on the evaluative criteria.
j. Therefore, the preceding suggests that with high likelihood the ratings of each brand on the evaluative criteria should match closely with the market share of each brand.
k. Survey results show that ratings of brand attitude closely parallel market shares.
1. This explains the market shares of different brands of detergents.

The preceding reconstruction, which attempts to accurately summarize the underlying logic of the explanation, shows in graphic relief the different kinds of statements in the explanatory structure. Taken in order, statement (a) is an *assumption* that probably could be validated empirically if challenged. Statement (b) is *classificatory;* however, the underlying classificatory schema is not given explicitly. Therefore, determining the acceptability of the classificatory schema is not possible. Statement (c) is a *logical* classificatory statement. Given (b), then (c) must be true. Statement (d) is another assumption that probably could be empirically verified, if necessary. Statements (e) and (f) are both *observation* statements; here the authors bring in "real-world" evidence. The next three statements contain the heart of the explanation; (g), (h), and (i) are the *laws* or *lawlike* statements that carry the brunt of the explanation. Statement (j) is a *hypothesis* suggested by the preceding arguments. Finally, (k) is an *observation* sentence, and (i) is the *explanandum,* or conclusion.

4.7.2 Structural Analysis of the Explanation

What is the basic form of the explanation? As reconstructed, the nature of the lawlike statements (g), (h), and (i) makes the explanation inductive-statistical. The qualifier, "with high likelihood," in (j) suggests an I-S model. The conclusive test, however, is the following question, "Can (k) be false and (j) still be true?" The answer must be yes; even with no measurement error, a nonrepresentative sample might give us the wrong results.

The reconstruction also shows that the original narrative explanation was an *enthymeme* (elliptical); some necessary statements were skipped over or suppressed. For example, the logical equivalent of statement (h) was suppressed. Although often heuristically unimportant, the suppression of premises can frequently impede analysis.

Next, note the *teleological* character of the explanation. That is, the lawlike generalizations are heavily purposive in kind. Statements (g), (h), and (i) suggest that the brand purchase decision is purposive to the extent that people seek brands that they perceive will correspond most closely to their evaluative criteria.

Now consider the depth of the explanation chain. The most basic statements in the chain concern components of the evaluative criteria that are product-specific, for example, cleaning ability and low suds. These components are presumably under the control of the

detergent firms. Therefore, the explanation attempts to explain a phenomenon (market share) by relying on factors (cleaning ability, etc.) that potentially enable the firm to *influence* the explanation chain. This suggests that, pragmatically speaking, the explanation is respectably deep.

An examination of causal considerations casts a shadow on the pragmatics of the explanation. The explanation implies that a firm might increase its market share by changing its product and promotion along lines suggested by the evaluative criteria. This presumes that changes in the evaluative criteria will cause subsequent changes in purchase and market share. However, the *temporal* sequence in this instance may be counterintuitive. Studies suggest that the "real" causal chain may be the reverse: changes in purchase (market share) cause subsequent changes in the evaluative criteria. Therefore, policy decisions based on such explanations may prove faulty.

Finally, consider the underlying habitual decision-process behavior (H-D-P-B) model. Many of the concepts in the model are not explicitly included or discussed in the explanation, for example, personality, information, experience, income, culture, and family. What was the role of the H-D-P-B model in generating the explanation? Essentially, the H-D-P-B model provided numerous constructs that fit together in a theoretical manner and that the authors suggest might be useful in explaining habitual purchase behavior. The *users* of the model then select several constructs as potentially explanatory.

4.8 A PRICE DISCRIMINATION EXPLANATION

Firms often engage in price discrimination; they charge different prices to different customers for the same product or service. Theaters charge different prices for adults than they do for children. Universities charge brilliant students less than average students (by providing scholarships). For the services provided by the federal government, wealthier citizens are charged more than poorer citizens (because the income tax rate is progressive). The same phenomenon occurs in newspaper advertising. *Why do newspapers charge lower rates to local advertisers than to national advertisers?* The late Julian Simon states one possible explanation:

> *Factual problem.* It is an observed fact that newspapers charge lower advertising rates to local retailers than to nationally advertised brands of goods. To explain why they do is a research problem.
>
> *Assumptions.* We *assume,* first, that businessmen (newspaper owners, in this case) will charge that price to each group of people that will result in *maximum profit.* (This is the "economic man" assumption.) . . . Second, we *assume* that businessmen know how groups of customers (retailers and national advertisers) react to various prices. (This is the "perfect knowledge" assumption.) . . .
>
> *Deduction.* We *deduce* that, if one customer group is *less sensitive* to a price increase than another group is, it will be profitable to charge a higher price to the less sensitive customer. (This can be shown with a standard logical chain of economic deduction.) . . .

> *Hypothesis.* We then hypothesize that, if the deduction is correct, the newspaper publishers believe that national advertisers are less sensitive to price changes. This hypothesis can be tested by finding out what the newspaper advertisers believe about the relative sensitivity of local and national advertisers. A questionnaire study found that publishers do indeed believe that national advertisers are less sensitive to price changes and thus confirmed the hypothesis. (Simon 1969, p. 38)

Simon's proposed explanation is well formed, succinct, and rigorous. It thus requires little elaboration or analysis. Some of the terminology differs in minor ways from the present treatment. For example, his *factual problem* would be our *explanandum;* this is the phenomenon to be explained. His *assumptions* would be our *laws,* insofar as these are generalized statements that do most of the explaining. The term *assumption* carries unfortunate connotations in the social sciences, such as "cannot be tested," "should not be tested," and "need not conform to reality." Because formal analysis of axioms and assumptions will be deferred to the next chapter, we need here only observe that few terms have been so thoroughly abused in the social sciences as have the terms *assumption* and *axiom.* They should definitely be labeled "handle with care."

A final observation concerning Simon's excellent explanation of newspaper price discrimination focuses on his *hypothesis* section. Simon deduces certain predictions that should occur *if his explanation is correct.* He then tests these predictions. Thus, Simon provides a graphic illustration of what it means to require explanations to be empirically testable and intersubjectively verifiable.

4.9 A WHEEL OF RETAILING EXPLANATION

The 1970s saw the successful rise of so-called budget motels in the lodging industry. These motels lacked many of the amenities (room service, bellhops, chandeliers, and posh furnishings) but featured very low prices for rooms. *Why was the introduction of budget motels such a successful retail innovation?* A marketing explanation might involve the wheel of retailing, first proposed by McNair (1958).

Stanley Hollander's (1960/1969) conceptualization of the wheel of retailing holds that new types of retailers "usually" enter the market as low-status, low-margin, low-price operators. Gradually, they acquire more elaborate establishments and facilities with both increased investments and higher operating costs. Finally, they mature as high-cost, high-price merchants, vulnerable to newer types who, in turn, go through the same pattern.

A wheel explanation of budget motels would start with the observation that the original motels began as cabins along the highways. They were low-cost, low-margin, low-price, and low-status operations. Over time, motels added many services and upgraded their facilities to the point where the features distinguishing motels from hotels were difficult to find. Concomitant with the added services and upgraded facilities were increases in costs and prices. Therefore, the entry of budget motels can be explained by demonstrating that it is another instance of the wheel of retailing.

The wheel constitutes a form of inductive-statistical explanation because of the claim that

new retailers "usually" enter the market. Hollander acknowledges that *not all* new retailers enter as low-cost, low-margin, and so on. Vending machine retailing, department store branches, and convenience stores are retail institutions whose entries did not conform with the wheel notion. Unfortunately, no one has been able to suggest the particular conditions that must prevail for new retail institutions to enter in accordance with the wheel.

Essentially, the wheel attempts to explain the entry of budget motels by demonstrating that the history of the motel industry is consistent with an *empirical regularity* (see Chapter 5) observed in other industries, that is, the upgrading of services and facilities, and, therefore, the increasing of costs and prices. The explanation seems incomplete because it does not explain *why* the motel industry continually increased services, facilities, costs, and prices. Perhaps integrating the wheel notion with the theory of competition for differential advantage might make the explanation more complete.

4.9.1 The Wheel of Retailing and Competition for Differential Advantage

The economist J.M. Clark (1954, 1961) promulgated a theory of "effective competition" that stressed the concept of "differential advantage." (See section 14.2 for more on Clark's works.) Clark held that new firms or institutions enter an industry when they believe they will have some differential advantage over existing firms in serving some subset of customers. Competition consists of a series of initiatory moves by some firms to gain a differential advantage and of subsequent countermoves by rivals to neutralize that advantage. Alderson captures the essence of the concept:

> Every business firm occupies a position which is in some respects unique. Its location, the product it sells, its operating methods, or the customers it serves tend to set it off in some degree from every other firm. Each firm competes by making the most of its individuality and its special character. It is constantly seeking to establish some competitive advantage. Absolute advantage in the sense of an advanced method of operation is not enough if all competitors live up to the same high standards. What is important in competition is differential advantage, which can give a firm an edge over what others in the field are offering. (Alderson 1957, p. 101)

We can now attempt to integrate the wheel of retailing into the theory of competition for differential advantage.[2]

1. All new retail institutions enter the market because the participants perceive that they will have some form of differential advantage over existing retail forms. The basis for differential advantage may be some innovation that yields greater convenience in location to certain customers (e.g., vending machines), speedy service (e.g., fast-food restaurants), low prices (motels), or some other benefit to a subset of potential customers. The particular differential advantage of early motels, when competing against hotels, was both convenient locations along the highway and low prices. Motels were low cost, low price, and low status, and they competed primarily against existing *hotels.*

2. The number of motels increased rapidly as a large segment of the market desired both convenient locations away from downtown areas and the low prices. As the number of motels increased, the nature of competition changed from motel versus hotel to motel versus motel. That is, the primary thrust of competition for motels changed from trying to lure customers from hotels to trying to lure customers from *other motels.* Then, individual motels began to seek some differential advantage over other motels.

3. Price reductions are the form of differential advantage that would be neutralized most easily and quickly. Therefore, the upgrading of services and facilities would be the most common means used to gain differential advantage. For example, most motels followed this progression: no television, pay television, free television, color television, cable color television. Each motel attempted to neutralize the differential advantage of its competitors by matching its competitors' services and facilities.

4. Therefore, over time the motel industry gradually increased its costs, margins, and prices. The motel industry thus became a tempting target for budget motels stressing low costs, margins, and prices.

The preceding integration of the wheel with competition for differential advantage shows why the wheel must state that new types of retailers *usually* enter as low cost, low price, and so on. There is no basis for supposing that *all* new forms of retailing would seek low price as their basis for differential advantage. In addition, the revised structure appears testable, at least in principle. In particular, the generalization that as a new form of retailing matures, the thrust of competition will change in the directions noted seems amenable to empirical confirmation. Finally, integrating the wheel of retailing into the theory of competition for differential advantage has made the explanation of the phenomenon of budget motels substantially deeper and more complete.

The theory of competition for differential advantage, as initiated by Clark and further developed by Alderson, would seem to be a viable alternative to neoclassical perfect competition theory. Yet, the theory lay fallow for decades. This situation changed with the development of resource-advantage ("R-A") theory (Hunt 2000b; Hunt and Morgan 1995, 1996, 1997). Indeed, "resource-advantage theory draws more strongly from differential advantage theory than from any other research tradition" (Hunt 2000b, p. 63). (See Chapters 13 and 14 for more on R-A theory.)

4.10 SUMMARY AND CONCLUSIONS

This chapter has explored a variety of issues concerning explanation and has used the tools developed so far to analyze several explanations of marketing phenomena. The thesis of structural symmetry was examined and found to be half correct: all adequate explanations of phenomena must be potentially predictive. However, all adequate predictions of phenomena are not necessarily adequate explanations because predictions can be made without the use of lawlike generalizations. And lawlike generalizations are necessary for the scientific explanation of phenomena. Furthermore, the ability to *retrodict* phenomena is *not* a necessary condition for explanation.

The relationships among scientific *understanding, explanation,* and *prediction* were

explored via an analysis of Dubin's power paradox. The paradox disappears upon the realization that prediction is necessary for explanation and that explanation is necessary for understanding. Therefore, models and theories in marketing that do not explain and predict do not contribute to scientific understanding.

The epistemologically troublesome notions of causality and causal explanations were shown to be problematic with respect to generating *sufficient* conditions for classifying an explanation as *causal.* However, various kinds of evidence can be introduced to *suggest* that a particular relationship or explanation is causal. These include temporal sequentiality, associative variation, nonspurious association, and theoretical support.

All explanations are incomplete in one way or another. In principle, all explanations involve an explanation chain and are, therefore, incomplete. Some explanations are incomplete because they are enthymemes, that is, they skip over or leave out some premise. Some explanations are better termed partial explanations or explanation sketches.

The product life cycle was explored as an explanatory device and found to lack explanatory power. A consumer behavior model was found to be useful in the context of discovery. Simon's price discrimination explanation was found to be well formed and illustrated how explanations can be empirically testable. Finally, the wheel of retailing was integrated into the theory of competition for differential advantage and used to explain the phenomenon of budget motels.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Summarize the thesis of structural symmetry. Why have so many marketing and social science theorists been reluctant to accept the conclusion that every acceptable explanation is a potential prediction?
2. Find an instance in the marketing literature where the author has used the term *cause,* and evaluate the appropriateness of the author's usage. What criteria, either explicit or implicit, did the author use to justify the author's causal assertion? Evaluate those criteria.
3. Can there be any such thing as a "final explanation" of any marketing phenomenon?
4. Find an example of an explanation chain in the marketing literature. Is it acceptably deep? Evaluate the explanation.
5. How do enthymemes, partial explanations, and explanation sketches differ? Are there other ways in which explanations can be incomplete?
6. This chapter found the product life cycle to be explanatorily impotent. If this is so, why does the product life cycle receive so much attention in the marketing literature? Is the attention justified?
7. The explanation of marketing phenomena is the cornerstone of research in marketing. Yet, the term *explanation* does not appear in the index of most prominent marketing research texts. How can this paradox be reconciled?

8. Empirical studies frequently cite the coefficient of determination (R^2), and they discuss the percentage of the dependent variable "explained" by the independent variable(s). When used in this context, why is the term *explained* usually enclosed in quotation marks? Should it be in quotation marks? Why?
9. What does it mean to propose that all marketing phenomena can "ultimately be reduced" to one of the four sets of fundamental explananda? Chapter 1 proposed that one dimension of macromarketing is the "impact of society on marketing systems." Could this dimension be "reduced"? Go through a recent issue of *JMR*. See whether all of the dependent variables can be "reduced." If not all of them can, what additional fundamental explananda are required?
10. Bagozzi (in Hunt 1983b) suggests that marketing researchers want to understand and control marketing behavior. He contends that "both objectives—understanding and control—rest fundamentality on the identification and analysis of causal relationships." Could understanding not come about simply through the use of regularity relationships? Why or why not? Similarly, could control not come about by way of regularity relationships? Why or why not?
11. Chapter 4 identifies four criteria for classifying explanations as potentially causal: (1) temporal sequentiality, (2) associative variation, (3) nonspurious association, and (4) theoretical support. John Stuart Mill—discussed by Bagozzi (in Hunt 1983b)—suggests the methods of agreement, difference, residues, and concomitant variation. Compare and contrast these two sets of criteria.
12. The chapter puts forth a classificational schema with five "levels" of prediction. Go through an issue of a major marketing journal. What percentage of the predictions fall into each category? Are there predictions that do not "fit"? If so, suggest a new schema that will accommodate all predictions. To what extent is the new schema superior or inferior to the schema in this chapter? Why? (See section 8.1.)

NOTES

1. Yes, it can be done. The sidewalk temperature in Madison is correlated with the ambient temperature in Madison. The latter is correlated with the ambient temperature in Oslo. The temperature in Oslo is related to the rate of conception in Oslo (higher in winter). QED.

2. This analysis parallels in part the integration of the wheel into the concept of intertype competition; see Bucklin (1972, p. 120 ff).

5

ON THE MORPHOLOGY OF SCIENTIFIC LAWS

> Several kinds of statements about behavior are commonly made. When we tell an anecdote or pass along a bit of gossip, we report a single event—what someone did upon such and such an occasion. . . . These accounts have their uses. They broaden the experience of those who have not had firsthand access to similar data. But they are only the beginnings of a science. The next step is the discovery of some sort of uniformity.
>
> *—B.F. Skinner*

Commentators (e.g., Brodbeck 1968, p. 673) on the history of science have found that the notion of descriptive or scientific laws evolved from the older conception of prescriptive or normative laws. The earliest laws were normative commandments that asserted rules for proper (moral) conduct: "Thou shalt not kill." Whether these normative laws were sanctioned by the established church or civil government, or frequently both, all people were universally obliged to obey them. From this genesis, the term *law* has been extended to descriptive regularities in science because these, too, apply "universally" to all phenomena. However, at that point the similarity ends. Normative laws *prescribe* what people *ought* to do; scientific laws concerning human behavior *describe* what people *actually* do.

When a citizen fails to *obey* a normative law, the appropriate authorities may invoke sanctions, for example, excommunication, public humiliation, imprisonment, flogging, or even death. As conspirators find out in antitrust cases, when a marketer disobeys a governmental normative law such as "Thou shalt not conspire to fix prices," both fines and imprisonment may ensue. In contrast, the consequences of finding behaviors that "disobey" a descriptive or scientific law suggest a reexamination of the law in question rather than punishment. If behaviors do not follow the law, then either the law should be rejected or the conditions under which the law is believed to be true should be modified. When the term *law* is used in this monograph, we refer to scientific laws, not normative laws.

Both normative laws and scientific laws should be carefully distinguished from normative decision rules or normative decision models. Normative decisions rules or models

prescribe the most appropriate courses of action to follow in order to attempt rationally to achieve some stated objective. Thus, for example, linear programming can provide a set of normative decision rules for optimally allocating the advertising budget among the various media. As a second example, consider the marketing concept. Note that it is not a *concept* in the normal sense of the term. Rather, it is a philosophy of doing business that (historically) has been based on a set of three normative decision rules: (1) firms should be customer oriented; (2) all marketing activities of the firm should be integrated; and (3) profit rather than sales should be the orientation of the firm. Although many normative decision rules are nothing more than crude *rules of thumb,* some are firmly based on scientific laws.[1] Bunge (1967b, p. 132) suggests that normative decision rules that are based or founded on some set of scientific laws be referred to as *grounded rules.*

In summary, normative laws prescribe what people *morally* or *legally* ought to do. Normative decision rules or models prescribe what people ought to do rationally to achieve some stated objective. Scientific laws in human behavior describe what people actually do.

This chapter concerns the morphology of scientific laws and their role in scientific inquiry. First, we explore the role of laws in marketing research. Then, we provide some criteria for separating lawlike generalizations from ordinary conversational generalizations, before evaluating several examples of marketing lawlike generalizations.

5.1 ROLE OF LAWS IN MARKETING RESEARCH

Laws and lawlike statements play vital roles in marketing inquiry. As indicated in Chapter 3, the development of laws in marketing is a requirement for explaining marketing phenomena. All of the models discussed in Chapter 3 that have explanatory power (deductive-nomological, deductive-statistical, and inductive-statistical) explain phenomena by deductive or inductive subsumption under lawlike generalizations. That is, we can *explain* the market shares of various detergents by showing that the shares are consistent with certain lawlike statements relating brand purchases with the components of brand attitudes (section 4.7). In addition to explaining marketing phenomena, lawlike statements in marketing facilitate the *prediction* of marketing phenomena. All models that can provide satisfactory scientific explanations of *past* marketing phenomena must be potentially capable of predicting future marketing phenomena; or, as Chapter 4 indicated, all satisfactory explanations are potential predictions. The laws in the models provide the predictive power. Taken together, the ability to explain marketing phenomena systematically and the ability to predict marketing phenomena lead to the *scientific understanding* and potential *control* of marketing phenomena. If, by employing certain laws, marketers can predict the consequences of changing certain resources under their control, then they have at least some ability to *control* the system. Thus, the intellectual goal of all scientific endeavor is scientific understanding, and a pragmatic consequence of scientific endeavor is increased control.

The next section will discuss the basic form of laws, that is, generalized conditionals. Also, some distinctions will be made concerning laws, lawlike generalizations, and

principles. One caveat prefaces our discussion: the term *law* is another slippery term (like the term *explanation*) that seems intuitively clear at first glance but provides stout resistance to rigorous analysis.

5.2 THE FIRST CRITERION: GENERALIZED CONDITIONALS

All laws specify a relationship in the form of a generalized conditional.[2] *Conditional* implies some kind of "if-then" relationship. Thus, a common basic form is: "Every time *A* occurs, then *B* will occur." Or, in a more developed form, "For any *x,* if *x* is *A,* then *x* is *B*" (or, alternatively, "All *A* are *B*"). For example, "All consumers who systematically avoid purchasing private-label merchandise are low in generalized self-confidence." Note that this statement can be recast in generalized conditional form: "For any *x* (consumer), if *x* is *A* (systematically avoids purchasing private-label merchandise), then *x* is *B* (low in generalized self-confidence)." Not all statements of generalized conditional form constitute laws. In particular, we have previously suggested that *accidental generalizations,* although having the form of generalized conditionals, are not laws.

Confusion continues concerning the requirement that all laws must specify a relationship in the form of a generalized conditional. Consider the interesting article by Sheth and Sisodia (1999) entitled "Revisiting Marketing's Lawlike Generalizations." They argue that (1) marketing is a context-driven discipline, (2) the context for marketing is changing radically due to electronic commerce, market diversity, new economics, and coopetition, (3) as marketing academics we need to question and challenge well-accepted lawlike generalizations in marketing. Their Table 2 identifies four groups containing twelve entities that they consider to be "old" lawlike generalizations that should be changed: (A) retail gravitation, sales and distribution channels, and intermediary-based advertising and promotion, (B) diffusion of innovation, product life cycles, and brand loyalty, (C) market segmentation, customer satisfaction, and market-driven orientation, and (D) market share, generic competitive strategies, and vertical integration.

Kerin and Sethuraman (1999) provide a commentary on the Sheth and Sisodia article. First, they discuss the criteria for considering an entity to be a lawlike generalization. They argue for the following four criteria: (1) generalized conditionals, (2) empirical support, (3) systematic integration into a coherent scientific structure or framework, and (4) insight/importance. They then evaluate the twelve entities that Sheth and Sisodia maintain are lawlike generalizations that should be challenged and conclude:

> We believe nine of the twelve generalizations do not qualify as lawlike generalizations. The product lifecycle is probably a tautology (see Hunt 1983b, p. 131). Others [e.g., generic competitive strategies] are normative statements or decision rules that prescribe a course of action to achieve some end. . . . Market-driven orientation is an organizational philosophy, not unlike the marketing concept. Four of the purported generalizations—physical distribution channels, location-based advertising,

> marketing segmentation, and, to some extent, vertical integration—represent current or conventional marketing practice or strategy. No discernable conditional lawlike statements are offered in their support. Thus, they fail the first requirement for a lawlike generalization. (Kerin and Sethuraman 1999, p. 103)

Agreed, "It may very well be that our progress and status as a discipline in the twenty-first century will be gauged by our success or failure in . . . the quest for lawlike generalizations that explain and facilitate the prediction of marketing phenomena" (Kerin and Sethuraman 1999, p. 104). Also agreed, Sheth and Sisodia provide some interesting thoughts as to topics in marketing that warrant reconsideration due to electronic commerce, market diversity, the new economics, and "coopetition." However, confusing *lawlike generalizations* with concepts, normative decision rules, strategies, and conventional wisdom detract from their argument.

Confusion also arises concerning the differences among the following concepts: "principles," "laws," and "lawlike generalizations." First, let's distinguish between laws and lawlike generalizations. Lawlike generalizations (or "lawlike statements" or "lawlike propositions") are statements in generalized conditional form that fulfill all the criteria of laws but have not yet been tested and confirmed or corroborated.[3] Although the subject of criteria for corroboration (or confirmation) of laws and theoretical constructions will be extensively investigated in Chapter 7, a few preliminary observations can be made here. To say that a lawlike statement is highly confirmed, or corroborated, or believed to be true, is different from saying that it is absolutely true, or true-with-certainty, or "True" (with uppercase "T"). (See Chapter 11 and Hunt [1990] for further discussion on this issue.) Therefore, we do not require lawlike statements to be "True" to be a law. Rather, we require them to be highly confirmed or corroborated by the evidence, which gives us reason to believe them to be true or isomorphic with the real world.

A second distinction is between laws and principles (sometimes referred to as "high-level laws," "most fundamental laws," or, simply, "Laws" with an uppercase "L"). The distinction is largely honorific. In any discipline a *law* becomes a *principle* when it is widely held to be of extreme significance or importance to that discipline and when the evidence corroborating it is overwhelming. Thus, we have the Law of Demand in economics, the First Law of Thermodynamics in physics, and Weber's Law in psychology.

For example, Simon (1969, p. 38, his emphasis) seems to have blurred the distinction between *law* and *principle:* "If an empirical test of the hypothesis confirms the hypothesis, the generalization might be called a *law,* provided that the finding is sufficiently *important.*" The unfortunate consequences of this confusion have led many writers to decry the absence of *laws* in the behavioral sciences when what they have really observed is the absence of *principles* (or *Laws* with uppercase "L") in those areas. Over four decades ago, Berelson and Steiner (1964) were able to document a host of reasonably well-supported generalizations worthy of being called *laws* in the behavioral sciences. Kincaid defends both the possibility and existence of laws in the social sciences. Indeed, he finds that "parts of the social sciences produce laws, evidence, and explanations quite similar in form to those of evolutionary biology and ecology" (1990, p. 58). And in marketing,

Bass and Wind (1995), identify over three dozen generalizations that (at least potentially) qualify as lawlike.

As examples of generalizations that at least potentially qualify as lawlike in marketing, Blattberg, Briesch, and Fox (1995) offer the following in the area of promotions: (1) Temporary retail price reductions substantially increase sales. (2) Higher market share brands are less deal elastic. (3) The frequency of deals changes the consumer's reference price. (4) The greater the frequency of deals, the lower the height of the deal spike. (5) Display and feature advertising have strong effects on item sales. In the area of reference price research, Kalyanaram and Winer (1995) propose the following: (1) Reference prices have a consistent and significant impact on consumer demand. (2) Internal reference prices utilize past prices as part of the consumer's information set. (3) Consumers react more strongly to price increases than to price decreases. In the area of consumer choice, Meyer and Johnson (1995) offer: (1) Subjective attribute valuations are a nonlinear reference-point dependent function of the corresponding objective measure of the attribute. (2) The algebraic integration rule that best describes how valuations are integrated into overall valuations is a multiplicative-multilinear function that recognizes an overweighting of negative attribute information. (3) Overall valuations of an option are linked to choices by a function that recognizes the proximity or similarity of the option to others in the set. In the area of market entry, Kalyanaram, Robinson, and Urban (1995) propose: (1) For mature consumer and industrial goods, there is a negative relationship between order of market entry and market share. (2) For consumer packaged goods, the entrant's forecasted market share divided by the first entrant's market share roughly equals one divided by the square root of order of market entry. I invite readers to evaluate these examples.

5.3 THE SECOND CRITERION: EMPIRICAL CONTENT

What kinds of statements that have the basic form of generalized conditionals should be characterized as *lawlike*? Alternatively, what criteria should be applied to distinguish lawlike statements from nonlawlike statements? One extremely desirable criterion is that *all lawlike statements must have empirical content* (Lambert and Brittan 1970, p. 38). The empirical-content criterion rules out both nonsense statements and strictly analytical statements. An example of a nonsense generalized conditional might be: "All marketing maloglops are high priced." Clearly, according lawlike status to such statements would be patently ridiculous since maloglops are nonexistent.

Much more important than just ruling out nonsense laws, the empirical-content criterion also excludes strictly analytical statements from being considered lawlike. Before discussing the importance of excluding strictly analytical statements, we need to distinguish between two basic kinds of statements: analytic and synthetic.

Consider the following two statements: (1) marketing activities consume a large portion of the consumer's dollar; and (2) either marketing activities consume a large portion of the consumer's dollar, or marketing activities *do not* consume a large portion of the consumer's dollar. Both statements are true, yet they are true for different reasons. The first statement is known to be true because of studies conducted by Reavis Cox (1965),

Harold Barger (1955), Louis P. Bucklin (1978), and others. That is, statement 1 is known to be true only after we examine the facts in the real world. Such statements are called *synthetic* (Bergmann 1957). Conversely, Statement 2 is true no matter what the real-world facts are. Statement 2 is true because it makes no assertion about the real world: it does not say anything at all! Such statements are called *purely analytic,* and, strictly speaking, they are true only because of the order and nature of the logical terms (such as *either* and *or*) and the way in which they define certain descriptive terms (such as *marketing*). True analytic statements are *tautologies,* and false ones are *contradictions.*

Bergmann (1957, p. 27) suggests that in tautologies the descriptive words appear only vacuously; that is, the truth content of tautologies is independent of the descriptive words. Therefore, to show that statement 2 is really tautological, we need to define the descriptive words:

p = marketing activities
q = consume a large portion of the consumer's dollar

As constructed, then, statement 2 merely asserts that "either p is q, or p is not q." We could then insert any descriptive terms for p or q that we desire, and the statement would still be true. Therefore, the descriptive terms in statement 2 appear only *vacuously.* A similar reconstruction of statement 1 will reveal it to be a synthetic statement. (Try it!)

So the *empirical-content* criterion successfully weeds out strictly analytic statements from lawlike statements because we want our laws to "say something" about the real world. We want lawlike statements to be empirically testable. However, the analytic/synthetic distinction may not always be as clear-cut as has been implied. Consider this assertion: "No consumer can be brand loyal to more than one brand at a time in the same product class." The statement is certainly a generalized conditional of the form "*If* for any *X,* if *X* is a consumer, and if the consumer is loyal to brand *A,* and if brands *A* and *B* are in the same product class, *then* the consumer cannot at the same time be loyal to brand *B*." Does the statement pass the empirical content criterion for being considered lawlike? Is the statement analytic or synthetic?

Whether the brand-loyalty assertion is analytic or synthetic depends primarily on how brand loyalty is defined. Consider the following definition: "Consumer *X* is considered to be brand loyal to brand *A* if, and only if, the consumer purchases over 50 percent of his/her requirements of the product class from brand *A*." Given this definition, the brand-loyalty assertion is obviously analytic, because it would be mathematically impossible for a consumer to purchase in excess of 50 percent of his/her requirements from brand *A* and at the same time purchase over 50 percent from brand *B.* Therefore, given this definition, the "brand-loyalty" generalization would fail the empirical-content criterion.

On the other hand, Tucker (1964) suggests denoting consumers as brand loyal if they make three successive choices of the same brand. Consider the following sequence of purchases over a twelve-month period:

CBAAACABBBAB

Using Tucker's definition, the consumer would be brand loyal to *both* brands *A* and *B* during the same twelve-month period, and the assertion that a consumer will be brand loyal to only one brand in a single time period becomes synthetic, not analytic. That is, with the revised definition, it is now *possible* to show that the brand loyalty generalization is empirically false. Therefore, the statement would pass the *empirical content* criterion.

Halbert (1965, p. 66) maintained that many of the generalizations in the marketing literature seem to be "either tautologies, truisms, or so overly general that they are of very limited use in developing marketing science." As one example, Halbert points out a generalized statement by Jastram (1955): "If it appears to be profitable to plan advertising at all, there will be some one rate of outlay for which it will be most profitable to plan." Readers should decide for themselves whether the statement is a tautology.

In his "theory of social initiative," Robert Bartels (1968, p. 32) states, "Society, not the business entrepreneur, is the basic undertaker of all activity." Is this analytic or synthetic? Would it pass or fail the empirical-content criterion for lawlikeness? As previously illustrated, the key depends on how certain terms (such as *basic undertaker*) are defined, and, unfortunately, Bartels provides no definitions. However, because innumerable "basic" activities do seem to be undertaken by business entrepreneurs, *if* the statement is intended to be synthetic, it is probably false. Therefore, there would probably be a strong temptation to define the term *basic undertaker* in such a way that the truth content of the statement would be assured. Consequently, to the extent that the statement is synthetic, it is probably false. And to the extent that the statement is analytic, it will fail the empirical-content criterion. In either case, the statement should probably not be considered lawlike.

5.4 THE THIRD CRITERION: NOMIC NECESSITY

The previous discussion suggests that lawlike statements must have (1) the basic form of generalized conditionals and (2) empirical content. The third criterion states that *all purportedly lawlike statements must possess nomic* (nō'mĭk) *necessity* (sometimes referred to as "nomological universality" or "nomic universality"). The purpose of the nomic-necessity criterion is systematically to prevent *accidental* generalizations from being considered laws. Nomic necessity implies that the occurrence of some phenomenon *must* be associated with some other phenomenon; the relationship cannot be, simply, by *chance.* The classic illustration of an accidental generalization has been provided by Nagel (1961, p. 52): "All the screws in Smith's current car are rusty." Note that the statement is a generalized conditional with empirical content. Nevertheless, few people would like to accord lawlike status to such a generalization precisely because it somehow seems to describe an *accidental* relationship.

As Rescher (1970b, p. 103) has observed, although there is widespread agreement that scientific laws involve a necessity that transcends simple accidental regularity, the issue of explicating what exactly is meant by nomic necessity remains a major problem. A few examples of generalizations might help to clarify the issue. Consider the following five generalizations: (1) all the coins in my pocket are half-dollars; (2) all products produced

by Procter and Gamble are distributed through supermarkets; (3) all products with the trade name Maxwell House have a coffee base; (4) two cities attract retail trade from an intermediate town in the vicinity of the breaking point (where 50 percent of the trade is attracted to each city) in direct proportion to their populations and in inverse proportion to the square of the distances from the two cities to the intermediate town (Converse 1949, p. 379); and (5) in any survey, the percentages of people who express intentions to purchase a brand are directly proportional to the square roots of the percentages of informants who currently use the brand (Ehrenberg 1971, p. 34).

Note that all five of the statements in the previous paragraph are generalized conditionals of the "all *A* are *B*" variety and that all five have empirical content. Nevertheless, most scholars would find it intellectually disquieting to accord lawlike status to the first three and would be more than willing to consider the last two as suitable candidates for lawlike status. (Remember, this does not necessarily mean that we have enough empirical evidence to consider [4] and [5] to be *laws* but only that they pass muster for consideration as *lawlike.*) Intuitively, the generalizations embodied in the first three statements seem qualitatively different, more *accidental,* than those embodied in the last two. But precisely how do we analytically (rather than intuitively) separate accidental from nonaccidental generalizations? The answer lies in the fact that *generalizations exhibiting nomic necessity have a kind of hypothetical power that is different from that of accidental generalizations.*

The major purposes of scientific laws are to explain and predict phenomena. To accomplish these tasks, laws must have the power to generate hypotheses such as "If phenomenon *X* occurs, then phenomenon *Y* will occur." To demonstrate that accidental generalizations lack hypothetical power, consider the following statements:

A. If this coin (which is not in my pocket) were placed in my pocket, it would be a half-dollar.
B. If this product (which is not labeled Maxwell House) were labeled Maxwell House, then it would have a coffee base.
C. If this automobile were produced by Procter and Gamble (which it is not), it would be distributed through supermarkets.

Statements A, B, and C are all called *counterfactual conditionals* because the premises of the statements are not true.[4] That is, the premises are "counter to the facts": The coin is not in my pocket, the product is not labeled Maxwell House, and the automobile is not produced by Procter and Gamble.

Referring to the five generalizations, none of the first three generalizations support their respective counterfactual conditionals. No reasonable person would believe that statements A, B, and C were true *even though* he or she knew that generalizations (1), (2), and (3) were true. The generalization "All the coins in my pocket are half-dollars" does *not* support (i.e., give someone good reason to believe) statement A. The generalization "All products with the trade name Maxwell House have a coffee base" does *not* support statement B. Finally, the generalization "All products produced by Procter and

Gamble are distributed through supermarkets" does not support statement C because there is nothing to prevent Procter and Gamble from distributing a product through another channel of distribution if it chooses to do so. The Procter and Gamble generalization (like the others) thus lacks the element of *must.* Alternatively, the Procter and Gamble generalization lacks the nomic necessity required of genuine lawlike statements because it lacks the *hypothetical power* to support counterfactual conditionals. As the next paragraph will show, genuine lawlike statements will exhibit nomic necessity by supporting counterfactual conditionals.

Consider the following statements:

D. If city *K* had four times the population of city *J* (*K* actually has only twice the population of *J*), then city *K* would double the percentage of retail trade that it draws from intermediate city *I.*
E. If the usership of brand *X* had been 16 percent (it actually was only 4 percent), then in this survey the percentage of people who expressed an intention to purchase brand *X* would have doubled.

Note, once again, that both D and E are counterfactual conditionals like A, B, and C. However, this time the generalizations *can support* their respective counterfactual conditionals if in fact the generalizations accurately represent the real world (a strictly empirical question). That is, generalization (4), "Two cities attract retail trade from an intermediate town . . . ," can support statement D if generalization (4) accurately depicts the real world. Similarly, generalization (5) can support statement E. *Therefore, in order for a statement to be considered lawlike, our third criterion is that it must exhibit nomic necessity, which rules out accidental generalizations. Accidental generalizations can be identified by their lack of hypothetical power, as evidenced by their inability in principle to support counterfactual conditionals.*

5.5 THE FOURTH CRITERION: SYSTEMATIC INTEGRATION

The analysis thus far has shown that lawlike statements have the form of generalized conditionals that have empirical content and exhibit nomic necessity. The final requirement provides that *all purportedly lawlike statements must be systematically integrated into a body of scientific knowledge.* Stated negatively, a simple empirical regularity (even a well-confirmed one) is not a lawlike generalization. An empirical regularity does not qualify as a lawlike statement until it is systematically integrated into a coherent scientific structure or framework.

An empirical regularity is a statement summarizing observed uniformities of relationships between measures of two or more concepts or variables. That empirical regularities should not be classified as lawlike until they have found a niche in a systematic framework has been observed both by philosophers of science and by theoreticians. Lambert and Brittan (1970, p. 45) suggest, "What leads us to reject the red sky in the morning/rain in the afternoon as a law, is that it is an isolated assertion having no apparent theoretical

ramifications." Kaplan (1964, p. 92) believes that a "nomic generalization must be derivable from other laws, that is, play a part in scientific theory. Otherwise, we obtain what might be called an empirical generalization rather than a law." Merton (1994/1968, p. 149) lamented that the literature in sociology abounds with isolated propositions that have not been assimilated into sociological theory. And Bass (1995, p. G8) admonishes marketing researchers to incorporate empirical generalizations into lower-level and higher-level theories because such incorporations lead to "low-level explanations" and "high-level explanations." Finally, Rescher plainly states the systematically integrated criterion:

> An empirical generalization is not to be viewed as fully adequate for explanatory purposes until it can lay claim to the status of a law. Now a law is not just a summary statement of observed-regularities-to-date; it claims to deal with a universal regularity purporting to describe how things inevitably are: how the processes at work in the world must invariably work, how things have to happen in nature. Such a claim has to be based upon a stronger foundation than any mere observed regularity-to-date. The *coherence of laws* in patterns that illuminate the "mechanisms" by which natural processes occur is a critical element—perhaps the most important one—in furnishing this stronger foundation, this "something more" than a generalization of observations. An "observed regularity" does not become a "law of nature" simply by becoming better established through observation in additional cases; what is needed is *integration* into the body of scientific knowledge. (Rescher 1970b, pp. 15–16)

The requirement that generalizations must be systematically integrated with other statements in the total corpus of knowledge points out the importance of theories. Because theories are systematically related sets of statements that include some lawlike generalizations, theories provide a crucial mechanism for according lawlike status to empirical regularities and other isolated propositions. As Barwise (1995, p. G31) argues, empirical generalizations in marketing should be "explained by, or at least linked in some way with theory."

Consider Ehrenberg's (1971, p. 33) "duplication of viewing" (hereafter referred to as "D-V") relationship: $d_{ts} = kr_t r_s \pm 1$. Ehrenberg was interested in determining what percentage of the audience would be duplicated if X percentage of the population viewed a television program on Monday night and Y percentage of the population viewed a program on Tuesday night. This information, of course, would be of great value to potential sponsors of the programs. Ehrenberg suggested that a wide range of empirical conditions support the relationship $d_{ts} = kr_t r_s \pm 1$, where:

d_{ts} = the audience (in rating points) that is duplicated at two times, s and t, on two different days of the week
r_t = the rating of the program at time t
r_s = the rating of the program at time s
k = a constant

Is the D-V relationship an empirical regularity, or is it a lawlike generalization? Certainly, the relationship is in the basic form of a generalized conditional and it also has empirical content. Ehrenberg indicates that several hundred cases examined support the relationship. However, if the D-V relationship is to be considered a lawlike statement, it must be *systematically integrated* into a body of scientific knowledge, and Ehrenberg provides no clues as to how the relationship fits existing knowledge. Actually, it appears to run *counter to* known facts about television viewing. For example, we know that different kinds of television programs attract different kinds of viewers, and that similar kinds of programs attract similar kinds of viewers. Nevertheless, the D-V relationship would lead one to conclude that the duplication of audience between a situation comedy on Monday and a situation comedy on Tuesday would be identical to the duplication between the same situation comedy on Monday and a Wednesday documentary on the political situation in Brazil (provided that the appropriate ratings are the same). On the other hand, the notion that similar programs draw similar audiences suggests that the duplication between the two situation comedies would be greater than the duplication between the situation comedy and the documentary. Therefore, the D-V relationship runs counter to our existing body of knowledge about television viewing.

Headen, Klompmaker, and Rust tested the D-V law in the United States. They conclude:

> The authors attempted to examine the major conclusions of Goodhardt and Ehrenberg in terms of a national sample of U.S. data. The basic conclusions are that, because of the differences between the U.S. and U.K. media environments, the simplifying assumptions of Goodhardt and Ehrenberg (i.e., the use of only within- and between-channel *K*-values) cannot be used if accuracy is desired. For example, because of the longer broadcast day in the U.S., duplication patterns should take into account the daypart in which audience flow patterns are being studied. Also, whereas, there seems to be no program-type loyalty in the U.K., the authors' results show positive evidence of this phenomenon. (Headen, Klompmaker, and Rust 1979, p. 340)

Is the D-V relationship a lawlike generalization? The previous analysis suggests that it is not. The D-V relationship (to the extent that it is true) is an empirical regularity because (a) it has not been systematically integrated into a body of knowledge about television viewing and (b) it actually contradicts a well-corroborated existing body of knowledge. It is important to note that classifying the relationship as an empirical regularity in no way disparages the importance or value of the discovery (if it is true). Requiring a relationship to be systematically integrated into a body of knowledge in order to be considered lawlike is one way of ensuring that we focus on the scientific explanation of phenomena (answering "why" questions), not simply the prediction of phenomena. And, we have found, prediction does not imply explanation.

In contrast to the D-V relationship, consider the sources of price reliance proposed by Shapiro (1973). He investigated the phenomenon of consumers using the price of a product as an indicator of the perceived quality of the product. He found that the tendency of consumers to rely heavily on price (price reliance) as an indicator of quality was a

generalized mental construct, an attitude or trait. He found that some people seemed price reliant regardless of the product under consideration and that some people were not price reliant. Shapiro then tested hypotheses regarding the relationships that determined the existence of price reliance. Both personal and situation-specific factors were examined. Among the relationships tested were:

1. Price reliance increases as the *credibility* of the source of price information increases.
2. Price reliance increases as the *perceived risk* in the purchasing situation increases.
3. Price reliance increases as the *specific self-confidence* of the consumer decreases.

Shapiro empirically tested these relationships (and others) and found corroborative evidence.

Are these determinants of price reliance to be considered empirical regularities or lawlike statements? They do have the basic form of generalized conditionals, and they do have empirical content—both requirements for lawlike status. However, are the observed relationships systematically integrated into a body of scientific knowledge? An affirmative reply seems justified. Shapiro demonstrates that all three relationships logically flow from the previous theoretical and empirical work of Bauer (1960, 1965/1973, 1967a, 1967b), Brody and Cunningham (1968), and Cox and Bauer (1964). Therefore, the determinants of price reliance relationships should be accorded lawlike status. Note that this does not necessarily imply that the price reliance relationships are *laws.* Lawlike statements become laws only after substantial empirical corroboration. What is meant by "substantial empirical corroboration" is a subject for Chapter 7, where we discuss the criteria for confirmation of lawlike and theoretical constructions.

The criterion that statements must be systematically integrated into a coherent body of knowledge in order to be considered lawlike raises some potentially serious problems, one of which is conservatism. There is no question but that the *systematic integration* criterion casts a conservative bias on scientific inquiry—newly discovered relationships that do not fit into some overall framework may, unfortunately, be automatically categorized as "spurious." Some observed regularities may then not receive the careful attention and further exploration that they deserve. Similarly, the systematically integrated requirement may be drastically distorted. Rescher speaks to this problem:

> The law must certainly fit into *some* pattern, but this need not of course necessarily be *the presently accepted* pattern. It is a convenient but unwarranted step to condemn the unfamiliar as unscientific, and to bring to bear the whole arsenal of scientific derogation (as "occult," "supernatural," "unscientific") that one sees, for example, orthodox psychologists launch against parapsychology.
>
> But the fact that the requirement of coherence [systematic integration] for explanatory laws can be abused does not show that it should not be used. (Every useful instrument can be misapplied.) And, of course, the proper use of this requirement

> must always be conditioned by reference to the primary requirement of correspondence—the evidence of tested conformity to fact. (Rescher 1970b, pp. 16–17)

Therefore, although researchers in marketing should require their lawlike statements to fit systematically into some larger framework, we must consciously avoid either ignoring "nonfitting" empirical regularities or rejecting them out of hand because they do not fit into some currently accepted framework or theory. All scientific knowledge is tentative.

5.5.1 Role of Empirical Generalizations

The preceding section should not be interpreted as in any way minimizing the importance of empirical generalizations. Unquestionably, empirical generalizations or empirical regularities play a prominent role in science. As noted previously, empirical regularities may become lawlike statements after they are systematically integrated into a body of scientific knowledge. A second prominent role of empirical generalizations lies within the *context of discovery.* Undoubtedly, the observation of empirical regularities by scientists is a frequent stimulus for scientific inquiry, research, and the discovery of corroborated lawlike generalizations.

For example, Edward Jenner's development of a vaccine for smallpox can be cited as an instance of an empirical regularity leading to the discovery of scientific knowledge (Hopkins 1983; Fenner and White 1976). Jenner observed that, though almost the entire population of eighteenth-century England at one time or another had smallpox, milkmaids almost never had smallpox. This observed empirical regularity sparked his scientific curiosity and led him to observe that rural women did contract a mild disease known as cowpox. He theorized that, somehow, the contraction of cowpox kept the rural women from being susceptible to smallpox. Thus, an observed empirical relationship led directly to the ultimate discovery of a vaccine for smallpox. The process that led Jenner to develop a smallpox vaccine illustrates the frequently important role that empirical regularities play in the context of discovery.

It would appear that some of the work of early synthesizers of marketing thought was spurred by observed empirical regularities. In recounting the origins of Arch W. Shaw's marketing writings, Joseph C. Seibert (in Bartels 1962) comments:

> Mr. Shaw's intellectual curiosity led him to devote a great deal of his time to the discovery of [business systems] and brought him conferences with leaders of many different types of industries. The outstanding discovery of these meetings, to Mr. Shaw, appeared to be the uniformity of procedures in spite of the variety of products produced and the outward differences of the separate organizations. (Bartels 1962, p. 234)

In the preceding case, the development of Shaw's concepts can be traced to his observation of an empirical regularity concerning the similarity of business systems across different kinds of industries and products.

A final caveat on the role of empirical generalizations seems desirable. We must carefully distinguish between asserting that observing empirical regularities may often be a

first step toward establishing lawlike relationships and asserting that the *only* way or the *best* way to discover lawlike relationships is to first observe some empirical regularities. Unfortunately, the latter assertion seems to be implied in much marketing literature. Consider the perennial debate on the best process for scientific progress in marketing. Obviously, science progresses through a process that includes both empirical work and the development of theory. One view has it that the best way to proceed is to first develop empirical generalizations from sets of data, then to search for and/or create theories that might explain the empirical generalizations, then to return to investigating the empirical generalizations in terms of the theory proposed, and so forth. Such a procedure might be illustrated as E → T → E → T, and so forth. A second process would be to develop a theory, then empirically test the theory, then modify the theory to reflect the empirical testing, then test the revised theory, and so forth. Such a process might be modeled as T → E → T → E, and so forth. Ehrenberg (1994, 1995) is the strongest, most articulate, and most consistent advocate of ETET. Most marketing researchers, however, seem to follow the TETE route. As argued by Bass (1995, p. G12), "Empirical does not always precede Theory, and thus Ehrenberg's schema [ETET] excludes TETE. The distinction is important because the theory may have other empirical applications beyond the immediate empirical generalization." Similarly, Rossiter (1994, p. 117) argues that, contra-Ehrenberg, "Many theories in marketing *are* tested extensively" (italics in original). He provides several examples of theories that have been thoroughly tested and are generally considered to be success stories in marketing research. These examples include the Fishbein model of multiattribute attitude formation, information overload theory, the elaboration likelihood model, and the attitude-toward-the-ad theory.

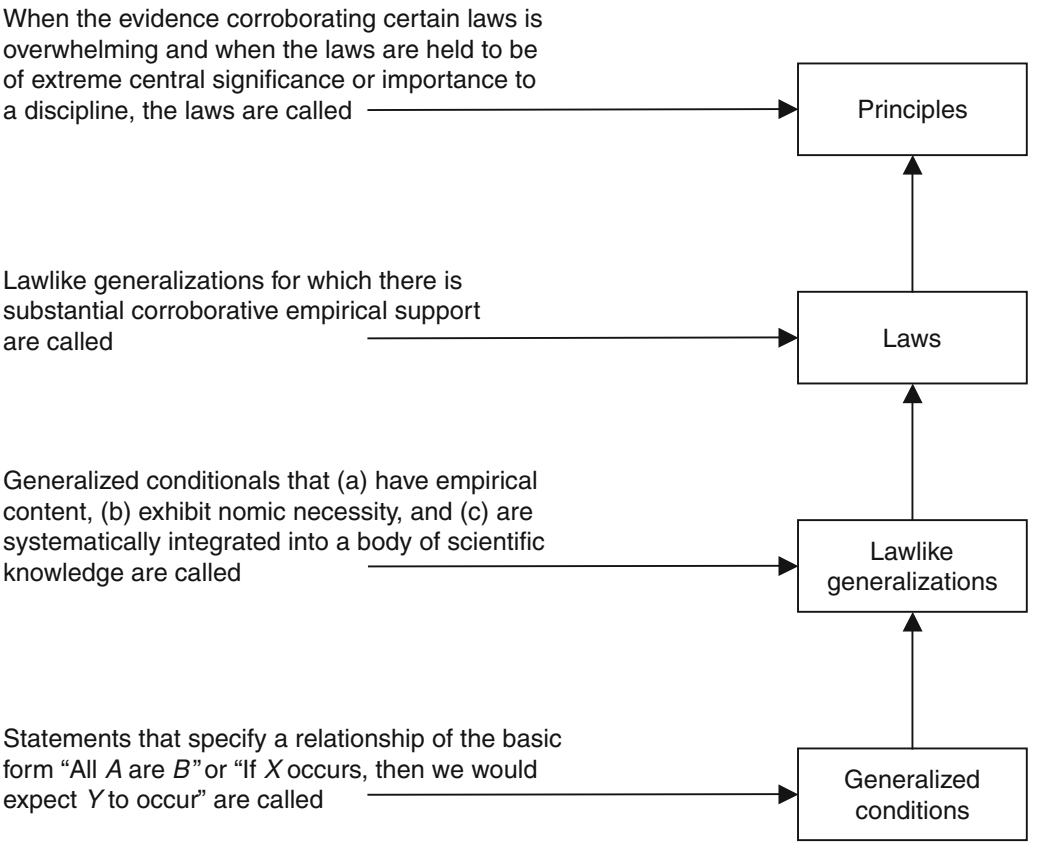
The view here, as discussed in Chapter 1, is that it is certainly possible that lawlike statements may be discovered by first amassing data on many different variables and then continuously sifting the data through ever more sophisticated mathematical and statistical sieves, as in "data mining." However, to state that this is the *only* procedure or the most preferable procedure seems totally unwarranted.

5.6 SUMMARY

We are now in a position to build a comprehensive framework for principles, laws, and lawlike statements. This framework is illustrated in Figure 5.1, which suggests that all statements that purport to be of lawlike form must specify a relationship in the form of a *generalized conditional.* Common examples of generalized conditionals are: "All instances of *A* are also instances of *B,*" and "Every time *X* occurs, then *Y* will occur."

In order for a generalized conditional statement to be a *lawlike generalization* or, alternatively, a lawlike statement, it must (a) have empirical content, (b) exhibit nomic necessity, and (c) be systematically integrated into a body of scientific knowledge. The empirical-content criterion successfully weeds out strictly analytic statements, tautologies, and nonsense generalizations from being considered lawlike. The nomic-necessity criterion serves the useful purpose of distinguishing lawlike statements from accidental generalizations such as "All products with the trade name Maxwell House have a

Figure 5.1 **Laws and Lawlike Statements**



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coffee base." Finally, the systematic integration criterion enables us to differentiate lawlike statements from strictly empirical regularities. Empirical regularities have been shown to play an important role in the context of scientific discovery.

Lawlike generalizations become *laws* when a substantial body of corroborative empirical evidence has been developed. What is meant by a "substantial body of corroborative empirical evidence" will be the topic of Chapter 7. Finally, a law becomes a *Principle* or *Law* (note uppercase "L") when the evidence corroborating the law is overwhelming and the law is held to be of extreme significance or importance to the scholars in a particular discipline.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. A "perfect system" for playing blackjack would destroy the game of blackjack, which would therefore render useless the "perfect system." Similarly, the discovery of marketing "laws" would be self-defeating. Evaluate.

2. John D.C. Little (1970) suggests that good normative decision models should be: (a) simple, (b) robust, (c) easy to control, (d) adaptive, (e) complete on important issues, and (f) easy to communicate with. Should normative decision models, like scientific laws, be required to be empirically testable? If no, why not? If yes, how would you empirically test a normative decision model? Is the distinction between normative decision models and scientific laws a useful one? How about the distinction between normative laws and normative decision models?
3. Evaluate the statement: "It is curious that marketing practitioners disclaim any interest in marketing laws because every time they make a strategic decision, they must rely either explicitly or implicitly on a presumed marketing law."
4. Alderson's (1957, p. 424) principle of postponement "requires that changes in form and identity occur at the latest possible point in the marketing flow; and changes in inventory location occur at the latest point in time." Is this a principle, law, lawlike generalization, empirical regularity, theory, or what? Is it empirically testable?
5. Suppose Congress, in its infinite wisdom, passed a law making it illegal for Procter and Gamble to distribute products through any channel other than supermarkets. Would the generalization "All products produced by Procter and Gamble are distributed through supermarkets" then pass the nomic necessity criterion?
6. Schwartz (1965, p. 8) suggests, "A law is a former theory which over time, and for the geographic area to which it is supposed to apply, has been demonstrated to yield perfectly accurate predictions each time it is used." Evaluate the usefulness of this conceptualization of law. Why did Schwartz insert his "geographic area" qualifier but not others (e.g., he could have qualified his conceptualization with reference to a particular culture or industry)?
7. Economic theory is sometimes criticized because its lawlike statements are supposed to be true only *ceteris paribus.* The phrase *ceteris paribus* is seldom found in the physical science or marketing literature. What does *ceteris paribus* imply? Is it a "defect" in economic theory? Should the physical sciences use it more often? Should marketing? Could the use of *ceteris paribus* be abused? How?
8. If the distinction between laws and principles (or Laws) is strictly honorific, is it therefore, unimportant?
9. Ehrenberg (1971, p. 32) suggests that "the *validity* of a scientific law depends only on its range of empirical generalization, that is, on the different conditions for which it is known to hold or not to hold, as shown by direct observation and analysis." Evaluate this perspective of *validity.* Does the concept of *ceteris paribus* relate to Ehrenberg's statement?
10. Evaluate the following argument:
    a. No purely analytic statement can be a lawlike generalization (such statements fail the empirical content criterion).
    b. The statement "2 + 2 = 4" is purely analytic.
    c. *All* mathematical statements have a basic form similar to "2 + 2 = 4."
    d. Therefore, all statements in mathematics are purely analytic.

e. Since no analytic statement can be lawlike, no mathematical statement can be lawlike.
f. All theories contain lawlike generalizations.
g. Therefore, mathematics contains no theory.
h. All sciences must contain theory.
i. Therefore, mathematics is not a science.
j. Mathematicians will dislike statement (i).

11. "The nomic necessity criterion is really only requiring that laws be theoretically supportable because it is really theories that enable us to 'support' counterfactual conditionals." Evaluate.
12. Bunge (1961, p. 260) proposes that "a proposition is a law statement if and only if it is *a posteriori* (not logically true), general in some respect (does not refer to unique objects), has been satisfactorily corroborated for the time being in some domain, and belongs to some theory (whether adult or embryonic)." Evaluate.
13. Is empirical evidence alone necessary and sufficient for according the status of law to a generalization? Is it necessary but not sufficient? Is it sufficient but not necessary? Is it neither necessary nor sufficient?

NOTES

1. These three normative decision rules and the phrase "marketing management concept" first appeared in General Electric's 1952 annual report. The marketing concept and McCarthy's (1960) "4 Ps" model became foundational components of the marketing management approach to the study and teaching of marketing in the 1960s (Hunt and Goolsby1988; Kerin 1996). By then, the word "management" had been dropped, and the "marketing concept" became the customary way to refer to these rules. By the 1970s many, if not most, marketing academics and practitioners had dropped rule (3) and maintained that the marketing concept meant, simply, a customer orientation backed by integrated marketing. In the 1990s, the concept of "market orientation" came to be used in conjunction with the marketing concept. The notion of market orientation, contrasted with the marketing concept, has (among other things) a dual focus on both customers and competitors (Kohli and Jaworski 1990; Narver and Slater 1990). (See Hunt and Morgan [1995] for a discussion of the relationship between the marketing concept and a market orientation.)

2. For excellent discussions of laws, see Nagel (1961, pp. 47–73); Hempel (1965c, p. 264 ff); Lambert and Brittan (1970, p. 37 ff); Kaplan (1964, p. 84 ff); and Bunge (1961).

3. Many writers use the term *confirmed* instead of the word *corroborated.* Popper (1959, p. 251 ff) suggests that confirmed, more than corroborated, is likely to indicate that the lawlike statement is known to be absolutely true, which is not a requirement for a law.

4. The classic treatment of counterfactual conditionals is in Goodman (1965). For a good review and articulation of counterfactuals, see Lange (1999). For a good discussion of counterfactual conditionals in marketing, see Gaski (1985).

6

SCIENTIFIC LAWS: ISSUES AND ASPECTS

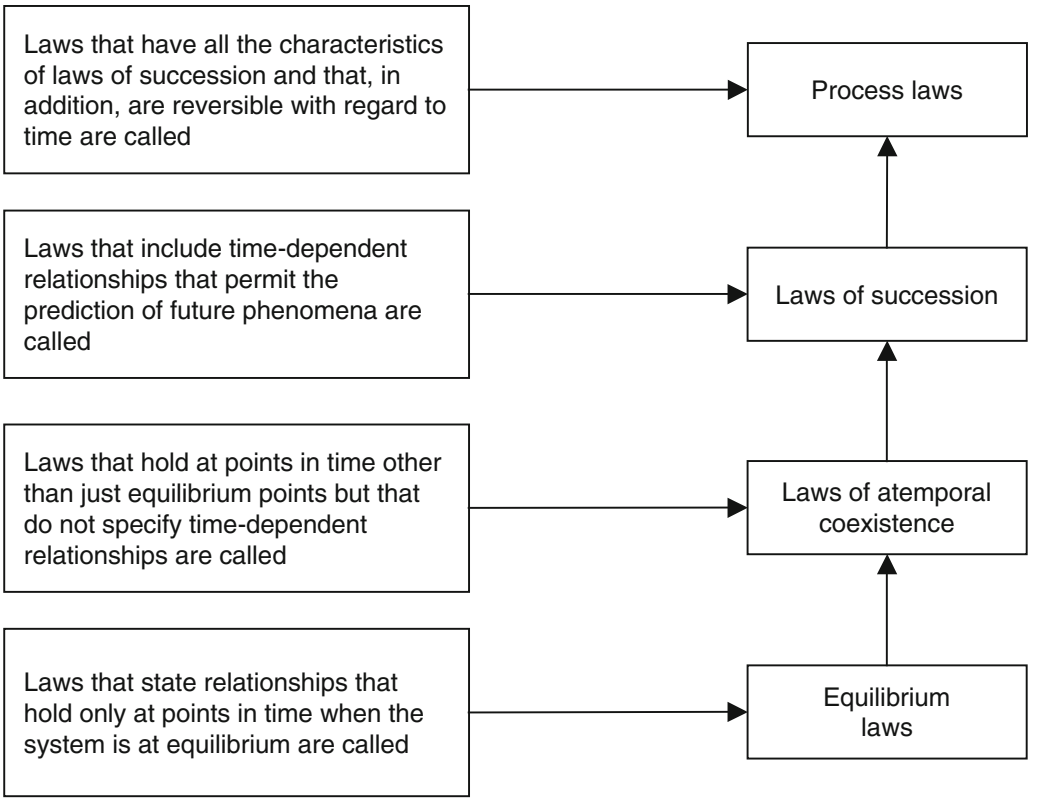
> Don't tell me our problem lies in prediction. No doubt it will remain our greatest weakness. But prediction runs parallel to explanation: the two problems are really the same one. And the better we are able to explain what has happened, the better we shall be able to predict what will.
>
> —*George C. Homans*

The previous chapter examined the basic nature of laws and lawlike statements. Lawlike statements were found to be generalized conditionals that (a) have empirical content, (b) exhibit nomic necessity, and (c) are systematically integrated into a body of scientific knowledge. Laws are lawlike generalizations for which there is substantial corroborative support. The challenge of the present chapter will be to explore the various kinds of scientific laws and to examine certain issues related to them. First, we explore how different kinds of laws incorporate time as a variable, and, thus, examine equilibrium laws, laws of atemporal coexistence, laws of succession, and process laws. Second, we systematically inquire into the nature of, and the proper role for, axioms or "assumptions" in theory construction. Third, lawlike statements will be shown to differ as to their extension and universality. In this context we examine singular statements, existential statements, statistical laws, and universal laws. Finally, the importance of carefully delimiting the extension of laws will be discussed, along with an evaluation of an extension of Weber's law and the so-called psychophysics of prices controversy.

6.1 THE TIME ISSUE

One way of exploring the various kinds of laws and lawlike statements is to analyze the manner in which the time dimension is handled. Figure 6.1 shows four kinds of laws that differ with respect to the time dimension: process laws, laws of succession, laws of atemporal coexistence, and equilibrium laws. Some very powerful lawlike statements not only explicitly incorporate time variables to facilitate the prediction of *future* phenomena but also permit the retrodiction of *past* phenomena. Bergmann (1968, p. 416) refers to these powerful statements as *process laws.* Following Hempel (1965a, p. 352), lawlike

Figure 6.1 **Laws and the Time Variable**



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statements that incorporate time-dependent relationships and allow the prediction of future phenomena but not the retrodiction of past phenomena can be called *laws of succession.* Contrasted with process laws and laws of succession, *laws of atemporal coexistence* do not specifically incorporate time as a real variable at all. Finally, certain kinds of laws of atemporal coexistence state relationships that occur only when the system is "at rest." Such statements are called *equilibrium laws* (Brodbeck 1968, p. 417). The next sections systematically explores and elaborates on the unique characteristics of each of the previously mentioned kinds of laws, each of which differs with respect to the time dimension. We begin with an analysis of laws of equilibrium.

6.1.1 Equilibrium Laws

Equilibrium laws are laws of atemporal coexistence stating that certain specified relationships in a particular system will be true only if the values of the variables are not changing over time; that is, the system must be at rest. Thus, the basic form of such laws is:

$$Y_{t(e)} = f\,[X_{t(e)}]$$

That is, specified values of *Y* are associated with specified values of *X* only when the system is at a point in time *e* when *X* and *Y* are not changing (the definition of equilibrium).

Conventional demand theory analysis prescribes that, at *equilibrium,* the consumer will purchase the particular combination of goods *X* and *Y* for which the marginal rate of substitution of good *X* for good *Y* equals the ratio of the price of *X* to the price of *Y.* The marginal rate of substitution shows the rate at which consumers are *willing* to substitute *X* for *Y,* and the price shows the rate at which they *can* substitute *X* for *Y.* Note that the relationship holds only if the system is at equilibrium. Of equal importance for demand theory, the converse is true: if the relationship does not hold, the system is not at equilibrium.

Equilibrium laws, of course, play an important part in neoclassical economics. They are also important in the approach to science proposed by functionalists. As discussed in Chapter 3, functional analysis seeks to understand a behavior pattern or sociocultural institution by determining the role it plays in keeping the given system in "proper working order" (an equilibrium kind of notion) or maintaining it as a going concern (Hempel 1965b, p. 305). Alderson, as a functionalist, suggested that marketers recognize that there are three different levels of equilibrium in organized human activities:

> First, there is market equilibrium which pertains to the network of external relations among organized behavior systems. Secondly, there is an organizational equilibrium which is a form of internal balance within an individual system. Finally, there is the more embracing concept of ecological equilibrium pertaining to the adjustment between a society and its environment. (Alderson 1965, p. 304)

Other marketing theorists have also extolled the virtues of viewing the markets for products as illustrations of equilibrating systems. Lawrence C. Lockley advocates this position:

> The concept of an equilibrium is a universal natural concept. The populations of fish and food tend to reach an equilibrium in each stream or body of water. The action of osmosis brings about an equilibrium between separated bodies of liquids. Human populations tend to reach a point of equilibrium in terms of their sources of support. Whatever organization of natural phenomena we consider, we see a tendency for forces ultimately to come to rest in an equilibrium.
>
> Strangely, this same tendency is apparent in the marketplace. Consider the case of staple foods (as will be discussed in more detail later) offered in cans. Some products are promoted by food packers as nationally advertised brands and some offered by wholesalers or retailers as private brands. Many years ago, when canned foods were relatively new in the market, some of the well-advertised brands had a disproportionate share of the market. But as time has passed, there seems to have

> developed a sort of rough equilibrium in this field, an equilibrium which is only temporarily upset by special promotional efforts.
>
> This same type of equilibrium is present, actually or incipiently, among competitors in many individual consumer markets. Men's shoes, household mechanical appliances, breakfast cereals, soaps and detergents, and bed sheets and blankets illustrate the tendency toward equilibrium. Among dentifrices, for example, the advertising, the point-of-purchase promotional material, the package design, the product formulation, and the channels of distribution have come to be about the same. Share of the market may have reached a point of stability. In the absence of any marked modification of these forces, we may have an excellent illustration of a market equilibrium. (Lockley 1964, p. 39)

Lockley then shows how the notion of equilibrium led to the conceptualization of several lawlike statements, including the principle of *drift.* "There will always be a tendency for merchandise to drift down from a 'specialty' to a 'shopping' to a 'convenience' goods classification. This is another way of saying of course that an equilibrium tends to be reestablished as soon as it is disturbed" (1964, p. 44).

Is the principle of drift an equilibrium law? The answer must depend on whether the stated relationship has time as a real variable and whether the relationship holds only at an equilibrium position. Clearly, time *does appear* as a real variable in the principle of drift, insofar as we can restate the principle as, "There will always be a tendency *as time passes* for merchandise to drift . . ." To state that an equilibrium will appear is not the same thing as specifying certain lawlike relationships that will hold *at equilibrium.* It is the latter requirement that separates equilibrium laws from laws of succession. Therefore, to the extent that the principle of drift is lawlike, it should be considered as a kind of law of succession.

A caveat seems desirable here concerning equilibrium laws and equilibrating systems. Students of marketing should carefully distinguish between (1) the assertion that there exist equilibrium laws concerning marketing phenomena and (2) the assertion that all (or even most) marketing processes and systems are inherently equilibrating (i.e., marketing systems universally tend toward the establishment of equilibrium positions). Note that statement (1) is a positive existential statement (see section 6.3.2) that, in principle, can be verified by finding examples of equilibrium laws in marketing. However, statement (2) appears to have an essentially metaphysical quality. The usefulness of believing in statement (2), as Lockley appears to do, may lie more in the context of discovery. Therefore, the appropriate question would be, "To what extent has the belief that marketing processes are inherently equilibrating led to the discovery of well-supported lawlike statements?" If this is truly the appropriate question, then there would appear to be insufficient evidence either to accept or reject the assertion that marketing systems universally or even commonly tend toward the establishment of equilibrium positions. Indeed, resource-advantage theory (Hunt 2000b), which is argued to be a general theory of competition and to be *toward* a general theory of marketing, maintains that competition is *disequilibrating.* (See Chapters 13 and 14.)

6.1.2 Laws of Atemporal Coexistence

The previous section explored one specific type of coexistence law, that is, equilibrium laws. Now, all equilibrium laws are laws of atemporal coexistence, but not all laws of atemporal coexistence are equilibrium laws. That is, many laws of atemporal coexistence state relationships that occur at points in time other than just equilibrium positions. Therefore, the basic form of laws of atemporal coexistence would be:

$$Y_t = f(X_t)$$

Specified values of Y at any time t are associated with specified values of X at the same time t.

Bergmann (1957, p. 102) refers to all laws of atemporal coexistence as *cross-sectional laws* and states that the concept of "cross-sectional [law] is taken from the metaphor that considers a state a temporal cross-section of a process. Such laws state functional connections obtaining among the values which several variables have at the same time."

Dubin takes a slightly different position on atemporal coexistence laws. Identifying these laws as *categoric laws,* Dubin says:

> The recognition of a categoric law of interaction is facilitated by noting that its typical form employs the words *is associated with.* Synonyms for this phrase serve, of course, to provide the same identification. . . . Categoric laws are symmetrical. It does not matter whether one or the other of the units comes first in the statement of the law. Thus, "juvenile delinquency and broken homes are positively related" is identical with "broken homes and juvenile delinquency are positively related." The symmetry of categoric laws is emphasized, for this fact buttresses that a law of interaction is not a statement of causality. What, indeed, is the meaning of cause if the units *juvenile delinquency* and *broken homes* can be interchanged without restriction in the law of interaction between them? (Dubin 1969, pp. 100–101)

Dubin then admonishes readers to avoid jumping to the conclusion that the broken-home condition preceded in time the juvenile delinquency and, therefore, *caused* it.

Dubin's caveat merits the scrutiny of marketing students. Caution must be exercised when attempting to infer the temporal sequence of a relationship from data that are strictly cross-sectional or associative. For example, there may be a strong temptation to infer that high advertising expenditures cause high sales. Such an inference may be unwarranted if the supportive data are cross-sectional because it is common knowledge that many firms set their advertising budgets at specified percentages of sales. Thus, the data may equally support either of the two assertions: "advertising causes sales" or "sales cause advertising." There are numerous instances where the temporal sequentiality of the relationships remains in doubt. Are changes in attitude lawfully related to subsequent changes in purchase behavior? Or, are changes in purchase behavior lawfully related to subsequent changes in attitude? Or, does the relationship go in both directions depending

on the situation? The major conclusion to be drawn from the preceding discussion would appear to be: if the data used to test lawlike statements are cross-sectional (i.e., all data are drawn from the same time period), adopt the agnostic position of treating any corroborated or confirmed lawlike statements as *laws of coexistence* rather than *sequential laws* or *causal laws.* By adopting this agnostic position the researcher will, it is hoped, remain alert to the possibility that the actual direction of the temporal sequence may be opposite to intuition.

However, though scientists have the luxury of the agnostic position with respect to the causal direction in laws, managers and policymakers do not. Consider again Dubin's "juvenile delinquency–broken home" law of interaction. Whether the relationship is (1) juvenile delinquency → broken homes or (2) broken homes → juvenile delinquency impacts on policy in terms of the temporal order of possible corrective actions. Should the emphasis of policy be on preventing broken homes in order to reduce juvenile delinquency or on curbing juvenile delinquency and, thus, preventing broken homes? For policymakers, choices must be made. Why did Dubin—a social scientist—not recognize specifically (or even acknowledge) that not all stakeholders of science have the luxury of the agnostic position? The answer is that he was so strongly influenced (as were the logical positivists and logical empiricists) by Humean skepticism. Thus, the tripartite rule is: (1) the scientist *qua* scientist may/should be agnostic on causal direction; (2) the scientist *qua* advocate or consultant may not and/or should not be agnostic; (3) the policymaker and manager *cannot* be agnostic (for choices must be made).

6.1.3 Laws of Succession

The preceding sections have discussed the two kinds of laws that do not include time as a real variable. *Laws of succession,* however, provide that specified values of one or more variables will be succeeded in time by specified values of one or more other variables (Hempel 1965a, p. 352). Dubin (1969, p. 100) refers to such laws as *sequential laws,* and Kaplan (1964, p. 109) uses the term *temporal laws.* The basic form of such laws is:

$$Y_{t+n} = f(X_t)$$

One extremely useful perspective for evaluating laws of succession is to examine the *level of specificity* of the time variable in the proposed relationships. Varying degrees of specificity are found in the relationships in marketing models and theories. Many marketing theorists only minimally specify the time variable in their theoretical constructions. Others are much more explicit in identifying which variables are hypothesized to influence other variables over time.

Consider the Engel, Kollat, and Blackwell (1973) model of consumer behavior. The authors, in their discussion, often do not explicitly state which relationships are time-dependent and which are not. There is little doubt about some of the proposed relationships. For example, the authors clearly imply that changes in exposure precede changes in attention, which precede changes in comprehension and retention. But what about the

relationships among these constructs: attitude, evaluative criteria, and personality? What is the proposed temporal sequence of changes in these constructs? Possibly the authors mean to propose that these relationships be considered laws of atemporal coexistence. The ambiguity results from the minimal specification of the time variable. Unfortunately, many marketing models only minimally specify the time-dependent relationships. Sometimes, the implied time-dependent relationships are intuitively clear; at other times, they are not.

Next, consider the "hierarchy of effects" model investigated by Palda (1966) and based on the cognitive-affective-conative sequence of psychological states proposed by Lavidge and Steiner (1961). As stated by Terrence O'Brien (1971, p. 284), the hierarchy of effects model consists of three statements:

1. Awareness influences attitude over time, and the relationship is expected to be positive.
2. Attitude influences intention to purchase over time, and the relationship is expected to be positive.
3. Intention influences actual purchase over time, and the relationship is expected to be positive.

Note that the time-dependent relationships are specified: changes in awareness precede changes in attitude, which precede changes in intention. O'Brien (1971, p. 289) then tested the relationships using the method of cross-lagged correlations on panel data on 636 women and concluded that the results basically supported the hierarchy predictions. The point of the illustration is that when the time-dependent relationships are unambiguously specified, the process of empirical testing is facilitated.

Although the hierarchy of effects model is superior to many marketing models in specifying the time-dependent relationships, it is far from optimal in this regard. Note that the hierarchy of effects model states that changes in awareness precede changes in intention. However, the model does *not* specify the length of time between these expected changes. A few marketing models are very specific in identifying the time dimension. The Nicosia (1966) model of consumer behavior is one such model. The equations composing Nicosia's linear model and his explanation follow:

1. $dB(t) / dt = b[M(t) - \beta B(t)]$
2. $M(t) = mA(t)$
3. $dA(t) / d = a[B(t) - \alpha A(t)] + cC(t)$
4. $C(t) = C$

Specifically, Equation 1 says that the time rate of change in the level of buying of a certain brand by a consumer of a certain type is directly proportional to the difference $[M(t) - \beta B(t)]$. Equation 2 states that motivation M is dependent on attitude A. Then, Equation 3 says that the time rate of change in level of attitude is directly proportional to the difference $[B(t) - \alpha A(t)]$ plus a constant multiple of the advertising level C.

Finally, the level of C is defined by Equation 4 to be constant with respect to time (Nicosia 1966, p. 209).

Thus, the nature of the time-dependent relationships is very clearly specified in the Nicosia model. Once again, this should greatly facilitate testing the model. However, this should not be interpreted as meaning that the entire model is easily testable. Severe measurement and parameter estimation problems exist with the Nicosia model. In evaluating the falsifiability of the Nicosia model, Zaltman, Pinson, and Angelmar (1973, p. 108) conclude that "one can even fear that no test of the model will be possible without a significant alteration of its very nature."

6.1.4 Process Laws

Up to this point, we have considered equilibrium laws, laws of atemporal coexistence, and laws of succession, and how each of these kinds of laws incorporates the time variable. However, laws of succession are not the final way to deal with time. Bergmann (1968, p. 416) uses "process law" to refer to lawlike statements that have all the characteristics of laws of succession and, in addition, are reversible with regard to time. Laws of succession enable one to predict future phenomena; process laws enable one to both predict future phenomena and retrodict past phenomena. This is the meaning of "reversible with regard to time." Thus, if we know the position and velocity of a planet at a single instant in time, the laws of Newtonian celestial mechanics enable us not only to predict the position and velocity of the planet in the future but also to retrodict the position and velocity of the planet at points of time in the past.

As the reader will note, process laws are very powerful kinds of statements. To the best of the writer's knowledge, at present there exist no examples of process laws in marketing. This should not be too depressing because, in fact, there probably exist no examples of process laws in any of the social sciences. The development of process laws, and, hence, the accumulation of what Bergmann calls "process knowledge," would still be a useful goal or ideal point for marketing and the social sciences, even if the objective were never to be reached.

6.2 AXIOMS, FUNDAMENTAL LAWS, AND DERIVATIVE LAWS

Few issues in the methodology of the social sciences have spawned as much controversy as the nature of, and the proper role for, axioms or "assumptions" in scientific inquiry. The debate has been especially lively in economics, with Friedman, Samuelson, and other writers expressing sharp differences of perspective.[1] Although all of the major issues in the so-called Friedman-Samuelson debate cannot be specifically evaluated here, two aspects of the controversy are appropriate here: (1) What are axioms or assumptions? and (2) What does it mean to say that "the axioms of a theory are assumed to be true"?

To appreciate the nature and role of axioms in scientific inquiry requires some elaboration of the notions of *fundamental laws* versus *derivative laws.* Hempel (1965c, p. 267) suggests that all lawlike statements in any theory can be categorized as either fundamental

or derivative. The set of derivative laws in a theory consists of all those laws that can be deduced from other laws *in the same theory.* Thus, Kepler's laws concerning the motions of planets can be deduced or derived from the more fundamental Newton laws. In marketing, the "square root law," which states that to double the attention-getting power of an advertisement, the size of the advertisement must be increased fourfold, can be derived from the more fundamental Weber's law (Meyers and Reynolds 1967, p. 13).

The fundamental laws of a theory are those that (1) are used to deduce other laws and (2) cannot themselves be deduced from other laws in that same theory. The fundamental laws of a theory are the axioms of that theory, and the derived laws are often called theorems. As Bergmann (1957, p. 131) has observed, "The laws of a theory are deduced from its axioms."

Two observations need emphasizing with regard to fundamental versus derivative laws. First, laws that are fundamental (axioms) in one theory can be derived (theorems) in some other theory (Bunge 1961, p. 268). For example, Newton's laws are fundamental in Newtonian mechanics but are derived laws vis-à-vis Einstein's theory of relativity. This implies that the categorization of fundamental versus derived is *theory specific.* Second, for some theories there may be a choice between which laws should be considered fundamental and which should be considered derived. Consider a hypothetical theory composed of five laws, L_1, L_2, L_3, L_4, and L_5. One construction of the theory may consider L_1 and L_2 to be fundamental, since they can be used to derive L_3, L_4, and L_5. On the other hand, another construction may consider L_1 and L_3 to be fundamental and use them to derive L_2, L_4, and L_5. In such cases, the theorist has a choice of which statements should be considered as fundamental.

So, if the axioms of a theory are the fundamental laws of the theory, what does it mean to assert that "the axioms of a theory are *assumed* to be true"? Unfortunately, the preceding assertion seems often to be interpreted as meaning either that the axioms *should not* be empirically tested to see whether they are consistent with reality or that the axioms cannot be so tested. Nothing could be further from the truth! The selection or recognition that certain lawlike statements in a theory are fundamental, and, thus, to be called "axioms," convey to them no sanctuary from the criterion that all purportedly lawlike statements must be empirically testable. Empirically testing the axioms of a theory is both possible and desirable. The assertion that the axioms are assumed to be true does not mean that the axioms are assumed to be true *empirically.* Rather, the axioms of a theory are assumed to be true *analytically.*

Understanding the difference between assuming that axioms are true empirically and assuming that axioms are true *for strictly analytical purposes* is crucial for clearing up many of the misconceptions about the proper role of axioms in theory construction and evaluation. Therefore, what does the phrase "for strictly analytical purposes" imply? Recalling our distinction between fundamental and derivative laws, the statement that axioms are assumed to be true for strictly analytical purposes implies that we assume axioms to be true *only* for the purpose of generating derivative laws and other statements. Therefore, "for strictly analytical purposes" comprises the following kinds of processes: "if these statements (the axioms) are true or false, then the following statements (theorems

or hypotheses) are true or false." The preceding discussion can be summarized by noting that we assume that axioms are true for the purpose of *constructing* theory rather than for the purpose of *evaluating* theory.

6.2.1 Bridge Laws

One set of derivative laws deserving special attention is *bridge laws,* or what Hempel refers to as *bridge principles:*

> [Bridge laws] indicate how the processes envisaged by the theory are related to empirical phenomena with which we are already acquainted, and which the theory may then explain, predict, or retrodict. . . . Without bridge principles, as we have seen, a theory would have no explanatory power. . . . Without bridge principles, the internal principles of a theory would yield no test implications, and the requirement of testability would be violated. (Hempel 1966, pp. 72–75)

Bridge laws, then, are derivative laws that "bridge the gap" between the general laws in any particular theory and the specific classes of empirical phenomena under investigation. Sometimes when researchers refer to the "guiding hypotheses" or even "hypotheses" of their research, they are implicitly referring to bridge laws.

An example will better serve to illustrate the nature and function in theory of fundamental laws, derivative laws, and bridge laws. The marketing implications of Leon Festinger's (1957) theory of cognitive dissonance provide an excellent vehicle for illustrating these concepts. Before discussing the lawlike statements in Festinger's theory, four definitions are necessary:

Definition 1. Cognitions are the bits of knowledge that people have about themselves, about their behavior, and about their surroundings.
Definition 2. Two cognitions are in a dissonant state if, considering these two alone, the obverse of one cognition would follow from the other.
Definition 3. Two cognitions are consonant if one cognition does follow from the other.
Definition 4. Two cognitions are irrelevant if one cognition implies nothing at all concerning the other.

With the preceding four definitions in mind, the core of the theory of cognitive dissonance can be stated in terms of three *fundamental lawlike* (FL) statements:

FL_1: After a decision is made, there may exist dissonant or "nonfitting" relations among cognitive elements.
FL_2: The existence of dissonance gives rise to pressures to reduce the dissonance and to avoid increases in dissonance.
FL_3: Dissonance can be reduced by (a) changing one or more of the cognitive elements,

(b) adding new consonant elements, and (c) decreasing the importance of the elements in the dissonant relations.

Scores of dissonance theorists have used these fundamental lawlike statements to generate derivative laws. While conducting research on appliance purchases, the present writer (1970) used the fundamental laws of dissonance theory to derive the following bridge laws (BL):

BL_1: Consumers will experience cognitive dissonance after the decision to purchase a major appliance.

BL_2: If retailers provide information to recent purchasers of major appliances, and if the information reassures the consumers that they made wise decisions, then the information will assist the consumers in their efforts to reduce cognitive dissonance.

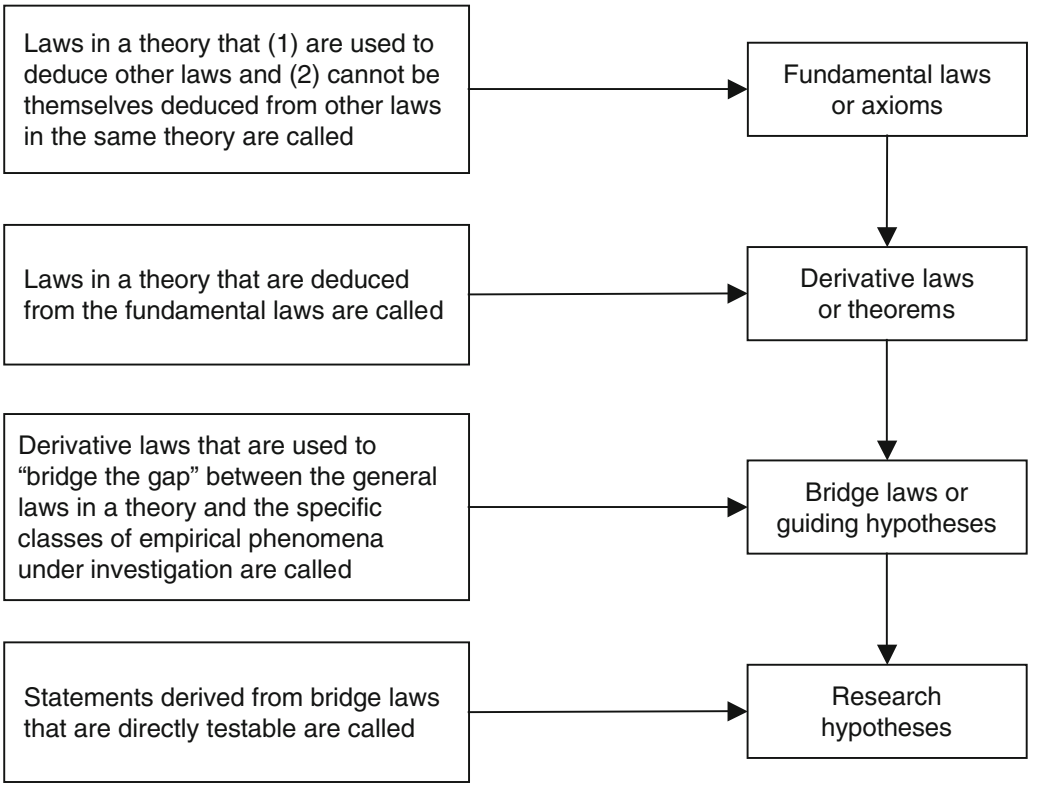
The first bridge law can be derived from the set of fundamental laws on the basis that the purchase of a major appliance would be an important decision for consumers. Furthermore, consumers would have dissonant cognitions because it is unlikely that the chosen brand model would be rated the best on all relevant characteristics, for example, style, color, and price. The second bridge law follows from the notion that the post-transaction reassurances provided to the consumer would be perceived as adding consonant cognitions, which is one method for reducing the dissonance of the consumer. Note that both bridge laws enable the researcher to span the gap between the general relationships in the fundamental laws and the specific relationships in real-world phenomena.

The writer then used the preceding bridge laws, a sample of 152 recent purchasers of refrigerators, and an experimental research design to test, among other things, the following *research hypothesis:* the subjects who received the post-transaction reassurances will have lower perceived dissonance scores (i.e., will be less dissonant) than the subjects in the control group. The results of the study provided modest support for the research hypothesis, and, therefore, the underlying bridge laws, and, finally, the entire theory in general.

All research that purports to test some theoretical construction will employ bridge laws. They may be either explicitly stated or implicitly implied. The more explicitly the bridge laws are stated, the lower the chance that the researcher will make a logical error. Often the hypotheses that are actually tested in a research project are not derivable from the theory that is supposedly being evaluated, simply because the researcher has not specifically stated the bridge laws. Such research is useless at best and misleading at worst.

In summary, Figure 6.2 shows that the fundamental laws or axioms of a theory (1) are used to deduce other laws and (2) cannot themselves be deduced from other laws in that same theory. Axioms are not assumed to be true empirically; rather, they are assumed to be true for the analytical purpose of deriving other statements. The derivative laws in a theory are deduced from the fundamental laws. Bridge laws are derivative laws that bridge the gap between the general laws of a theory and the specific classes of phenomena under investigation. As the cognitive-dissonance example illustrated, bridge laws are needed to

Figure 6.2 **Fundamental and Derivative Laws**



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derive the research hypotheses for testing purposes. Researchers often do not specifically state the bridge laws and, instead, jump directly from other more general derived laws or even fundamental laws to the directly testable research hypotheses. Such procedures run the risk of testing research hypotheses that cannot in fact be derived from the set of lawlike statements that comprises the theory.

6.3 EXTENSION AND UNIVERSALITY

Bunge (1961, p. 266) suggests that both lawlike and nonlawlike statements should be categorized by their extension or range. The *extension* of a statement consists of the set of all possible objects (past, present, and future) covered by the statement. The work of Weitz (1981) illustrates the importance of carefully delineating the extension of theories and propositions. Weitz reviews the research that has been conducted on salesperson performance using variables such as sales behaviors, behavioral predispositions, and salesperson capabilities. He concludes that past research results were contradictory or inconclusive, in part because the researchers were attempting to uncover universal char-

acteristics or behaviors that would be predictive of performance over too wide a range of situations. That is, the researchers were seeking generalizations with an *extension* that was unlikely. Weitz (1981, p. 361) then suggests a contingency framework for sales effectiveness based on the following basic postulate: "The effectiveness of sales behaviors across customer interactions is contingent upon or moderated by (a) the salesperson's resources, (b) the nature of the customer's buying task, (c) the customer-salesperson relationship, and interactions among (a), (b), and (c)."

Knowledge of the extension of a statement is valuable both because statements differing in extension have different roles in theory construction and because statements differing in extension vary with respect to their confirmability and falsifiability. *Confirmable* means the extent to which a statement is capable of being shown to be empirically true; that is, does the statement accurately describe the real world? *Falsifiable* means the extent to which a statement is capable of being shown to be empirically untrue; that is, how conclusively can we show that the real world is not arranged in accordance with the statement? As will be demonstrated, not all statements that are confirmable are at the same time falsifiable. Likewise, some statements are falsifiable but not confirmable; some are *neither* confirmable nor falsifiable. Others are only *weakly* confirmable or falsifiable. We shall restrict the discussion to four basic kinds of statements that have differing degrees of extension: singular statements, existential statements, statistical laws, and universal laws.

6.3.1 Singular Statements

Singular statements, sometimes referred to as particular (Popper 1959, p. 27) or observation (Hempel 1965b, p. 103) statements, extend only to specific phenomena that are bound in time and space. Singular statements are never lawlike statements because they do not have the form of generalized conditionals, which is a basic requirement for any statement to be considered lawlike. Singular statements play a crucial role in the confirmation or validation of theories and laws because the research hypotheses that are used to test theories and laws are usually singular statements.

The research hypothesis concerning cognitive dissonance discussed in the preceding section is illustrative of singular statements in marketing research: "The subjects who received the post-transaction reassurances will have lower perceived dissonance scores (i.e., will be less dissonant) than the subjects in the control group." That the statement is singular is evident from the fact that it refers to specific subjects in specific groups and specific scores taken at identifiable points in time. The dissonance hypothesis also shows the tremendous value of singular statements in testing theories. The statement is (a) derived from dissonance theory and (b) both confirmable and falsifiable. If the actual data show that the dissonance scores of the subjects in the experimental group are lower than the scores of the subjects in the control group, then the hypothesis (singular statement) is *confirmed.* Because the hypothesis is derived from the theory, the confirmation of the hypothesis is evidence in support of dissonance theory; that is, it *corroborates* (or tends to confirm) dissonance theory. Conversely, if the data show the opposite results, then the hypothesis is *falsified,* and, thus, we have evidence noncorroborative of dissonance theory.

It is in this sense that singular statements are generally both confirmable and falsifiable, and, consequently, they play a vital role in theory validation.

6.3.2 Existential Statements

Existential statements are statements that propose the existence of some phenomenon. "There exist products that have life cycles." "People have psychological needs." "The abominable snowman exists!" All of the previous statements are existential in basic form. Even though their extension is greater than that of singular statements, note that existential statements are not lawlike because they do not have the form of generalized conditionals.

Zaltman, Pinson, and Angelmar (1973, p. 66) suggest that all existential statements are purely confirmable but not falsifiable. To illustrate their point they cite Martilla (1971, p. 173) as an example of an existential statement: "There are opinion leaders in industrial firms." In contrast, the position taken here is that, though all existential statements are purely confirmable, only those statements whose extension or range is unqualified or unbounded are not falsifiable. Qualified or bounded existential statements are both confirmable and falsifiable. Zaltman et al.'s example is actually a bounded existential statement and, hence, is capable, at least in principle, of being falsified. That is, there is a bounded or finite number of industrial firms. Therefore, one could, in principle, examine the entire set of industrial firms and potentially falsify the statement "There are opinion leaders in industrial firms." Contrast the preceding with the unqualified existential statement, "There are opinion leaders." Finding a single opinion leader would confirm the statement, but, because of the unbounded extension of the statement, it is not falsifiable.

In attempting to explore the foundations of consumer behavior, Tucker (1967, p. 134) asserted two "propositions":

Proposition 1. Someone goes through some process and acquires something with some effect.
Proposition 2. Someone uses something in some way with some effect.

These "propositions" are in reality unqualified existential statements, and, thus, are confirmable but not falsifiable. The first proposition can be confirmed as true if one can discover a single person who has gone through some process and acquired something with some effect. Nevertheless, no possible research design could possibly show the statement to be false.

The primary role of these "propositions" and other existential statements in marketing research is probably heuristic. For example, if one adopts the existential belief that there exist lawlike relationships among marketing phenomena, then one may attempt to discover the relationships. On the other hand, if one holds firmly the belief that the relationships among marketing phenomena are nonlawlike, then why conduct research? It is precisely in this context that the belief or nonbelief in existential statements plays a heuristic role in research.

6.3.3 Statistical Laws

The nature and form of statistical laws were first broached in the evaluation of various methods of scientific explanation in Chapter 3. Laws of basically statistical form gain prominence when there are a large number of variables (many of which are often unknown) that influence the phenomenon to be explained or predicted. Therefore, the exact specification of the relationship between changes in the phenomenon to be explained or predicted and any other single variable is thwarted by other variables. Under such conditions, theoreticians frequently rely on statistical laws that state an indeterministic relationship between variables. Such lawlike statements have substantially greater extension than either singular statements or existential statements because statistical laws *do* have the form of generalized conditionals. That is, the relationships implied in statistical laws extend to a far greater number of objects and phenomena than do singular or existential statements.

Kaplan (1964, p. 97) refers to one particular subset of statistical laws as *tendency* laws. Such laws state that there *tends* to be a relationship, usually rather loosely specified, between two variables. Commentators on laws in marketing would probably agree that *most of the lawlike statements in marketing are (explicitly or implicitly) tendency laws.* For example, the generalizations discussed in section 5.2 are tendency laws. Other examples of tendency laws in marketing include: "Opinion leaders [tend to] meet more salesmen than nonleaders" (Schiffman and Gaccione 1974, p. 50). "Brand loyalty [tends to vary] directly with perceived satisfaction with the old brand" (Newman and Werbel 1973, p. 406). "Lower income consumers [tend to] prefer credit contracts that include the lowest monthly payments" (Walker and Santer 1974, p. 73). "The greater the cost of the product considered, the greater the tendency for two or more family members to be involved in the decision process" (Granbois 1971, p. 196). A final illustration: "Audiences tend to expose themselves selectively to those messages which best fit their existing predispositions or inclinations" (Bogart 1962, p. 53).

Unlike singular statements, tendency laws (like all statistical laws) are neither *strictly* confirmable nor *strictly* falsifiable. To illustrate this, consider how one would test the "opinion leader" statement mentioned in the previous paragraph. The procedure would probably involve obtaining a sample of opinion leaders and nonleaders and then measuring their respective contact with salespeople. No matter how strong the relationship found, the evidence would never be *conclusive* in favor of or against the "opinion leader" statement. On the one hand, if the data contradicted the statement, defenders could always claim that the data base was too small, or was biased, or that the data were "contaminated" or "noisy." For example, Farley and Ring (1970, p. 435) found very low coefficients of determination in their test of the Howard-Sheth theory of buyer behavior. They then postulated that "noisy data" was the problem, rather than the low explanatory power of the theory.

If, on the other hand, the results of a research project *supported* the "opinion leader" statement, attackers could claim that the observed relationship was "spurious" and that the "relationship could disappear with a larger sample or a different kind of test." Therefore,

tendency laws are neither *strictly* confirmable (i.e., able to be proved conclusively true) nor *strictly* falsifiable (i.e., able to be proved conclusively false). Rather, test results can be shown to be either *consistent* or *inconsistent* with the tendency law in question, and, thus, *corroborative* or *noncorroborative.*

Observations of the preceding kind lead many scholars to debunk the whole notion of statistical laws. Bunge suggests that this would be a mistake:

> Some die-hard classical determinists claim that stochastic statements do not deserve the name of law and are to be regarded, at their best, as temporary devices. This anachronistic view has no longer currency in physics, chemistry, and certain branches of biology (notably genetics), especially ever since these sciences found that all molar laws in their domains are stochastic laws deducible (at least in principle) from laws concerning single systems in conjunction with definite statistical hypotheses regarding, e.g., the compensation of random deviations. Yet the prejudice against stochastic laws still causes some harm in psychology and sociology, where it serves to attack the stochastic approach without compensating for its loss by a scientific study of individuals. (Bunge 1967a, p. 336)

The preceding advice by Bunge is also wise counsel for marketing. Tendency laws have played, do play, and will continue to play a central role in marketing theory. The fact that such laws are only *weakly* confirmable and *weakly* falsifiable should be no cause for methodological alarm.

There are other kinds of statistical laws besides tendency laws, the most prominent of which is the *probability* law. Recall that the relationship between the variables in a tendency law is usually very loosely specified. In contrast, the relationship between the variables in a probability law is clearly specified in the form of a probability or relative frequency statement:

$$P(G, F) = r$$

That is, the probability of event G, given that event F has occurred, is r. Or, alternatively, in the long run, the proportion of cases of F that are also G is r. "The probability of throwing an 'ace' given a 'fair die' is 1/6." (The reader may want to review the section on theories of probability in Chapter 3 at this point.)

Although, as Popper (1959, p. 189) has pointed out, probability laws are not falsifiable, they are in general more powerful than tendency laws precisely because the relationships between the variables are more clearly specified. This increases their predictive and explanatory power, and thus their susceptibility to empirical testing and corroboration.

Probability laws are less common in marketing than tendency laws because the existence of a probability law between two variables presupposes that the other variables that influence the process interact either in a random manner or at least in a consistent way with the phenomenon in question. That is, the law that the probability of throwing an ace with a fair die equals 1/6 presupposes that such factors as initial velocity and the

direction of the throw, which do in fact influence the results of each single toss of the die, will be randomly distributed over time. Therefore, the factor determining the probability of an ace on any throw will be the geometry of the cube. The lack of probability laws in marketing can be ascribed to the fact that in most marketing processes the other variables that might influence the phenomenon in question do not exert either random or consistent interactions over time.

The work of Bass (1969, pp. 215–27) on the rate of diffusion of innovations illustrates a probability law in marketing. Bass classified the initial purchasers of new consumer durable goods into innovators and imitators, where the latter group included early adopters, the early majority, the late majority, and laggards. From the basic notion that imitators are primarily influenced in their purchase of durables by other buyers, Bass proposed the following probability law: "The probability that an initial purchase will be made at *T,* given that no purchase has yet been made, is a linear function of the number of previous buyers." Bass then tested his probability law on purchasers of eleven consumer durable goods and found substantial corroborative support. Readers can satisfy themselves that the Bass proposition is in fact a probability law by observing that it has the basic form $P(G, F) = r$. In this case, the probability r is a linear function of the number of previous buyers, rather than a simple constant.

The probability law proposed by Bass, like many statistical laws, is as significant for what it excludes as it is for what it includes. The law excludes certain variables from playing significant roles in determining the rate of purchase of new durable goods. For example, one might propose *a priori* that different levels of advertising would influence the rate of purchase. However, since this variable is not explicitly included in the relationship, then, if the probability law is true, we must conclude that either (a) advertising does not influence (or only minimally influences) the rate of purchase or (b) advertising influences the rate of purchase, but it does so in a consistent manner across different products. In the latter case, advertising would be *implicitly* incorporated in the probability statement.

6.3.4 Universal Laws

Laws of *strictly universal form* take the form of universal generalized conditionals and constitute the prototypes of all laws. Universal laws state: "Every time *A* occurs, then *B* will occur," or "All *A* are *B,*" or "For any *x,* if *x* is an instance of *A,* then *x* is an instance of *B*." Note that universal laws do *not* simply state that "*B* exists," as would an existential statement. Neither do they state that "*B* tends to be associated with *A,*" as would a tendency law. Nor do universal laws state that "the probability of *B* happening, given *A,* is *r,*" as would a probability law. Because laws of strictly universal form extend to all instances of *A,* they have greater extension than singular statements, existential statements, or statistical laws.

The tremendous power of universal laws lies in their being falsifiable in a very strict sense. As Popper (1959, p. 69) has observed, laws of universal form can be alternatively expressed as negative existential statements or "there-is-not statements." An example from Newtonian mechanics will illustrate this point. Newton's third law of motion states

that for every action there is a reaction equal in magnitude and opposite in direction. Note that this law can be stated alternatively as a negative existential statement, "There exists *no* action for which there is *not* a reaction equal in magnitude and opposite in direction." All laws of strictly universal form can be similarly reconstructed as negative existential statements.

As previously discussed, positive existential statements are strictly confirmable. That is, to confirm the statement "Opinion leaders exist," one need only find a single opinion leader. Similarly, negative existential statements are strictly falsifiable. The negative existential statement "Opinion leaders do not exist" can be falsified by finding a single opinion leader. *Therefore, because all universal laws can be alternatively expressed as negative existential statements, all universal laws are strictly falsifiable.*

Universal laws have greater explanatory and predictive power than statistical laws. Recall that the deductive-nomological (D-N) model of scientific explanation, discussed in chapter 3, has the following structure:

$$\begin{array}{l} C_1, C_2, \ldots C_k \\ L_1, L_2, \ldots L_k \end{array} \Big\} \text{ Explanans } S$$
$$\overline{\qquad\qquad}$$
$$\text{E} \qquad \} \text{ Explanandum } E$$

In this model, the characteristics (C_1, C_2 . . .) of the situation and the strictly universal laws (L_1, L_2, . . .) deductively imply the phenomenon to be explained, *E.* Thus, the D-N model explains the occurrence of a phenomenon *E* by invoking a universal law stating that certain antecedent circumstances C_k are invariably followed by phenomenon *E* and then noting that circumstances C_k had, indeed, occurred.

The most common form of statistical explanation is the inductive-statistical (I-S) model:

$$\begin{array}{l} C_1, C_2, \ldots C_k \\ SL_1, SL_2, \ldots SL_k \end{array} \Big\} \text{ Explanans } S$$
$$\overline{\overline{\qquad\qquad}} \text{ [it is very likely that]}$$
$$\text{E} \qquad \} \text{ Explanandum } E$$

In the I-S model of explanation, unlike the D-N model, the phenomenon to be explained is not a logical, deductive consequence of the explanans. The I-S model states that, given circumstances C_k and certain statistical laws SL_k, then it is *very likely* that *E* would have occurred (or would tend to occur). Because the laws are statistical, not universal, the explanandum *E* is not a logical consequence of the premises *S,* in the sense that even if *E* does not occur, *S* could still be true. Therefore, universal laws have greater explanatory power than statistical laws, and a structurally similar argument could be developed to demonstrate that they have greater predictive power as well.

As Leone and Schultz (1980, p. 12) point out, "There are no universal laws in marketing." In this regard, marketing does not differ from the other social or behavioral sciences,

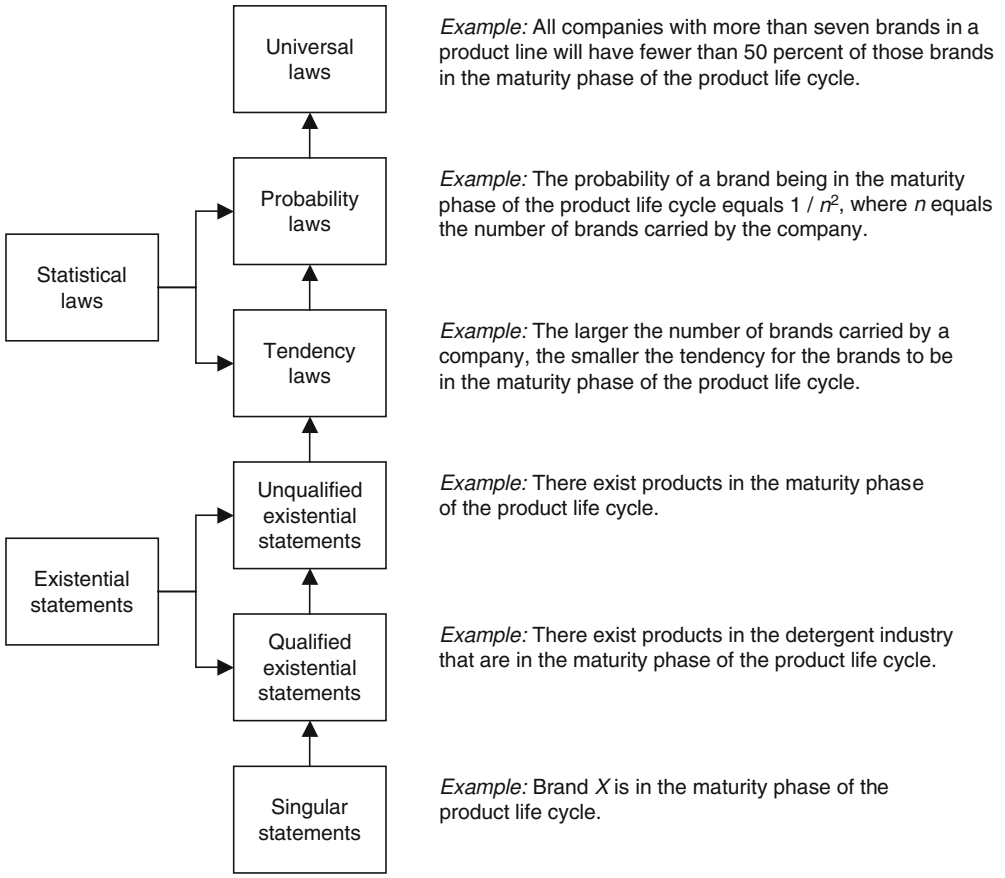
none of which contain laws of strictly universal form (Kaplan 1964, p. 97). The previous statements do not mean that theorists in marketing (or the other behavioral sciences) never state their lawlike generalizations in universal form. Rather, even though some theorists *state* their lawlike generalizations in universal form, they expect their readers to *interpret* the laws statistically. Readers should ask themselves the following question when they confront any lawlike generalization that is stated in strictly universal form: "Would the theorist be willing to accept the conclusion that his law is false if he/she were shown the results of a *single experiment* where the law did not predict correctly and where the theorist was convinced that there were neither methodological nor observational errors in the experiment?" If the answer to this question is affirmative, then the theorist truly wants his law to be interpreted in a strictly universal manner, insofar as the essence of this question is whether the law being examined is *strictly falsifiable.* Strict falsification is a requirement of all laws of truly universal form. However, most theorists would probably reply in the negative to the previous question and then defensively retort, "Don't take my words so literally!"

We may conclude that no existing laws in marketing have the form of strictly universal, generalized conditionals. Indeed, some would take the position that marketing phenomena (like other behavioral phenomena) are *inherently* indeterministic, and, thus, the best that can ever be accomplished will be laws of basically statistical form.[2] *If* marketing phenomena were assumed to be inherently indeterministic, this should not be intellectually discomforting to marketing researchers. Although strictly universal laws have greater explanatory and predictive power than statistical laws, in many situations statistical laws perform quite adequately.

6.4 SUMMARY AND CONCLUSIONS

This chapter has attempted to explore the extension and universality of both lawlike and nonlawlike statements. The reader may wish to consult Figure 6.3, which delineates the various kinds of statements that have differing extension and gives hypothetical examples of each kind. The examples were specifically constructed so that each would involve the common construct of the "maturity phase of the product life cycle." Singular statements have the least extension of all statements because they refer to specific phenomena that are bound in time and space. Singular statements are both strictly confirmable and strictly falsifiable. Thus, their primary role in research lies in the *testing* of laws and theories. Existential statements propose the existence of some phenomenon. Bounded or qualified existential statements are both confirmable and falsifiable, whereas unqualified existential statements are confirmable but not strictly falsifiable. Tendency laws are a kind of statistical law stating that two variables tend to vary together in a systematic way. Most lawlike statements in marketing are tendency laws. Probability laws are a genus of statistical law where the relationship between two variables is clearly specified in the form of a probability or relative frequency statement. Both tendency laws and probability laws are neither confirmable nor falsifiable. Universal laws state a relationship between two variables in the form of a strictly uni-

Figure 6.3 **Extension and Universality**



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Note: All examples are hypothetical and are for illustrative purposes only.

versal generalized conditional. Such laws have the greatest extension and universality and are falsifiable but not strictly confirmable.

6.5 PROBLEMS IN EXTENSION: THE PSYCHOPHYSICS OF PRICES

The previous sections have attempted to classify various kinds of statements according to their differing extension. The extension of a statement was defined as the set of all possible objects (past, present, and future) covered by the statement. That is, to what is the statement referring: apples, oranges, attitudes, or preferences? A careful delimitation of the extension of statements is essential to systematic, scientific inquiry. Note, however, that theory and research in the marketing discipline has been awash with lawlike statements that have been borrowed from other disciplines and then extrapolated to fit marketing

problems. This extrapolation often extends lawlike statements far beyond their legitimate domain or "universe of discourse." To illustrate the problems of questionable extension or extrapolation, we shall consider some of the issues raised by the well-known, much discussed, "psychophysics of prices and Weber's law" controversy.

Kamen and Toman (1970) used preference behaviors of consumers concerning various pricing combinations of major branded gasolines and independent branded gasolines to attempt to test their "fair price" theory versus Weber's law. The research sparked critical comments by Monroe (1971) and by Gabor, Granger, and Sowter (1971) and a reply by Kamen and Toman (1971). Although many issues were raised during the controversy, only the issue of the extension of Weber's law will be explored here.

The crux of the fair price theory of Kamen and Toman (hereafter referred to as K-T) is that consumers have specific notions about what constitutes a fair price for a product and that when the actual price exceeds this fair price, consumers will take courses of action in an attempt to maintain the fair price. Thus, for their gasoline data, K-T suggest:

> According to the "fair price" theory, as the price of major brand gasoline exceeds the perceived fair price, more and more motorists will turn to the Independents. Thus, suppose that the price difference between Majors and Independents is two cents. This theory would predict that when the price of gasoline is high—for example, 42 cents for Majors and 40 cents for Independents—more people would be attracted to the Independents than when the price of gasoline is low—for example, 28 cents for Majors and 26 cents for Independents. This prediction is exactly opposite to the one inferable from Weber's Law. (Kamen and Toman 1970, p. 27)

K-T thus imply that the following *bridge law* is inferable from Weber's law: when the price of gasoline is high, more people will be attracted to the majors than to the independents than when the price of gasoline is low, if the same price *differential* is maintained at both price levels. Since K-T's results supported that fair price prediction, the major issue is whether their bridge law was a valid inference from Weber's law. That is, can Weber's law be extended to cover K-T's concepts in the predicted relationship?

First, a review of Weber's law and its original extension seems desirable. All participants in the controversy agree that Weber's law states:

$$\Delta I / I = K$$

where ΔI equals the smallest increase in the intensity of a stimulus that will be just noticeably different from the previous intensity of the stimulus, I equals the original intensity of the stimulus, and K equals a constant that varies according to the nature of the stimulus. Substantial research has confirmed that the relationship *extends* to the following stimuli: pressure, visual brightness, lifted weights, tone, smell, and taste (Berelson and Steiner 1964, p. 96). K-T (1971, p. 253) concede that they have extrapolated Weber's law beyond its original extension but claim that their extrapolations are consistent with the current marketing literature. They cite, as an example, Engel, Kollat, and Blackwell:

> Assume, for example, that a price increase of $1 is to be put into effect. That increase would be highly apparent on a 50-cent item, whereas it probably would escape detection on an $80 item. (Kamen and Toman 1970, p. 27)

Are K-T just testing reasonable extensions or extrapolations of Weber's law? To this issue we now turn.

The original extension of Weber's law concerned people's ability to discriminate between different intensities of selected stimuli (smell, taste, etc.). The marketing literature, like the above quotation from Engel, Kollat, and Blackwell, has extended Weber's law to a different stimulus, that is, *price.* K-T, on the other hand, have extended Weber's law beyond the literature in at least two other respects.

The literature has retained the notion of discriminability (note that Engel, Kollat, and Blackwell use the phrases "highly apparent" and "escape detection"). K-T extend Weber's law to *consumer preferences* instead of *discriminability.* K-T (1971, p. 253) recognize that they have done this but provide no logic to justify the extension. Perhaps the logic exists, but it was not presented. K-T (1971, p. 253) state that "any price difference is discriminable" and that "there simply is no JND (just noticeable difference) for price." These statements seem curious in light of K-T's own research, which appears to indicate that some price changes for some people were not discriminable. To wit, K-T's following paragraph is presented:

> An opportunity for a cleaner validation study arose a little less than a year after general gasoline price levels rose slightly over one cent in the same 24 markets mentioned previously. Approximately 1,500 motorists, randomly selected from city directories, were interviewed by telephone on their reactions to the price increase. Approximately 47 percent believed that the general gasoline price level went up during the past year, 2 percent believed that it went down, 44 percent that it remained the same, and 7 percent did not know or failed to answer this question. (Kamen and Toman 1970, p. 34)

In any respect, even if it were true that all price changes were discriminable, this would be insufficient reason to extend Weber's law from *discriminability* to *preferences.* To repeat, there may be a logic available to justify extending discriminability to preference, but the logic was not presented.

The third major extension of Weber's law by K-T concerns the notion of ΔI, the *differential.* In the original extension of Weber's law, the differential referred to different intensities of the *same stimulus* (smell, taste, etc.). However, the differential that K-T refer to is different intensities of *different stimuli.* That is, their differential is the difference in price between major branded gasoline and independent gasoline (not a difference for the same gasoline). Once again, no logic is presented to show that this is a reasonable extension.

In summary, K-T extended the original version of Weber's law (1) to a different stimulus (price), (2) from discriminability to preferences, and (3) from different inten-

sities of the same stimulus to different intensities of different stimuli. Taken in total, and without any justifying logic, these extensions (especially [2] and [3]) seem unwarranted because they so drastically alter the basic statement of the law. The "fair price" theory of K-T is intriguing; their research is interesting; and their data appear sound. Unfortunately, it is difficult to see what any of their results have to do with Weber's law. They certainly did not test Weber's law in any meaningful sense of the word *test.* It is hoped that, if the "psychophysics of prices" controversy has done nothing else, it has underscored the tremendous importance of carefully delimiting the extension of lawlike statements. One must always stop to ask, "What is this statement really saying?" Also, "To which kinds of circumstances and situations does this statement apply, and to which does it not?"

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. "Models are really the bases for marketing theories since they are the *axioms* on which marketing theories are founded" (Lazer 1962, p. 10). Evaluate the view that models are axioms.
2. Find a theory or model in the marketing literature. Determine which statements purport to be lawlike. How do the statements handle the time dimension? Examine the extension of the statements.
3. Laws are "hypotheses that are empirically corroborated to a degree regarded as satisfactory at a certain point in time" (Zaltman, Pinson, and Angelmar 1973, p. 71). To what extent is this perspective consistent or inconsistent with the treatment presented herein?
4. All lawlike statements have empirical content. Any statement that has empirical content can be empirically tested. Any statement that can be empirically tested can be shown to be true or false. Therefore, all lawlike statements can be shown to be empirically true or false. Evaluate.
5. Alderson (1965, p. 345) proposed "a research agenda for functionalism" that included 150 falsifiable propositions. "For a proposition to be falsifiable, it must make a flat assertion with no authority behind it except its apparent consistency with other propositions that have already been accepted into the body of theory." Further, "a proposition is not falsifiable, or testable, if it has been hedged and qualified until it is almost certainly true under some circumstances." Analyze a sample of Alderson's propositions. What kinds of lawlike statements are they? What is their extension? To what extent are they *falsifiable?*
6. "As the degree of extension of a lawlike generalization increases, its accuracy of prediction will decrease." Evaluate.
7. Pinson, Angelmar, and Roberto take great issue with the term *lawlike generalization:*

> The term "lawlike generalization" . . . is not commonly used in the philosophy of science literature. Instead, most authorities use the term "lawlike statement." . . . The reason for preferring the term "lawlike statement" over "lawlike gen-

> eralization" relates to the redundance of the latter term [because] a minimum necessary condition of any scientific statement proposed as lawlike is that it can be a universal generalization. (Pinson, Angelmar, and Roberto 1972, p. 67)

Evaluate the charge made by Pinson, Angelmar, and Roberto. Are all generalizations *lawlike?* Are all lawlike statements *universal?*

8. Lockley (1964, p. 48) proposes the following tendency law in marketing, which he calls the principle of nonprice competition: "For products for which product or marketing differentiation becomes difficult, there will be an increasing tendency toward nonprice competition, and the extent of nonprice competition will tend to be in proportion to the size and resources of the competing vendors." Does this imply that for products for which differentiation is *easy* there will be a *decreasing* tendency for nonprice competition? Evaluate.
9. The channels of distribution literature often discuss the notions of power and conflict. A major problem has been to get an adequate definition of conflict. The following two perspectives are typical: (1) "The term conflict refers neither to its antecedent conditions, nor to individual awareness of it, nor certain affective states, nor its overt manifestations, nor its residues of feeling, precedent, or structure, but to all of these taken together as the history of a conflict episode" (Pondy 1967, p. 319). (2) "Conflict in our scheme refers to overt behavior arising out of a process in which one unit seeks the advancement of its own interests in its relationship with others" (Schmidt and Kochon 1972, p. 363).

   Are these *definitions* of conflict, *explanations* of conflict, *lawlike generalizations* involving conflict, or what? Is it possible for a statement to be both a lawlike generalization and a definition at the same time? How useful would the two preceding perspectives be in conducting research on conflict in channels of distribution?
10. Is it possible to have a set of nontrue theorems that are derived from a set of true axioms? Does your answer differ depending on whether the axioms were universal or statistical in nature?

NOTES

1. See: Friedman 1953; Samuelson 1963, 1964, 1965; Garb 1965; Machlup 1964; Lerner 1965; Masey 1965; Nagel 1963; Blaug 1980.

2. The reader should be analyzing all statements by now and should recognize that this statement is falsifiable but not strictly confirmable. Can the statement be alternatively constructed as a negative existential statement?

7

ON THE MORPHOLOGY OF THEORY

> Once upon a time two explorers came upon a clearing in the jungle. In the clearing were growing many flowers and many weeds. One explorer says, "Some gardener must tend this plot." The other disagrees, "There is no gardener." So they pitch their tents and set a watch. No gardener is ever seen. "But perhaps he is an invisible gardener." So they set up a barbed-wire fence. They electrify it. They patrol with bloodhounds. (For they remember how H.G. Wells's The Invisible Man could be both smelt and touched though he could not be seen.) But no shrieks ever suggest that some intruder has received a shock. No movements of the wire even betray an invisible climber. The bloodhounds never give cry. Yet still the Believer is not convinced. "But there is a gardener, invisible, intangible, insensible to electric shocks, a gardener who has no scent and makes no sound, a gardener who comes secretly to look after the garden which he loves." At last the Sceptic despairs, "But what remains of your original assertion? Just how does what you call an invisible, intangible, eternally elusive gardener differ from an imaginary gardener or even from no gardener at all?"
>
> *—A.G.N. Flew*

This chapter will attempt to explicate the nature and role of theory in scientific inquiry and research. After discussing various perspectives on the concept of theory, a consensus conceptualization of theory will be offered. A review of some basic misconceptions of the nature of theory will show that, as a result of these misconceptions, marketing theory has taken a "bum rap." The body of the chapter will be devoted to developing in some detail the full import of the three key ideas embodied in the consensus conceptualization of theory. First, we explore some perspectives on the notion of theory.

7.1 THE NOTION OF THEORY

What constitutes a theory? Is the term *theory* synonymous with *law?* How does a *theory* of *X* differ from an *explanation* of *X?* How do theories differ from hypotheses? Is a theory simply a model? Strangely enough, although *theory* would have to rank high among the most abused terms in marketing, there is probably more unanimity among philosophers of science as to what constitutes a theory than there is agreement among them concerning

the nature of laws and explanations. This is not to say that there is a universal consensus concerning the nature of theoretical constructions. Rather, different uses of the term *theory* in philosophy of science are more apparent than real, more superficial than substantive, as a representative sample of perspectives will demonstrate.

Kaplan (1964, p. 297) defines theory thus: "We may say to start with that a theory is a system of laws. But the laws are altered by being brought into systematic connection with one another, as marriage relates two people who are never the same again." Similarly, Bergmann (1957, p. 31) notes, "If there has to be a formula again, one might say that a theory is a group of laws deductively connected." Blalock suggests:

> It has been noted that theories do not consist entirely of conceptual schemes or typologies but must contain lawlike propositions that interrelate the concepts of variables two or more at a time. Furthermore, these propositions must themselves be interrelated. (Blalock 1969, p. 2)

Bunge is much more specific and detailed in his description of theory:

> In ordinary language and in ordinary metascience "hypothesis," "law," and "theory" are often exchanged; and sometimes laws and theories are taken to be the manhood of hypotheses. In advanced science and in contemporary metascience the three terms are usually distinguished: "law" or "law formula" designates a hypothesis of a certain kind—namely, non-singular, non-isolated, referring to a pattern, and corroborated; and "theory" designates *a system of hypotheses, among which law formulas are conspicuous*—so much so that the core of a theory is a system of law formulas. In order to minimize confusions we will provisionally adopt the following characterization: A set of scientific hypotheses is a scientific theory if and only if it refers to a given factual subject matter and every member of the set is either an initial assumption (axiom, subsidiary assumption, or datum) or a logical consequence of one or more initial assumptions. (Bunge 1967a, p. 381, italics added)

In his classic work *The Logic of Scientific Discovery,* Popper (1959, p. 59) metaphorically suggests that *theories* are "nets to catch what we call 'the world': to rationalize, to explain, and to master it. We endeavor to make the mesh finer and finer." Braithwaite (1968, p. 22) believes that "a scientific theory is a deductive system in which observable consequences logically follow from the conjunction of observed facts with the set of the fundamental hypotheses of the system." Finally, the marketing theoretician Wroe Alderson (1957, p. 5) proposes that a "theory is a set of propositions which are consistent among themselves and which are relevant to some aspect of the factual world."

Although the previous perspectives on theory differ, a careful examination will reveal that the differences are noteworthy primarily for their superficiality. Note how often these similar terms and phrases are repeated: "system of laws," "systematic connection," "interrelated lawlike propositions," "set of scientific hypotheses," "factual subject matter," "group of laws," and "deductively related." All of these key concepts can be incorporated

into a consensus definition of theory, which will serve as the focal point for this chapter, and which was originally proposed by Richard S. Rudner:

Definition: A theory is a systematically related set of statements, including some lawlike generalizations, that is empirically testable. The purpose of theory is to increase scientific understanding through a systematized structure capable of both explaining and predicting phenomena. (Rudner 1966, p. 10)

Much of the rest of this chapter will be devoted to fully explicating the import of this conceptualization of the nature and role of theory. In the process we will show that a full articulation of the three key criteria of theory—(1) it is systematically related, (2) it includes lawlike generalizations, and (3) it is empirically testable—will demonstrate that this conceptualization can be truly described as consensus. Concurrently, we will show that the correct application of the three key criteria will both systematically *exclude* all constructions that should not be given the status of *theory* and, at the same time, will systematically *include* all constructions that should be referred to as theories. However, before we proceed further, a brief analysis of theoretical misconceptions will reveal that careless usage of the term *theory* has resulted in much mischief in marketing.

7.2 MISCONCEPTIONS OF THEORY

No marketing academician would dispute this assertion: the term *marketing theory* is often viewed with disfavor by both marketing students and marketing faculty. Few criticisms in academia are more common than "This course is too theoretical!" A suggested opening gambit for a course in marketing theory is to ask the students to try to think of another course they have had that they disliked because it was "too theoretical." The instructor then begins to probe the students to determine exactly what each meant when he or she thought some particular course was *too theoretical.* After some discussion, the students' criticisms begin to center on four recurring themes:

1. The too theoretical course was difficult to understand.
2. The too theoretical course was conjectural rather than factual. ("That's just a theory, not a fact.")
3. The too theoretical course was not related to the real world.
4. The too theoretical course was not practical enough. ("It's all right in theory but not in practice.")

Although the complaint that *too theoretical* courses are difficult to understand is often justified, this complaint should be met with sympathy and compassion, but not with alarm. Theories often deal with abstract concepts and complicated relationships, and, thus, they may be difficult to comprehend. Simplicity is a desirable characteristic of theory, but reality is often complex, and the theoretical constructions used to explain reality must often be complex. However, the theory presented in courses is sometimes difficult to

understand because of the theorist's obtuse and nonlucid manner of writing, rather than because of the inherent difficulty of the theoretical relationships. Even great theorists (such as J.M. Keynes) have often expressed their ideas in a manner susceptible to great variance in interpretation. While discussing Wroe Alderson's theoretical constructions, Hostiuck and Kurtz (1973, p. 141) note that "the authors have heard even recognized scholars of marketing groan at the mere mention of Alderson and intimate that they never really understood him." Perhaps it is just too much to expect creative theorists to present their theoretical constructions lucidly, but a nonlucid articulation of a theory will make students groan that it is difficult to understand and will also retard the testing and future development of the theory.

The second complaint states that too theoretical courses are *conjectural* rather than *factual.* One characteristic of the scientifically immature mind is to be uncomfortable in the presence of uncertainty. True scientists are always ready to revise their beliefs in the light of fresh evidence. The really important issues in any discipline are always conjectural rather than factual. To recognize that consumers' preferences are shifting toward smaller automobiles is useful, but the real challenge is to develop theories, which by their very nature will be conjectural, to explain past shifts in consumer preferences and to predict future shifts. Likewise, knowing that distribution channels are now shorter is less useful than theorizing *why* they are now shorter and attempting to predict what will happen to the length of distribution channels in the future. Although theories must be empirically testable (hence, not "purely conjectural"), because of the nature of any theory's constituent lawlike generalizations, a theory can never be *confirmed* in the same sense that simple descriptive or singular statements can be confirmed. Nevertheless, except for people who just like to read telephone books or census data, the most interesting issues in a discipline are usually more conjectural than factual.

The charges that the content of too theoretical courses is "not related to the real world" or is "not practical" are serious indeed. In fact, these charges are perhaps the most common and serious of all the charges that are made against theoretical courses. *The resolution of these charges lies in the realization that all purportedly theoretical constructions must be empirically testable and must be capable of explaining and predicting real-world phenomena.* Two conclusions immediately follow: (1) All purportedly theoretical constructions *must* be related to the real world. (2) All purportedly theoretical constructions *must* be practical, because the explanation and prediction of real-world phenomena must rank high on any list of practical concerns. Rather than "it is all right in theory but not in practice," the truth of the matter is that *if it is not all right in practice, it cannot be all right in theory!* Courses filled with complex mental gymnastics (often couched in mathematical terms) that have no relevance to the real world and no explanatory or predictive power are not *too theoretical* at all. On the contrary, such courses are completely devoid of theoretical content.

Unfortunately, all too many marketing students and academicians have tended to bestow the term *theory* on locutions that are nothing more than obtuse armchair philosophy or mathematical mental gymnastics with no explanatory or predictive power. It is little wonder that the label "too theoretical" has truly become an epithet. One of the objectives

of this book is to challenge marketing theorists to cull out the obtuse armchair philosophy and to ensure that their purportedly theoretical constructions are empirically testable and have explanatory and predictive power. For too long, marketing theory has taken a "bum rap" because it has been awash in nontheoretical constructions masquerading as theory.

In summary, since the real world is often complex, the theoretical constructions with which we attempt to explain reality will often be complex. Because by their very nature theories cannot be conclusively shown to be *true* (in the sense that a singular statement can be shown to be true), theories are, of necessity, *conjectural.* But the most important issues (such as explanation and prediction) in any discipline are always of the conjectural variety. Finally, the notion that *theoretical* and *practical* are at opposite ends of a continuum is false. Any construction that purports to be a theory must be capable of explaining and predicting real-world phenomena. And the explanation and prediction of phenomena are eminently *practical* concerns. With these misconceptions of theory disposed of, we can now turn our attention to the three criteria distinguishing theoretical from nontheoretical constructions.

7.3 THE "SYSTEMATICALLY RELATED" CRITERION

So, a theory is a *systematically related* set of statements, including some lawlike generalizations, that is empirically testable. This section will explore two basic questions: (1) Why should the statements in a theory be required to be systematically related? (2) In what precise way should the statements in a theory be systematically related? Rudner provides one response to the first question:

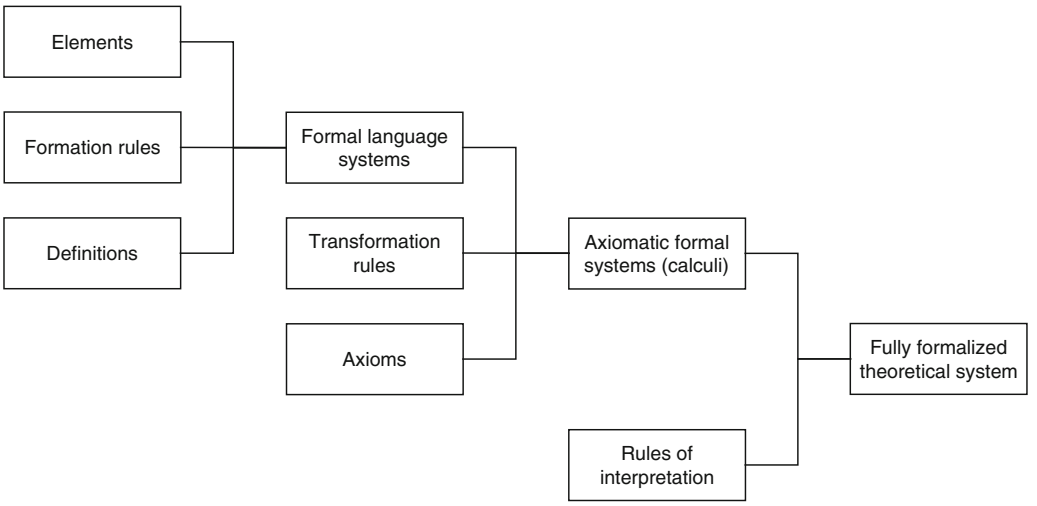
> We are all familiar with the view that it is not the business of science merely to collect unrelated, haphazard, disconnected bits of information; that it is an ideal of science to give an *organized* account of the universe—to connect, to fit together in relations of subsumption, the statements embodying the knowledge that has been acquired. Such organization is a necessary condition for the accomplishment of two of science's chief functions, explanation and prediction. (Rudner 1966, p. 11)

Robert K. Merton addresses the same issue in sociology:

> [A] miscellany of . . . propositions only provides the raw materials for sociology as a discipline. The theoretic task, and the orientation of empirical research toward theory, first begins when the bearing of such uniformities on a set of interrelated propositions is tentatively established. The notion of directed research implies that, in part, empirical inquiry is so organized that if and when empirical uniformities are discovered, they have direct consequences for a theoretic system. (Merton 1949/1968, p. 149)

The view taken here is that we require theories to contain systematically related sets of statements in order to increase the *scientific understanding* of phenomena. To understand

Figure 7.1 **The Full Formalization of a Theory**



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scientifically the occurrence of a phenomenon requires more than simply being able to explain and predict it using *isolated* lawlike generalizations. In addition, we must be able to show how the statements used to explain and predict a phenomenon are incorporated into the total body of scientific knowledge. The view that the *systematically related* criterion represents a consensus position can be supported by a careful examination of the previously cited perspectives on theory. For example, Kaplan mentions "systematic connection," Bergmann talks about "deductively connected," Blalock requires "interrelated propositions," and Braithwaite discusses a "set of fundamental hypotheses." Therefore, all of these writers allude to what we are calling the systematically related criterion.

The second question asks, "In *what precise way* should the statements in a theory be systematically related?" As Dubin (1969, p. 16) has observed, simply to have a collection of propositions is not necessarily to have a theory. The propositions or statements in a theory must have a high degree of internal consistency. To check for internal consistency, all of the *concepts* in each statement in the theory must be clearly defined, all of the *relationships* among the concepts in each statement must be clearly specified, and all of the *interrelationships* among the statements in the theory must be clearly delineated.

The complete articulation of the nature of the "systematically related" criterion requires an elaboration of the notion of "full formalization" in the philosophy of science. The essence of the full formalization of a theory is the complete, rigorous articulation of the entire syntactic and semantic structure of the theory. Bergmann (1957, p. 38) suggests, "One formalizes a scientific theory by replacing its descriptive words by 'marks on paper.' The logical words, which remain, are the only ones we 'understand.'" But full formalization implies much more than simply replacing descriptive words with "marks on paper." Figure 7.1 illustrates that *a fully formalized theory consists of a formal language*

system that has been axiomatized and appropriately interpreted.[1] We must now turn to an examination of (1) formal language systems, (2) axiomatized formal systems, and (3) appropriately interpreted, axiomatized formal systems because to require theories to contain systematically related sets of statements implies that theories must, in principle, be amenable to formalization.

7.3.1 Formal Language Systems

Formal language systems must first be differentiated from natural language systems, such as English. Both formal language systems and natural language systems include (1) elements, (2) formation rules, and (3) definitions. The English language elements are the *words* of English, and the formation rules (the grammar or syntax) state the permissible ways in which words can be combined to form correct English sentences. For example, the statement "marketing advertising beneficial" is not a correct English sentence because it violates the *formation rule* that each English sentence must contain a verb.

In addition to elements and formation rules, both natural languages and formal language systems must have sets of definitions. The kinds of definitions that are required here are *nominal* definitions (Hempel 1970, p. 654) or *rules of replacement* (Rudner 1966, p. 15). These types of definitions should be carefully distinguished from definitions of the so-called *operational* variety. Roughly speaking, nominal definitions have to do with relationships among *terms alone* (syntactic considerations), and operational definitions have to do with relationships between terms and the *real world* (semantic considerations). We will treat operational definitions in the section probing the "empirically testable" criterion of theoretical constructions and will focus our present attention on nominal definitions.

A nominal definition states that one term, the *definiendum,* is equivalent to another term or groups of terms, the *definiens.* So, Alderson (1965) defines assortment:

$$\textit{assortment} =_{\text{df}} \textit{meaningful heterogeneous collection}$$

In this instance, "assortment" is the definiendum and "meaningful heterogeneous collection" is the definiens. The significance of calling nominal definitions "rules of replacement" is that the truth value of any statement that includes the definiendum is maintained if the definiendum is replaced by the definiens (Rudner 1966, p. 16). That is, any statement that is true and contains the term *assortment* will likewise be true if "assortment" is replaced by "meaningful heterogeneous collection." Similarly, any false statement that contains the term *assortment* will remain false if "meaningful heterogeneous collection" is substituted for it.

But how does one define *meaningful heterogeneous collection*? We must either introduce other terms to define *meaningful heterogeneous collection,* or we must suffer circularity by defining:

$$\textit{meaningful heterogeneous collection} =_{\text{df}} \textit{assortment}$$

In either case, a little reflection reveals that in any language system there will be a set of *primitive* elements or terms. These primitive elements will be undefined within that system but will not necessarily be undefined within some other system. Also, all of the nonprimitive elements within the given language system can be defined by means of the primitives. Alderson (1965, p. 25) suggested that all of the subject matter in his conceptualization of marketing could be ultimately reduced by a series of definitions to three primitive terms: sets, behavior, and expectations. Although this conclusion may be open to question, Alderson clearly realized that all language systems contain primitive or undefined elements.

We require theories to contain systematically related sets of statements. The *systematically related* criterion implies a kind of systematization that is, at least in principle, amenable to formalization. A fully formalized theory implies, among other things, a formal language system. Insofar as both formal language systems and so-called natural languages include (1) elements, (2) formation rules, and (3) definitions, how do formal language systems differ from natural languages? Formal language systems differ from natural languages in that they identify all of the primitive elements, and they develop a complete "dictionary" that shows how all of the nonprimitive terms are derived from the primitive elements. Furthermore, rather than having the loose and continually evolving formation rules of natural languages, such as English, formal language systems rigorously and exhaustively specify the formation rules delineating the permissible ways of combining elements to form statements.

Summarizing, the full formalization of a theory requires the construction of a formal language system that includes a complete list of the primitive elements of the system, a "dictionary" showing how all of the system's nonprimitive terms are derived from the primitive elements, and a complete explication of the formation rules specifying how elements can be combined to form permissible statements (often called "wffs" or "well-formed formulations" in the philosophy of science literature). Nevertheless, the full formalization of a theory requires more than just a formal language system. The system must also undergo *axiomatization,* a subject to which we now turn.

7.3.2 Axiomatic Formal Systems

Figure 7.1 indicates that a fully formalized theory includes a formal language system that has been axiomatized. An axiomatized formal language system is referred to as a *calculus* in the philosophy of science. Axioms and their role in theory construction have already been discussed in section 6.2, which the reader might find helpful to review. The axiomatization of a formal language system requires (1) the adoption of rules of transformation and (2) the selection of appropriate fundamental statements or axioms. Recalling that formation rules detail the permissible ways in which elements can be combined to form statements, *transformation rules detail how statements can be combined to deduce other statements in the system.* A syllogistic example of a transformation rule from consumer demand theory should illustrate the kinds of rules that are required:

1. Bundle of goods *A* contains four oranges and two apples.
2. Bundle of goods *B* contains three oranges and three apples.
3. Bundle of goods *C* contains two oranges and four apples.
4. Consumer *X* indicates a preference for bundle *A* over bundle *B.*
5. Consumer *X* indicates a preference for bundle *B* over bundle *C.*
6. Therefore, consumer *X* will indicate a preference for bundle *A* over bundle *C.*

In consumer demand theory, statement (6) is deducible from statements (1) to (5) because demand theory assumes that consumer preferences follow the logical transformation rule known as *transitivity.* That is, if *A* is preferred to *B,* and *B* is preferred to *C,* then *A* is preferred to *C.* Therefore, to axiomatize a formal language system requires first that we adopt a series of transformation rules that dictate how some statements can be deduced from other statements.

After the permissible ways in which certain statements can be deduced from other statements have been delineated, the axiomatization of a formal language system requires the selection of appropriate fundamental statements or axioms to separate fundamental statements from derived or deduced statements. According to Popper (1959, p. 71), there are four criteria for selecting the *appropriate* fundamental statements for axiomatization. They must be: (1) free from contradiction, (2) independent, (3) sufficient, and (4) necessary. The first criterion requires that the fundamental statements be internally consistent to the extent that mutually exclusive outcomes or statements cannot be deduced from the fundamental statements. That is, if an appropriate set of transformations on the fundamental statements produces the statement that "*X* will occur," then there must *not* be some other permissible set of transformations that will produce the statement that "*X* will *not* occur." Thus, the first requirement is an internal consistency criterion. The second requirement, that the fundamental statements be *independent,* implies that no statement in the final set of fundamental statements can be deducible from the other statements. That is, the axioms must truly be *fundamental* in the system. The third requirement, that the fundamental statements be *sufficient,* implies that all of the statements that are part of the theory proper can be derived from the set of fundamental statements. Finally, to be *necessary* implies that all the statements in the fundamental set are *used* to derive other statements; that is, there are no superfluous statements.

Two points should be reemphasized here. First, as discussed in section 6.2, the fundamental statements or axioms of a theory are assumed to be true for *analytical* purposes only. That is, they are assumed to be true for the purpose of deriving other statements. The axioms are *not* assumed to be true for *empirical* purposes. Therefore, it is entirely appropriate and desirable to empirically test the axioms of a theory. Second, at least some of the fundamental or derived statements in the axiomatic formal system must have the characteristics of lawlike generalizations. Otherwise, the axiomatic formal system would not be a theoretical construction.

In summary, the axiomatization of a formal language system requires (1) the specification of the transformation rules that state the permissible ways in which statements can be combined in order to derive or deduce other statements, and (2) the delineation of a

set of fundamental statements or axioms that are free from contradiction, independent, sufficient, and necessary. Every theory is, at least in principle, susceptible to axiomatization because every theory is composed of statements, and it should be possible to classify the statements in terms of whether they are (1) derived or (2) fundamental *within that theory.* For an excellent axiomatization of consumer demand theory, the reader is urged to consult the first fifty pages of Peter Newman's (1965) *The Theory of Exchange.* This writer knows of no strictly marketing theory that has been axiomatized. Whether marketing theorists should attempt to axiomatize and formalize their theories will be discussed later in this chapter.

7.3.3 Rules of Interpretation

Recapitulating, a fully formalized theoretical structure will include a formal language system that has been axiomatized. Recall, however, that the essence of the full formalization of a theoretical system is a complete, rigorous articulation of *both* the syntactic and semantic structure of the theory. The analysis, so far, has been purely syntactic; that is, only the requirements for the logical relationships among elements and combinations of elements (statements) have been articulated. We have developed the requirements for a formalized analytical-conceptual schema. Insofar as everything up to this point has dealt solely with Bergmann's "marks on paper," it is now time to bring in the meanings of the marks on paper. Alternatively stated, it is now time to bring in the real world by discussing the semantic rules of interpretation.

Referring again to Figure 7.1, an axiomatic formal language system becomes a fully formalized theoretical system when a complete set of appropriate semantic rules of interpretation for the elements or terms in the formal language system has been developed. Because theoretical systems are used to explain and predict phenomena, the elements in the theories must somehow be linked to observable entities and the properties of observable entities in the real world. The semantic rules of interpretation that accomplish this linkage are variously referred to as measures, indicants, operational definitions, coordinating definitions, correspondence rules, or epistemic correlations (Nagel 1961, p. 93).

Although a complete analysis of semantic rules of interpretation will be deferred to section 7.5 on the "empirically testable" criterion of theories, the *ideal goal* of these semantic rules should be stated here. The semantic rules of interpretation are optimal when for each possible interpretation of the axiomatized formal system by semantic rules that makes the fundamental statements (or axioms) true, all of the derived statements (or theorems) will likewise be true. Such a set of optimal semantic rules of interpretation would thus achieve a kind of *isomorphism* or "one-to-one correspondence" between the marks on paper of the theory and the real world.

Summarizing the preceding three sections, a theory is a systematically related set of statements, including some lawlike generalizations, that is empirically testable. To be *systematically related* is a desirable and consensus criterion of theory because science endeavors to increase scientific understanding by giving an organized account of the universe. A set of statements will fulfill the *systematically related* criterion when it exhibits

a kind of systematization that is, at least in principle, amenable to full formalization. A fully formalized theoretical system consists of a formal language system that has been axiomatized and completely interpreted. Formal language systems contain (1) elements, (2) formation rules, and (3) a set of definitions (all three are rigorously specified). An axiomatic formal language system includes a set of transformation rules showing how some statements can be derived from other statements and a set of fundamental statements that are (1) free from contradiction, (2) independent, (3) sufficient, and (4) necessary. An axiomatized formal language system becomes a fully formalized theoretical system when a complete set of semantic rules of interpretation has been developed.

7.3.4 Issues in Formalization

Although theories are required to have a kind of systematization that is susceptible to formalization, four points must be made regarding formalization. First, the preceding discussion of *full formalization* in no way attempts to capture or describe the actual processes that theorists use to discover or create a theoretical structure. The formalization of a theory is *ex post.* That is, the process of formalization customarily begins in earnest only *after* the theory has been proposed. Second, some writers warn against the premature formalization of theories on the ground that formalization may actually inhibit scientific creativity. Thus, Kaplan suggests:

> The demand for exactness of meaning and for precise definition of terms can easily have a pernicious effect, as I believe it often has had in behavioral science. It results in what has been aptly named the *premature closure* of our ideas. That the progress of science is marked by successive closures can be stipulated; but it is just the function of inquiry to instruct us how and where closure can best be achieved. . . . There is a certain kind of behavioral scientist who, at the least threat of an exposed ambiguity, scurries for cover like a hermit crab into the nearest abandoned logical shell. But there is no ground for panic. That a cognitive situation is not as well structured as we would like does not imply that no inquiry made in that situation is really scientific. On the contrary, it is the dogmatisms outside science that proliferate closed systems of meaning; the scientist is in no hurry for closure. Tolerance of ambiguity is as important for creativity in science as it is anywhere else. (Kaplan 1964, p. 70)

Third, the complete formalization of any theory is an arduous task requiring great effort. Fourth, it should be noted, few theories in any of the sciences have been fully formalized.

Many philosophers of science have questioned the role of formalization in theory development. Suppe (1977b, pp. 110–15) has summarized their arguments. First, the systematic interconnections among the concepts of many theories are insufficiently specified to enable *fruitful* axiomatization. Suppe cites examples such as Darwin's theory of evolution, Hoyle's theory on the origin of the universe, Pike's theory of language structure, and Freud's psychology. Second, the formalization of a theory often leaves untouched many

of the truly interesting philosophical problems. This is because formalization usually emphasizes syntactic rather than semantic considerations. Third, formalization is a static analysis revealing at best a "snapshot" of a theory at a point in time. Thus, formalization ignores the dynamics of theory development.

Suppe (1977b) then replies to these criticisms by pointing out that the fact that some theories cannot be completely formalized ignores the possibility (and usefulness) of *partial* formalization. Furthermore, for studying the fine details of a structure, a "snapshot" is often much preferred to a "videotape." Suppe (1977b, p. 62) concludes, "Rather surprisingly the various criticisms of the Received View have left this claim [that theories should be formalized] essentially unchallenged." Suppe's conclusion coincides with that of most writers. For example, MacKinnon suggests:

> Perhaps the most basic and obvious question to be asked concerning rational reconstructions of scientific theories is "Why bother?" Rational reconstructions have contributed little if anything either to the understanding of historically developing theories or to advancing their future development. The rather pragmatic point of view . . . is that in a rational reconstruction a scientific theory becomes an object of study, rather than a tool for studying some other domain. Reconstructing a theory is a help to understanding it, at least in the sense that we have some understanding of anything we can take apart and put back together. (MacKinnon 1979, p. 510)

The preceding analysis implies that the primary purpose of formalization lies in *evaluating* theoretical structures, not in discovering or creating them. Often, the attempt even to partially formalize a theory, by baring its essential structure or morphology, can sharpen the discussion of the theory and put it into a framework suitable for testing. *For many marketing theories, the partial formalization of the theory is an absolutely necessary precondition for meaningful analysis.* Two examples should illustrate this point. First, we shall consider the so-called "general theory of marketing" proposed by Robert Bartels (1968), and second, we shall explore the partial formalization of the Howard-Sheth "theory of buyer behavior" (1969) that was generated by Farley and Ring (1970).

7.3.5 The "General Theory of Marketing": A Partial Formalization

An article by Robert Bartels (1968) on marketing theory generated substantial debate concerning the nature of theoretical constructions in general and the components of a general theory of marketing in particular. Bartels proposed a general theory of marketing that included seven subtheories: (1) theory of social initiative; (2) theory of economic (market) separations; (3) theory of market roles, expectations, and interactions; (4) theory of flows and systems; (5) theory of behavior constraints; (6) theory of social change and marketing evolution; and (7) theory of social control of marketing. In order to analyze the basic nature of these seven theories, the present writer found it necessary to partially formalize them. The reconstructions or partial formalizations of the seven theories were

then used for evaluative purposes (Hunt 1971). A review of two of Bartels's theories will help to illustrate the process used for partial formalization. Bartels states his theory of flows and systems as follows:

> Flows are the movements of elements which resolve market separations. Marketing does not occur as a single movement, but rather as a number of movements, in series, parallel, reciprocal, or duplicatory. They occur in the complex relations among the individuals who have found an economic basis for their existence and for their participation in the marketing process. (Bartels 1968, p. 33)

Similarly, Bartels states his theory of behavior constraints in this way:

> Action in the marketing system is not determined wholly by any one individual or set of participants. It is governed by many determinants and occurs within constraints defined by society. Some of these constraints are economic in nature. Only that can be done which can be done within the bounds of economic feasibility. This may be determined through experience in the profitable combining of economic factors of production. However, much feasibility is predetermined and set forth in the form of marketing technology, know-how, or generalizations for behavior. This is reason for having thorough knowledge of marketing mechanics, or the relations of commodities-functions-institutions as set forth in conventional marketing theory.
>
> Constraints are also social, rather than economic or technical, in nature. These may be of an ethical nature, as that term is used broadly, indicating what is "right" to do under certain circumstances. Rightness may be determined by personal, legal, societal, and theistic standards, and each of these may differ from one society to another. As marketing is viewed more as a personal process rather than only a physical one, such constraints play a more prominent role in marketing theory. (Bartels 1968, p. 33)

The first steps in the formalization of any theory are to generate the basic statements of the theory in precise, succinct fashion and to array the statements in an orderly manner to facilitate theoretical analysis. One such reconstruction or (very) partial formalization of the first "theory" would be:

1. The elements in marketing can be classified into those that flow and those that do not flow.
2. The flowing elements of marketing can be further classified by type—series, parallel, reciprocal, and duplicatory.
3. a. The marketing flows are very important and should be studied by marketing students.
   b. The relationships among marketing flows are very complex.

Similarly, one possible reconstruction of the second "theory" would be:

1. Marketing behavior is constrained behavior.
2. a. Some of the constraints are designed by society.
   b. The societal constraints may be classified as economic, social, ethical, or technical.

As can easily be seen, these reconstructions, *even though they represent only the first modest steps toward formalizations,* are much more amenable to rigorous analysis than the original narrative discussions of the theories. The present writer analyzed these reconstructions and concluded that none of the seven constructions were theories at all. Rather, the seven constructions were shown to be an assemblage of classificational schemata, some intriguing definitions, and exhortations to fellow marketing students to adopt a particular marketing perspective in attempting to generate marketing theory (Hunt 1971, p. 68). Pinson, Angelmar, and Roberto (1972) then analyzed these same partial formalizations and came to different conclusions regarding their theoretical adequacy. The discussion of Pinson, Angelmar, and Roberto prompted a rebuttal that concluded that Bartels's "theory" was "neither a theory of marketing nor a 'general' theory of marketing" (Hunt 1973, p. 70).

The point to be emphasized here is not whether Bartels's theory is or is not really a theoretical construction. Rather, the partially formalized reconstructions greatly facilitated theoretical analysis. In addition, a caveat is needed at this point: if the reconstructions do not accurately capture the basic structure, then, of course, any subsequent analysis will not do justice to the theory. In conclusion, in response to the question "Why formalize?" we respond, "In order to facilitate the analysis of theoretical and purportedly theoretical constructions." A second example of a partial formalization will reveal another benefit.

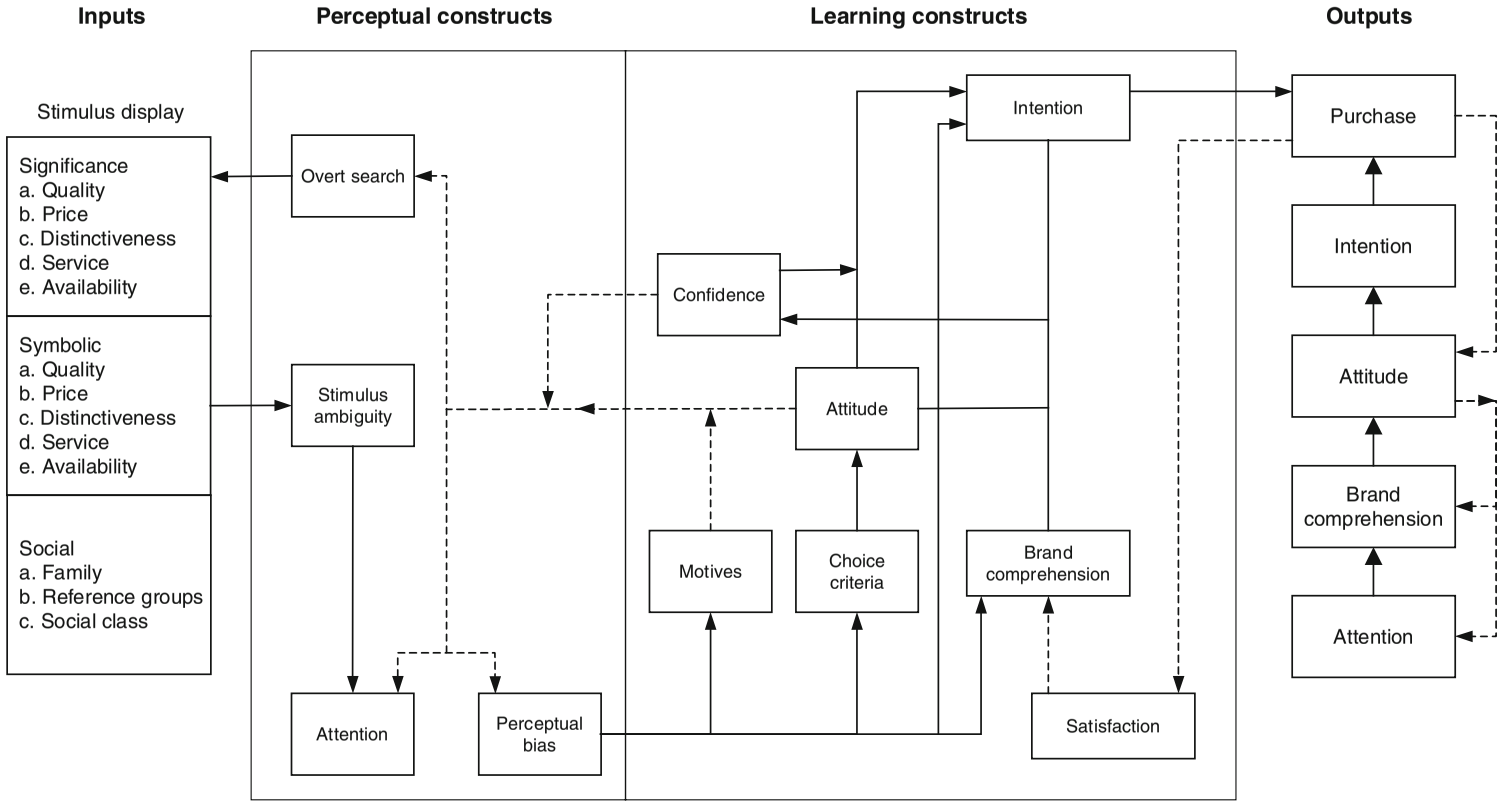
7.3.6 The Theory of Buyer Behavior: A Partial Formalization

Few theories in marketing have sparked more scholarly interest than the Howard-Sheth (1969) theory of buyer behavior. Figure 7.2 reproduces a summary of this theory. As can be observed, the theory consists of a large number of constructs, both exogenous and endogenous to the system. The constructs are interconnected by both direct causal linkages (the solid lines) and by feedback effects (the dashed lines).

Now, the fundamental question to be asked of any theoretical structure is: *how well does this theory represent the real world by explaining and predicting real-world phenomena?* To answer this question requires that the theory undergo empirical testing. Unfortunately, the theory as depicted in Figure 7.2 is not constructed in a form suitable for testing. Both the syntactic and semantic structure of the theory must first be reconstructed in at least partially formalized form to make the structure amenable to empirical testing. Farley and Ring (1970) addressed themselves to this task in their trailblazing study, "An Empirical Test of the Howard-Sheth Model of Buyer Behavior."

Farley and Ring's partial formalization of the Howard-Sheth (H-S) theory required the rigorous specification of the exact nature of the linkages among the constructs. They lament, "Indeed, in its [the model's] present form, the functional relationships among

Figure 7.2 **The Howard-Sheth Theory of Buyer Behavior**



Source: Howard and Sheth (1969, p. 30). Reprinted by permission of John Wiley.

the variables are generally unspecified, although their directions are known" (Farley and Ring 1970, p. 427). The formalization of the theory culminated in a series of eleven simultaneous equations, each having the basic form:

$$Y_{(i)} = \sum_{\substack{j=1 \\ (j \neq i)}}^{11} \beta_{i,j} Y_{(j)} + \sum_{k=1}^{K} Y_{i,k} X_{(k)} + Y_{i,o} + u_{(i)}; i = 1,\ldots,11.$$

Farley and Ring then obtained measures (the semantic rules of interpretation previously discussed) for each construct and conducted a test of the theory using both ordinary least squares and two-stage least squares regressions. Their results can be interpreted as weakly supporting the H-S theory.

Farley and Ring's efforts also sparked a critical appraisal of the basic structure of the H-S model by Hunt and Pappas (1972). This appraisal found that, because the actual variables used in the H-S model had been common knowledge in consumer behavior for some time, the major substantive contribution of the H-S theory was the postulation of certain *developmental linkages.* For example, Howard and Sheth propose the developmental linkage that attitude influences purchase only *through* the mediating variable of intention to purchase. Any complete test of the H-S model must test for the existence or nonexistence of these developmental linkages. Because structural equation modeling programs were unavailable at the time, a method using partial correlation coefficients was suggested as a possible procedure for testing the developmental linkages in the H-S model (Hunt and Pappas 1972, p. 347).

Subsequent empirical research by Lehmann and colleagues (1974) specifically tested for the developmental linkages postulated by the H-S model. Using the partial correlation coefficient procedure and cross-lagged correlation analysis, they concluded:

> Confidence levels for the Howard-Sheth model, as well as for the other two structures, were high enough to conclude that they are statistically significant, if yet very imperfect, representations of consumer information processing and decision making over time. In terms of finding strong support for the model, however, the results were disappointing. (Lehmann et al. 1974, p. 51)

Because of the weak support for the H-S version, Lehmann and colleagues constructed a revised model with different developmental linkages.

The pioneering efforts of Farley and colleagues conclusively demonstrate the desirability of formalization. Without their partial formalization of the Howard-Sheth theory, it would still be largely untested. Their partial formalization led to empirical testing (Farley and Ring 1970), then to theoretical appraisal and evaluation (Hunt and Pappas 1972), and then to retesting and redevelopment (Lehmann et al. 1974). In conclusion, the bogeyman of premature closure should deter no one from the formalization of theories, at least to the extent that formalization facilitates both theoretical analysis and empirical testing.

7.4 THE "LAWLIKE GENERALIZATIONS" CRITERION

The preceding sections have discussed the import of requiring the statements comprising a theory to be systematically related. Nevertheless, not all systematically related sets of statements are theoretical in nature. For example, definitional schemata, purely analytical schemata, and classificational schemata all contain systematically related statements, but they are not theories. For a systematically related sets of statements to be a theory, at least some of the statements must be in the form of lawlike generalizations. A discussion of the nature of lawlike generalizations need not be repeated here, insofar as Chapter 5 has already explored this topic. It will suffice to recall that lawlike generalizations are statements having the basic form of generalized conditionals (statements of the "If *X* occurs, then *Y* will occur" variety) that (a) have empirical content, (b) exhibit nomic necessity, and (c) are systematically integrated into a body of scientific knowledge.

Why must theories contain at least some statements taking the form of lawlike generalizations? Because the purpose of theory is to increase scientific understanding through a systematized structure capable of both *explaining* and *predicting* phenomena. Being able to scientifically explain a phenomenon implies the ability to predict that phenomenon (see Chapter 4). Now, all of the models discussed in Chapter 3 that have explanatory power require lawlike generalizations to explain phenomena. That is, the deductive-nomological, deductive-statistical, and inductive-statistical models all rely on lawlike generalizations for their explanatory power. Models that do not include lawlike generalizations, for example, the pattern model, have been shown to yield inadequate scientific explanations of phenomena. *Therefore, all purportedly theoretical constructions must contain lawlike generalizations because a major purpose of theory is to explain phenomena, and all scientific explanations of phenomena contain lawlike generalizations.*

Once again, the *lawlike generalizations* criterion represents a consensus position in the philosophy of science. Thus, Kaplan (1964, p. 297) requires a "system of *laws*"; Bergmann (1957, p. 31) suggests a "group of *laws*"; Blalock (1969, p. 2) demands that theories contain "*lawlike* propositions"; and, finally, Bunge (1967a, p. 381) insists on "a system of hypotheses, among which *law* formulas are conspicuous." All of these authors are requiring theoretical constructions to contain what is referred to here as "lawlike generalizations."

Unfortunately, marketing theorists often seem to ignore the lawlike generalizations criterion in their efforts to develop marketing theory. Theorists create elaborate structures of systematically related statements composed exclusively of definitional and classificational schemata. As previously noted, a definition is a rule of replacement whereby an element (the definiendum) in a statement can be replaced by another element or elements (the definiens) without losing the truth value of the statement. A *definitional schema* is simply a systematically related set of definitions. All theories will contain definitional schemata, but a definitional schema is, by itself, not a theory.

Also frequently confused with theoretical schemata, a classificational schema is a kind of system that sets forth the conditions for the applicability of its categorial or classificatory terms. Classificational schemata always attempt to *partition* some universe of elements or statements into homogeneous groups.

7.5 THE "EMPIRICALLY TESTABLE" CRITERION

Having established that theories must contain systematically related statements, that at least some of these statements must be lawlike generalizations, and that classificational schemata should not be confused with theoretical schemata, there remains the task of exploring (1) what it means to require theories to be *empirically testable,* and (2) why they must be so required. First, why must theories be empirically testable? One powerful reason has been suggested by Popper (1959, p. 44). Scientific knowledge, in which theories are primal, must be *objective* in the sense that its truth content must be *intersubjectively certifiable.* Requiring a theory to be empirically testable ensures that it will be intersubjectively certifiable because different (but reasonably competent) investigators, with differing attitudes, opinions, and beliefs, will be able to make observations and conduct experiments to ascertain the truth content of the theory. Hempel (1970, p. 695) concurs, "Science strives for objectivity in the sense that its statements are to be capable of public tests with results that do not vary essentially with the tester." Scientific knowledge rests on the bedrock of empirical testability, which makes it intersubjectively certifiable. Most other kinds of knowledge (for example, theological knowledge, whose cornerstone is "faith") lack the capacity to be intersubjectively certifiable.

The second reason for requiring theories to be empirically testable springs from the purpose of theory itself. As previously discussed, the major purpose of theory is to increase scientific understanding through a systematized structure capable of both explaining and predicting phenomena. Any systematized structure that is *not* empirically testable will suffer from explanatory and predictive impotence: it will not be able to explain and predict phenomena. Hence, any structure that is not empirically testable will not be able to perform the tasks expected of genuine theoretical structures. Many purportedly theoretical constructions in marketing seem to lack explanatory and predictive power.

A final justification for requiring theories to be empirically testable lies in the desirability of distinguishing between theoretical schemata and what Rudner (1966, p. 28) refers to as *purely analytical schemata.* An analytical schema contains a systematically related set of statements, all of which are purely *analytic* rather than *synthetic.* "Products have life cycles" is a synthetic statement. "Either products have life cycles or products do not have life cycles" is purely analytic. Whether a synthetic statement is true or false can be ascertained only by examining the real-world facts. In contrast, whether a purely analytical statement is true or false can be determined solely by examining the order and nature of the logical terms (such as *either* and *or*) and the way in which certain descriptive terms (such as *products*) are defined. The real-world facts are completely irrelevant to the truth value of a purely analytical statement. Therefore, requiring theories to be empirically testable will screen out purely analytical schemata from being considered theories, which is desirable because we want the truth value of our theoretical construction to be relevant to the real would.

Summarizing, theories are required to be empirically testable in order that they be (a) intersubjectively certifiable, (b) capable of explaining and predicting phenomena, and (c) differentiated from purely analytical schemata. So far, we have been using the expression

"empirically testable" as if it had perfect antecedent clarity. It is time to explore much more carefully exactly what it means to test a theory empirically.

7.5.1 The Nature of Empirical Testing

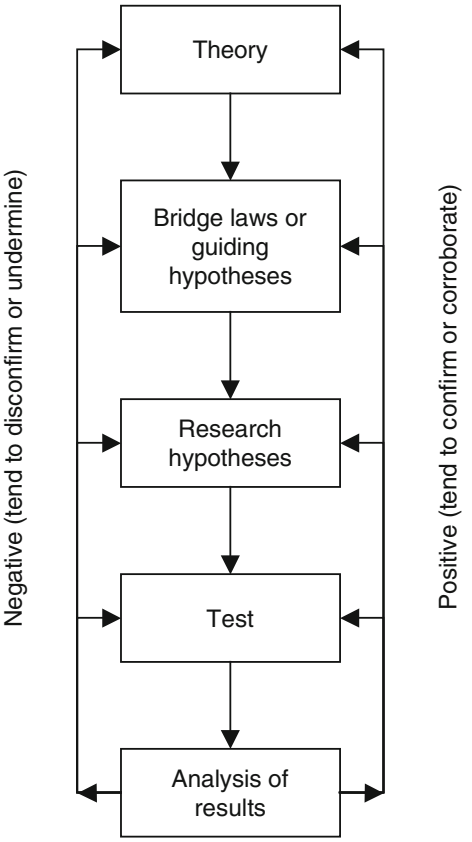
When confronted with any theory, ask the basic question, "Is the theory true?"[2] Less succinctly, ask the questions: "To what extent is the theory isomorphic with reality? Is the real world actually constructed as the theory suggests, or is it not? To what extent has the theory been empirically confirmed?" Numerous criteria have been proposed to evaluate the adequacy of theoretical constructions. Popper (1959, p. 32) has proposed the four criteria of internal consistency, logical form, comparison with other theories, and empirical testing. Dodd (1968, p. 31) reviewed the literature regarding criteria to evaluate theories and recorded seventy commonly used criteria, including accuracy, applicability, brevity, brilliance, and clarity. He then reduced the seventy to twenty-four criteria considered most relevant, including verifiability, predictivity, consistency, and reliability (Dodd 1968, p. 49). J.T. Clark (1969, p. 109) proposes the criteria of clarity, explanatory power, simplicity, and confirmation. Bunge (1967a, pp. 352–54) suggests twenty criteria to evaluate theories, grouped into (1) formal criteria, (2) semantic criteria, (3) epistemological criteria, (4) methodological criteria, and (5) metaphysical criteria. Zaltman, Pinson, and Angelmar (1973, p. 104) used the Bunge groupings to develop their set of sixteen criteria, which they then employed to evaluate the Nicosia model of consumer decision processes, the Howard-Sheth theory of buyer behavior, and the Engel-Kollat-Blackwell model of consumer behavior.

What may be getting *lost* in all this generating of evaluative criteria is the realization that one criterion stands supreme over all others: *Is the theory true? To what extent has it been empirically confirmed?* Bunge (1967a, p. 347) clearly wanted his nonempirical criteria to be applied only for empirically equivalent theories: "Experience will be weighty, perhaps decisive at one point, in the evaluation of empirically *inequivalent* theories. But how does one proceed in the presence of one or more empirically equivalent theories?" Kaplan expresses the same belief:

> Norms of validation can be grouped according to the three major philosophical conceptions of truth: correspondence or semantical norms, coherence or syntactical norms, and pragmatic or functional norms. The first set is the basic one somehow; the others must be regarded as analyses or interpretations of correspondence. Science is governed fundamentally by the reality principle, its thought checked and controlled by the characteristics of the things it thinks about. (Kaplan 1964, p. 312)

The way to determine the truth content of any theory is to test it empirically. Figure 7.3 represents one (grossly compacted) conceptualization of the process of empirical testing.[3] For a detailed, scientific realist model of the process of empirical testing, see Chapter 12.

Figure 7.3 **The Empirical Testing Process**



Source: Hunt (1991a). Reprinted by permission of the author.

7.5.2 The Empirical Testing Process

Figure 7.3 shows that the first step in testing a theory empirically is to derive some bridge laws or guiding hypotheses from the theory proper. As discussed in section 6.2.1, a bridge law is a kind of derivative law whose function is to bridge the gap between a theory and the specific classes of phenomena under investigation. A researcher desirous of testing the role of *risk* in consumer behavior might develop bridge laws dealing specifically with the perceived risk involved in purchasing new homes. Similarly, the "fair price" theory is couched in terms of the prices of goods in general. To test the "fair price" theory, one must develop bridge laws concerning *specific* stimuli, for example, the prices of gasoline used by Kamen and Toman (1970).

Neither theories nor bridge laws are directly testable; they are only *indirectly* testable. Because both theories and bridge laws are composed of statements in the form of generalized conditionals, neither can be tested by a direct confrontation with data. To illustrate, consider the bridge laws mentioned in section 6.2.1:

BL_1: Consumers will experience cognitive dissonance after the decision to purchase a major appliance.

BL_2: If retailers provide information to recent purchasers of major appliances, and if the information reassures the consumers that they made wise decisions, then the information will assist the consumers in their efforts to reduce cognitive dissonance.

As stated, these bridge laws are not susceptible to a direct comparison to data; they are not *directly* testable. In contrast, research hypotheses are directly testable. These are predictive-type statements that are (a) derived from the bridge laws and (b) amenable to direct confrontation with data. For example, a research hypothesis derived from the preceding bridge laws and actually tested was: "The subjects who received the post-transaction reassurances will have lower perceived dissonance scores (i.e., will be less dissonant) than the subjects in the control group" (Hunt 1970, p. 48). The research hypothesis is directly testable because it refers to specific subjects in specific groups (rather than consumers in general) and to specific scores on a measuring instrument (rather than dissonance in general).

The requirement that research hypotheses be predictive-type statements that are amenable to direct confrontation with data implies that all of the descriptive terms in the statements must have rules of interpretation containing empirical referents. These referents are sometimes referred to as reflective measures, operational definitions, formative indicators, or epistemic correlations. (See Arnett, Laverie, and Meiers [2003]; Diamantopoulos and Winklhofer [2001]; Howell, Brevik, and Wilcox [2007] for good discussions of reflective versus formative indicators.) This does not mean that all of the terms in the theory proper must be "observables" or that every term in the theory must have an empirical referent. Rather, to require theories to be empirically testable implies that they must be capable of generating predictive-type statements (hypotheses) whose descriptive terms have empirical referents. The notion of empirical referents deserves some elaboration, especially insofar as it is closely intertwined with the school of thought commonly known as *operationalism,* from which social science has inherited the term "operational definition."[4]

The position taken here should be carefully distinguished from the early forms of logical positivism or what Hempel (1970, p. 675) refers to as the *narrow thesis of empiricism.* According to the narrow thesis, "Any term in the vocabulary of empirical science is definable by means of observation terms; i.e., it is possible to carry out a rational reconstruction of the language of science in such a way that all primitive terms are observation terms and all other terms are defined by means of them." This *radical empiricist* position was adopted by the school of thought known as *operationalism,* which was originated by P.W. Bridgman (1927). Three key propositions are central to the doctrine of operationalism:

1. Only terms that have operational definitions are "meaningful." Terms without operational definitions are "meaningless," and, hence, have no value in scientific endeavor (Bridgman 1938).

2. Operational definitions always make reference ultimately to an instrumental operation (Bridgman 1951). That is, there must be some experiment or manipulation involved.
3. There must be only one operational definition for each scientific term (Bridgman 1927, p. 6).

The three propositions embodying the radical empiricism of the operationalists have been successfully challenged by Hempel (1970), Fetzer (1977), and others (Bergmann 1957, p. 58). Hempel (1970, p. 684) suggests that proposition (1) is untenable because (among other reasons) *theoretical constructs* play a vital role in science. These highly abstract concepts, for example, "channel of distribution," "self-image," "transvection," and "utility," often stoutly resist direct operational definitions. Such terms are not introduced piecemeal into a theory. "Rather, the constructs used in a theory are introduced jointly, as it were, by setting up a theoretical system formulated in terms of them and by giving this *system* an experiential interpretation, which in turn confers empirical meaning on the theoretical constructs" (Hempel 1970, p. 684). Thus, proposition (1) of the operationalists seems much too restrictive.

Hempel (1965b, p. 126) suggests that proposition (2) is also overly restrictive. Requiring direct experimenter manipulations is unnecessary to give experiential import to a term. Measures based on unobtrusive observations will often yield equally satisfactory results, maintaining intersubjective reliability. The important requirement is that different investigators be able to observe the phenomenon with reasonable agreement as to whether the requisite test conditions have been realized and the appropriate response obtained.

Concerning proposition (3), Bergmann (1957, p. 58) notes that some researchers refused "to 'generalize' from one instance of an experiment to the next if the apparatus had in the meantime been moved to another corner of the room or if the experimenter had, in the one case but not the other, blown his nose." The replication of an experiment would be logically impossible if proposition (3) were interpreted literally, because each instance would be a "different" operational definition. Furthermore, testing a theory under a variety of circumstances of application is extremely desirable in the process of confirmation. Yet, testing a theory under different circumstances may entail using different operational definitions for key constructs. The adoption of proposition (3) would imply that each time we change the circumstances in the testing of a theory, we must consider ourselves to be testing a "new" theory. Science seems hardly to be advanced by the adoption of such a position. A much more viable position seems to be to acknowledge that alternative and equally valid operational definitions or measures may exist for the same concept, while maintaining constant vigilance to ensure that the alternative operational definitions or measures are, indeed, equally valid.

In conclusion, the preceding analysis suggests that the position of the radical empiricists is untenable. At the same time, metaphysical excesses are to be avoided. *Consequently, the requirement that theories be empirically testable shall be construed as being satisfied when a theory is capable (at least in principle) of generating predictive-type statements (hypotheses) whose descriptive terms have empirical referents, thus ensuring that the statements*

are amenable to a direct confrontation with real-world data. This requirement is similar to the criterion of significance for theoretical terms suggested by Carnap (1956).

7.5.3 On Confirmation

Consider how "research hypothesis" is used in Figure 7.3. Wartofsky has observed that

> No term in science suffers a greater ambiguity than does *hypothesis.* One could make up a list of contradictory statements about hypotheses and their status and use in scientific discussion which would make the scientific community look like something on the other side of Alice's looking glass. (Wartofsky 1968, p. 183)

Alas, the term *hypothesis* is commonly used as a general-purpose label synonymous with concepts such as law, lawlike generalization, derived law, theory, explanation, model, axiom, and theorem. The term *hypothesis* is overworked, overused, overbroad. Such abuse may result in little harm in ordinary conversation but may have serious, unintended consequences in research and scientific writing. Therefore, this writer suggests using the term *hypothesis* (or *research hypothesis*) to represent statements that are derived from laws or theories and are susceptible to direct testing by confrontation with real-world data. This usage is by no means a consensus position because there is no consensus concerning the term. However, the viewpoint expressed here is similar to Dubin's (1969, p. 212) position: "An hypothesis may be defined as the predictions about values of units of a theory in which empirical indicators are employed for the named units in each proposition."

The important consequence of reserving the label "hypothesis" for directly testable statements is that it highlights the notion of the empirical confirmation of theories. Because theories are not directly testable, they are not *strictly confirmable.* Theories cannot be shown to be conclusively true in an empirical sense. One can only say that certain research hypotheses have been derived from a theory and that these hypotheses have been directly tested. If the hypotheses are confirmed, then this provides empirical support that the theory is, indeed, empirically true; that is, the theory has been empirically corroborated by the confirmation of the research hypotheses. If the hypotheses are rejected by the data, then this provides empirical evidence that either (a) the theory is false (reality just is not constructed as the theory suggests), or (b) errors have been made in the empirical testing procedures, or (c) the rejected hypothesis was not properly derived from the theory, or some combination of a, b, and c.

The preceding discussion of the empirical testing procedure suggests that both *deduction* and *induction* play vital roles in the process of empirical confirmation. Because theories are not directly testable, one must *deduce* from them research hypotheses that are susceptible to direct confrontation with data. Once the tests of the research hypotheses have been conducted, the procedure of empirical confirmation is inherently *inductive.* To believe that empirically confirming a research hypothesis will strictly confirm a theory is to fall prey to the logical fallacy of *affirming the consequent.* It would be claiming the following to be a valid syllogism:

If theory *X* is true, then hypothesis *h* is true.
Hypothesis *h* is empirically true.

Therefore, theory *X* is true.

Obviously, hypothesis *h* could be true and yet theory *X* could be false. Therefore, when a theory has been tested many times and its hypotheses have been confirmed, we cannot say that the theory is empirically true; rather, we can say that the empirical tests have provided strong *inductive* support for the truth of the theory. In a sense, we are "weighing" the empirical evidence (Bunge 1967b, p. 319). The "heavier" the weight of the empirical evidence, the more likely it is that the theory accurately represents reality, and, thus, the more highly confirmed the theory is.

7.6 SUMMARY

This chapter has attempted to explicate the fundamental underpinnings of the nature and role of theory in scientific inquiry. The treatment here has proposed that the major role of theory is to *increase scientific understanding through a systematized structure capable of both explaining and predicting phenomena.* Consequently, theories become systematically related sets of statements, including some lawlike generalizations, that are empirically testable. Theories must contain a systematically related set of statements because science seeks to give an organized account of phenomena. Theories must contain lawlike generalizations because it is precisely these statements that give theories their explanatory and predictive power. Theories must be empirically testable in order that they may be (a) intersubjectively certifiable, (b) capable of explaining and predicting real-world phenomena, and (c) differentiated from purely analytical schemata. A theory is capable of being empirically testable when it is possible to derive from the theory certain predictive-type statements (hypotheses) that are amenable to direct confrontation with real-world data.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Define and differentiate: theories, laws, hypotheses, models, generalizations, empirical regularities, propositions, and concepts. Show how these terms are related.
2. Do theories generate hypotheses, or do hypotheses lead to theories? How are theories generated?
3. Would marketing theory be useful to a marketing practitioner? To a government official? To a teacher of a basic marketing course?
4. "A theory is a systematically related set of statements, including some lawlike generalizations, that is empirically testable." We may therefore conclude that all theories can be shown to be either "true" or "false." Discuss.

5. Someone has said: "The problem with marketing research is the lack of an isomorphic relationship between concepts and their respective operational definitions." Evaluate.
6. Robert K. Merton makes a plea for "theories of the middle range" in sociology:

> Throughout this book, the term *sociological theory* refers to logically interconnected sets of propositions from which empirical uniformities can be derived. Throughout we focus on what I have called theories of the middle range: theories that lie between minor but necessary working hypotheses that evolve in abundance during day-to-day research and the all-inclusive systematic efforts to develop a unified theory that will explain all the observed uniformities of social behavior, social organization, and social change. (Merton 1994/1968, p. 39)

   Is Merton actually pleading for theories in sociology with greater extension? (See Chapter 6.) Evaluate the current work in marketing theory. How much of it appears to be in the middle range? Give examples of works that are in the middle range, more specific than the middle range, and more general than the middle range. Does Merton's plea also apply to marketing?
7. A major issue in marketing theory concerns the development of a general theory of marketing. Bartels (1968) suggests that "the broadest statement of marketing thought in any period is the 'general theory' of that day." Similarly, the subtitle of Alderson's (1965) last book was *A Functionalist Theory of Marketing.* Do you agree with Bartels's definition of a general theory of marketing? If yes, why? If no, suggest an alternative definition and show how it is superior to Bartels's definition. Will we ever have (i.e., is it possible to have) a general theory of marketing? What would be the role or purpose of a general theory of marketing if we did have one?
8. What is the relevance to the content of the chapter of the "garden in the jungle" epigraph at the beginning of the chapter?
9. Evaluate Bartels's perspective on the nature of theory:

> Theory is a form in which knowledge is expressed, and the term is used with two meanings. First, it designates a tentative, speculative, or unproven generalization concerning a subject. In this sense, it is synonymous with "hypothesis" and presents an early stage in the logical process. Second, it means a summary, or considered conclusion reached after analysis and synthesis of information, and, as such, it represents a mature stage in the development of thought. This second concept of theory is the sense in which the term is used hereafter. (Bartels 1970, p. 2)

10. Many theories or models in marketing consist primarily of a diagram with little boxes, each having a single concept or construct and various arrows connecting the boxes. Is this a theory proper or a pictorial representation of

a theory? To what extent is this procedure appropriate or inappropriate? Can it be abused?

11. Evaluate the following three characteristics of a good theory proposed by Baumol (1957):
    1. The model should be a sufficiently simple version of the facts to permit systematic manipulation and analysis.
    2. It must be a sufficiently close approximation to the relevant facts to be usable.
    3. Its conclusions should be relatively insensitive to changes in its assumptions.
12. In "Lawlike Generalizations and Marketing Theory," Hunt (1973) contends that "in order for generalizations to be considered lawlike, the *minimum* necessary conditions are that the generalizations specify a relationship in the form of a universal conditional (such as my example in the original note), which is capable of yielding predictive statements (hypotheses), which are composed of terms that have empirical referents and, thus, permit empirical testing." Are they necessary conditions? Are they the minimum necessary conditions?
13. El-Ansary (1979) in his "The General Theory of Marketing: Revisited" proposed that "a vertical marketing system, distribution channel, is a key integrative concept in marketing." Evaluate the contention that in a general theory of marketing the marketing channel would be the key integrative concept.
14. El-Ansary (1979) suggests that a major section of a general theory of marketing would be "a theory of micromarketing." Apparently, he believes that such a theory would include subtheories of product and brand management, pricing, promotion, physical distribution management, marketing research, financial aspects, and marketing program productivity. What would these subtheories attempt to explain and predict? Would such a collection of subtheories be appropriately referred to as a theory of micromarketing? Would a theory of micromarketing be positive or normative? Would it be possible to have a general theory of marketing that included both positive and normative components?
15. There is a famous paradox on how theories and laws are confirmed or corroborated by empirical testing. First proposed by Hempel (1945), the paradox is usually referred to as the "raven paradox" and can be roughly stated as:
    a. The statement *P* "All ravens are black" is logically equivalent to the statement *P** "All nonblack things are nonravens."
    b. One may empirically explore the validity of *P* by examining ravens and seeing whether they are black (tend to confirm) or nonblack (tend to disconfirm).
    c. One may empirically examine *P** by looking at nonblack objects (e.g., roses) and determining whether they are nonravens (tend to confirm) or ravens (tend to disconfirm).
    d. Any evidence that tends to confirm *P** must logically tend to confirm *P.*
    e. Therefore, the fact that a rose may be red tends to confirm that ravens are black.
    f. Statement *e* is intellectually disquieting.

Gardner discusses the paradoxical nature of ravens thus:

> We look around and see a yellow object. Is it a raven? No, it is a buttercup. The flower surely confirms (albeit weakly) that all nonblack objects are not ravens, but it is hard to see how it has any *relevance* at all to the statement "All ravens are black." If it does, it equally confirms that all ravens are white or any color except yellow. (Gardner 1976, p. 121)

Evaluate. (Remember that logic, like nature, is often surprising, sometimes fascinating, but never paradoxical.)

16. Do you agree that logic, like nature, is never paradoxical? If yes, why? If no, why not?
17. Alderson's law of exchange states: If *X* is an element in the assortment *A,* and *Y* is an element in the assortment *B,* then *X* is exchangeable for *Y* if, and only if, the following three conditions hold:
    1. *X* is different from *Y.*
    2. The potency of the assortment *A* is increased by dropping *X* and adding *Y.*
    3. The potency of the assortment *B* is increased by adding *X* and dropping *Y.*

    Evaluate the law of exchange. To what extent does the law of exchange differ from conventional microeconomic demand theory? To what extent does the law of exchange have empirical content?
18. "In order for an economy to be characterized by many variations of the same basic product, it is necessary and sufficient that there be heterogeneity of demand." True? False? Why?
19. For Alderson (1965, p. 86), "a transvection is the unit of action for the system by which a single end product such as a pair of shoes is placed in the hands of the consumer after moving through all the intermediate sorts and transformations from the original raw materials in the state of nature." How does a transvection differ from a channel of distribution? How does a transvection differ from Michael Porter's concept of "value chain"? Discuss the actual and potential usefulness of Alderson's transvection concept in marketing. Develop a hypothetical example of a transvection.
20. Alderson discusses his "discrepancy of assortments" as follows:

> Goods are associated in different patterns at various levels in the channel. . . . Goods are associated at the manufacturing level because they can be made on the same equipment or in the same plant. They are associated at the wholesale level because of similarities among trade customers and similar requirements for shipment and storage. They are associated at the retail level because of consumer purchasing habits and convenience. . . . While in general there is some degree of resemblance between retail assortments and wholesale assortments in the same line of trade, the matching is far from perfect. . . . This phenomenon in the channels of trade is called the discrepancy of assortments. (Alderson 1965, p. 78)

How does this concept attempt to explain the existence of intermediaries? Why is a wholesaler "most vulnerable when it purchases only a part of what the manufacturer supplies and sells to retailers only a small portion of what their customers demand" (p. 80)? Do you agree? Discuss the actual and potential usefulness of the concept of "discrepancy of assortments."

21. Bagozzi (1979) in "Toward a Formal Theory of Marketing Exchanges" proposes that "in their interactions with each other and with other social actors, the parties to an exchange are presumed to maximize"

$$U_d = U(Z_a, Z_c, Z_{mb})$$

where U_d is the utility for the dyad and Z_a = affect, Z_c = cognitions, and Z_{mb} = moral beliefs. To what extent does this equation differ from Alderson's Law of Exchange? Do you agree that the parties to an exchange maximize U_d?

22. Ferrell and Perrachione (1980, p. 159) contend that "one of Bagozzi's recurring goals or self-imposed criteria is to construct a theory that will go beyond description to explanation (and eventually prediction and control). Yet the models he has proposed, in spite of their frequent descriptive richness, are consistently insufficient when measured against the explanation criterion." Do you agree or disagree with this criticism?

NOTES

1. The succeeding discussion follows, in part, Rudner (1966, pp. 10–18) and Kyburg (1968).

2. See Chapter 11 and Hunt (1990b) for a discussion and defense of the role of *truth* in marketing theory and research.

3. See Chapter 12, Bunge (1967b, p. 309), and Hunt (1992a, 1993, and 1994a) for more on empirical testing.

4. Most social scientists and marketers today use "operational definition" and "measure" interchangeably. However, "operational definition" in operationalism had a precise meaning, as shown in proposition (2).

8

THEORY: ISSUES AND ASPECTS

> A science is served in many ways: by intelligent discussion and fresh proposals, by the extension or completion of previously presented theories, by the fair-minded and unflinching evaluation of current proposals, by justly protesting, blowing the whistle, and pointing out that this kingly theory or that is not wearing a shred of evidence, by sometimes synthesizing and sometimes isolating, by daring to be explicit and—ironically—by daring to be suggestive. It is when scientists and philosophers of science cannot make up their minds as to which role they are playing or—what is worse—try to fill several roles at once, that matters go awry. Then the Ivory Tower and the Tower of Babel sound disturbingly alike.
>
> *—Paul Surgi Speck*

The preceding chapter explored the nature of theoretical constructions. The purpose of this chapter is to examine several specific issues in marketing theory. Because classificational schemata are often confused with theoretical schemata, we begin by analyzing the nature of classifications in marketing. Next, we delineate the differences between positive theory, normative theory, deterministic theory, and stochastic theory. The chapter concludes by explicating the nature of general theories.

8.1 CLASSIFICATIONAL SCHEMATA

Marketing is replete with classificational schemata. There are classificational schemata for different kinds of goods (convenience, shopping, etc.), stores (department stores, limited line stores, etc.), wholesalers (general merchandise, general line, etc.), pricing policies (cost-plus, demand-oriented, etc.), and numerous others. Classificational schemata play fundamental roles in the development of a discipline in that they are the primary means for *organizing* phenomena into classes or groups that are amenable to systematic investigation and theory development. Nevertheless, classificational schemata, no matter how elaborate or complex, are not by themselves theoretical, though most theoretical constructions will contain classificational schemata as components. As previously noted, an analysis of the seven subtheories of Bartels's general theory of marketing revealed them to be primarily classificational schemata that lacked the requisite lawlike generalizations to be considered theories (Hunt 1971, p. 68).

Marketing has long-suffered from a lack of standardized classificational schemata. As Engel, Kollat, and Blackwell (1973, p. 659) pointed out, "The lack of standardized variable categories . . . makes it difficult to compare and integrate research findings." If having a variety of nonstandard classificational schemata for the same phenomenon is dysfunctional, how does one select the best classificational schema from the available alternatives? Because classificational schemata help to organize the elements of the universe, and because organizing phenomena often represents the first step in theory development, how can one differentiate a good classificational schema from a bad one? This section will attempt to answer these questions, first, by discussing the two basic approaches to generating classificational schemata and then by developing some criteria for evaluating any classificational schema.

Classificational systems always involve a partitioning of some heterogeneous universe of objects, events, or other phenomena into classes or sets that are homogeneous with respect to some categorical properties. There are two distinctly different procedures or methods for generating classificational schemata. Following the essence of Harvey's (1969, p. 334) terminology, one procedure is *logical partitioning*, and the second is *grouping.* The procedure referred to here as logical partitioning is sometimes called "deductive classification," "*a priori* classification," or "classification from above." Grouping is a common label for the second procedure (more accurately, second *set* of procedures). The grouping procedures are often called "inductive classification," "*ex post* classification," "classification from below," "numerical taxonomy," or "quantitative classification." The essential difference between the logical partitioning and grouping procedures is that with the former, the classificational schema is always developed *before* the researcher analyzes any specific set of data (hence, "deductive," "*a priori,*" and "from above"). In contrast, when using grouping procedures, researchers generate their schemata only *after* they analyze specific sets of data (hence, "inductive," "*ex post,*" and "from below"). With logical partitioning, the researcher *imposes* a classificational system on the data; with grouping, the researcher lets the data suggest the system. Both kinds of procedures are used in marketing, and both have their strengths and weaknesses. After a (very) modest elaboration of the two procedures, we shall explore some criteria for evaluating any classification system.

8.1.1 Logical Partitioning

Logical partitioning starts with the careful specification of the marketing phenomena to be categorized—families, retailers, wholesalers, types of goods, brands of goods, and so forth. Next comes the delineation of the categorial terms. These are the properties or characteristics of the phenomena on which the classificational schema is to be based—for families this might be age, marital status, and number of children; for retailers it might be number of units and type of ownership. Finally, labels are given to the various categories that emerge from applying the categorial terms to the phenomena—thus, for families we have "newly married couples," the "full nest I," "empty nest," and so on; and for retailers we have independents, chain stores, and so on.

Several observations concerning logical partitioning are important to keep in mind. First, Sneath and Sokal (1973) point out that logical partitioning usually results in *monothetic* classifications. With monothetic classification systems, *all* members of a category possess *all* of the characteristics or properties used to identify the category. To illustrate this point, consider the commonly used stage in the family life cycle schema reproduced in Table 8.1. This schema is an example of logical partitioning with monothetic classifications. In order for a family to be classified as "full nest III," the family must satisfy *all* of the criteria; that is, the couple must be over forty-five years old and married, and the youngest child must be six years or older. If the couple satisfied the age and marital status criteria but had, by chance, a child under six years old, then it could not be classified as "full nest III." (In fact, it would not fit any category in the schema, a point to be discussed later.) Now, it may intuitively appear that all classificational schemata would be monothetic. But this is not the case, as will be shown when we discuss grouping procedures.

The second observation concerning logical partitioning is that the procedure can result in either single-level or multilevel schemata. Prominent among multilevel schemata are *hierarchical* classification systems that involve the ordering relation $\subset$ from set theory. Thus A $\subset$ B should be read, "The class of phenomena designated as *A* are contained in the class of phenomena designated as *B*." Hierarchies can be displayed by means of Euler-Venn diagrams, from set theory, or by *trees*. Figure 8.1 illustrates the common hierarchical classification of wholesalers by means of a tree. The ordering relation $\subset$ is both asymmetrical and transitive. Thus, $A \subset B$, being asymmetrical, implies that $B \not\subset A$ (brokers are a subset of the class known as agent wholesalers, but agent wholesalers are *not* a subset of the class called brokers). Similarly, $A \subset B$ and $B \subset C$ implies, by transitivity, $A \subset C$ (if a mail-order wholesaler is a kind of limited-function wholesaler, and if a limited-function wholesaler is a kind of merchant wholesaler, then a mail-order wholesaler is a kind of merchant wholesaler). Hierarchical classifications are preferable (other things equal) to single-level classifications because of their greater systemic power. That is, hierarchical classifications have greater power to systematically organize the phenomena under investigation.

The third observation about logical partitioning is that there may exist *empty classes*. That is, a proper application of the categorial terms may generate a class to which no phenomenon belongs. The importance of empty classes lies in the context of discovery; observing that empty classes exist may spur the researcher to inquire about the circumstances under which phenomena might be classified into the now empty set. For example, the existence of holes or empty classes in Mendeleev's periodic table of elements in chemistry suggested the existence of, and spurred the search for, previously unknown elements.

A final observation, suggested by Harvey (1969, p. 366), is that logical partitioning "presupposes a fairly sophisticated understanding of the phenomena being investigated, [or] else the classifications involved may be totally unrealistic, nothing better than an inspired guess." Any universe of phenomena can be classified in an infinite variety of ways. Which properties or characteristics are the important ones to use for classificatory purposes? Which classes would be most meaningful for research? Logical partitioning

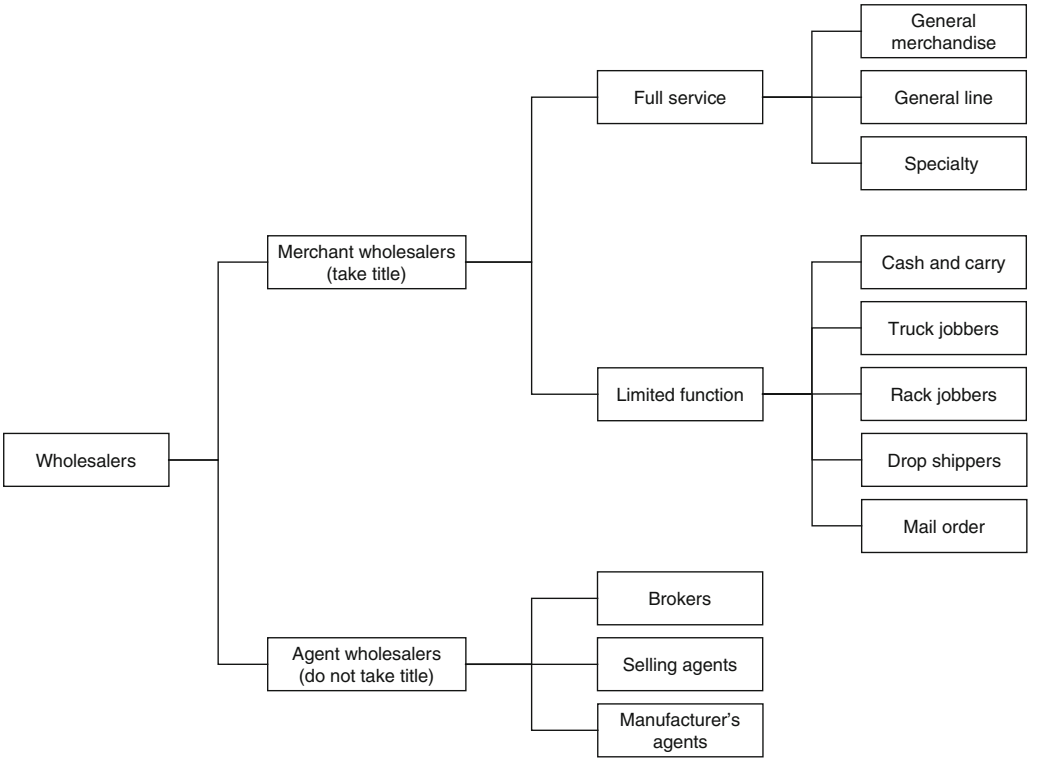
Table 8.1

The Stage in the Family Life Cycle Schema

	Age		Marital status		Children			
Category	Less than 45 years	45 years or older	Single	Married	No children	Youngest child under 6 years	Youngest child 6 years or over	No dependent children
1. The bachelor stage	X		X					
2. Newly married couples	X			X	X			
3. The full nest I	X			X		X		
4. The full nest II	X			X			X	
5. The full nest III		X		X			X	
6. The empty nest		X		X				X
7. The solitary survivors		X	X					X

Source: Hunt (1991a). Reprinted by permission of the author.

Figure 8.1 **Classification of Wholesalers**



Source: Hunt (1991a). Reprinted by permission of the author.

assumes substantial *a priori* knowledge about how to answer these questions. The procedures referred to previously as grouping procedures, which will be discussed next, require much less *a priori* knowledge about the phenomena to be classified.

8.1.2 Grouping Procedures

Like logical partitioning, all grouping procedures for classification start with the specification of the phenomena to be classified and the properties or characteristics on which the categorizing is to be done. However, grouping procedures are designed to conveniently accommodate larger numbers of properties than is the case with logical partitioning. As Frank and Green point out:

> Almost every major analytical problem requires the classification of objects by several characteristics—whether customers, products, cities, television programs, or magazines. Seldom are explicit classification systems with some combination of attributes, such as those used for measuring a customer's social class or stage in life cycle, found. Such classification systems typically represent self-imposed

> taxonomies; that is, taxonomies the researcher believes to be relevant because of a theory or prior experience. Although this approach [logical partitioning] can be useful, it has limitations. Regardless of the complexity of reality, it is difficult to classify objects by more than two or three characteristics at a time. If reality requires greater complexity, researchers are severely constrained by their conceptual limitations. (Frank and Green 1968, p. 84)

So, the first difference between logical partitioning and grouping is that the grouping procedures are better equipped to handle large numbers of categorial terms or properties. The second difference, and perhaps the most significant from a methodological perspective, is that *all grouping procedures share the common characteristic that they determine categories or classifications by an analysis of a specific set (or specific sets) of data.* Consequently, they are referred to as "inductive," "*ex post,*" or "classification from below" procedures.

Although all grouping procedures determine classifications by analyzing data, numerous basic models have been developed to accomplish the classificatory task, and many computer programs have been generated for each model. Among the most commonly used models are correspondence analysis, factor analysis, multiple discriminant analysis, multidimensional scaling, and cluster analysis. Thus, correspondence analysis has been used to classify soft drinks (Hoffman and Franke 1986), and factor analysis has been used to classify liquor preferences, job characteristics, and coffees (Stroetzel 1960; Hunt, Chonko, and Wood 1985; Mukherjee 1965). Multiple discriminant analysis has been used to classify types of patients (Dont, Lumpkin, and Bush 1990). Both pharmaceuticals and colors of automobiles (Neidell 1969; Doehlert 1968) have been classified by multidimensional scaling. Cluster analysis is the most frequently used technique (Hair et al. 1995). It has generated classifications of cities for test marketing and market survey respondents (Green, Frank, and Robinson 1967) and dissatisfaction response scores (Singh 1990). Finally, gasoline brands have been classified by Kamen (1970) using "quick clustering" and by Aaker (1971) using "visual clustering." Although some of these models are often used for purposes other than classification, researchers are ever more frequently using them to organize phenomena for classification. The tremendous differences among the models notwithstanding, they share the common property of separating phenomena into groups that maximize both the degree of "likeness" *within* each group and the degree of difference *between* groups, according to some objective function.

A detailed exposition of the specific models for grouping phenomena need not concern us here; interested readers are advised to consult the previously cited references for guidance as to which specific grouping model would be most appropriate for their needs. What shall concern us from a methodological perspective are certain fundamental characteristics of grouping procedures, when contrasted with logical partitioning. First, Sneath and Sokal (1973) observe that classificational schemata that are developed by grouping procedures usually have classes that are *polythetic.* Recalling that logical partitioning usually results in monothetic classes, with *polythetic* classes, the phenomena in any given class may

share many characteristics in common; however, no individual phenomenon need possess *all* of the characteristics of the class. One simple example will illustrate this point. W.A.K. Frost (1969) used a clustering program to classify different television programs. One cluster contained the following programs: *World of Sport, Football, Sportsview, Horse Racing, Motor Racing, Boxing,* and *News.* All of the programs, save one, share (among other things) the common characteristic of being either sporting events (boxing, etc.) or solely concerned with sporting events (*Sportsview*). The *News* program must be perceived by viewers as being similar to the other members of its cluster *in toto;* yet, it does not share the *sports* characteristic common to its companions. It is in this respect that grouping procedures are polythetic.

Second, unlike logical partitioning, grouping procedures do not generate empty classes because classes can be formed only from existing observations. To the extent that empty classes serve the heuristic function of suggesting fruitful avenues for research, the inability to generate empty classes might be disadvantageous. In addition, at the risk of sounding tautologous, if researchers use a clustering program, they will get clusters. Aaker emphasizes this point when he advocates "visual clustering":

> [The results] indicate that the structure is not very well defined and that an unambiguous set of clusters does not exist. This observation, it should be noted, is not insignificant. Often an output which lists several clusters fails to convey this type of conclusion to those not intimate with the program. (Aaker 1971, p. 331)

Grouping procedures require substantially less *a priori* knowledge concerning which specific properties are likely to be powerful for classifying phenomena than in the case in logical partitioning. Furthermore, grouping procedures are uniquely equipped to accommodate large numbers of potentially useful categorial properties. These are powerful advantages for grouping procedures. Nevertheless, these procedures have not yet produced many *general* classificational schemata. Classifications developed through grouping procedures seldom are generalized beyond their original database. Rather than developing classification systems for marketing phenomena in general, the use of these procedures has been restricted to problems that are highly situation specific. Perhaps this is what Frank and Green are implying when they suggest that we refer to grouping procedures as "preclassification techniques":

> These taxonomic procedures may be called preclassification techniques since their purpose is to describe the natural groupings that occur in large masses of data. From these natural groupings (or clusters) the researcher can sometimes develop the requisite conceptual framework for classification. (Frank and Green 1968, p. 84)

Irrespective of the procedure used for classifying, either logical partitioning or grouping, the resultant classificational schema can be evaluated using the same criteria. The next section develops such criteria.

8.1.3 Criteria for Evaluating Classificational Schemata

Numerous criteria have been suggested for evaluating alternative classificational schemata. Although the following five criteria do not exhaust the possibilities, they would seem to make a useful starting point for any researcher who has a classification problem:

1. Does the schema adequately specify the phenomenon to be classified?
2. Does the schema adequately specify the properties or characteristics that will be doing the classifying?
3. Does the schema have categories that are mutually exclusive?
4. Does the schema have categories that are collectively exhaustive?
5. Is the schema useful?

Criterion 1 inquires whether the schema adequately specifies the phenomenon to be classified. That is, *exactly* what is being categorized? What is the universe? An analysis of the familiar classification of goods schema should help to explicate this criterion. Table 8.2 reproduces four versions of the classification of goods schema: the 1960 American Marketing Association (AMA) definition (which closely follows the original version by Copeland [1923]), the 1988 AMA definition (which is very similar to the 1960 version), the version suggested by Richard H. Holton (1958), and the version suggested by Louis P. Bucklin (1963) in his classic article "Retail Strategy and the Classification of Goods."

What is the universe that is being partitioned by the classification of goods schema? Is it products, brands of products, consumers' perceptions of products, or consumers' perceptions of brands of products? The answer is not unambiguous. The two AMA definitions of convenience goods appears to be referring to *products,* yet both the Holton and Bucklin versions of convenience goods appear to be classifying consumers' *perceptions* of products. Holton (1958, p. 54) observed, "A given good may be a convenience good for some consumers and a shopping good for others." This would imply that the universe consists of perceptions of goods rather than goods *per se.* Thus, McCarthy (1971) suggests that the approach a marketing manager should take is to determine the proportion of consumers who perceive the product as a convenience good and compare this with the proportion who perceive it as either a shopping good or a specialty good. This approach implies that there will seldom, if ever, be anything that could be classified as a *convenience good*; rather, there are only a certain proportion of consumers who *perceive* a good to be a convenience good.

Similar ambiguity surrounds the specialty goods category, where people commonly cite specific *brands* as examples of specialty goods. Thus, for some consumers a particular *brand* of coffee would be a specialty good, and the universe being partitioned would be consumers' perceptions of brands of goods and not simply goods. Perhaps some of the debate that has centered on the classification of goods schema during the past decades can be explained by pointing out that this schema does not do well on criterion 1; it does not clearly specify the universe to be classified. Readers should ask themselves whether

Table 8.2

Classifications of Goods' Schemata

Category	American Marketing Association (AMA)	Richard H. Holton[c]	Louis P. Bucklin[d]
Convenience goods	AMA 1960.[a] Those consumers' goods which the customer purchases frequently, immediately, and with a minimum of effort. AMA 1988.[b] Consumer goods and services . . . that are bought frequently, often on impulse, with little time and effort spent on the buying process. They usually are low-priced, and are widely available.	Those goods for which the consumer regards the probable gain from making price and quality comparisons as small compared to the cost of making such comparisons.	Those goods for which the consumer, before his need arises, possesses a preference map that indicates a willingness to purchase any of a number of known substitutes rather than to make the additional effort required to buy a particular item.
Shopping goods	AMA 1960.[a] Those consumers' goods which the customer in the process of selection and purchase characteristically compares on such bases as suitability, quality, price, and style. AMA 1988.[b] Products . . . for which the consumer is willing to spend considerable time and effort in gathering information on price, quality, and other attributes. Several retail outlets are customarily visited. Comparison of product attributes and complex decision processes are common.	Those goods for which the consumer regards the probable gain from making price and quality comparisons as large relative to the cost of making such comparisons.	Those goods for which the consumer has not developed a complete preference map before the need arises, requiring him to undertake a search to construct such a map before purchase.
Specialty goods	AMA 1960.[a] Those consumers' goods on which a significant group of buyers are habitually willing to make a special purchasing effort. AMA 1988.[b] Products that have unique attributes or other characteristics which make them singularly important to the buyer. Multiple-store searching, reliance on brand, and absence of extensive product comparisons are the rule.	Those convenience or shopping goods which have such a limited market as to require the consumer to make a special effort to purchase them.	Those goods for which the consumer, before his need arises, possesses a preference map that indicates a willingness to expend the additional effort required to purchase the most preferred item rather than to buy a more readily accessible substitute.

[a]American Marketing Association, *Marketing Definitions* (Chicago, 1960)
[b]Bennett (1988).
[c]Holton (1958).
[d]Bucklin (1963).

other classification schemata in marketing, such as the *product life cycle* schema, suffer from the same ambiguity. For example, does the product life cycle schema refer to an industry's product or to an individual company's product?[1]

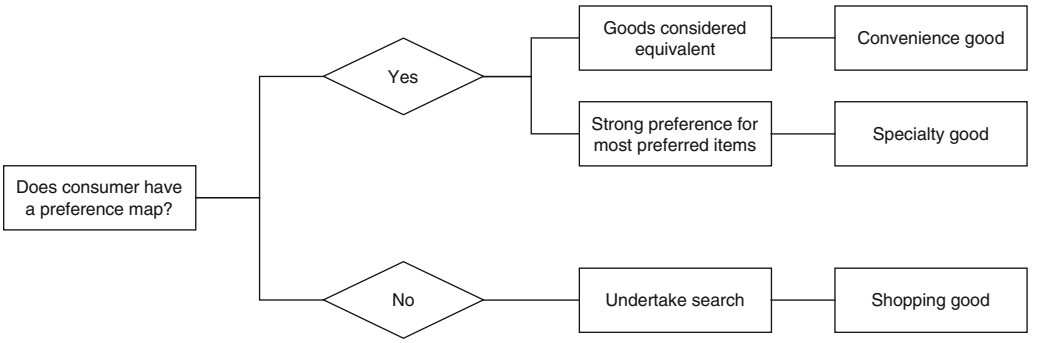
Criterion 2 inquires whether the properties or characteristics that have been chosen to do the classifying have been adequately specified. Implicit in criterion 2 is the question: "Are these properties the *appropriate* properties for classificatory purposes?" One clue that the chosen properties may be inappropriate is when different properties are used throughout the schema, "changing horses in midstream," so to speak. Bunge (1967a, p. 75) addresses this issue: "One of the principles of correct classification is that the characteristics or properties chosen for performing the grouping should be stuck to throughout the work; for example, a shift from skeletal to physiological characteristics in the classification of vertebrates will produce not only different classes but also different systems of classes, i.e., alternative classification."

Referring once again to the classification of goods schema in Table 8.2, note that the two AMA schemata use three properties to identify convenience goods: purchased (a) frequently, (b) immediately or "often on impulse," and (c) with a minimum of effort. This would lead one to expect some other category to contain goods purchased (a) infrequently, (b) not immediately or not on impulse, and (c) with great effort. Yet, shopping goods are classified on the basic property of *comparison* or "gathering of information," and specialty goods are classified on the basis of *willingness to expend effort* or "multiple-store searching." The shifting from one set of properties for the classification of convenience goods to other properties for specialty goods and shopping goods is a strong sign that the schema is structurally unsound.

Consider now the revision suggested by Holton and detailed in the second column of Table 8.2. Holton uses the same two properties for classifying convenience goods and shopping goods: the consumer's perceptions of (a) the probable gain for making comparisons versus (b) the probable cost of such comparisons. Unfortunately, Holton then scraps these properties and uses the size of the market to classify specialty goods. Only the Bucklin version of the classification of goods schema consistently uses the same properties to do the job of classifying. These properties are (a) the existence of a preference map and (b) the nature of the preference map if one exists. To see that this is indeed the case, refer to Figure 8.2, which reformulates the essence of Bucklin's schema by means of a tree diagram. Bucklin's efforts are unique in terms of classifying goods because only his schema retains the conceptual richness of Copeland's original concepts, while at the same time rigorously and consistently explicating the properties used for classificatory purposes. Bucklin's version has much to recommend it.

So one aspect of criterion 2 is whether the chosen properties are appropriate for classification. Second, one should ask whether the operational procedures for applying the classificatory properties are rigorous. *The procedures should be intersubjectively unambiguous,* a characteristic that is sometimes referred to as "high interjudge reliability." The procedures should be such that different people would classify the phenomena in the same categories. For example, the family life cycle schema in Table 8.1 has "marital status" as a categorial property with two categories, single and married. To be intersubjectively

Figure 8.2 **Bucklin's Classification of Goods Schema**



Source: Bucklin (1963). Reprinted by permission of the American Marketing Association.

unambiguous, the schema must have rigorous procedures to enable different people reliably to classify widows, widowers, and divorcees.

Is the product life cycle schema intersubjectively unambiguous? Will different people categorize the same product in the same stage? A randomly selected group of a dozen products given to a group of students familiar with the product life cycle notion is hereby guaranteed to produce sobering results.

The third criterion for evaluating classificational schemata suggests that all the categories at the same level of classification should be *mutually exclusive.* That is, if an item fits one category or class, it will not fit any other class. No single item may fit two different categories at the same level. "At the same level" must be emphasized because, of course, in a hierarchical classificational schema the same item will fit different categories at different levels. Therefore, in Figure 8.1 a general line wholesaler is both a full-service wholesaler and a merchant wholesaler.

Many classifications in marketing do not meet the mutually exclusive criterion. The normal distinction between industrial goods and consumer goods typifies the problem. Common usage in marketing suggests, "Consumer goods are those goods and services destined for the ultimate consumer. These contrast with industrial goods, which are those goods and services destined for use in producing other goods and services" (McCarthy 1971, p. 300). Holloway and Hancock (1973, p. 683) have commented that this partitioning of goods does not result in mutually exclusive categories. "Relatively few goods are exclusively industrial goods. The same article may, under one set of circumstances, be an industrial good and under other conditions a consumer good." The lack of exclusivity is not a mortal blow to a useful classificational schema (consider, for example, the unquestionably useful male/female schema). However, exclusivity is an appropriate goal.

Criterion 4 suggests that classificational schemata should be *collectively exhaustive.* Every item that is to be classified should have a "home." Consider, once again, the family life cycle schema in Table 8.1. Into which category would a couple fall if they had the following characteristics: (a) over forty-five years old, (b) married, and (c) youngest child under six years old? Obviously, there is no home for this family. Now, all classifi-

cation systems can be made collectively exhaustive by the simple expedient of adding that ubiquitous category "other." However, the size of this category should be monitored carefully. If too many phenomena can find no home except "other," then the system should be examined carefully for possible expansion by adding new categories.

Criterion 5 simply asks, "Is the schema useful?" Does it adequately serve its intended purposes? How well does it compare with alternative schemata? Of the five criteria, this one is "first among equals." Researchers do not create classificational schemata because they are possessed by some taxonomic devil. Rather, classifications are devised to attempt to solve some kind of problem. Harvey reminds us that we must keep in mind the purpose of the classificational system when evaluating it.

> Classification may be regarded as a means for searching reality for hypotheses or for structuring reality to test hypotheses. It may also be regarded as a beginning point or the culmination of scientific investigation. We possess, therefore, no means of assessing the adequacy or efficiency of a given classification independently of the job it is designed to do. (Harvey 1969, p. 326)

Therefore, the ultimate criterion is usefulness. For example, how useful is the schema for helping marketing managers solve problems? In this regard, the product life cycle schema may pass muster if it simply reminds managers constantly to monitor their product line, because a company cannot afford to have all of its products in the "decline" stage. How *theoretically fruitful* is the schema? Have the concepts embodied in the schema been useful for developing lawlike generalizations? Many popular classificational schemata in marketing (e.g., the product life cycle) have exhibited extremely limited usefulness in generating lawlike statements. Much work remains to be done in this area.

In conclusion, classificational schemata are important in theory development because they are our primary means for organizing phenomena. Generating useful classificational systems frequently represents one of the first steps in theorizing. For this reason, we have spent considerable time analyzing classificational schemata. Nevertheless, classificational systems are a kind of nontheoretical construction because they lack the requisite lawlike generalizations that all theoretical constructions must contain. To develop useful systems for classifying is an endeavor worthy of any researcher, but this should not be confused with the construction of theory proper.

8.2 POSITIVE VERSUS NORMATIVE THEORY

Most of this monograph focuses on the nature of positive theories: systematically related sets of statements, including some lawlike generalizations, that are empirically testable and that increase scientific understanding through the explanation and prediction of phenomena. However, much of the theorizing in marketing is normative, not positive. How do normative theories differ from positive theories? As we shall see, these two kinds of theories differ as to (1) structure, (2) purpose, and (3) validation criteria. First, what is the nature of normative theories?

Normative theories are of at least two kinds. One embodies an *ethical* "ought"; the other, a *rational* "ought." There are normative theories that are essentially *ethical theories* that prescribe morally correct, desirable, or appropriate behavior. Such theories are found in the branch of philosophy generally known as "ethics," which has been a subject of philosophical inquiry since time immemorial. (See section 13.3.3 and Hunt and Vitell [1986, 1993, 2005, 2006] for a theory of marketing ethics.)

However, when marketers discuss "normative theory," we are usually *not* referring to theories of ethical behavior. Rather, we are usually referring to some kind of model that assists a decision maker in rationally or systematically choosing from among a limited set of alternative actions or strategies, given certain (1) objectives, (2) consequences or payoffs, and (3) states of nature. As an example, consider the "high-assay model," reproduced in Figure 8.3, which was originally developed by the advertising agency of Young and Rubicam.

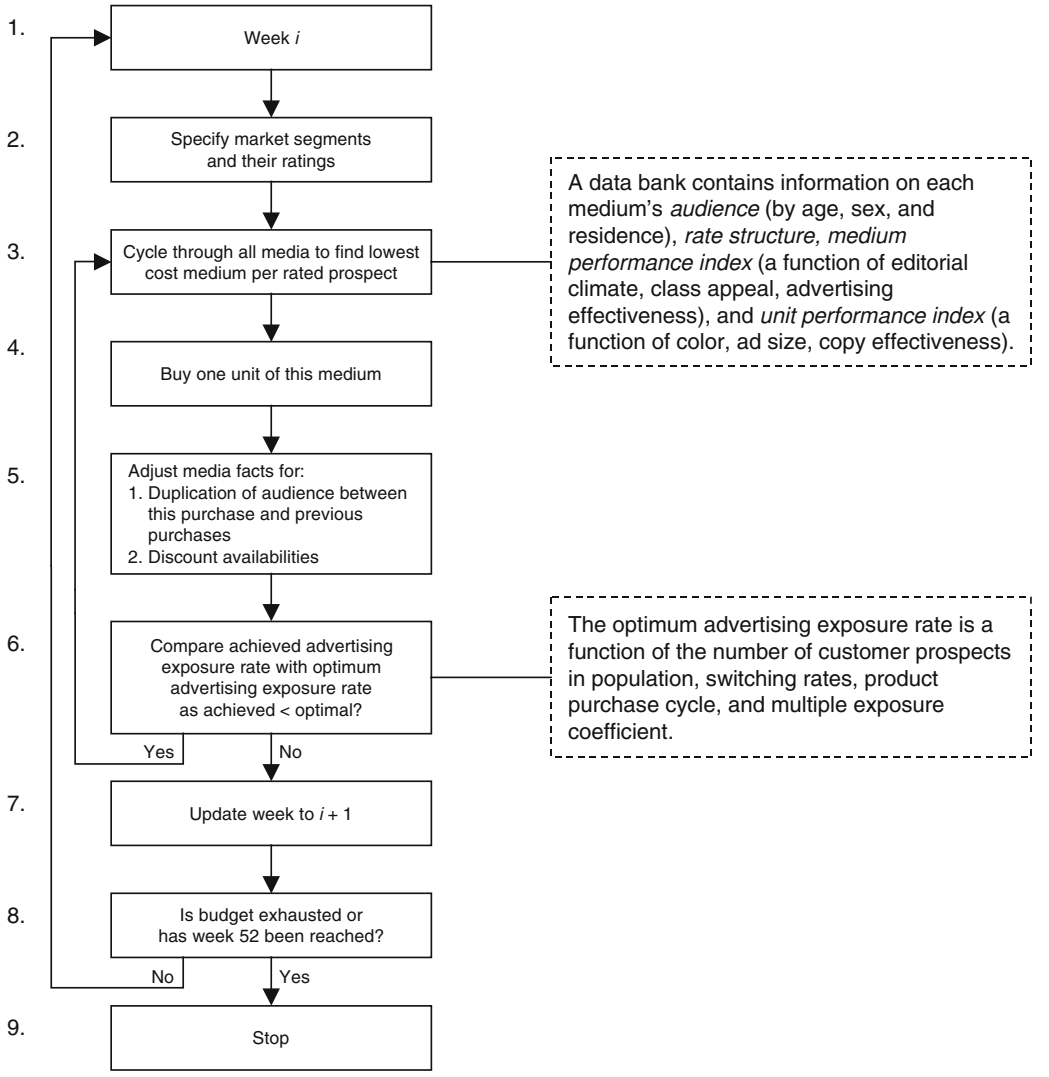
The high-assay model attempts to assist the advertising decision maker in scheduling media. The model incorporates the method of steepest ascent, which proposes that the user should purchase the insertion at each step that provides the greatest increase in effectiveness for the money expended. Statements 2 and 3 are typical of the statements in normative decision theories: "Specify market segments and their ratings" and "Cycle through all media to find lowest cost medium per rated prospect." Note the *prescriptive* nature of these statements. The basic form of these statements is: "Under circumstances $X_1, X_2, X_3, \ldots X_k$, one should do Y in order to achieve G." All normative decision theories must contain such prescriptive statements.

On the other hand, all positive theories must contain not prescriptive statements, but lawlike generalizations. This is the fundamental *structural* difference between normative and positive theory. Because theories that are purely normative contain no lawlike generalizations, such structures cannot explain phenomena. Recall that it is precisely the lawlike generalizations in positive theories that carry the brunt of the load of explaining and predicting phenomena.

A second difference between normative and positive theory is one of purpose. The purpose or objective of positive theory in marketing is to increase our understanding of marketing phenomena by providing systematized structures capable of explaining and predicting phenomena. Because of their ability to explain and predict phenomena, positive theories are often extremely useful aids to decision making. Indeed, a stronger assertion is probably warranted: good normative theory is based on good positive theory. Understanding how the world *works* enables one to develop theories and models about how to make the world work *better*. Nevertheless, aiding decision making remains a favorable consequence of using theories. The *objective* of theory is to explain, predict, and understand.

In contrast, consider the purpose of the high-assay normative decision theory. Its objective is to help the advertising decision maker to better schedule media. Such is the situation with all normative decision theories. As another example, Massy and Weitz (1977) have developed a normative theory of market segmentation. The purpose of their theory is to provide specific guidelines for allocating marketing expenditures among various market

Figure 8.3 **High-Assay Model**



Source: Kotler (1971, p. 457). Reprinted by permission of the author.

segments. *Indeed, the entire purpose of creating normative decision models in marketing is to assist marketers in making better decisions.*

The final distinction between positive theory and normative decision theory lies in their differing validation criteria. How does one go about determining whether the theories are valid? The validity of a positive theory is determined by (1) checking the internal logic and mathematics of the theory and then (2) exposing it to empirical tests in the real world. Roughly speaking, we develop testable hypotheses of the following kind: "If theory X is valid, then every time we see circumstances $C_1, C_2, C_3, \ldots C_k$ in the real world, we would

expect also to find phenomena $P_1, P_2, P_3, \ldots P_k$." Note that it is the existence of lawlike generalizations in positive theory that enables us to derive testable hypotheses.

Consider the appropriate validation procedures for normative decision theories. The internal logic and mathematics of such a theory can be checked and verified. Further, the *usefulness* of the theory can be evaluated. That is, compared with other relevant theories, to what extent are the necessary data available, how expensive is the theory to use, how much time does it take to use the theory, and does the theory use the most appropriate objective function?

Although the usefulness and logic of normative decision theories can be validated, such theories cannot be empirically tested in the real world in the same sense as positive theories. Because normative decision theories do not contain lawlike generalizations, they cannot explain and predict phenomena, and, therefore, they cannot generate empirically testable hypotheses. As MacDonald has observed:

> Let it be admitted that all or most human beings are intelligent or rational. And that what is known by reason is certainly true. But, also, what can be known by unaided reason is what *must* be true, and perhaps what *ought* to be [true] but never what *is* true of matter of fact. And statements which are logically certain are tautological or analytic and are neither verified nor falsified by what exists. *Statements about what ought to be are of a peculiar type which will be discussed later, but it is certain that they say nothing about what is.* (MacDonald 1968, p. 724, italics added)

It is precisely because normative decision theories are not empirically testable that many writers, including this one, prefer the label "normative decision *model*" instead of the label "normative decision theory."

8.3 DETERMINISTIC VERSUS STOCHASTIC THEORY

Unquestionably, much marketing theory uses tendency laws or tendency relationships. For example, "salespeople tend to be more satisfied with their jobs when they perceive that their immediate supervisor closely directs and monitors their activities" (Churchill, Ford, and Walker 1976, p. 331). It would seem that such relationships are inherently stochastic. Frank Bass was a prominent proponent of the view that *all* marketing behavior may well be *fundamentally* stochastic:

> Despite growing evidence that individual consumer choice behavior is characterized by substantial randomness, the underlying premise or rationale which guides most of the current research in individual consumer behavior is that, in principle, behavior is caused and can therefore be explained. Thus, works such as those by Howard and Sheth and Engel, Kollat, and Blackwell, however valuable they may have been in providing structure and framework for empirical research, may have misdirected research by implicitly overemphasizing deterministic models of behavior. (Bass 1974, p. 1)

Nevertheless, many authors have expressed discomfort at and/or criticism of the use of stochastic instead of deterministic models as bases for marketing theory. For example, Morrison states:

> These models have been and can continue to be useful devices for describing and sometimes predicting consumer behavior, but there are many inherent factors which greatly limit the ability of [stochastic] brand switching models to help develop a theory of consumer behavior. (Morrison 1978, p. 5)

Is marketing theory *inherently* stochastic? To evaluate this issue requires a precise specification of the characteristics of deterministic theory.[2]

8.3.1 The Nature of Deterministic Theory

Intuitive conceptions of determinism connote some type of certainty—that is, if *A,* then *B, always.* The discussion here will attempt to make this notion more precise. Because the determinism of almost any current marketing theory might be open to dispute, we look to the physical sciences for an instructive example. We will begin, then, by considering Newtonian mechanics as the prototypical example of deterministic theory.

The theory of classical mechanics derives from the three laws promulgated by Newton, which are of strictly universal form. Under these laws, the mechanical state of a particle is completely specified by its position and momentum. Position and momentum are, therefore, the state variables of the theory. For a system of point masses, the mechanical state of the system is given by the mechanical states of all the particles in the system. With this information concerning a system at an arbitrary point in time, we need only specify the force function applying to the system in order to completely, and uniquely, specify the mechanical state of the system at any future time. As Nagel (1961, pp. 277–335) points out in his argument against the claimed indeterminacy of quantum mechanics, *this ability to predict a unique state over time is the essence of the determinism of classical mechanics.*

It is important to note that classical mechanics is deterministic only with respect to the mechanical state of the particle system, that is, its state variables. It says nothing, for example, about the chemical and electromagnetic states, nor does it deal with changes caused by these unspecified factors. *Thus, it is not necessary that a deterministic theory completely specify all aspects of a system.*

Furthermore, in classical mechanics the positions and momenta of all particles are assumed to be known *precisely* at an instant in time. Obviously, in practice such perfect measurements can never be made. This imperfect knowledge of the initial state of the system causes mechanical equations to yield values for variables at a future time that are only approximate, that is, probabilistic. Again, uncertainty due to measurement does not damage the determinism of the theory.

In order to maintain that classical mechanics is a deterministic theory, it is necessary to treat it as an ideal state construct that is approximated in experiments. This is equivalent

to stating that, *ignoring experimental error, classical mechanics is deterministic because of the logical relationships between statements of the theory.* Formally stated, "a theory is deterministic if, and only if, given the values of its state variables for some initial period, the theory logically determines a unique set of values for those variables for any other period" (Nagel 1961, p. 292).

Nagel's (1961) conceptualization of determinism is quite inclusive. For example, there is no loss of uniqueness in allowing an infinite number of state variables. Furthermore, the state variables might be defined in terms of classes of individuals rather than individuals, for example, in terms of statistical parameters. If the state description of a theory is defined by the values of a set of statistical parameters, and if those parameters can be uniquely specified through the theoretical laws, given their values at some initial time, the theory would be deterministic.

8.3.2 Uncertainty in Explanation

As a prelude to analyzing the sources of uncertainty that lead to indeterminism in theory, we shall explore the sources of uncertainty in scientific explanations. The issues are the same in that scientific explanations contain exactly the same kinds of laws and lawlike generalizations as scientific theories. The analysis focuses on deductive-nomological (D-N), deductive-statistical (D-S), and inductive-statistical (I-S) explanations.

All three types of explanation suffer some kinds of uncertainty. However, this does not mean that all of the theories built on all three explanatory models are indeterministic. The definition of deterministic theory denotes uncertainty only in a specific sense. If the logical structure of the theory predicts two or more states of the system at a point in time based on the same initial conditions, this uncertainty with respect to prediction makes the theory indeterminate.

As noted earlier, one result of this restriction is that no form of measurement error affects the determinism of a theory. Such error does not affect the logic of the theory, only the ability of the scientist to test the predictions of the theory. The precision with which these predictions can be confirmed is a problem in determining the empirical adequacy of the theory, that is, its truth content. Whether a theory is empirically confirmed is a problem separate from that of logical analysis.

Because measurement error is the only form of uncertainty present in a deductive-nomological explanation, theories cast in this form must be deterministic. The explanandum is logically subsumed under the explanans, and the laws, universal in form, are assumed to be consistent. Therefore, the theory cannot predict two inconsistent versions of the explanandum.

In deductive-statistical explanations, the variables of the explanation are statistical parameters. The explanandum is logically subsumed and mathematically derived from the explanans. Given a fairly reasonable set of assumptions, a statistical parameter mathematically derived from another will be uniquely specified. Thus, with respect to statistical variables of state, any theory based on this type of explanatory model will be deterministic. Although the deductive form of the explanatory models in the deductive-

Table 8.3

Sources of Uncertainty in Scientific Explanation

Type of explanation	Uncertainty in measurement	Uncertainty in prediction	Uncertainty in logical relationship
Deductive-nomological	Yes	No	No
Deductive-statistical	Yes	Yes	No
Inductive-statistical	Yes	Yes	Yes

Source: Hunt (1991a). Reprinted by permission of the author.

nomological and deductive-statistical cases guarantees that any theory based on these models will be deterministic, the same reasoning does not apply to inductive-statistical explanations.

As noted earlier, inductive-statistical explanations confer some level of logical probability on the occurrence of the explanandum phenomenon. Conversely, they confer some likelihood of the nonoccurrence of the explanandum as well; that is, both the occurrence and the nonoccurrence of the explanandum are consistent with the truth of the explanans. In light of the definition given above, inductive-statistical explanations are not deterministic.

The preceding discussion is summarized in Table 8.3, which shows the three sources of uncertainty in the three kinds of explanations. The three sources of uncertainty result from (1) errors in measurement, (2) the logical relationships between statements in the explanans and the explanandum, and (3) the inability to predict the occurrence of individual phenomena. D-N explanations are deterministic because the only source of uncertainty arises from measurement error. D-S explanations have both measurement error and uncertainty resulting from their inability to predict the occurrence of individual phenomena. Yet, D-S explanations are deterministic because their explananda (a statistical parameter) are logically subsumed under the explanans. I-S explanations are indeterministic because they contain all three sources of uncertainty.

8.3.3 Determinism and Marketing Theory

The foregoing discussion of the nature of deterministic theory, the relationship of deterministic theory to explanatory models, and the uncertainty inherent in explanation can be used to assess the role of deterministic theory in marketing. Bass (1974) argued that the presence of a stochastic element in consumer behavior makes it impossible to construct deterministic marketing theory. His argument centers on the claim that the stochastic element inherent in the individual actions of consumers makes it impossible to construct laws of universal form. Bass notes that even if this assumption were incorrect, the number of variables affecting consumer behavior might be so large that it would be pragmatically impossible to build deductive-nomological explanations of buyer behavior. Granting the assumption, it is true that it would be impossible to construct a deterministic

theory predicting each consumer purchase, that is, deductive-nomological explanations of consumer behavior.

However, one could consider the long-run behavior of consumers toward a product or the behavior of a group of consumers toward a product and construct theories explaining brand loyalty, market share, or other statistical parameters or probabilities. Such theories, predicting particular values for the statistical variables of state, would be deterministic in that they would be based on a deductive-statistical model of explanation.

Thus, some type of deterministic theory is a legitimate goal of research in marketing. A critical question arising from this discussion is whether deterministic marketing theory is a desirable goal. And, if so, what implications does such a theoretical focus have for the conduct of theory construction and testing? In order to analyze this issue, it is necessary to return to the fundamental motivations for theory development in marketing.

Consider the buying behavior of a consumer. A theory of consumer behavior that gave rise to deductive-normological explanations of the consumer's purchasing would explain buying behavior completely. Regarding a consumer facing a particular buying decision, the theory would allow one to predict the decision outcome with probability 1.0. Failing this, the theory might predict the outcome of the decision with some high likelihood—for example, "The consumer in this situation will buy with probability .9." This is an inductive-statistical explanation. As noted earlier, most marketing theory to date has been of this type.

Alternatively, by changing the focus of our considerations from one to many decisions, for example, the buying decision of an aggregate of consumers in a particular situation, the theory might predict market share. Depending on the precision of the estimate, the theory could be a deterministic one. Many so-called stochastic models of consumer choice provide exact predictions of parameters such as market share. To the extent that these models have a systematic basis, they represent deterministic theories. Again, the restriction of the determinism might be relaxed and a theory of inductive-statistical form developed to provide a range of estimates with varying probabilities.

The essential notion embodied in the above examples is that deterministic theory is an end toward which marketing theory is moving in a natural fashion. The implication is that, from a structural perspective, both the "deterministic" and "stochastic" schools of research in marketing are developing theory whose ultimate end, if reached, is deterministic.

8.4 THE NATURE OF GENERAL THEORIES

Starting from the perspective that theories are systematically related sets of statements, including some lawlike generalizations, that are empirically testable, how do general theories differ from the ordinary kind? In short, what is it that makes a general theory *general?*[3]

There are several ways that one theory can be more general than another. First, recalling that the purpose of theories is to increase scientific understanding through structures that

can explain and predict phenomena, general theories can be more general by explaining and predicting *more* phenomena. (See sections 5.3 and 5.5.) That is, more general theories have a larger extension or domain than do less general theories. Dubin (1969, p. 41), for example, proposes that "the generality of a scientific model depends solely upon the size of the domain it represents." Zaltman, Pinson, and Angelmar (1973, p. 52) concur: "A second formal syntactical dimension of a [theoretical] proposition is its degree of generality. All propositions purport to refer to a particular segment of the world, their universe of discourse."

Second, theories can be more general by accommodating, integrating, or systematically relating a large number of concepts and lawlike generalizations from other theories. As Farber (1968, p. 173) suggests, in psychology "comprehensive theories, i.e., those serving to organize a considerable number of laws, depend on the state of knowledge in a given area." For example, perfect competition theory is more general than the theories of demand and supply because it integrates these more specific theories into a more general structure. Likewise, in neoclassical economics, general equilibrium theory takes the conditions for partial equilibrium (i.e., for equilibrium in each industry) and integrates them into a more general case.

A third and very important way that one theory can be more general than another is total theory *incorporation.* As used here, one theory may be said to incorporate another totally when the more general theory can satisfactorily explain the more limited theory's explanatory and predictive successes (Sellars 1963; Levy 1996). That is, it is not the case that the general theory adopts some or all of the concepts and assumptions of the less general theory. Rather, it is the case that the general theory explains why and under what circumstances the concepts and assumptions of the less-general theory explain and predict well.

The classic example of total incorporation, of course, is that Newtonian theory (which maintains that the acceleration of two masses increases as they approach each other) incorporates Galileo's law of descent (which assumes that acceleration is constant between two bodies), and it, thereby, explains all the predictive successes of Galileo's law. Simply put, if d is the distance of a body from the surface of the earth and D is the radius of the earth, Galileo's law predicts well for most falling objects because the ratio d/D is—as the general principle is argued for in economics by Friedman (1953)—"close enough" to zero that assuming g to be constant in Galileo's $S = 1/2\ gt^2$ is nonproblematic. Therefore, the foundations of Newtonian theory are such that they incorporate Galileo's law as a special case. Similarly, as discussed in section 14.1.2, resource-advantage theory is more general than perfect competition theory because the former totally incorporates the latter.

A fourth way that theories can be general is that their constructs may have a high level of abstraction. Blalock states:

> The general theory will be stated in highly abstract terms, with as few assumptions as possible as to the form of the equations, the values of the parameters, or (in the case of statistical theory) the specific distributions of the error terms. It will often be

> found that this very general theory cannot yield useful theorems, and so additional assumptions will be made in order to study important special cases . . . the principal value of a highly general theoretical formulation is that it enables one to place the various special cases in perspective and to prove general theorems appropriate to them all (Blalock 1969, p. 141).

Howard and Sheth were cognizant of the relationship between "level of abstraction" and "level of generalization." Thus, they indicate "first, the theory is said to be at a moderate level of abstraction, because it deals only with buying behavior, but nevertheless to be abstract enough to encompass consumer buying, institutional buying, distributive buying, and industrial buying" (1969, p. 391).

Unfortunately, the phrase "high level of abstraction" does not have perfect antecedent clarity, and at least three different meanings seem possible. First, a high level of abstraction may indicate "more encompassing." This seems to be the usage suggested by Howard and Sheth when they propose that their theory of buyer behavior encompasses not only consumer buying, but other forms of buying as well. This meaning of level of abstraction would make it consistent with the notion that a general theory encompasses and explains a large number of phenomena.

A second possible meaning of high level of abstraction might be that the terms in the theory are "far removed" from directly observable phenomena. Thus, empirical referents or measures of the "highly abstract" constructs may be difficult, if not impossible, to develop. Given the requirement that all theories must be empirically testable, there appears to be a significant danger in developing theories that are too abstract, in the sense of being far removed from observable reality. To address this second meaning of "too abstract," the original logical positivist position required all terms or constructs in a scientific theory to have direct empirical referents, that is, be directly observable. Recognizing that this position was untenable, the positivists' successors, the logical empiricists, required all abstract or "theoretical" terms to be linked to directly observable terms via devices known as "correspondence rules."

Current analysis in the philosophy of science suggests that even the logical empiricist position is too stringent. Keat and Urry (1975) propose that attention be focused on the testability of *statements* rather than the observability of all terms *in* statements. Thus, they propose the following: "A statement is scientific only if it is possible to make observations that would count in some way for or against its truth or falsity" (p. 38). This principle suggests, as discussed in section 7.5.2, that the constructs in a theory cannot be allowed to become so abstract (so far removed from reality) that they render the theory incapable of generating hypotheses that can be empirically tested, because such a theory would necessarily be explanatorily and predictively impotent.

In conclusion, one theory may be more general than another because it (1) explains and predicts more phenomena, (2) accommodates, integrates, or systematically relates a larger number of concepts and lawlike generalizations, (3) totally incorporates the less-general theory, and (4) has a higher level of abstraction.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Evaluate the classificational schema, discussed in Chapter 1, that used the three dichotomies of profit/nonprofit, micro/macro, and positive/normative to classify issues in marketing. Would the schema be a useful pedagogical tool? How well does it satisfy our criteria for classificational schemata?
2. Distinguish between normative and positive theory. Is price theory normative or positive? Is it appropriate for economists to use price theory to recommend that large firms be broken up?
3. How would you classify (i.e., as a theory, law, definition, etc.) the statement "Effort in marketing takes two primary forms—either sorting or transformation" (Alderson 1965, p. 49)? Why?
4. Differentiate between *normative* theory and *positive* theory. Must the development of positive theory necessarily precede the development of normative theory? Do all normative theories rest upon an essentially positive base?
5. Someone has said, "We should always carefully distinguish between developing criteria for evaluating what constitutes a theory and developing criteria for evaluating what constitutes a good theory." Are these really two different notions? Could any single criterion be used for both purposes?
6. Find examples of two theories in marketing that are alike in that they address at least one phenomenon in common. Which theory is more general?
7. Would all theories that explain and predict more phenomena necessarily be better than theories that explain and predict fewer phenomena?
8. Wells (1993) is highly critical of certain kinds of consumer research. In particular, the "[s]earch for abstract, universal Theories has not increased our understanding of common behavior" (p. 500). He argues:

   > So here we have four counts against Theory-oriented consumer research. Ego-involved Theorists persevere indefinitely in the face of Theory-disconfirming results. Theory "tests" are so imperfect that they can always be written off. When consumer behavior Theories are "tested," they do not get better or even change. And, Theory-oriented consumer researchers neither replicate their findings nor systematically investigate the range and limits of their work. In the absence of this essential segment of the research process, the "body of knowledge" cannot be expected to explain or predict real-world events. (Wells 1993, p. 497)

   And he recommends:

   > An alternative would be to start small and stay real—to forsake pursuit of high-level, abstract representations and seek ground-level generalizations that actually work. These generalizations might be called "theories" with a small *t* (Olson 1982); but they would always refer to real events. (pp. 497–98)

   Evaluate (1) the problem that Wells identifies and (2) his proposed solution.

9. Consider the interests of firms (i.e., what is good and bad for them) and the interests of society (i.e., what is good and bad for *them*). Is deterministic marketing theory a *desirable* goal? An undesirable goal? Why?

NOTES

1. Polli and Cook (1969) discuss this issue.
2. Parts of the following analysis are drawn from Nakamoto and Hunt (1980).
3. Parts of the following analysis are drawn from Hunt (1983a).

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PART 3

CONTROVERSY IN MARKETING THEORY

This section is titled *Controversy in Marketing Theory.* Its subject is the controversies, often referred to collectively as the "philosophy debates," that arose in the early 1980s and continue in many different guises today (e.g., controversies in marketing involving relativism, postmodernism, interpretivism, humanism, and feminism) (Easton 2002). Examples of specific controversies in the philosophy debates include: (1) Does science (and, therefore, *marketing* science) differ from nonscience in any fundamental way (or ways)? (2) Does "positivism" (i.e., logical positivism and logical empiricism) dominate marketing research? (3) Does positivism imply quantitative methods? (4) Would positivist research be causality-seeking, adopt the machine metaphor, adopt realism, be deterministic, reify unobservables, and adopt functionalism? (5) What is philosophical relativism? (6) Is relativism an appropriate foundation for marketing research? (7) Does relativism imply pluralism, tolerance, and openness? (8) Should qualitative methods (e.g., naturalistic inquiry, humanistic inquiry, ethnographic methods, historical method, critical theory, literary explication, interpretivism, feminism, and postmodernism) be more prominent in marketing research? (9) Do qualitative methods imply relativism? (10) What is the philosophy known as "scientific realism"? (11) Is scientific realism an appropriate foundation for marketing research? (12) Are true theories, as emphasized by realism, an appropriate goal for marketing research? (13) Is objective research in marketing possible? (14) Should marketing pursue the goal of objective research?[1]

The controversies addressed in this section that prompt its title are those that have been prominent and contentious in marketing's philosophy debates. This section argues (1) for a reasoned approach to marketing's controversies, (2) for realism as a foundation for marketing theory and research, and (3) for truth and objectivity as goals for marketing to pursue and regulative ideals for marketing to maintain. These "fors," in turn, stem from the foundations of the philosophy underlying this monograph, in general, and our method of analysis in this section, in particular. These foundations are seven (Hunt 2001b).

First, the monograph's philosophical foundations hold that Plato's "critical discussion" (see Hunt 2003a, section 2.2.2) is essential for knowledge development. In this view, the pursuit of truth is furthered by proposing penetrating, highly critical questions, which are to be followed by equally insightful, thoughtful answers. Second, civility in critical discussion is a virtue. For example, the use of *ad hominem* is proscribed: discussion should always be directed at the ideas of people, not the people themselves. Third, the use of sophistry is prohibited: it is impermissible to employ disingenuous argumentation, even when such arguments might be persuasive to one's audience. Fourth, one should respect reason and evidence: the fallibility of all research methods implies that one must be open to all alternative views that provide

well-reasoned arguments and evidence. Fifth, clarity in scholarship is a virtue: to be obscure should never be confused with being profound. Sixth, of all the "isms" in the philosophy of science, scientific realism seems to make the most sense for marketing: no other philosophy is coherent (without being dogmatic), is critical (without being nihilistic), is open (without being anarchistic), and—at the same time—can account for the success of science.

Seventh, and finally, the philosophy underlying this work emphasizes critical pluralism. "Critical pluralism" is a label originally suggested by Siegel (1988) to characterize how scholars should view their own and others' theories and methods. The "pluralism" part reminds us that dogmatism is antithetical to science; we should adopt a tolerant, open posture toward new theories and methods. The "critical" half stresses that nonevaluational, nonjudgmental, or noncritical pluralism (which would view the supposed encapsulation of rival theories and alternative methods as thwarting comparison and evaluation) is just as bad as dogmatism. All methods, theories, and their respective knowledge claims can (and must) be subjected to critical scrutiny; nothing is exempt. Critical pluralism, therefore, implies that the philosophy underlying the monograph is open-minded, but not empty-headed.

Chapter 9 begins our analyses of the controversies in marketing's philosophy debates and focuses on the question: which philosophical foundation is most appropriate for grounding marketing research? The chapter shows why relativism was rejected in the philosophy of science in the 1970s and argues that scientific realism merits marketing's consideration as a foundation for theory and research.

Chapter 10 addresses two questions. First, do sciences differ from nonsciences in any fundamental ways? Second, should researchers adopt qualitative methods because "positivism is dead"? As to the first question, the chapter argues in the affirmative. As to the second question, the chapter concludes that the "positivism is dead" argument, as a basis for proposing qualitative methods, is fatally flawed.

Chapter 11 examines the "truth controversy" in marketing's philosophy debates. The chapter begins by developing a scientific realist model of truth (Figure 11.1, p. 290) that focuses on the successes and failures of empirical tests. It then shows that the arguments used by relativists and others against truth are uninformed by reason, before arguing for truth on the basis of marketing's adopting a philosophy that recognizes and stresses the importance of marketing researchers' generating trustworthy knowledge.

Chapter 12 analyzes the "objectivity controversy" in marketing's philosophy debates, that is, the question: can marketing research be objective? The chapter evaluates and finds faulty the arguments that have led so many relativists, social constructionists, postmodernists, feminists, historicists, subjectivists, and humanists to deny the possibility of objectivity in marketing research. It then puts forth the "positive case" for pursuing objectivity, which, like the case for truth, focuses on trust. The chapter concludes by developing a scientific realist model of empirical testing and using the model to analyze issues related to the objectivity controversy in marketing. The controversies in marketing theory await. Let's get started.

NOTE

1. For brief introductions to the history of the philosophy debates in marketing, see Hunt (2001b) and Easton (2002).

9

ON SCIENTIFIC REALISM AND MARKETING RESEARCH

> The pluralist does not hold, with the relativist, that there is no possibility of non-neutral or non-question-begging evaluation of alternative claims, theories, schemes, versions, or the like. The pluralist subscribes, rather, to a willingness to tolerate and utilize a diversity of ideas and approaches, while at the same time acknowledging criteria which afford the possibility of objective comparison and evaluation of the diverse alternatives tolerated and utilized.
>
> *—Harvey Siegel*

This chapter is the first of four that analyze controversies that have been prominent in the philosophy debates in marketing. The controversy that will be analyzed here can be stated as a question: which philosophical foundation is most appropriate for grounding marketing research? Note that the question assumes—correctly, most writers agree—that all research projects have philosophical foundations. That is, when scholars engage in a research project, there are always underlying assumptions as to what entities exist (i.e., ontological assumptions), what research designs are appropriate for generating new knowledge (i.e., methodological assumptions), and what criteria are appropriate for evaluating knowledge-claims (i.e., epistemological assumptions). Therefore, though the philosophical foundations of a research may be explicit or implicit, they cannot be avoided. Options are available; choices should be made for sound reasons.

As to available options, philosophy of science researchers have long focused on the issue of what is the most appropriate philosophy for providing a foundation for science. After the acknowledgment of the deficiencies of positivistic philosophy of science in the 1960s and the subsequent repudiation of relativism (of both the Kuhnian and Feyerabendian varieties) in the early 1970s, philosophers of science in the latter part of the 1970s turned sharply toward a *realist* orientation: "Contemporary work in philosophy of science increasingly subscribes to the position that it is a central aim of science to come to knowledge of how the world *really* is, that correspondence between theories and reality is a central aim of science" (Suppe, 1977a, p. 649). Nevertheless, as Leplin (1984, p. 1) observes, "scientific realism is a majority position whose advocates are so divided as to appear a minority."

The purpose of this chapter is to review the philosophy of scientific realism and discuss it as an appropriate foundation for marketing research. Specifically, because relativism has been argued in marketing to be a viable foundation for marketing research, this chapter first reviews several reasons why philosophers of science rejected relativism as a foundation for science in the 1970s. Then, the chapter (1) examines the historical development of realism, (2) explicates the four fundamental tenets of modern, scientific realism, (3) discusses the implications of scientific realism for physics, biology, and marketing and the social sciences, (4) discusses how scientific realism can explain the success of science, (5) details the scientific realist approach to understanding how science progresses, (6) contrasts scientific realism with logical empiricism, (7) contrasts scientific realism with constructive empiricism, and (8) evaluates two versions of critical realism.

9.1 WHY RELATIVISM WAS REJECTED

To understand why philosophy of science rejected relativism in the 1970s, one needs to know what relativism is. All forms of philosophical relativism embrace two theses: (1) the relativity thesis that something is relative to something else, and (2) the nonevaluation thesis that there are no objective standards for evaluating *across* the various kinds of "something else" (Siegel 1987, 1988).[1] With the preceding in mind, we can define several forms of relativism:

1. *Cultural relativism* holds that (a) the elements embodied in a culture are relative to the norms of that culture, and (b) there are no objective, impartial, or nonarbitrary criteria to evaluate cultural elements across different cultures.
2. *Ethical relativism* holds that (a) what is ethical can only be evaluated relative to some moral code held by an individual, group, society, or culture, and (b) there are no objective, impartial, or nonarbitrary standards for evaluating different moral codes across individuals, groups, societies, or cultures.
3. *Conceptual framework relativism* holds that (a) knowledge-claims are relative to conceptual frameworks (theories, paradigms, worldviews, or *Weltanschauungen*), and (b) knowledge-claims cannot be evaluated objectively, impartially, or nonarbitrarily across competing conceptual frameworks.
4. *Reality relativism* (a view often associated with constructionism) holds that (a) what comes to be known as "reality" in science is constructed by individuals relative to their language (or group, social class, theory, paradigm, culture, worldview, or *Weltanschauung*) and (b) what comes to count as "reality" cannot be evaluated objectively, impartially, or nonarbitrarily across different languages (or groups, etc.).
5. *Subjectivism,* an "ism" customarily paired with relativism, holds that there is something basic to the human condition—usually something about human perception and/or language—that categorically prevents objective knowledge about the world.

To understand why relativism and subjectivism are minority views within the philosophy of science, consider how these "isms" would respond to the following six questions: "Does the sun revolve around the earth, or does the earth revolve around the sun?" Conceptual framework relativism, for example, Kuhn's (1962) relativism, implies the following answer: "First I must know whether you subscribe to the paradigm of Copernicus or Ptolemy, for these paradigms—like all paradigms—are incommensurable and, therefore, there is no *truth* to the matter independent of the paradigm you hold." And subjectivism implies the following answer: "Because scientists see what their theories and paradigms tell them is there, the theory-ladenness of observation tells us that an *objective* answer to your query is impossible."

Question two: "Was Great Britain morally *right* in leading the drive in the nineteenth century to abolish slavery in cultures throughout the world?" Relativism responds: "Since slavery is a cultural element that cannot be evaluated independently of the norms of the culture within which it exists, no judgment on this matter can be made—to apply one's own norms elsewhere is simply cultural ethnocentrism." Question three: "Should Great Britain work toward the abolition of slavery in the few remaining states in which slavery continues to exist?" Answer: "See response to previous question." Question four: "Did the Holocaust occur?" Answer: "Since the Holocaust is a 'constructed' reality (Lincoln and Guba 1985, p. 84), just one of many 'multiple realities,' the Holocaust's occurrence or nonoccurrence cannot be objectively appraised independent of the worldview of a particular social grouping or culture."

Question five: "Is a culture that is tolerant of individuals from other cultures preferable to a culture that oppresses everyone outside the dominant culture?" Answer: "Although the predisposition toward tolerance is a cultural element that varies widely across different cultures, no judgment can be made across cultures as to the moral superiority of tolerant versus intolerant cultures." Question six: "Should an academic discipline be open to the views of those outside the discipline?" Answer: "Although it is true that different academic disciplines differ in their relative openness to the views of outsiders, no judgment can be made across disciplines as to the relative desirability of such openness."

It should be easy now to understand why relativism and subjectivism were rejected in the 1970s and are now minority views in the philosophy of science. Relativism does not imply a constructively critical stance toward knowledge-claims, nor does it simply imply acknowledging that the knowledge-claims of science are fallible. Relativism implies nihilism—the belief that we can never have genuine knowledge about anything. Relativists, incoherently, claim to *know* that no one else can ever know anything. (If it is true that *all* knowledge is impossible, how can one *know* that "all knowledge is impossible"?) Furthermore, relativism does not imply a tolerant stance toward outside ideas and other cultures; it implies *indifference* to the norm of tolerance. Moreover, relativism does not imply ethical sensitivity; it implies ethical impotence: one cannot make moral evaluations. Finally, subjectivism does not caution science to work at minimizing bias; it maintains that the human condition makes the very idea of objectivity a chimera. Therefore—like truth—objectivity should be abandoned.

For the preceding reasons, among others, philosophy of science rejected relativism as

a foundation for science in the 1970s. Marketing should, too. A major benefit of scientific realism, the philosophy discussed here, is that, contrasted with many other philosophies, it produces intelligible, coherent discourse about science. With the exceptions of Bagozzi (1980, 1984), Blair and Zinkhan (1984), Easton (1998, 2002), and my own works (Hunt 1989b, 1990b, 1991a, 1991b, 1992a, 1992b, 1993, 1994a, 1994b, 1994c, 1995a, 1995b, 2001b, 2003a, 2005), authors have given little attention to realist philosophy in marketing's philosophy debates. This omission is unfortunate, because not only do "the majority of philosophers of science profess to be scientific realists" (Causey 1979, p. 192), but much, if not most, marketing research seems implicitly to assume a realist perspective. The next section examines the historical development of realism, the philosophy that this chapter puts forward as appropriate for grounding marketing research.

9.2 HISTORICAL DEVELOPMENT OF REALISM

From the very beginnings of the scientific revolution in the sixteenth century, science and philosophy were closely related. Indeed, prior to the nineteenth century, science was a branch of philosophy, and scientists were referred to as "natural philosophers." However, this situation changed in the latter half of the nineteenth century when philosophy came to be dominated by Hegel (1770–1883) and his idealism: "He ruled the philosophical world as indisputably as Goethe the world of literature, and Beethoven the realm of music" (Durant 1954). Hegel's idealism was hostile to mathematics and unsympathetic to science. Its central tenet was that the external world does not exist unperceived: "All reality is [for idealism] mental (spiritual, psychical). Matter the physical, does not exist" (Angeles 1981, p. 120). Thus, Hegel's "identity of reason and reality" denied the existence of tangible objects (e.g., rocks and trees) and proclaimed only *reason* to be *real.*

Hegelian idealism's dominance in philosophy began to crack at the turn of the century from the efforts of G.E. Moore (1873–1958) and Bertrand Russell (1872–1970), who offered three major arguments against idealism: First, idealism confuses the act of perception with the object being perceived. Once the object of a mental act is distinguished from the awareness of it, there is no reason to deny the existence of the object independently from its being perceived. Second, idealism uses the concept *real* in ways that violate principles of intelligible discourse. That is, the meaning of the term "real" derives from such exemplars as "this table exists." Denying the fundamental examples that give meaning to a term, while at the same time continuing its use in other contexts, produces unintelligible speech. Third, idealism constitutes sophistry, for the behaviors of idealists are inconsistent with their stated beliefs. Although they claim that objects such as chairs do not exist, when entering rooms, idealists approach and sit on chairs, just as if they believe such chairs *do* exist. The philosophy that Moore and Russell argued for was, in today's terminology, "classical" or "commonsense" realism, whose central tenet is that the external world of tangible objects exists independently of perception. (See Moore [1903/1992] and Russell [1929] for further details on their arguments against idealism.)

The second crack in idealism's philosophical hegemony developed from a discussion group at the University of Vienna that was formed in 1907 by the mathematician

Hans Hahn, the physicist Philipp Frank, and the social scientist Otto Neurath. By the 1920s, the "Vienna Circle" group had added other physicists, including Moritz Schlick (1882–1936), who had studied under Max Planck and who had already received acclaim for his interpretations of Einsteinian relativity. Under Schlick's leadership, the Vienna Circle sought a philosophy that would (1) heal the rift between science and philosophy and (2) provide a means for interpreting quantum mechanics. The philosophy they developed, "logical positivism," was not opposed to the common-sense realism of Moore and Russell. Indeed, the positivists were allies with the realists in their philosophical battles with advocates of Hegelian idealism. Schlick's (1932) classic article on the foundations of logical positivism framed the idealism-realism question as: "If the phrase 'external world' is taken with the signification it has in everyday life, . . . [then] are there, in addition to memories, desires, and ideas, also stars, clouds, plants, animals, and my own body?" He answered: "It would be simply absurd to answer this question in the negative" (p. 101). Therefore, "logical positivism and realism are not in opposition; whoever acknowledges our fundamental principle must be an empirical realist" (p. 107).

But if the logical positivists had no problems with according reality status to tangible, observable entities, they strongly questioned giving such status to any "transcendent world" that allegedly "stood behind" the observable world, but about which nothing could be verified by observational means. Because the positivists' "verifiability principle" equated the meaningfulness of a proposition with the possibility of its verification, for Schlick (1932, p. 107), "The denial of the existence of a transcendent external world would be just as much a metaphysical statement as its affirmation. Hence, the consistent empiricist does not deny the transcendent world, but shows that both its denial and affirmation are meaningless." A major reason the positivists questioned the meaningfulness of any proposition in which transcendent or unobservable concepts are included is that they believed that this was the best interpretation of quantum mechanics. Understanding how they came to this conclusion requires at least some understanding of the world implied by quantum mechanics—a world that is anything but commonsensical.

9.2.1 Quantum Mechanics, Realism, and Positivism

The development of quantum mechanics began with attempts to solve the "black body" problem at the turn of the century.[2] A black body is one that perfectly absorbs and then reemits all radiation falling upon it. In the smoothly continuous world of classical physics, the radiation emitted from a black body would also be perfectly continuous. Max Planck, however, proposed in 1900 that the radiant energy emitted takes place *only* in the form of discrete packets, which he called energy *quanta.* Electromagnetic radiation, he proposed, is made up of a whole number of packets of energy, with each packet having the energy hv, where h is Planck's constant and v is the frequency of oscillation. Einstein used Planck's idea of energy quanta in 1905 to discredit the (then firmly established) view that light is fundamentally wavelike. He theorized that construing light as being made up of individual particles or *photons* would explain how electrons are emitted from metals

by an incident beam of light. Thus was born what has become known as the wave-particle duality: light is *simultaneously* both wavelike and particle-like.

In 1911, Ernest Rutherford developed his solar system model of the atom, in which negatively charged electrons orbit a positively charged, nuclear "sun." His model, however, had a major problem: if electrons could occupy any of the infinite number of possible orbits, they would spiral ever closer to the nucleus, and the atom would be unstable. A young Dane, Niels Bohr, solved this problem by applying quantum theory. He theorized that electrons could occupy only *discrete* orbits around the nucleus, and he used Planck's constant to identify the specific orbits that would be possible. In 1923, Lewis de Broglie proposed that *all* subatomic particles, not just photons, are actually "wave-particles" and developed equations that connected the energy and momentum of any such particle with the frequency of its associated wave. Erwin Schrödinger then used de Broglie's ideas in 1926 as a basis for accommodating the wave-particle duality through his justly celebrated wavefunction equation. In 1927, Heisenberg proposed his indeterminacy principle: the experimental act of investigating the position (momentum) of a subatomic particle *necessarily* destroys the possibility of measuring its momentum (position) to arbitrary accuracy. At the limit, if one knows precisely where any subatomic particle is, one has absolutely no idea what it is doing. Dirac then used wave mechanics in 1928 to develop quantum field theory. If interrogated in a particle-like way, the formalism of quantum field theory gives probability predictions of particle behavior; but if interrogated in a wavelike way, the theory gives probability predictions of wavelike behavior.

Since the late 1920s, predictions of quantum mechanics have been confirmed in thousands of experiments. Given its radical break with classical mechanics, its interpretation prompted a great debate between Einstein, who argued for a realist interpretation, and Bohr, who, influenced by the Vienna Circle, argued for a positivist view. Bohr and his positivist allies developed an interpretation of quantum mechanics that is now referred to as the "Copenhagen interpretation," which is often used interchangeably with "instrumentalist interpretation" and "positivist interpretation." Its basic premise is that what we can know about the quantum world is only the effects we can observe *after* an intervention. As Bohr put it, "The entire formalism is to be regarded as a tool for deriving predictions . . . under experimental conditions" (Bohr, quoted in Polkinghorne [1984, p. 79]). That is, the uncertainty described in Heisenberg's principle does not reflect science's ignorance of the laws of nature—uncertainty *is* a law of nature. Prior to an act of measurement (observation), it is meaningless speculation even to talk about where a subatomic particle *really* is, or its momentum, or the direction of its spin. All particles exist in a *superposition* of potential states.

Einstein and his realist allies attacked the Copenhagen view with appeals to (1) rhetoric (e.g., Einstein's famous claim that God does not "play dice" with the universe), (2) "hidden variable" theories that posited entities standing behind the wave-particle duality (e.g., David Bohm's hypothesized "pilot wave"), and (3) numerous "thought experiments." Of the thought experiments that attempted to undermine the view that uncertainty is a law of nature, Einstein's most famous one, with Boris Podolsky and Nathan Rosen (hence the "EPR" experiment), argued that quantum mechanics implied, at times, that the in-

formation that a particle is being investigated would be transmitted *instantaneously* to a second particle. Since speeds in excess of the speed of light are impossible, argued EPR, quantum mechanics violates "local reality" and must be deficient.

Bohr responded to Einstein's rhetoric on "playing dice" with the gentle chide that it is not for scientists to prescribe to God how he should run the world. As to the various hidden variable theories, Bohr and his positivist allies argued that such theories were *ad hoc* and, in any case, the hidden variables (e.g., Bohm's "pilot wave") seemed even more bizarre than the Copenhagen view. The Hungarian mathematician John von Neumann then joined the argument and argued that any hidden variable theory was bound to disagree with some of the verified empirical results of quantum mechanics' experiments. After Bohr and his colleagues had rebutted Einstein's thought experiments, John Bell in the 1960s developed some experimentally testable consequences of the EPR thought experiment. Since then, the results of experiments have tended to favor the Copenhagen interpretation: Einsteinian local reality seems incorrect. Indeed, the positivist, Copenhagen view, as unsettling as its nonrealistic interpretation is to many, continues to be prominent among physicists.

9.3 SCIENTIFIC REALISM: FOUR FUNDAMENTAL TENETS

Realism suffered a heavy blow in the quantum mechanics debate. However, beginning in the 1960s, the "received view" that *all* theories (and not just quantum mechanics) should be interpreted according to the dictates of the positivism of the Vienna Circle began steadily losing ground to the realism now generally referred to as "scientific realism" (Suppe 1977b). Scientific realism is associated with such philosophers as Boyd (1984), Levin (1984), Magnus and Callender (2004), Manicas (1987), Maxwell (1962), McMullin (1984), Mikkelson (2006), Psillos (1999, 2001), Putnam (1962, 1990), Sellars (1963), and Siegel (1983, 1987). A major problem for realism is that there are so many different versions of it: "Scientific realism is a majority position whose advocates are so divided as to appear a minority" (Leplin 1984, p. 1). That is, there is no "grand theory" of science according to realism. Rather, there is the transcendental realism of Bhaskar (1979, 1986, 1993, 1998), the ontic realism of MacKinnon (1979), the methodological realism of Leplin (1984, 1986, 1997), the critical realism of Sayer (1992), the evolutionary, naturalistic realism of Hooker (1985), the referential realism of Harré (1986), the critical scientific realism of Niiniluoto (1999), and the constructive realism of Giere (1985). The approach in this section is not to advocate any particular version of scientific realism, but to examine the fundamental, unifying beliefs underlying all versions of it. These fundamental tenets we shall refer to as classical realism, fallibilistic realism, critical realism, and inductive realism.

First, *classical* realism is the common-sense realism of Moore and Russell, which holds that the world exists independently of its being perceived. For example, the "external realism" advocated by Searle (1995, p. 150) maintains that "the world (or alternatively, reality or the universe) exists independently of our representations of it." An interesting argument for classical realism is that of Thagard (2007, pp. 29–30). He points out

that, because the best "scientific evidence strongly suggests that the universe is over 10 billion years old, but that representations constructed by humans have existed for less than a million, . . . we can infer that there was a world existing independent of human representation for billions of years . . . [and] truth is not a purely mental matter." Thus, classical realism contrasts with idealism and deconstructive, postmodernist relativism, which hold that all reality is "in here" (the mind) and, therefore, all reality is relative to the mind that knows it. That is, contra Olson's (1981, 1987) relativism, there really is something "out there" for science to theorize about. To hold otherwise is to make all of science a sham. To hold that science does not "touch base" with some reality separate from its own theories is to make inexplicable the enormous success of science over the past 400 years (Stove 1982).

Although scientific realism assumes classical or commonsense realism, it does not embrace "naive" or "direct" realism. These forms of realism hold that our perceptual processes result in a direct awareness of, or a straightforward confrontation with, objects in the external world. Thus, direct realism maintains that our perceptual processes always result in a veridical representation of external objects, which, in turn, results in knowledge about external objects that is known with certainty (Hooker 1985). Clearly, such a realism would warrant the pejorative adjective "naive." Advocates of scientific realism, though agreeing that our perceptual processes can yield genuine knowledge about an external world, emphatically reject direct realism. Instead, believing that some of our perceptions may be illusions or even hallucinations, they argue that some of our perceptions may be true and others false or, alternatively, some of our perceptions are "more accurate" or "closer to the truth" than others. Hence scientific realism is a middle-ground position between direct realism and relativism. (Recall that reality relativism and constructionism—see section 9.1—hold that each perception constitutes one of many "multiple realities" and, because alternative realities cannot be objectively, impartially, nonarbitrarily evaluated, all perceptions (realities) must be—by logic—equally valid.)

Second, scientific realism argues for *fallibilistic* realism, which maintains that, though the job of science is to develop genuine knowledge about the world, such knowledge will never be known with certainty. The concept of "know with certainty" belongs in theology, not science. For scientific realism, there is no "God's-eye view," nor does science need one to fulfill its goal of being a truth-seeking enterprise. As Siegel (1983, p. 82) puts it, "To claim that a scientific proposition is true is not to claim that it is certain; rather, it is to claim that the world is as the proposition says it is." As Hooker (1985) points out, a consequence of fallibilistic realism is that scientific realism rejects the *direct* realism discussed in the previous paragraph. Scientific realism rejects (and should reject) direct realism; it accepts (and should accept) fallibilism.

Third, scientific realism adopts *critical* realism, which (1) recognizes the fallibility of scientists' perceptual (measurement) processes involved in the testing for the truth-content of knowledge-claims and (2) contends that the job of science is to use its method to improve our perceptual (i.e., measurement) processes, separate illusion from reality, and thereby generate the most accurate possible description and understanding of the world. For scientific realism, all of science's knowledge-claims are provisional, subject

to revision on the basis of further evidence. Critical realism stresses the importance of the continuing efforts of science to develop ever better (1) measures of constructs, (2) research procedures for empirical testing, and (3) epistemological norms for developing scientific knowledge. For example, the practice of developing multiple measures of constructs and testing them in multiple contexts in social science stems from this critical orientation (Cook and Campbell 1986). As a second example, the critical orientation of science and science *education* serves as a foundation for arguing that the development of students' critical thinking should serve as a regulative ideal for *all* of education (Siegel 1989, 1997).

Fourth, scientific realism adopts *inductive* realism, which maintains that "the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, p. 26). Because the logical positivists believed that science's implicit acceptance of inductive realism in the nineteenth century had wrongly encouraged it to believe in the absolute truth of Newtonian mechanics, they rejected inductive realism and accepted Humean skepticism's position with respect to unobservable constructs (McMullin 1984; Stove 1982; Suppe 1977b). Scientific realism, in contrast, maintains that Humean skepticism, which "denies that one can progress by logical reasoning from perceptual experience to any genuine knowledge of an external world" (Watkins 1984, p. 3), is wrong-headed. For scientific realism, the positivists were "throwing out the baby with the bathwater." Therefore, for inductive realism, and contrary to logical positivism and logical empiricism, concepts that are *unobservable* are appropriate in theories that purport to explain observable phenomena. Similarly, and contrary to Popperian falsificationism, the positive results of empirical tests—not just falsifications—provide evidence as to the truth content of the theories tested.

In short, scientific realism proposes that (1) the world exists independently of its being perceived (classical realism), (2) the job of science is to develop genuine knowledge about that world, even though such knowledge will never be known with certainty (fallibilistic realism), (3) all knowledge-claims must be critically evaluated and tested to determine the extent to which they do, or do not, truly represent or correspond to that world (critical realism), and (4) the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists (inductive realism).

9.4 IMPLICATIONS OF SCIENTIFIC REALISM

Scientific realism has implications for all branches of science. Here, I sketch some implications for physics, biology, and marketing and social science.

9.4.1 Physics

Consider the case of Newtonian mechanics. Is not Newtonian mechanics false? On the contrary, scientific realism contends that the 300-year story of Newtonian mechanics gives

us reason to believe that something like the entities of Newtonian mechanics actually exists (i.e., apples, trees, planets, and stars actually exist). Equally important, scientific realism contends that the successes of Newtonian mechanics give us reason to believe that something like the structure of relationships, or "forces," postulated by Newtonian mechanics exists. That is, we are warranted in believing that Newtonian mechanics, within its validity limits, gives us significant truth about the world (Rohrlich and Hardin 1983). Scientific realism, therefore, joins theory acceptance and (approximate) truth: "To rationally accept a theory as a basis for action is to accept it as telling us something or other about the world, and that is to accept the theory as being more or less true" (Newton-Smith 1981, p. 287). Therefore, for example, it was rational for NASA to accept and rely on the approximate truth of Newtonian mechanics to put astronauts on the Moon.

How about quantum mechanics? The general acceptance of quantum mechanics in the early part of this century (as discussed in section 9.2.1) was the precipitative cause of the rejection of naive realism with respect to science, and rightly so. The view that our perceptual processes always give a veridical representation of the world and that current science is known with certainty *ought* to be rejected. However, in part as a reaction to the excesses of naive realism, many philosophies of science then went to the opposite extremes of relativism, subjectivism, constructionism, irrationalism, and nihilism. None of the extremes is satisfactory.

There are many interpretations of quantum mechanics. Indeed, "the Copenhagen interpretation, which informs the textbook presentation of quantum mechanics, . . . has as many versions as it has adherents" (Gomatam 2007, pp. 736–37). Nonetheless, the customary version of the Copenhagen interpretation suggests that quantum mechanics should be interpreted in a positivist, instrumental manner (Polkinghorne 1984). In this view, quantum mechanics is "just" a series of equations, albeit a series of equations that has been extraordinarily successful in predicting subatomic phenomena. Realism is often attacked for ostensibly being committed to finding "hidden variables" that will turn quantum mechanics from an indeterministic set of equations to a deterministic process (McMullin 1984). However, the scientific realism discussed here is not committed to the position that all theories must contain "entities" or "hidden variables" that will turn all indeterministic theories into deterministic ones. Rather, the inductive realism tenet of scientific realism posits that the success of those theories that contain entities gives us reason to believe that something like the entities contained in the theories actually exists. Therefore, with respect to quantum mechanics, if the best interpretation of quantum mechanics is that it posits no "hidden variables," or "entities," so be it; no damage occurs to scientific realism. However, scientific realism is also relevant to quantum mechanics because of the inductive realism tenet. That is, the long-run predictive success of quantum mechanics gives reason to believe that it truly "says something" about the world. In short, quantum mechanics has truth-content and can be relied on (Dickson 1995).

The "Lessons" of Quantum Mechanics

Many of the major participants in the philosophy of science debate about the appropriate foundations for understanding science have had strong backgrounds in physics. As a

consequence, how the participants interpreted quantum mechanics influenced their views about how *all* of science should be interpreted. For example, Thomas Kuhn's formal training was in physics and Feyerabend's background leaned heavily toward physics. The philosophies of both were strongly influenced by the Copenhagen interpretation of quantum mechanics (Howard 2004). Indeed, Feyerabend acknowledges that it was a discussion with Professor C.F. von Weizsäcker on the foundations of quantum mechanics that played a decisive role in his turn toward relativism and irrationalism (Feyerabend 1987, p. 117). For Kuhn and Feyerabend, the "lesson" of the Copenhagen interpretation of quantum mechanics was that philosophy of science should assume relativism and irrationalism.

However, it is not the case that quantum mechanics implies relativism and irrationalism. Howard's historical work shows that "what is commonly known as the Copenhagen interpretation of quantum mechanics, regarded as representing a *unitary* Copenhagen point of view, differs significantly from Bohr's complementarity interpretation" (2004, p. 669; italics added). Specifically, Bohr's complementarity interpretation "does not employ wave packet collapse in its account of measurement and does not accord the subjective observer any privileged role in measurement." Indeed, Bohr was "a direct *realist* at the experimental level" (Gomatam 2007, p. 743; italics in original). The label "Copenhagen interpretation" was introduced in the 1950s by Heisenberg (1955), not Bohr: "Simply put, the image of a unitary Copenhagen interpretation is a postwar myth, invented by Heisenberg" (Howard 2004, p. 675). So, we have the strange situation of Niels Bohr, to whom is customarily attributed the distinction of being a, if not the, lead creator of the "Copenhagen interpretation," actually disagreeing strongly with some of the interpretation's provisions. Regarding the myth of the unitary Copenhagen view, Howard's penultimate conclusion bears reporting in full:

> Until Heisenberg coined the term in 1955, there was no unitary Copenhagen interpretation of quantum mechanics. There was a group of thinkers . . . [who] agreed that interdeterminacy, complementarity, and entanglement were important lessons of the quantum theory, lessons whose import went beyond simple empirical claims to a revision in our thinking about how physical theories represent natural phenomena. But they did not all believe that quantum mechanics entailed observer-induced wave packet collapse, a privileged role for the observer, subjectivism, or positivism. Heisenberg and Bohr, in particular, disagreed for decades about just these issues. That Heisenberg succeeded in convincing us otherwise is unfortunate. It is time to dispel the myth. (Howard 2004, p. 680)

The "lesson" of quantum mechanics has been highly influential in convincing many behavioral scientists to adopt reality relativism and constructivism. The difficulty of understanding the *meaning* of the equations of quantum mechanics has led to what the philosopher of science Howard (2004, p. 669) refers to as "silliness" and the physicist Polkinghorne refers to as cases of "quantum mechanical folklore" (Polkinghorne 1984, p. 61). One such example of folklore is "Schrödinger's cat":

> The unfortunate animal in question is incarcerated in a closed box which also contains a radioactive atom with a 50–50 chance of decaying in the next hour, emitting a gamma-ray in the process. If this emission takes place it triggers the breaking of a vial of poison gas which instantaneously kills the cat. At the end of the hour, before I lift off the lid of the box, the orthodox principles of quantum theory bid me consider the cat to be in a state which is an even-handed superposition of the states "alive" and "dead." On opening the box the wave packet collapses and I find either a cooling corpse or a frisking feline. (Polkinghorne 1984, pp. 61–62)

Many advocates of reality relativism cite such treatments of quantum mechanics as that found in Zukav's *The Dancing Wu-Li Masters* (1979), which emphasizes one interpretation of "Schrödinger's Cat": By opening the box, the "reality" (either a dead or live cat) has been "created." For example, Lincoln and Guba are prominent proponents of reality relativism-constructivism. They propose that "it is dubious whether there is a reality. If there is, we can never know it" (Lincoln and Guba 1985, p. 83). On what do they base their position? Their conclusion rests almost exclusively on citing examples such as "Schrödinger's cat" drawn from the quantum mechanical folklore advanced by authors such as Zukav.

With respect to Schrödinger's cat, Gomatam (2007, p. 744) points out that "the 'cat paradox' is not a problem at all for Bohr." Likewise, Polkinghorne (1984, p. 62) notes: "It is scarcely necessary to emphasize *the absurdity* of the proposition that this state of affairs, whichever it is, has been brought about by my action in lifting the lid. It must surely be the case that the cat is competent to act as observer of its own survival or demise and does not need me to settle the issue for it." In fact, Polkinghorne points out that there is absolutely nothing in quantum mechanics that compels us to abandon realism. "It is astonishingly anthropocentric . . . to suppose that in the thousands of millions of years before conscious life emerged in the world—and still today in those extensive parts of the universe where no conscious life has yet developed—no wave packet has ever collapsed, no atom for certain decayed . . . that quantum mechanics as we know it is a biologically induced phenomenon" (1984, p. 66). Thus, he concludes: "If in the end, science is just about the harmonious reconciliation of the behavior of laboratory apparatus, it is hard to see why it is worth the expenditure of effort involved. *I have never known anyone working in fundamental science who was not motivated by the desire to understand the way the world is*" (1984, p. 79).

Quantum mechanical folklore has also strongly influenced the "relativist/constructionist" perspective of Peter and Olson (1983), and the "critical relativism" of Anderson in marketing. For example, Anderson's critique of Cooper (1987) claims to have "demonstrated that 'truth' is an inappropriate objective for science, and that consumer research will do well to abandon such a quixotic ideal" (Anderson 1988b, p. 405). Anderson justifies his conclusion concerning truth in sections attacking what he calls (following Laudan) "convergent realism" and "motivational realism." His section attacking "motivational realism" is based *exclusively* on the supposed "lesson" of quantum mechanics as interpreted by the well-known antirealist philosopher, Arthur Fine. However, the very same Fine who pronounces emphatically that "Realism is dead" (Fine 1984, p. 83) (relying on

quantum mechanics), in the very same article confidently asserts: "I certainly trust my senses, on the whole, with regard to the existence and features of everyday objects," and he continues by stating that he has no problem believing that "there really are molecules and atoms" (Fine 1984, p. 95). Thus, Fine's *antirealism* looks similar to the *realism* espoused here and by others elsewhere.

The subject of the philosophical implications of quantum mechanics, the "lessons" to be learned, like many other topics concerning the philosophy of science, has been woefully misunderstood in marketing. For example, Marsden (2001) develops what might be called the "postmodern physics argument" for marketing's postmodernism. The postmodern physics argument of Marsden is that (1) marketing is positivist; (2) Newtonian mechanics was positivist; (3) modern physics (i.e., quantum mechanics) rejects positivism; (4) postmodernism rejects positivism; (5) therefore, "postmodern marketing is, paradoxically, more scientific than positivist marketing" (Marsden 2001, p. 75).

The postmodern physics argument is uninformed because, contra Marsden (2001): (1) marketing is more realist than positivist, (2) Newtonian physics was *not* positivist, and (3) quantum mechanics does *not* reject positivism. Indeed, the prominent, non-Bohrian version of the Copenhagen interpretation of quantum mechanics is positivist, and logical positivism was actually developed *specifically* to accommodate the indeterminancy of quantum mechanics. Therefore, postmodernism's rejection of positivism does not imply that postmodernism is "more scientific" (i.e., more consistent with the modern physics of quantum mechanics). Rather, postmodernism's rejection of positivism implies a rejection of the prominent (i.e., positivist) interpretation of quantum mechanics.

Many scholars believe that postmodernism can be defended coherently (e.g., Griffin 1988). The modern physics argument is not such a defense. The postmodernists' cause is poorly served by such historically inaccurate and uninformed defenses.

9.4.2 Biology

The application of scientific realism to the biological sciences is straightforward. For example, the long-term success of theories such as the viral theory of diseases and the genetic theory of heredity provides reasons for believing that (1) something exists like the entities designated as "viruses," "genes," "AIDS virus," "chromosomes," and "DNA" (deoxyribonucleic acid), and (2) something exists like the structures relationships and structures postulated by the theories containing these entities. That is, the long-run success of the viral theory of diseases provides reason to believe that something like what we label a "virus" exists and that it does in fact cause illnesses, such as smallpox and polio. Similarly, the long-run success of genetic theory provides reason to believe that the DNA molecule exists and transmits heredity. Scientific realism helps us understand the actual workings of modern science without mocking it. The warranted belief that viruses exist and cause diseases provides justification for medical scientists, when confronted with a new disease (such as AIDS), to search for a new virus as its cause. Similarly, the warranted belief that the DNA molecule exists justifies the search for the description of the characteristics of that molecule, that is, the "double helix" (Watson 1968).

The preceding discussion of the DNA case history provides a striking illustration of why many philosophers of science, as well as most philosophically oriented, practicing scientists, believe that only some version of realism can explain the actual workings of much of science without reducing it to a charade. (Because no rational person searches for the characteristics of a "nonexisting entity," what, other than the warranted belief that DNA *exists,* could motivate the search that resulted in the "double helix"?) Even though both logical positivism and logical empiricism held truth in high regard, both were also under the spell of Humean induction (Stove 1982) and refused to countenance the real existence of "unobservable entities." Similarly, the acceptance of Humean induction was a cornerstone of Popper's falsificationism: "I regard Hume's formulation and treatment of the logical problem of induction . . . as a flawless gem . . . a gem of priceless value . . . a simple, straightforward, logical refutation of any claim that induction could be a valid argument, or *a justifiable way of reasoning*" (Popper 1972, pp. 86, 88; italics added). Thus Popper, by claiming that all positive results of a theory test are irrelevant to science (not a "justifiable way of reasoning"), fell into a form of irrationalism (Stove 1982).

9.4.3 Marketing and the Social Sciences

Applying scientific realism to marketing and the social sciences differs only in that most of the entities postulated in physical and biological theories are, at least in principle, tangible. In contrast, many, but not all, of the entities postulated by theories in marketing and the social sciences are intangible (and, therefore, unobservable in principle). The reason for the qualifying phrase "but not all" is that people occupy central positions in most social science theories, and people are, to say the least, *tangible.* Furthermore, most social science and marketing theories have manifestations or consequences that are tangible by any meaningful interpretation of that word. For scientific realism, intangible entities may be real in their consequences.

Applied to marketing and social science, the inductive realism tenet of scientific realism maintains that, to the extent that there are theories that have long-run success in explaining phenomena, predicting phenomena, or assisting in the solution of pragmatic problems in marketing and society, we are warranted in believing that something like the theories' postulated entities and structures of relationships exists. That is, the theories truly represent or correspond to some reality external to the theorist. (See section 11.2.1 and Figure 11.1.) In sociology, if a proposition such as "racist beliefs in a society generally result in the unfair treatment of a racial group" is successful, then we have reason to believe that something like "racist beliefs" exists, and they do result in the "unfair treatment of racial groups." In political science, if a proposition such as "totalitarian political regimes have a tendency to repress all human rights" is successful, then we are warranted in believing that something like the concept "totalitarian political regimes" actually exists, and that these regimes have their posited consequences—many of the manifestations of which will be, most assuredly, tangible.

Most research programs in marketing are at least consistent with scientific realism. For example, cognitive theories in consumer behavior, power and conflict theories in

channels of distribution, and relationship marketing theories in strategy are consistent with realism. Indeed, the resource-advantage (R-A) theory of competition (discussed in Chapters 13, 14, and 15) adopts realism as its philosophical foundation (Hunt 2000b; Hunt and Morgan 1995, 1996, 1997). Behavior modification theory in consumer behavior, a major exception, is positivistic in orientation because it admonishes the researcher to stay at the "observable" level of actual behaviors.

Because Bagozzi (1980, 1984) has been a prominent advocate of realism, many marketers seem to associate scientific realism only with his advocacy of structural equation modeling. But, though such modeling techniques *require* realism in order to be intelligible, scientific realism does not imply any specific mathematical or statistical technique. More strongly, scientific realism does not require any mathematical or statistical techniques at all. For example, Easton (2002) argues for case research based on critical realism. His approach centers on using cases to explore for entities that have causal powers and for the necessary and contingent relationships among the entities identified. As another example, the philosophical foundations of areas such as naturalistic, humanistic, and interpretive inquiry are unclear. Though these programs generally avoid mathematics and statistics, they need not avoid realism (Hunt 1989b). Similarly, at least some of their proponents seem to hold truth to be central in their research: "The humanities in general and artworks in particular contain truths that escape procedures of the hypothetical-deductive method" (Holbrook, Bell, and Grayson 1989, p. 40). Indeed, in ethnography, Stewart argues:

> Statistics-oriented researchers and ethnographers share an ultimate epistemic value. Whether or not they define themselves as "scientists," they both adhere to the fundamental purpose of science: to try to learn the truth about the world. . . . Certainly, it is difficult to see why people would put up with the tribulations of participant observation, would bother to *be there,* if they did not hope that their accounts would be more or less true. (Stewart 1998, p. 12; italics in original)

Scientific realism emphasizes the testing of marketing theories as a means for establishing their success. Therefore, theories comprising diverse concepts such as "attitudes," "intentions," "market segments," "purchase behavior," "channels of distribution," "retail store," "conflict," "brand awareness," "information search," "perceived risk," and so forth, warrant our believing (to the extent such theories are successful) that these entities have a real existence and the theories comprising these entities truly "say something"—and say something *objectively* (see Chapter 12)—about the world.

9.5 SCIENTIFIC REALISM AND THE SUCCESS OF SCIENCE

Theories can be successful in many ways. The inductive realism tenet of scientific realism focuses attention on the explanatory, predictive, and pragmatic success of a theory. Therefore, the phrase "long-term success" in the tenet identifies a theory that over some significant period of time has demonstrated its ability to explain phenomena, predict

phenomena, or be useful in solving pragmatic problems. However, as will be discussed in Chapter 11, scientific realism does not equate the meaning of "pragmatic success" with "truth." Likewise, for long-term success to "give reason," the inductive realism tenet does not imply that one "knows with certainty" because scientific realism specifically adopts the tenet of fallibilism. At the same time, it avoids the skepticism of the Humean view that only deductive methods are appropriate for generating knowledge. By "something like the entities," the inductive realism tenet rejects the view of naive or direct realism that the entities posited in the theory are (or must be) exactly as posited by the theory. Finally, by "something like the structure," the tenet claims that the success of a theory in explanation, prediction, and the solution of practical problems (usefulness) gives us reason to believe that the structure of relationships among the entities in the theory, both causal and otherwise, are as proposed in the theory. Again, however, this does not mean that the evidence will allow us to know with certainty that the structure of relationships is as posited.

9.5.1 Explaining the Successful Eradication of Smallpox

To further explicate how scientific realism can explain the success of science, we shall use an example from medical science: the eradication of smallpox. Such an example would seem particularly pertinent for marketing, since both marketing and medicine have "basic" and "applied" dimensions, and the claim is often made that marketing (and the marketing academic discipline) should be "more like" medicine. (See Chapter 2.)

Background

The disease called smallpox plagued the human community for thousands of years.[3] There are numerous references to a disease with symptoms like smallpox in the records of ancient India and Africa. For example, an analysis of the remains of several Egyptian mummies shows evidence of smallpox. By the Middle Ages smallpox was a scourge in most of Asia, Africa, and Europe: 3 million people died of smallpox in India's 1769 epidemic; by the end of the eighteenth century, Europe was losing over 400,000 people each year to smallpox; and it was responsible for over one-third of all the blindness in eighteenth-century Europe.

By the early part of the eighteenth century, it was widely recognized that people who once had smallpox seemed never to get the disease again. Furthermore, several European countries began adopting inoculation (as distinguished from "vaccination") procedures to prevent people from acquiring the disease during an epidemic. (Inoculation procedures had been used in Asia for several hundred years.) With the inoculation procedure, a subject was injected with material from a smallpox lesion obtained from an infected person. Inoculation became commonplace in England after a publication of the Royal Society, authored by James Jurian, showed that the risk of dying from smallpox from the inoculation was about 1 percent; whereas the overall risk of dying from smallpox was about 12 percent, and in times of epidemics the risk rose to about 20 percent. However,

in addition to the risk of death, the inoculation procedure also had the disadvantage that the inoculated person spread the disease to others.

Edward Jenner (1749–1823) was a physician who lived in Berkeley, England. He, for years, had heard rumors to the effect that rural women who had been infected by the mild disease called "cowpox" never contracted smallpox. Intrigued, Jenner began gathering data systematically on those who claimed to be immune from smallpox on the basis of having already had cowpox. The data seemed to confirm the claims of the rural women. However, would someone *inoculated* with cowpox be immune from smallpox? On May 14, 1796, Jenner inoculated one James Phipps with cowpox taken from a sore on the hand of a milkmaid who had recently become infected. The child developed a mild reaction similar to that of a favorable smallpox inoculation. On July 1, Jenner inoculated the boy with smallpox taken from another patient, and the inoculation produced no significant reaction. Elated with the results of his experiment, Jenner repeated his experiment on five more children in 1798, when a fresh outbreak of cowpox next made the virus available to him. After the experiments yielded the same positive results, Jenner triumphantly announced his findings in a small pamphlet. Within a few short years, Jenner's experiments were repeated on much larger samples, and "vaccination" procedures with cowpox virus became commonplace. Even so, smallpox continued to kill thousands of people over the next 200 years.

It should be pointed out that, of course, Jenner never *saw* a virus in his entire life. Although the "germ" theory of contagious diseases was actively being promulgated during his time, it was not widely accepted. In 1836, using the recently invented achromatic microscope, Agostino Bassi (1773–1856) was the first person to isolate a specific microscopic organism that causes a disease. In this case, he isolated a parasitic fungus that causes a disease common in silkworms. In 1898 Loeffler and Frosch demonstrated that some diseases were caused by microorganisms so small that they would pass through a very fine filter. Such microorganisms came to be known as "filterable viruses," and the organism causing smallpox soon came to be known as one of these, as did the organism causing poliomyelitis. In fact, viruses are so small that it was not until the invention of the electron microscope in 1947 that anyone saw a virus. The advent of the electron microscope brought about rapid advances in our knowledge of the characteristics and properties of the smallpox virus, including its internal chemistry and how it is able to attack human cells and force them to reproduce the virus.

It was already well known by the time of the electron microscope that the smallpox virus could not reproduce itself outside a human host. Therefore, if there ever came a time when no one in the world had smallpox, the disease—according to theory—should be completely eradicated. This was the program adopted by the World Health Organization (WHO) at its 1966 meeting in Geneva. At that time, forty-four countries were still reporting smallpox, and the disease was endemic in thirty-three of them. The WHO set a deadline of ten years for the eradication of the disease through massive vaccination programs. The last known case of smallpox—as of this writing—was in 1978, when the virus escaped from a laboratory in Birmingham, England. In 1980, the thirty-third World Health Assembly accepted the Final Report of the Global Commission for the Certification of Smallpox Eradication.

Scientific Realism's Explanation

Most people would consider the eradication of smallpox to be a significant "success story" for science. How can this success story be explained? Scientific realism posits the following theory to explain the "micro" successes that can then explain the "macro" success of the smallpox eradication program.

If it is true that:

1. something like the phenomenon denoted by "smallpox disease" exists; and
2. something like the entity denoted by "smallpox virus" exists; and
3. something like the "cowpox virus" exists; and
4. vaccinating people with cowpox virus will cause the body to produce antibodies that can successfully attack the smallpox virus; then
5. the preceding explains (the "micro" successes of) why vaccinating people with cowpox virus prevented them from contracting smallpox.

Therefore, if it is *also* true that:

6. the smallpox virus cannot exist outside human subjects; then
7. the preceding explains (the "macro" success of) why instituting a massive vaccination program throughout all parts of the world where smallpox existed was successful in eradicating smallpox.

The preceding argument shows (in highly summarized form) how scientific realism can explain the millions of "micro" success stories making up the "macro" success story of smallpox eradication by science. Note the crucial role that "entities" play, for example, "something like the smallpox virus exists." Furthermore, "structures" of entities play a key role, for example, "if injections of cowpox virus cause the body to produce antibodies." Now, though the smallpox eradication is a highly visible "macro" success story, over the past 400 years science has produced, literally, countless such episodes where the knowledge obtained through science has been (1) pragmatically successful in solving real-world problems and (2) empirically successful in explaining and predicting phenomena.

What else, other than some form of scientific realism, can explain the pragmatic and empirical success of science? That is, explaining the *failure* of a program like the smallpox eradication program would be easy. For example, if the injections of cowpox virus *do not* cause the body to produce antibodies, then the eradication program would likely fail. But what other viable theory is there, except scientific realism, to explain the success of the smallpox eradication program in particular and the overall pragmatic success of science in general? Aside from divine intervention, there currently exists no rival theory. Indeed, "The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle" (Putnam 1975, p. 69). Therefore, a reason for adopting scientific realism as a foundation for marketing research is that it, alone among philosophies of science, can explain the success of science.

9.6 SCIENTIFIC REALISM AND SCIENTIFIC PROGRESS

How does science progress? Most theories of science fail miserably in enabling us to understand both the nature and the mechanisms of scientific progress. Scientific realism contributes significantly to our understanding of how science progresses. First of all, as did the logical empiricists, scientific realism holds that science progresses by means of developing new theories, the falsifying of existing theories, expanding the scope of existing theories, and reducing specific theories into more general theories (Hunt 2003a, pp. 86–90). However, scientific realism goes much further. In particular, scientific realism maintains that science progresses by (1) discovering new entities, (2) describing better the attributes and characteristics of entities, (3) measuring entities better, and (4) discovering the structures of the relationships among entities, including, most important, the structures of causal relationships.

Given that the long-term success of a scientific theory gives scientists reason to believe that something like the entities postulated by the theory actually exists, scientific realism holds that science progresses when researchers explore for the existence of new entities. For example, the belief in the "germ" theory of disease warranted researchers to search for other entities that may cause other diseases. Thus, by 1909 medical science had isolated the causal agent of poliomyelitis as a "filterable virus." Likewise, the warranted belief that entities exist prompted researchers to explore (and find) the virus resulting in the AIDS (acquired immune deficiency syndrome) disease. If one does not have good reason to believe that the entities—including unobservable entities—in one's theory exist, why engage in a search for them?

Scientific realism also holds that science progresses by better descriptions and better measures of the entities postulated by its theories. For example, how large is the viral entity? What is its shape? What is its chemical composition? By what means does it penetrate the cells of humans? Only scientific realism warrants the exploration of these kinds of questions. To work toward better descriptions and better measures of *nonexisting* entities is irrational. Absent the belief in scientific realism, the questions could only be explored by the researcher doing the following: "Even though viruses do not exist, I shall attempt to explore precisely how large these nonexisting entities are." Scientific realism holds that researchers do not in fact engage in such elaborate rituals of self-deception.

Given that scientific realism holds that the long-term success of a theory is reason to believe that something like the *structure* postulated by the theory exists, scientific realism warrants the search for how the entities postulated by the theories interact with other entities. For example, how do viruses cause human cells to propagate the production of copies of the original virus? By what means does the viral entity produce the negative complications of the disease, that is, high temperature, rash, and so forth? By what mechanism and through what route does the cowpox virus cause the human body to produce antibodies? By what mechanism do the antibodies protect the human body against the smallpox virus?

As can be seen, scientific realism dramatically expands the concept of scientific progress to include many of the "lower-level" aspects of science. But "lower-level" does not

mean unimportant. On the contrary, by focusing exclusively on "grand theories," many commentators on science have missed much of the real progress in the development of science.

In summary, scientific realism recognizes that science has been enormously successful over the past 400 years. This success, particularly its pragmatic and empirical aspects, warrants the belief that "something like" the entities and structures postulated by scientific theories actually exist. Perhaps the easiest way to understand scientific realism is to start with "belief," rather than "success." Scientific realism holds that the *belief* that "something like" the entities and the structure postulated by a scientific theory exists provides *warrant* (good reason) for interventions, that is, for using the theory to take action. For example, the belief that the cowpox virus exists and that vaccinating people with cowpox virus will protect them from smallpox *warrants* the action of vaccinating people. The action of vaccinating people results in certain consequences. These consequences may be favorable (successes) or unfavorable (failures). This pragmatic success (failure) involved in the use of a scientific theory contributes to warranting the belief (disbelief) in the truth of the theory. By "truth of the theory" we mean that "something like" the entities and structure postulated by the theory likely exists. The "something like" and "likely" become synonymous with the notion of "approximate truth."

The reader should recognize that the preceding does *not* imply the acceptance of the much-maligned "pragmatic theory of truth." Scientific realism does not contend that the *meaning* of the statement "theory *X* is true" is the same as accepting the pragmatic consequences of "theory *X*." Rather, scientific realism holds that the pragmatic and empirical consequences of a theory (its success) give *warrant* (good reason) for believing the theory to be true, that is, the world is "something like" the theory. Therefore, a reason for adopting scientific realism as a foundation for marketing research is that it enables us to understand the nature and mechanisms involved in scientific progress.

9.7 SCIENTIFIC REALISM CONTRASTED WITH LOGICAL EMPIRICISM

Although scientific realism and logical empiricism share a common belief that science does in fact make progress, scientific realism differs sharply from logical empiricism on several issues, two of which I discuss here. First, most scientific realists do not share the logical empiricists' confidence in formal logic. The logical empiricists and logical positivists were enamored with the formal logic developed by Bertrand Russell and Gottlob Frege (see Hunt 2003a, sections 2.5.2 and 3.1.2). They believed that most philosophical problems could be solved (or at least can and should be addressed) through the application of formal logic. Furthermore, they believed that science could be reconstructed using formal logic. Although some scientific realists, such as Niiniluoto (1999), make use of the tool of formal logic, most realists have less confidence than did the logical empiricists that reconstructing science using this tool can solve important philosophy of science issues. The second, and more important, difference between scientific realism and logical empiricism is the issue of the "theoretical term/observation term" dichotomy.

Following their logical positivist predecessors, the logical empiricists believed in a sharp distinction between "observation terms" and "theoretical terms" in a scientific theory. Adopting, essentially, the Wittgenstein "picture theory" of meaning (see Hunt 2003a, section 3.1.3), observation terms referred in a nonproblematical manner to entities in the real world. Thus, the logical empiricists adopted a kind of "empirical realism" with respect to observation terms in a theory. However, some terms in some theories did not *refer* in the direct manner of the observation terms. The logical empiricists labeled all terms that did not directly refer to some aspect of the observable world as "theoretical terms." In order for scientific theories to be "meaningful" (as opposed to meaningless metaphysics), theoretical terms would have to be given meaning by being defined through "correspondence rules" with observation terms. However, this posed an enormous problem for the logical empiricists: the problem of theoretical dispensability. As discussed in Hempel's famous article, "The Theoretician's Dilemma" (1958, reprinted in Hempel 1965d), if (1) all theoretical terms can be defined through correspondence with observation terms and (2) the purpose of science is to determine relationships among observation terms, then (3) theoretical terms are "unnecessary" in science. (See Hunt 2003a, section 3.3.3, for more on the "theoretician's dilemma.")

For scientific realism, the "theoretician's dilemma" is no dilemma at all. Scientific realism dismisses the theoretical term/observational term dichotomy as a false dichotomy. That is, scientific realism acknowledges that all the terms in a theory are, properly speaking, "theoretical terms." The phrase "theoretical term" means nothing more than "a term in a theory." For scientific realism, some terms in a theory may denote something more observable, more detectable, more easily measurable than other terms. In fact, some terms may denote nothing, in principle, observable at all. However, all the terms in a theory (excepting, of course, mathematical and logical terms) can lay a legitimate claim to denote the existence of some entity based on the senses (the classical realism tenet) and/or the success of the theory (the inductive realism tenet).

Consider, for example, the concept of "intelligence." For the logical positivists and empiricists, "intelligence" would always be a theoretical concept whose *meaning* was totally contained within its measure ("when I say 'intelligence,' this is to be interpreted as a 'shorthand' way of saying 'the results that a person has obtained on the Stanford-Binet intelligence quotient test'"). From the perspective of a scientific realist, the concept of "intelligence" *may* refer to a real, existing, nontangible entity, whose characteristics can be measured or indicated through a variety of measuring devices. Stated in modern measurement terms, scientific realism acknowledges the possibility of *both* a reflective measurement model and a formative measurement model being appropriate, depending on the particular circumstances. In contrast, logical empiricism assumes a (kind of) *formative* measurement model as being the *only* appropriate tool (Diamantopoulos and Winklhofer 2001; Fornell and Bookstein 1982; Howell 1987; Howell, Brevik, and Wilcox 2007).

Why could the logical empiricists not adopt the position that theoretical terms could be real, yet nonobservable? Several factors made it impossible for them to adopt a realistic interpretation of theoretical terms. First, like Popper and many other philosophers, the logical empiricists were firmly convinced that Hume was correct: It is impermissible to

go from observables to unobservables (e.g., going from observations of *X* and *Y* to the claim that *X causes Y*), because only deductive logic is an acceptable method of reasoning. Therefore, no nonobservable entity may be *inferred* from the conjoining of experience and observation. Second, the logical empiricists implicitly accepted the doctrine of "foundationalism," which implies that, in order for science to be secure, it (1) must rest on incorrigible foundations (the certainty of observation), and (2) it must proceed by infallible methods. Third, the "lesson" that the logical empiricists had learned from the downfall of Newtonian mechanics was that never again should science allow itself to believe it had found genuine knowledge about nonobservable entities existing in the world. Fourth, and finally, the "lesson" of quantum mechanics was that, because no one knew how to interpret quantum mechanics realistically, *all* theories should be interpreted instrumentally. That is, theories are calculation devices for making predictions about observable phenomena and do not refer to or represent any underlying reality.

Because reflective measurement models—in which the indicators reflect the existence of some real, but unobservable entity—play a central role in empirical research in marketing, logical empiricism is not a good candidate for grounding marketing research. In contrast, because scientific realism can accommodate unobservable variables that can be measured reflectively, it is a good candidate for grounding marketing research.

9.8 SCIENTIFIC REALISM CONTRASTED WITH CONSTRUCTIVE EMPIRICISM

Consider, now, the philosophy of Bas C. van Fraassen (1980), "constructive empiricism." It shares some of the same tenets of the logical positivists and logical empiricists, since constructive empiricism is decidedly antirealist. Ever since the famous paper by Maxwell (1962), philosophy has recognized that the concept of "observation" should be interpreted as a continuum. On this view, we have a large number of sense-extending instruments that enable us to "see," for example, microscopes and telescopes of varying powers. Actually, "observable" is often used, and most appropriately so, as roughly synonymous with "detectable" or "measurable" in modern philosophy of science literature.

Van Fraassen's theory of science rejects totally the notion of observation being a continuum and contends that we do not "see" through a microscope. For van Fraassen, only those entities exist that can be, in principle, observed by the unaided human eye. Therefore, genes, bacteria, and viruses do not exist (but the "back sides" of the moon and stars do) (Hacking 1985, p. 135). Van Fraassen proposes: "That the observable phenomena exhibit these regularities, because of which they fit the theory, is merely a brute fact, and may or may not have an explanation in terms of unobservable facts 'behind the phenomena'—it really does not matter to the goodness of the theory, nor to our understanding of the world" (Van Fraassen 1980, p. 54).

What is the aim of science? For van Fraassen's constructive empiricism, "science aims to give us theories which are empirically adequate; and acceptance of a theory involves a belief only that it is empirically adequate" (Van Fraassen 1980, p. 12). How does van Fraassen reach his conclusion that "empirical adequacy" is the only appropriate aim for

science? Because he interprets this to be the "lesson" of quantum mechanics: "He [van Fraassen] takes it that the realist is committed to finding hidden variables in quantum mechanics" (McMullin 1984, p. 34).

For a detailed discussion of van Fraassen's constructive empiricism, see Alspector-Kelly (2001). For our purposes, readers should note that, as with most versions of antirealism, van Fraassen's philosophy is an impoverished one. He would have us believe, for example, that the Nobel Prize committee was somehow totally confused about the nature of science when they awarded the Nobel Prize in medicine in 1962 to James D. Watson for his discovery that DNA had the structure of a double helix. How can nonexisting entities have any structure, let alone a double helix? Therefore, constructive empiricism is not a viable alternative to scientific realism as a philosophy for grounding marketing research.

9.9 SCIENTIFIC REALISM AND CRITICAL REALISM

A fundamental tenet of scientific realism is that it is *critical.* That is, all knowledge-claims must be critically evaluated and tested to determine the extent to which they do, or do not, truly represent or correspond to the world. Many philosophers of science use "critical realism" to designate their approaches to science. However, the word "critical" may be used differently in the different versions of critical realism. This section discusses the critical realisms of Niiniluoto (1999) and Sayer (1992). We select these two approaches because, first, they are widely cited and their proponents argue well for their respective positions. Second, though both are *critical,* their approaches differ in their implications for marketing research. Third, in marketing, Easton (2002) advocates the realism of Sayer. Fourth, in marketing, Wensley (2002, p. 233) has mockingly criticized the Niiniluoto approach as an "Alice in Wonderland" philosophy of science. We begin with Niiniluoto.

9.9.1 The Critical Realism of Niiniluoto

For Niiniluoto, what he labels "critical scientific realism" may be distinguished from its rivals on the following theses:

R0. At least part of reality is ontologically independent of human minds.
R1. Truth is a semantical relation between language and reality. Its meaning is given by a modern (Tarskian) version of the correspondence theory, and its best indicator is given by systematic enquiry using the methods of science.
R2. The concepts of truth and falsity are in principle applicable to all linguistic products of scientific enquiry, including observation reports, laws, and theories. In particular, claims about the existence of theoretical entities have a truth value.
R3. Truth (together with some other epistemic utilities) is an essential aim of science.
R4. Truth is not easily accessible or recognizable, and even our best theories can fail to be true. Nevertheless, it is possible to approach the truth, and to make rational assessments of such cognitive progress.

R5. The best explanation for the practical success of science is the assumption that scientific theories in fact are approximately true or sufficiently close to the truth in the relevant respects. Hence, it is rational to believe that the use of the self-corrective methods of science in the long run has been, and will be, progressive in the cognitive sense. (Niiniluoto 1999, p. 10)

Niiniluoto argues that the "ontological realism" of R0 (i.e., our classical realism) distinguishes his critical scientific realism (hereafter, CSR) from the works of these philosophers who advocate subjective idealism, solipsism, and phenomenalism. In turn, the "semantical realism" of R1 separates CSR from pragmatism (e.g., the works of William James), neopragmatism (e.g., Richard Rorty), and relativism/constructivism (e.g., David Bloor and Bruno Latour). The "theoretical realism" of R2 (a variant of our inductive realism) and the "axiological realism" of R3 distinguish CSR from "half-realism" (e.g., Nancy Cartwright), methodological nonrealism (e.g., Larry Laudan), and constructive empiricism (e.g., Bas C. van Fraassen). The fallibilistic realism of R4 separates critical realism from both naive realism and dogmatic skepticism (e.g., Paul Feyerabend). For Niiniluoto, this "fallibilist tradition in epistemology . . . has been advocated by such diverse thinkers as Friedrich Engels, Charles Peirce, Karl Popper, and Wilfrid Sellars" (1999, p. 13). Most of Niiniluoto's book represents a detailed exposition, often using the tool of formal logic, of his six theses. In doing so, he defends his critical scientific realism against critics, such as those advocating internal realism (Niiniluoto 1999, pp. 206–26), relativism (1999, pp. 227–41), feminism and postmodernism (1999, pp. 242–51), and social constructivism (1999, pp. 252–78).

Readers should note that the word "critical" does not appear in any of Niiniluoto's six theses. How, then, is critical scientific realism *critical?* CSR is critical in two ways. First, note that "truth" appears in five of the six theses, and that "systematic inquiry using the methods of science" is in R1, and "the self-corrective methods of science" appears in R5. For Niiniluoto, therefore, CSR is critical in that it maintains that science uses systematic, self-corrective methods to seek, and in fact often achieve, linguistic products (e.g., observation reports, laws, and theories) that are "approximately true or close to the truth in relevant respects" (1999, p. 10). Indeed, for society, "trust in science depends on the fact that the community of its practitioners is employing the critical method of scientific inquiry" (1999, p. 299).

CSR earns the appellation "critical" in a second manner. Niiniluoto argues against "naturalism" in philosophy of science, which holds that "scientific rationality has to be grounded in the actual practice of science" (1999, p. 15). He points out that a normative *ought* cannot be derived from, or rebutted by, a historical *is.* Specifically, he maintains: "While it is important for the scientific realists to have a realistic picture of scientific activities, and therefore to pay serious attention to historical and sociological case studies, they should also maintain the possibility of *criticizing* the way science is actually done" (1999, p. 17; italics added). That is, CSR is critical in that it may criticize science, scientists, and scientific communities. Both of Niiniluoto's senses of "critical" are consistent with the critical realism of section 9.1.

9.9.2 The Critical Realism of Sayer

Consider, now, the critical realism of Sayer (1992), which is advocated in marketing by Easton (2002). Sayer provides, as he puts it, eight "signposts" to identify the premises of his version of realism:

1. The world exists independently of our knowledge of it.
2. Our knowledge of that world is fallible and theory-laden. Concepts of truth and falsity fail to provide a coherent view of the relationship between knowledge and its object. Nevertheless knowledge is not immune to empirical check, and its effectiveness in informing and explaining successful material practice is not mere accident.
3. Knowledge develops neither wholly continuously, as the steady accumulation of facts within a stable conceptual framework, nor wholly discontinuously, through simultaneous and universal changes in concepts.
4. There is necessity in the world; objects—whether natural or social—necessarily have particular causal powers or ways of acting and particular susceptibilities.
5. The world is differentiated and stratified, consisting not only of events, but objects, including structures, which have powers and liabilities capable of generating events. These structures may be present even where, as in the social world and much of the natural world, they do not generate regular patterns of events.
6. Social phenomena such as actions, texts and institutions are concept-dependent. We therefore have not only to explain their production and material effects but to understand, read or interpret what they mean. Although they have to be interpreted by starting from the researcher's own frames of meaning, by and large they exist regardless of researchers' interpretations of them. A qualified version of 1 therefore still applies to the social world. In view of 4–6, the methods of social science and natural science have both differences and similarities.
7. Science or the production of any other kind of knowledge is a social practice. For better or worse (not just worse) the conditions and social relations of the production of knowledge influence its content. Knowledge is also largely—though not exclusively—linguistic, and the nature of language and the way we communicate are not incidental to what is known and communicated. Awareness of these relationships is vital in evaluating knowledge.
8. Social science must be critical of its object. In order to be able to explain and understand social phenomena we have to evaluate them critically. (Sayer 1992, pp. 5–6)

Sayer's purpose, he points out, is not to do a philosophical treatise. Rather, it is to discuss method in social science, in conjunction with considering social theory and philosophy of science. He wishes to counter two types of "imperialism:" (1) the "scientism" that centers "around the search for regularities and hypothesis testing, to derogate or disqualify practices such as ethnography, historical narrative, or explorative research," and (2) the "kind of imperialism . . . which tries to reduce social science wholly to the interpretation

of meaning" (Sayer 1992, p. 4). In this regard, he condemns how interpretivists use the terms "positivist" and "empiricist" as "purely pejorative" epithets (1992, p. 7).

As with Niiniluoto, we can make several observations concerning Sayer's eight premises. First, premise 1, in conjunction with 6, adopts classical realism. Second, premise 2 embraces fallibilistic realism. Third, unlike the realism of Niiniluoto, the word "truth" appears only once, in premise 2; and even on this occasion its purpose is not to put forth truth as a goal (as does Niiniluoto), but to claim that "truth and falsity fail to provide a coherent view of the relationship between knowledge and its object." Fourth, premise 3 rejects both the Kuhnian "revolution"—see (Hunt 2003a), section 4.2—and the Baconian "bricks in a pile"—see Hunt (2003a), section 8.3.1—views of scientific progress. Fifth, premises 4 and 5 stress the importance of causal powers of objects, as emphasized and developed in marketing by Easton (2002).

Sixth, unlike Niiniluoto, the word "critical" appears in his premises—as number 5. However, again unlike Niiniluoto, by "social science must be critical of its subject," Sayer is using "critical" in the manner of the "critical theory" of the Frankfurt School. Whereas Niiniluoto advocated the criticism of *science,* Sayer advocates that social scientists should be social critics (i.e., criticize *society*) and be social *activists.* That is, in their roles as social activists, social scientists "should develop a critical self-awareness in people and indeed assist in their emancipation" (Sayer 1992, p. 42). As pointed out (and advocated) in marketing by Hetrick and Lozada (1994, p. 549), "critical theory is a neo-Marxian critique of both capitalist societal arrangements and positivist science." Therefore, for critical theorists, the discussion of critical theory in, for example, Murray and Ozanne (1991) is a "sanitizing" of the very essence of critical theory, because it may "sustain social groups sympathetic to capitalism" (Hetrick and Lozada 1994, p. 549). Therefore, those marketers hostile to economic freedom may favor the "critical" in Sayer's critical realism; those sympathetic to economic freedom will not. Those marketers concerned that social activism may compromise objectivity will look "critically" at the "critical" in Sayer's realism; those marketers unconcerned will not. (See also Nagel's arguments on the issue of whether social science is inherently subjective in section 12.2.1.)

Seventh, Sayer's premise 2 indicates that observational evidence is problematic because it is "theory-laden." Furthermore, contrasted with Niiniluoto, who maintains that "we choose the language L, and the world decides which sentences of L are true" (Niiniluoto 1999, p. 267), Sayer's premise 7 assumes that "the nature of language and the way we communicate are not incidental to what is known and communicated." Therefore, he suggests that "it may help to replace (or if you prefer, modify) the concept of truth with 'practical adequacy'" (Sayer 1992, p. 69). Indeed, he does so even though he maintains that observational evidence does provide an "external check" on the "objectivity of our knowledge" (1992, p. 65).

Both Niiniluoto's and Sayer's versions of critical realism fall within the domain of scientific realism. Clearly, however, the "critical" in the two different versions of critical realism means different things. Therefore, marketers who choose to adopt critical realism as a foundation for their research are well advised to choose their version of critical realism carefully.

Also clearly, even some realist philosophers of science, such as Sayer (1992), believe that both the "theory-ladenness" of observation and the "nature of language" pose difficulties for the pursuit of objectivity and truth in social science research. These issues will be addressed in Chapters 11 and 12.

9.10 CONCLUSION

What can we conclude about scientific realism? First of all, the prevailing view among philosophers of science is that most sciences and most scientists embrace some version of realism. As noted by Suppe, "Science is overwhelmingly committed to metaphysical and epistemological realism" (1977a, p. 716). Also, as Meehl relates:

> As to realism, I have never met any scientist who, when doing science, held to a phenomenalist or idealist view; and I cannot force myself to take a nonrealist view seriously even when I work at it. So I begin with the presupposition that the external world is really there, there is a difference between the world and my view of it, and the business of science is to get my view in harmony with the way the world really is to the extent that is possible. There is no reason for us to have a phobia about the word "truth." The idea that you shouldn't ask whether a scientific statement is true, separate from the anthropologists or the Hogo Bogos' belief in it, *because you can't be absolutely certain,* is a dumb argument. (Meehl 1986, p. 322; italics added)

What does scientific realism imply? First, some parts of the actual workings of science are totally incomprehensible and irrational if not viewed from a realist prospective. Indeed, many research programs require scientific realism (Leplin 1986, p. 38). If a scientist does not believe that viruses exist, then such activities as engaging in experiments to determine the size, shape, and structure of "nonexisting viruses" is irrational. Second, realism gives the practicing scientist prescriptive warrant for engaging in certain kinds of research activities. For example, the belief that viruses exist (ontological realism) and that they have caused smallpox and polio (epistemological realism) gives warrant for the practicing scientist to attempt to discover if there is a virus that may cause another disease (such as AIDS).

Third, many of the attacks on scientific realism seem to be attacks on straw-men caricatures of scientific realism, or unintelligibly incoherent, or fundamentally misguided. It is very curious and highly suspect that antirealists rely so heavily on the difficulty of interpreting realistically one scientific theory (i.e., quantum mechanics) and then generalize (in a monumental act of inductive *hubris*) that the entire universe of scientific theories should, therefore, be treated in a nonrealist fashion. To consider the absurdity of this situation, how about if the facts of the matter were in reverse? Suppose that quantum mechanics were the only theory that could be interpreted realistically, and that it was very difficult, if not impossible, to interpret all other theories in a realist manner? Are we to believe that those who currently adopt the antirealist position would suddenly argue

powerfully in favor of realism? My strong suspicion is that they would not. Although the "lesson" of quantum mechanics may truly motivate some antirealists, there is good reason to believe that many others have radically different agendas.

Fourth, scientific realism is the only philosophy of science that provides an explanation for science's success over the past 400 years in explaining phenomena, predicting phenomena, and guiding interventions in the real world. Realism's explanation of the success of science is that science's theories are approximately true representations of the world. (The role of truth in marketing theory and research will be further explicated in Chapter 11.)

In conclusion, there are numerous "isms" in the philosophy of science, for example, logical positivism, logical empiricism, constructive empiricism, idealism, relativism, and scientific realism. Of all these "isms," scientific realism seems to make the most sense for marketing, for no *other* philosophy is coherent (without being dogmatic), is critical (without being nihilistic), is open (without being anarchistic), is tolerant (without being relativistic), is fallible (without being subjectivistic), and—at the same time—can account for the success of science. It is a good candidate for providing a philosophical foundation for marketing research.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. A student has commented: "The analysis of relativism, constructionism, and subjectivism [as discussed in section 9.1] is totally unnecessary. Because it is *obvious* that relativism has no merit, one need not develop arguments against it." Evaluate the student's comment. If you agree that "it is obvious," explain why so many philosophers of science in the 1960s and 1970s, as well as marketing researchers in the 1980s and 1990s, promoted it. If you disagree that "it is obvious," why do you think the student would make such a comment?
2. Rachels (1986), an ethicist, discusses cultural relativism as follows:

> Cultural Relativism would preclude us from saying that any of these practices [e.g., slavery and genocide] were wrong. We would not even be able to say that a society tolerant of Jews is *better* than the anti-Semitic society, for that would imply some sort of transcultural standard of comparison. The failure to condemn *these* practices does not seem "enlightened"; on the contrary, slavery and anti-Semitism seem wrong *wherever they occur. Nevertheless, if we took cultural relativism seriously, we would have to admit that these social practices are also immune from criticism.* (Rachels 1986, pp. 17–18)

What does Rachels mean when he says, "if we took cultural relativism seriously"? Under what circumstances will individuals propose philosophies that are "not to be taken seriously"? How do we *know* when a philosophy is to be "taken seriously"? Is it *ever* appropriate to propose philosophies that one does not "take seriously"?

3. Section 9.1 contends that reality relativism implies "ethical impotence." What's would be the difference between a philosophy's being ethically or morally impotent and its being ethically or morally wrong? Many philosophers maintain that the distinction between moral relativism and cognitive relativism (e.g., conceptual framework relativism) is often difficult to make. Why should this be the case? Why should, or should not, the *morality* of cognitive relativism be an issue?
4. Suppe (1984) points out that there is a distressing tendency for the behavioral sciences to uncritically accept many theories of science from the philosophy of science, even after the philosophies have been long repudiated: "Increasingly, behavioral and social sciences are reading and being influenced by Thomas Kuhn's *Structure of Scientific Revolutions.* Within anthropology, economics, political science, psychology, and sociology there has been a spate of publications attempting to view these disciplines from a 'Kuhnian' perspective, either in an attempt to discern the current state of the field or in an attempt to resolve various methodological disputes within the discipline" (Suppe 1984, p. 89). He goes on to relate that "I am dismayed and worried by the burgeoning influence Kuhn is having on behavioral and social scientists. My fears here rest not on the fact that Kuhn's structure of scientific revolutions is bad history of science and fundamentally defective philosophy of science, but rather on the way in which behavioral and social scientists are resorting to and relying on Kuhn's work" (Suppe 1984, p. 69). Suppe concludes that "science must approach philosophy of science with a critical eye" and:

   > In short, scientific methodologies are strategies for ascertaining the likelihood that accepted theories or hypotheses are correct, and to the extent that science has been progressive it has involved the development of increasing more sophisticated strategies or canons of rationality for deciding which theories to accept or reject on the basis of increasingly smaller data bases, in such a way that such strategies lead to increasingly higher success rates in the acceptance of true theories or hypotheses and the rejection of false ones. The methodologies codified by philosophers of science or methodologically-concerned scientists are codifications of such strategies, often coupled with assessments of the relative merits of these strategies compared to other available ones. (Suppe 1984, p. 102)

   To what extent are Suppe's comments relevant to marketing and social science today? To what extent is Suppe arguing that scientific *methodology* is progressive? Would it be possible to know with certainty that scientific methodology is progressive? Why is the issue of the progressivity of scientific methodology important, or not important?
5. After the publication of Feyerabend's *Science in a Free Society,* Tibor Machan wrote a review for *Philosophy of Social Science* (Machan 1982). Feyerabend then commented on Machan's and referred to his views as "academic ratiofascism" (Feyerabend 1982, p. 191). Machan then replied:

> Oh my, what have I unleashed with my humble little review! A veritable Don Rickles of the journal circuit, only without the fun. But let me try, as hard as I can, to keep away from derision, not without saying first, however, that it does hurt. (Machan 1982, p. 197)

He then responded to the charges that he is a "ratiofascist":

> Feyerabend may wish to have us think that all he, but no one else, asks for is a tolerance for different lifestyles. But there is plenty of room in my own standard-ridden outlook . . . for enormous varieties of lifestyles within societies with the very same legal framework—the U.S. liberal melting pot is really far richer, though getting less so, than Feyerabend allows. What with his calling me a fascist for thinking and publishing my libertarian notions, I doubt that a polity of his conception would be anywhere near so open and pluralistic as one I have in mind. . . . It really makes me doubt that Feyerabend is a man with whom any kind of conversation that does not begin with total submission to his views could be carried out in a civilized fashion. (Machan 1982, p. 199)

Some writers have pointed out that many advocates of relativism, like Feyerabend, seem totally intolerant of others whose views may differ from their own. Other writers have noted that relativism became popular during the turbulent era of the 1960s. Why does relativism not imply a tolerance toward others' views? Why should a philosophy promote being tolerant of others' views? Would a philosophy promoting tolerance imply that we should be tolerant of the views of the Nazis who maintained that their attempted genocide of the Jews was justified? Discuss.

6. Why has "positivism" received so much attention in the marketing literature, but "realism" so little?
7. How does "naive" or "direct" realism relate to "foundationalism" and "objectivism"?
8. How long should be the "long" in "long-term success"?
9. Identify a research program in marketing. To what extent is the research program consistent or inconsistent with scientific realism? With positivism? With relativism?
10. What are the advantages for the view that social scientists should, at the same time, be social *researchers,* social *critics,* and social *activists?* What are the disadvantages?
11. How does scientific realism explain the success of science? In this respect, what does "success" mean? What is the "miracle theory" of scientific success? Evaluate it as a genuine rival for scientific realism. Is it true that the "miracle theory" is the only genuine rival to scientific realism for explaining the success of science? Philosophers like to hypothesize a world where we are all "brains floating in a vat." In such a world, everything is "illusion." Is this a genuine rival for scientific realism?

12. Scientific realism expands the concept of scientific progress to include many of the "lower-level" aspects of science. What does "lower-level" mean? How have philosophers erred by focusing exclusively on "grand theories"? Is marketing making progress at the "lower-level"? Give examples.
13. What is the difference between a correspondence rule *defining* a "theoretical term" and its *measuring* a "theoretical term"? To what extent does marketing rely on "correspondence rules" versus "measures"? To what extent should it?
14. What is scientific realism? How does it differ from logical positivism and logical empiricism? Many people believe that when the critics of science attack the logical positivists/empiricists, such critics are actually attacking scientific realism (but they do not realize it). Evaluate this thesis.

NOTES

1. See Hunt (2003a), especially Chapter 4, for a more complete discussion of the rise and fall of relativism in the philosophy of science.

2. See Polkinghorne (1984) for a good introduction to quantum mechanics.

3. Hopkins (1983) and Fenner and White (1976), on whom this section draws, provide good discussions of the history of smallpox.

10

ON SCIENCE/NONSCIENCE, QUALITATIVE METHODS, AND MARKETING RESEARCH

This chapter continues our analyses of controversies that have been prominent in the philosophy debates in marketing. The controversies addressed involve two questions. First, do the sciences differ from nonsciences in any fundamental ways? Second, should researchers adopt qualitative methods because positivism is dead? We begin with the sciences versus nonsciences controversy.

10.1 THE SCIENCES VERSUS NONSCIENCES CONTROVERSY

What is the nature of science? As first proposed in Hunt (1976a, 1976b), marketing theory has traditionally viewed a science as having four characteristics: (1) a distinct subject matter, (2) the description and classification of the subject matter, (3) the presumption that underlying the subject matter are uniformities, regularities, and causal structures that sciences seek to discover, and (4) the adoption of the scientific method.[1] The first criterion distinguishes the sciences in general from instances of science in particular. In marketing, for example, it has been argued that the transaction is the distinct subject matter of marketing science (Bagozzi 1974; Hunt 1976a, 1976b; Kotler 1972b). The second and third criteria, jointly, differentiate the sciences (such as psychology and chemistry) from the arts and humanities (such as music and English). The fourth criterion, scientific method, distinguishes the sciences (such as astronomy and medicine) from the nonsciences and pseudosciences (such as astrology and palmistry). The scientific method, called by many "the most significant contribution of Western civilization" (Morris 1955, p. 63), traces to the seventeenth century natural philosophers who conjoined (1) the emphasis on logic and speculation implicit in Plato's method of critical discussion, (2) the powerful tool of mathematics, as proposed by Pythagoras, (3) the belief in systematic observation and the syllogistic logic of Aristotle, and, very important, (4) the reliance, when possible, on experimentation, as advocated by Galileo (and subsequently articulated by Sir Francis Bacon). (See Hunt 2003a, section 2.2, for a detailed discussion of the development of the scientific method.)

The traditional view in marketing theory, as well as the mainstream view in philoso-

phy of science, has been challenged by the relativist conception of the nature of science. Relativism maintains that there are no fundamental differences between the sciences and nonsciences (Anderson 1983, 1989; Peter and Olson 1983, 1989). This section reviews the relativist argument, before (1) evaluating the argument, (2) revisiting a partial formalization of the argument, (3) examining whether the argument is a straw man, and (4) evaluating the "weak form" version of the argument.

10.1.1 Relativism and the Nature of Science

Relativists point out that the search for criteria that would separate science from nonscience dates from the very beginnings of Western philosophy, and that Popper has labeled the question "the problem of demarcation" (Anderson 1983, p. 18). However, "philosophers have been signally unsuccessful in their search for such criteria" and many "consider the question to be a chimera" (Anderson 1983, p.18). After rejecting the notion that sciences differ from nonsciences in any fundamental respect, relativists propose that marketing science should adopt a relativistic stance, and they distinguish between two different ways that the term "science" can be used. These two ways are designated science_1 and science_2:

> It is proposed that science_1 should refer to the idealized notion of science as an inquiry system which produces "objectively proven knowledge" (Chalmers 1976, p. 1). On this view, science seeks to discover "the truth" by the objective methods of observation, test, and experiment. Of course it should be clear that no such inquiry system has ever existed—nor is it very likely that such a system will ever exist. (Anderson 1983, p. 26)

Given their belief that science as a process that searches for truth by objective methods cannot exist, relativists propose that marketing adopt the notion of science_2:

> The defining element here [science_2] is that of societal consensus. On this view, science is whatever society chooses to call a science. In Western cultures this would include all of the recognized natural and social sciences. (Anderson 1983, p. 26)

On Relativism and the Nature of Science: For Reason

Are there no fundamental differences between sciences and nonsciences? As a point of departure, we should note that relativists cite Alan Chalmers (1976) as supporting the view that "science_1" has never existed and is unlikely ever to exist in the future. This is, I suggest, a fair reading of *What Is This Thing Called Science?* (Chalmers 1976). However, like Kuhn before him, Chalmers repudiated his 1976 views on objectivity, truth, and science_1 in his later work:

> This book is a sequel to *What Is This Thing Called Science?* In that earlier book I subjected some of the standard accounts of science and its methods to a critical

> scrutiny but did not elaborate on an alternative to them in any detail. I have become convinced that some such elaboration is necessary, especially given the extent to which, against my intentions, my position has been read as a radically skeptical one that denies any distinctive, objective status to scientific knowledge. This book contains an extension and elaboration of the argument of its predecessor. I persist in my rejection of orthodox philosophical construals of the so-called scientific method but show how a qualified defense of science as objective knowledge is possible nevertheless. Consequently, I will no doubt be scorned by many philosophers to my right and sociologists of science to my left. . . . I wish [in this book] to resist the subjectivist, relativist response to the critique of empiricism for which, it would appear, I am partly responsible. (Chalmers 1990, pp. xi, 41)

Even though Chalmers has repudiated the science_1 versus science_2 view, we may still inquire whether there are, indeed, differences between the sciences and nonsciences. Stated more precisely, do the knowledge-claims of the nonsciences have an epistemological status equal to the knowledge-claims of the sciences? This is equivalent to asking whether there are good grounds for accepting the knowledge-claims of sciences in preference to the knowledge-claims of nonsciences. An example from Hunt (1984) that uses medical science can provide a clearer perspective on the issue in question.

Suppose your father visited his family physician because he was feeling poorly. Suppose further that the physician conducted some tests and diagnosed your father's condition as a bone cancer that, if left unattended, would probably result in your father's death within a year. Upset with this diagnosis, your father visits his local palmist. The palmist reads your father's palm and tells him that he does not have bone cancer, and that he will live a long life without any medical treatment. The fundamental question here is whether there are good reasons for accepting the knowledge-claim (the diagnosis) of the physician (and acting accordingly) and for rejecting the knowledge-claim (the diagnosis) of the palmist? Answering the question requires asking: Do medical science and palmistry justify their claims about knowledge by equally acceptable methods?

When asked to justify his diagnosis of bone cancer, the family physician would refer to the results of experiments that have found that when the results of certain medical tests are "positive," then the patient usually has a kind of bone cancer. Furthermore, the physician would point out that over time medical scientists have observed that the average life expectancy of someone with this kind of bone cancer is approximately one year. When the palmist is asked to justify his diagnosis, he indicates that his knowledge is based on the "gift of reading." He was born with this "gift," and only others who have been similarly "blessed" with this gift can understand truly his knowledge or powers.

Scientific realists, as well as practicing scientists, would claim that the diagnosis of the physician is better justified than the knowledge-claim of the palmist. That is, mainstream philosophy of science maintains that open empirical testing of the knowledge-claims of medical science provides good reasons for accepting the diagnosis of the physician in preference to those of the palmist. This process is often referred to as simply "intersubjective certification" or "commitment to evidence" (see section 1.5).

Relativists reject the claim that empirical testing provides good reasons for preferring the knowledge-claims of medical science over palmistry. Relativists maintain not only that there is no "unique scientific method" but also that science is "subjective" (Peter and Olson 1983, 1989). Furthermore, because the observations used to test theory are "theory-laden," objective knowledge is impossible (Sauer, Nighswonger, and Zaltman 1982, p. 20; Anderson 1983, pp. 20, 26; Peter and Olson 1983, pp. 121–22; Mick 1986, p. 207; Jaworski and MacInnis 1987, p. 164; Olson 1987, p. 388; Hudson and Ozanne 1988, pp. 515, 518; Holbrook and O'Shaughnessy 1988, p. 401; Firat 1989, p. 95; Peter and Olson 1989, p. 26; Thompson 1990, p. 29; Peter and Dacin 1991, p. 280). Indeed, relativists can point out that many times medical science conducts the same test and the patient turns out *not* to have bone cancer. Thus, sometimes the palmist's diagnosis will be correct and the diagnosis of medical science will be incorrect. They can also argue that terms such as "disease" and even "death" are theory-laden. For example, perhaps what medical science calls a disease is in reality the normal state of affairs. Furthermore, what is to be our definition of "death"? Finally, relativists can contend that science is a "social process" and put forth Feyerabend's (1978b, 1987) argument that the primary reason that many people often accept the diagnoses of physicians over the diagnoses of palmists is that people have been in essence "brainwashed" by the self-serving interests and propaganda of the members of the scientific community. After all, is science not "whatever society chooses to call as science" (Anderson 1983, p. 26)? It would seem, therefore, that relativists must either (1) accept the view that medical science and palmistry have equal epistemological merit or (2) deny their relativism. Or must they?

10.1.2 Revisiting the Nature of Science Arguments

In addressing the medical science versus palmistry example, relativists claim that "we currently have *no* universally applicable criterion by which we can demarcate scientific knowledge from any other kind of knowledge" (Anderson 1989, p. 10). Citing Laudan (1980) as a reference, relativists lament that "unfortunately this is often thought to imply that all knowledge-claims are on an equal epistemic footing". They further claim that "very little follows from the fact that philosophers have been unable to come up with a *universal* demarcation criterion" and argue in favor of "science$_2$—the definition of science by societal consensus" (Anderson 1989, p. 10).

Is it the case that nothing of great importance follows from the demarcation issue? Stated more succinctly, is it the case that "societal consensus" alone constitutes the reason there are astronomy departments in universities but not astrology departments, that there are medical science departments but not palmistry departments? The mainstream philosophy of science view is that it is not just "societal consensus," but rather that the societal consensus is backed by good reasons. Embedded within the medical science versus palmistry example in the preceding section is both an argument for relativism and one for mainstream philosophy of science. As discussed in section 7.3, it is often useful to reconstruct (or partially formalize) arguments for purposes of clarifying and evaluating issues.

The "nature of science argument" according to the relativist point of view of science may be summarized concisely (i.e., reconstructed) as follows:

R1. *There are no fundamental differences separating the sciences and the nonsciences.* ("The search for [demarcation] criteria that separate science from nonscience" . . . has been "signally unsuccessful" [Anderson 1983, p. 18]. Since there are no objective criteria separating science from nonscience, "science is whatever society chooses to call science" [Anderson 1983, p. 26].)

R2. *The knowledge-claims of the nonsciences have as much epistemological warrant as the sciences.* (That is, we have no good reason to believe and act on the knowledge-claims of the sciences in preference to the nonsciences. Statement R2 is *logically implied* by R1 because, if we had good reasons to believe and act on the knowledge-claims of the sciences, such reasons would constitute "fundamental differences" that could be used to separate science from nonscience.)

R3. Therefore, statement R2 implies that if a palmist should diagnose a person as *not* having bone cancer (an example of a nonscience knowledge-claim), such a diagnosis would have equal warrant as the diagnosis of a medical doctor that the person *did* have bone cancer (an example of a science knowledge-claim).

A reconstruction of the nature of science argument according to mainstream philosophy of science would be:

M1. *There are fundamental differences separating the sciences and nonsciences.* (Sciences differ from nonsciences in their method of verifying knowledge-claims.)

M2. *The knowledge-claims of sciences have greater epistemological warrant than the knowledge-claims of the nonsciences.* (Because the knowledge-claims of the sciences are intersubjectively certifiable through open empirical testing, people have good reasons for accepting such claims and acting upon them in preference to the knowledge-claims of nonsciences.)

M3. Therefore, M2 implies that if a palmist should diagnose a person as *not* having bone cancer and a medical doctor should diagnose that same person as *having* bone cancer, the person has good reasons for believing the diagnosis of the medical doctor and acting accordingly. (Palmistry has not, among other things, adopted the verification system of open empirical testing, and, therefore, is not a science.)

Relativists have reviewed the narrative versions (as detailed in Hunt 1984) of the two natures of science arguments and lamented that empiricists often cast "proffered alternatives" as "relativistic flights of fancy that lead to epistemological anarchy and the abandonment of rationality and objectivity" (Anderson 1986, p. 156). Relativists do not deny that the second version accurately reflect the views of mainstream philosophy of science. Nor do they conduct a detailed analysis of the relativist version and point out logical flaws or empirical inadequacies. Rather, they dismiss the argument as a "straw

man." To avoid the impression of possibly mischaracterizing their reasons for dismissing the argument, I quote at length:

> The type of relativism attacked by Hunt (1984) has *never* been advocated by any of the participants in the current debate. The object of Hunt's critique is a straw man known as judgmental (Knorr-Cetina and Mulkay 1983) or "nihilistic" relativism. In this view, all knowledge claims are equally valid and there is no basis on which to make judgments among the various contenders. (*Indeed, a careful reading of even the most radical of contemporary relativists will reveal that this does not even approximate their views,* e.g., Collins 1975, 1981; Feyerabend 1975, 1978a). (Anderson 1986, p. 156; italics added)

Is it true that the above argument (indicating that relativism inexorably leads one to be indifferent between the claims of palmistry and medical science) is a "straw man"? Is it truly the case that "a careful reading of *even the most radical* of contemporary relativists will reveal that this does not even *approximate* their views"?

10.1.3 Is the Relativist Nature of Science Argument a Straw Man?

Because Feyerabend is used as an example of a relativist who would not subscribe to the so-called nihilistic relativism presented in the relativist argument, we shall examine Feyerabend's views on the subject. As discussed in Hunt (2003a, section 4.2.2), Feyerabend is one of the most prominent and widely cited supporters of a relativist view of science (Suppe 1977a). Indeed, all of the relativist writers in marketing and consumer research draw heavily from his works for intellectual sustenance, reference him liberally, and even refer to him as a relativist "hero" (Olson 1987). In *Science in a Free Society* (Feyerabend 1978b), he addresses and answers many of the criticisms of his work from other philosophers of science. He is quoted here at length (to avoid the possibility of a paraphrase mischaracterization) concerning his answer to a critique by a philosopher named Tibbets:

> [Tibbets] also asks: "If *he* had a child diagnosed with Leukemia would he look to his witchdoctor friends or to the Sloan Kettering Institute?" I can assure him that I would look to my "witchdoctor friends" to use his somewhat imprecise terminology *and so would many other people in California* whose experience with scientific medicine has been anything but encouraging. The fact that scientific medicine is the only existing form of medicine in many places does not mean that it is the best and the fact that alternative forms of medicine succeed where scientific medicine has to resort to surgery shows that it has serious lacunae: numerous women, reluctant to have their breasts amputated as their doctors advised them, went to acupuncturists, faith healers, herbalists and got cured. Parents of small children with allegedly incurable diseases, leukemia among them, did not give up, they consulted "witchdoctors" and their children got cured. How do I

> know? Because I advised some of these men and women and I followed the fate of others. (Feyerabend 1978b, pp. 205–6; italics in original)

Recall that relativists dismiss the relativist nature of science argument on the basis that "a careful reading of even the most radical of contemporary relativists will reveal that this does not even approximate their views" (Anderson 1986, p. 156). The above quote shows clearly that, to the contrary, careful observers of relativists' writings know that the relativist nature of science argument closely reflects the logical implications of their views. How do relativists (such as Feyerabend) reach such extreme positions that even their followers (such as Anderson and others) dismiss relativist views as "nihilistic" nonsense? The answer lies precisely in Feyerabend's fundamental beliefs—as detailed in Hunt (2003a, section 4.2.2)—about the existence of many, equally viable "ways of knowing" that he calls research "traditions." These fundamental beliefs are strikingly similar to, if not precisely the same as, those (supposedly) championed by relativist marketing and consumer researchers.

The discussion in Hunt (2003a, section 4.2.2) of Feyerabend's relativist views on research traditions shows clearly how he arrives at conclusions so extreme as to be labeled "nihilistic" and "a straw man" by Anderson. One might suppose that Feyerabend is simply engaging in academic gamesmanship or sophistry, and that he did not intend for readers to take his views seriously. May Brodbeck notes that relativists believe "science is in effect a game" (Brodbeck 1982, p. 3), which is consistent with the view that "the important question we must ask is how can I convince my colleagues in the scientific community that my theory is correct?" (Anderson 1982, p. 15). As Krausz and Meiland (1982, p. 6) have pointed out, the sophists in ancient Greece, from whom we get the term "sophistry," were among the first relativists in Western intellectual history. These philosophers delighted in weaving incredibly convoluted arguments that *they* knew to be false, but that the *less sophisticated* would (or could) not know.

Clearly, however, the late Feyerabend was not a sophist. The preceding long quote demonstrates that he recognized how extreme his premises were concerning the nature of the scientific "tradition" (the knowledge-claims of the sciences are no better than the knowledge-claims of the nonsciences), and he was willing to accept their logical consequences (palmistry and medical science should have "equal rights" and "equal access to key positions"), no matter how extreme those consequences may appear to others. Most important, he not only accepted intellectually these extreme consequences, but (unlike a sophist) he also acted upon them (referring now to the long quote of Feyerabend in which he indicates that he had both used and referred others to "witchdoctor friends").

Unfortunately, many relativists are not as consistent as Feyerabend. The issue is never whether relativists embrace nihilistic conclusions (like R3), but whether relativist beliefs (like R1) *imply* nihilism. Relativist advocates wish to emphatically proclaim Feyerabend's extreme premises concerning the nature of science. Yet, unlike him, they wish to deny (indeed, pejoratively dismiss) the logical consequences of those premises. Neo-Marxist, "critical theory" relativists have also been forced to address the nihilism/sophistry dilemma:

> Political radicals are quite as dedicated to the concept of privilege as their opponents: they believe, for example, that the level of food supplies in Mozambique is a weightier issue than the love life of Mickey Mouse. The claim that one kind of conflict is more important than another involves, of course, *arguing* for this priority and being open to disproval; but nobody actually believes that "power is everywhere" in the sense that any manifestation of it is as significant as any other. On this issue, as perhaps on all others, *nobody is in fact a relativist, whatever they may rhetorically assert.* (Eagleton 1991, pp. 8–9; italics added)

For the purposes of the present discussion, let us try to salvage a view that might be called "weak-form" relativism (consistent with Anderson's [1986, p. 164] "weak-form incommensurability"). In this view, one wants to avoid the charges of nihilism, sophistry, and/or the pain of logical inconsistency, yet still embrace a relativistic position concerning science. That is, one wants to be coherent, to make sense.

10.1.4 Weak-Form Relativism

Most (but not necessarily all) marketing and consumer behavior relativists disagree with the extreme conclusions on the nature of science held by writers like Feyerabend (such as statement R3 in section 10.1.2). Yet, they wish to embrace Feyerabend-like premises on science (such as statement R1). Is it possible, therefore, to salvage the weak-form relativism championed by them? A satisfactory answer to this question lies not in simply denying that they agree with the extreme conclusions of the relativist argument on the nature of science (that the diagnoses of palmists should be given equal epistemological status to the diagnoses of medical science). To do so is much like killing the messenger who brings bad news. Rather, because the conclusions of any argument are entailed in its premises, advocates of weak-form relativism must reexamine their premises in order to salvage their position, for extreme premises must yield extreme conclusions.

Modifying statement R1 to make it less extreme might be a useful starting place for weak-form relativists. Such a modification might be as follows:

R1a. Although in most cases it is impossible to find fundamental differences separating what society chooses to call "science" from "nonscience," there are isolated instances (like medical science versus palmistry) where such differences are obvious.

Weak-form relativists would then have to identify and state the characteristics that so obviously differentiate science from nonscience in each such isolated instance. Furthermore, they would have to show how these differentiating characteristics lack commonality (each "obvious" instance is totally idiosyncratic) and defend this lack of commonality as a reasonable position. Or, they would have to identify the *common* differentiating characteristics across the "isolated instances" and defend how these commonalities differ in some significant fashion from what practicing scientists and mainstream philosophers of science call the *scientific method* of verification through open empirical testing. Otherwise, weak-form

relativists would be forced to deny one of their most cherished precepts: that science does not differ from nonscience through its method of verifying its knowledge-claims.

Clearly, the preceding procedure for salvaging weak-form relativism would seem to be an extraordinarily difficult task at best, or devastating to the relativist agenda at worst. Therefore, modifying statement R2 as a possibility to salvage weak-form relativism should be examined.

Statement R2 of the relativist argument indicates that the knowledge-claims of the nonsciences have as much epistemological warrant as the knowledge-claims of sciences. Relativists have already made a useful start in the direction of modifying R2 when they note that "society bestows a high epistemological status on science because it believes that science generally functions in the best interests of society as a whole" (Anderson 1983, p. 26). Advocates of weak-form relativism could modify their position on R2 by *agreeing* with society:

R2a. Society does bestow and ought to bestow a higher epistemological status on the knowledge-claims of the sciences than on the knowledge-claims of the nonsciences.

Justifying statement R2a would pose a significant challenge for relativists. Although society may bestow a privileged epistemological position on science capriciously and arbitrarily (as Feyerabend states and Anderson implies in his science$_2$: "science is whatever society chooses to call a science"), weak-form relativists, as professed scholars of science, would have to explain exactly why society ought to bestow a superior position on the knowledge-claims of science. Precisely why is it reasonable to believe that science, in preference to nonscience, functions "in the best interests of society as a whole"? This would seem to imply necessarily that weak-form relativists would have to claim that the procedures that science uses to justify its knowledge-claims are somehow *better.* That is, its "way of knowing" or "tradition" is *superior* or should be *privileged.* But if the verification procedures for scientific knowledge-claims are better than the procedures used by nonsciences, then, on pain of continuing inconsistency, weak-form relativists would again have to give up their belief that there are no fundamental differences separating the sciences from the nonsciences. And therein lies the rub. In order to "save" relativism, weak-form relativists have to destroy it.

Contrary to the verdict of relativists, many philosophers of science continue to believe that there are fundamental differences separating science from nonscience, and these differences provide good reasons for societal consensus (Grove 1985). For example, as noted in section 1.5, in an article in *Philosophy of Science,* Siegel reviewed the demarcation debate and notes that "Laudan's rejection of a unique SM [scientific method] depends on a failure to distinguish between general principles of appraisal and specific instantiations of such principles" [or "techniques"] (Siegel 1985, p. 528). He goes on to note that the methodological criteria of science that constitute collectively the scientific method can best be expressed as a "commitment to evidence," as exemplified by "a concern for explanatory adequacy, however that adequacy is conceived; and insistence on testing, however testing

is thought to be best done; and a commitment to inductive support, however inductive inference is thought to be best made" (Siegel 1985, p. 528). Thus, "science's commitment to evidence, by way of SM, is what justifies its claim to respect—science is to be taken seriously precisely because of its commitment to evidence" (1985, p. 530). However, the preceding conclusions by Siegel about scientific method do not imply that he claims that science is the *only* domain in which the epistemic worthiness of beliefs, hypotheses, or claims may be putatively established on the basis of evidence. Rather, "SM extends far beyond the realm of science proper. But this is only to say that SM can be utilized widely. It is still properly labeled *scientific* method" (Siegel 1985, p. 530).

One of the problems with the entire demarcation issue in the philosophy of science is the word "demarcation" itself. This unfortunate choice of words (by Popper) tends to suggest (connote) that an *unequivocal* judgment can be made in all cases using a single, simple criterion (like "falsifiability"). Borderline cases, such as parapsychology, are then brought forth by relativists as examples that purportedly demonstrate the science/nonscience distinction to be just a societal convention, rather than a societal consensus based on good reasons. The fallacy of such a ploy is pointed out by Grove (1985): most, if not all, genuine and useful categorizational schemata have borderline cases. Should biology dispense with the male/female distinction because some entities share characteristics of both sexes? Should marketing research dispense with the consumer goods/industrial goods categories because some goods at some times can be either? Should advocates of qualitative methods dispense with the trustworthy research/nontrustworthy research distinction just because the truth-content of knowledge-claims cannot be known with certainty? How about rational/irrational? Objective/subjective? Or, as I shall discuss in Chapter 11, truth/falsity?

Should marketing adopt relativism? The preceding analysis should at the very least sound a cautionary note: *Any* philosophy whose underlying premises lead to conclusions so extreme that they are dismissed as "nihilistic" nonsense by the philosophy's own advocates must be considered highly suspect, if not intellectually impoverished. Such a philosophy would seem to poorly serve the needs of researchers of any kind. Nevertheless, Hudson and Ozanne (1988, p. 520) imply that consumer research should strongly consider relativism since it is "based on the premise that every approach to consumer research may have something to offer." Hudson and Ozanne (mistakenly, as Muncy and Fisk [1987], Vallicella [1984], Margolis [1983], and Hacking [1982] point out) equate the premises of relativism with diversity, tolerance, and pluralism. But relativism, as Feyerabend (1975, 1978a, 1978b, 1987) has so forcefully argued, implies epistemological anarchy, not a tolerant epistemological pluralism. And just as ethical relativism does not imply the tolerance of ethical diversity (Harrison 1982), and just as political anarchy does not imply a tolerant, pluralistic democracy, epistemological anarchy does not imply a tolerant, pluralistic science.

10.2 THE POSITIVISM VERSUS QUALITATIVE METHODS CONTROVERSY

We turn now to a second controversy in the philosophy debates: Should researchers adopt qualitative methods because positivism is dead? It is clear that studies in the social sci-

ences, marketing, management, and consumer research are increasingly using qualitative methods or "interpretivism" as alternative "ways of knowing" (Hudson and Ozanne 1988; Ozanne and Hudson 1989). Examples include naturalistic inquiry (Belk, Wallendorf, and Sherry 1989), humanistic inquiry (Heath 1992; Hirschman 1986), ethnographic methods (Arnould and Wallendorf 1994; Daft and Lewin 1990; Sherry 1983; Van Maanen 1993), historical methods (Fullerton 1987), "enchanted inquiry" (Monieson 1988), critical theory (Denzin 2001; Dholakia 1988; Murray and Ozanne 1991), semiotics (Holbrook and Grayson 1986; Mick 1986), feminism (Bristor and Fischer 1993; Hirschman 1993; Stern 1993), literary explication (Stern 1989a, 1989b), deconstructionism (Stern 1996), existential-phenomenological methods (Thompson, Locander, and Pollio 1989), and postmodernism (Firat and Venkatesh 1995; Sherry 1991).

Many, if not most, of the advocates of the various qualitative methods argue for their own preferred qualitative approach by noting that it represents an alternative to the "positivist research" that supposedly dominates marketing, management, and consumer research.[2] Thus, Van Maanen (1993, p. 224) maintains that "Ethnographers are learning to overcome . . . the conceits of positivism," and Putnam (1993, p. 233) states that both "ethnography and critical theory stand in opposition to positivism." Similarly, Daft and Lewin (1990, p. 2) decry the "normal science" paradigm in organizational studies, which they equate with "positivism," and liken to a "straitjacket" that restricts theory and research. More specifically, most advocates of qualitative methods use some version of the "positivism is dead" argument[3]:

1. Positivist research (i.e., research guided by the tenets of logical positivism) dominates marketing, management, and consumer research.
2. Positivist research is the same thing as quantitative research, and is causality seeking, adopts determinism and the machine metaphor, is realist, reifies unobservables, and is functionalist.
3. Positivism has been shown to be dead (or thoroughly discredited) in the philosophy of science.
4. Therefore, all research that is quantitative, causality seeking, and so forth, is also discredited.
5. Therefore, researchers should adopt some form of qualitative or "interpretivist" method.

As to the "positivism is dead argument," not only do some participants contend that their opponents' "criticisms largely reflect misrepresentations and misunderstandings" (Calder and Tybout 1989, p. 205), "misconceptions" (Peter and Olson 1989, p. 25), and "honest misunderstandings" (Anderson 1989, p. 11), but others contend more strongly that the debate is full of "mischaracterizations and caricaturizations" (Hunt 1989a, p. 185) or, even worse, "nastiness and purposeful distortions" (Hirschman 1989, p. 209) and "ridicule" (Pechmann 1990, p. 7). This section's underlying thesis is that these "misses" (misconceptions, misunderstandings, misrepresentations, and mischaracterizations) stem, at least in part, from the fact that much of the debate has been historically ill-informed about the origins and

fundamental beliefs of a group of philosophers called logical positivists. Therefore, this section shows that the first two premises of the "positivism is dead" argument are false. To show this, I use historical method to illuminate and clarify not only what logical positivism was, but what it was not. The purpose here is not to attack qualitative methods, for I believe such methods have much to offer. (Indeed, this section adopts a qualitative method, that is, historical method.) Rather, my purpose is to use historical method to expose the intellectually impoverished state of the now ritualistic "bashing of positivism" rhetoric and to encourage all qualitative researchers to move beyond (rise above?) such historically uninformed—and, when done knowingly, downright shameful—argumentation. I begin by detailing the misconceptions about logical positivism.[4]

10.2.1 Misconceptions About Positivism

As developed in detail in Hunt (2003a, Chapter 3), in the l920s and l930s, a group of German philosophers in Vienna (hence, the Vienna Circle) developed a philosophy—later given the label "logical positivism"—that relied heavily on Machian neopositivism, Humean skepticism, Wittgenstein's *Tractatus Logico-Philosophicus,* and Russell's *Principia Mathematica.* From Mach, the positivists drew their conviction that science should avoid metaphysical concepts and rely exclusively on observables. From Hume, they derived their belief that inductive reasoning is impermissible. For Hume, only the conclusions of deductive logic and the beliefs derived from direct observational experience could be known with certainty. Because, according to "foundationalism," science should restrict itself to knowledge with certainty, inductive reasoning is therefore impermissible. From Wittgenstein, the positivists developed both their famous "verifiability principle" (only statements that can be shown conclusively to be true or false are "cognitively meaningful") and the belief that the objective of philosophical inquiry should be the "critique of language" or "meaning analysis." From Russell, they adopted formal (symbolic) logic as an analytical tool for their meaning analyses.

The objectives of the logical positivists were to (1) help science make sense of the indeterministic nature of quantum mechanics, (2) help science avoid another Newtonian debacle, (3) help draw together or "unify" the various scientific disciplines, and (4) effect a rapprochement between science and the discipline of philosophy. (Under Hegelian idealism in the preceding half-century, philosophy had been, at best, irrelevant to modern science, and, at worst, openly hostile to it.) The positivists were successful in effecting a rapprochement between large portions of the philosophical and scientific communities, and they did explicate and emphasize the commonalities of apparently diverse scientific disciplines. They did, indeed, contribute to developing a philosophy that could accommodate a major interpretation of the indeterminism of quantum mechanics (the "Copenhagen view"), and, at least to the present, there has been nothing in science comparable to the Newtonian debacle (Stove 1982, p. 51). However, misconceptions about the logical positivists and their philosophy abound in the current literature. Here, I focus on five major ones: (1) quantitative methods, (2) causality, (3) determinism and the machine metaphor, (4) reality and reification, and (5) functionalism.

Quantitative Methods

Many writers equate "positivist" with "quantitative." For example, "very simply, the logical positivist view of the world is synonymous with the quantitative paradigm" (Deshpandé 1983, p. 102). In like manner, others claim the "opposing research camps" to be the "relevant" researchers versus the "rigorous" researchers, and contend that the latter value the "quantifiable results" of "positive empiricism" (Dholakia 1985, p. 3). Similarly, Belk, Wallendorf, and Sherry (1989, p. 13) state that "we have not called for the development of quantitative measures because the nature and experience of the sacred may be antithetical to such measurement. The ontological and epistemological assumptions of positivist methods are not sympathetic to the mystical and experiential nature of sacredness." Even Wilk (2001, p. 310), who maintains: "There is no particular reason why a positivist or a humanist cannot use any of the whole range of methodologies available," equates "quantitative" with "positivist."

Is the use of quantitative methods the same thing as adopting positivism? The logical positivists and logical empiricists did hold mathematics and statistics in high regard. Given the positivists' view that quantum mechanics is *just* mathematics, they were sympathetic to quantification in science. (In contrast, at least some advocates of qualitative methods not only avoid the use of mathematics and statistics, but, consistent with Hegelian idealism (see Hunt [2003a, section 2.4]), at times seem hostile to these tools.) Nonetheless, equating positivism with quantitative methods is ahistorical: "A positivist, *qua* positivist, is not committed to any particular research design. There is nothing in the doctrine of positivism that necessitates a love of statistics or a distaste for case studies" (Phillips 1987, p. 96). The distinctive features of positivism, such as the verifiability principle, do not mandate quantitative research.

August Comte (1798–1857) and his colleague Saint-Simon (1760–1825), the nineteenth-century originators of the word "positivism" to describe a philosophical position, actually opposed the use of statistics in sociology. For this reason, Comte coined a new word, "sociology," to distance his "positivism" from the statistical emphasis of what was then called "social physics." Moreover, contemporary qualitative researchers could draw upon such positivist writings as those of Brodbeck to buttress their case for qualitative methods: "The qualitative-quantitative dichotomy is spurious. . . . Although quantification has considerable merit, it is neither a necessary nor a sufficient condition for science" (Brodbeck 1968, pp. 573–74). In short, one can be a very good positivist and not engage in quantitative research, and one can be a very good quantitative researcher and not be a positivist.

Causality

The marketing, management, and consumer research literatures claim that the search for causal relations or causal explanations figures prominently in "positivistic social science." For example, in delineating "alternative ways of seeking knowledge in consumer research," Hudson and Ozanne (1988, p. 512) state: "The positivists, with their

goal of explanation and prediction, place a high priority on identifying causal linkages." Hirschman (1986, pp. 239, 241) contrasts the "positivistic metaphysic" with the "humanistic metaphysic" and claims that the former implies that "elements of reality can be segregated into causes and effects," "first stage causes," "second stage causes," and "third stage causes." Thompson, Locander, and Pollio (1989, p. 134) believe that a broad "set of assumptions underlies the use of positivist methods" and that "these assumptions are manifested in many normative methodological prescriptions," including the prescription that "science should uncover causal laws that explain the functioning of phenomena." Ryan and Bristor (1987, p. 193) claim that the "positivistic approach" emphasizes "causal explanation," and Lutz (1989, pp. 4, 7) asserts that "whereas, the Positivist aspires to causal explanation, the Naturalist eschews notions of linear causality." He further notes that "positivist consumer research" will seek "causal explanation" because the assumption of "real causes" is an "axiom" of the "Positivist paradigm." Ozanne and Hudson (1989, p. 3) claim that a "basic assumption" of positivism is that "real causes exist." Anderson (1989, p. 17), in discussing the "implications for positivism and interpretivism" of Wittgenstein's philosophy, asserts that "perhaps the most damaging implication for positivistic psychology is Wittgenstein's acausalism." He then maintains that positivistic research is discredited because, "on a Wittgensteinian construal, there can be no question of invoking a memory as a cause of behavior" (1989, p. 18). Finally, Wallendorf and Belk (1989, p. 74), in developing criteria for assessing trustworthiness in naturalistic consumer research, point out that "triangulation does not always mean that only one interpretation will emerge," and note that, "given the post-Positivist rejection of the assumption of a single causal reality, this is entirely appropriate."

Statements in the marketing, management, and consumer research literatures notwithstanding, it is historically false that research guided by a positivistic philosophy would seek causal explanations or linkages. It is also false that such research would assume that "real" causes or a "single causal reality" exist. Any research guided by positivism would necessarily avoid both the assumption of causality and the search for "real causes." For example, Brodbeck (1962, p. 250) defended Hempel's "thesis of structural symmetry in explanation" (which states that explanation equals prediction and nothing *more*) by derisively noting that Hempel's critics had adopted the "causal idiom" and that the truth content of "statements like 'C is the cause of E' is problematic." Kyburg (1968, p. 236) claimed that causality is in the "realm of metaphysics" and stated that "it is questionable to what extent causality is of scientific interest." Braithwaite (1968, p. 308) asserted that attempting to identify causal laws would be a "thankless task." Finally, in discussing the concept of causal explanations, Hempel (1965b, pp. 353–54) concluded, "It is not clear what precise construal could be given to the notion of factors 'bringing about' a given event, and what reason there would be for denying the status of explanation to all accounts invoking occurrences that temporally succeed the event to be explained."

The positivists rejected causality because they viewed "cause" as an unobservable, metaphysical concept that violated their Humean skepticism. As noted by Ayer (1959, p. 4), "It is indeed remarkable how much of the doctrine that is now thought to be especially characteristic of logical positivism was already stated, or at least foreshadowed, by Hume."

For the positivists, following Hume, if a scientist observes that phenomena *A* and *B* occur with uniform regularity, there is no way to show deductively anything other than a regularity relationship. In particular, one cannot (and should not) conclude that *A causes B*. Positivism, despite claims to the contrary, does not imply the search for causation.

Determinism and the Machine Metaphor

A third major misconception about "positivistic social science" is that such a science would necessarily be deterministic, machine-like, or mechanistic. Thus, Ozanne and Hudson (1989, pp. 3, 7) contend that a "basic assumption" of the "positivist approach" is that the "nature of social beings" is "deterministic" or, even more strongly, "entirely deterministic." Similarly, Thompson et al. (1989, pp. 134, 137) believe that one of the assumptions that "underlies the use of positivist methods" is the "machine metaphor": "Cartesianism's world view is a mechanistic view in which reality is perceived as a machine-like event determined by forces and constraints." Like the view that positivism implies causality, these views also are ahistorical.

"Determinism" is "the view that every event has a cause. . . . All things in the universe are 'governed' by, or operate in accordance with, causal laws" (Angeles 1981, p. 60). Because of the success of Newtonian mechanics, the philosophy of mechanistic materialism, with its Laplacian, deterministic "machine metaphor," was prominent at the close of the nineteenth century. In tracing the historical origins of logical positivism, Suppe (1977b, p. 10) notes that "by the turn of the century, the three main philosophic positions held in the German scientific community were mechanistic materialism, neo-Kantianism, and Machian neo-Positivism. . . . [However,] relativity theory and quantum theory were thought to be incompatible with all three of these philosophies of science." Because the logical positivists were all German-speaking scientists, primarily mathematicians and physicists, they were well aware that quantum mechanics was incompatible with mechanistic materialism, the machine metaphor, and Laplacian determinism. They were well aware of the "breathtaking revolutionary developments simultaneously taking place in mathematical physics and the foundations of mathematics" (Friedman 1999, p. xiii). Indeed, a primary objective of the Vienna Circle was to develop an alternative to such a view. They did so by creating one that replaced the Newtonian machine metaphor with the view that indeterministic, probabilistic prediction was appropriate for science (Carnap 1966).

In short, in light of the fact that the best that could be accomplished with quantum mechanics is probabilistic prediction (the positivist "Copenhagen view"), such an accomplishment must be acceptable for all science. Again, it should be emphasized that only prediction is sought, not "deeper" or causal explanations. Theories and laws, therefore, must be treated solely as calculation instruments for making predictions. Any research motivated by the "positivistic metaphysic" would view as naively misguided the belief that ultimate reality, either physical or social, follows the Newtonian/Laplacian "machine metaphor." Steven Hawking, the prominent physicist and originator of the big-bang theory, specifically acknowledges the debt that his (nonmachine metaphor) position owes to the positivists.

> You can say that the use of imaginary time [to explain the origins of the universe] is just a mathematical trick that doesn't tell us anything about reality. . . . But if you take a *positivist* position, as I do, questions about reality don't have any meaning. All one can ask is whether imaginary time is useful in formulating mathematical models that describe what we observe. (Hawking, as quoted in Strong 1990, p. 71; italics added)

Positivism, despite claims to the contrary, does not imply determinism or the machine metaphor.

Reality and Reification

How the positivists viewed the nature of reality (their "ontology") is a fourth area of confusion in the debate. Hirschman (1986, p. 239) contends that the "humanistic metaphysic" proposes that "human beings construct multiple realities," while the "positivistic metaphysic" believes that "there is a single reality composed of discrete elements." Similarly, Hudson and Ozanne (1988, p. 509; italics added) claim, "The positivists tend to take a *realist* position and assume that a single, objective reality exists independently of what individuals perceive. . . . In contrast, the interpretivists deny that one real world exists; that is, reality is essentially mental and perceived." The concept "reification" also figures prominently in the debate on reality: "Positivists reify subjective states and treat them like objects," according to Hudson and Ozanne (1988, p. 515). In much stronger terms, Monieson (1988, p. 7) claims that "positivistic social science" is pernicious because it "reifies social relations so that they . . . are forged into marketable traits, into commodities." Again, these claims are ahistorical.

In philosophy, the study of ontology asks, "What does 'to be,' 'to exist' mean?" (Angeles 1981, p. 198). Although ontology has been perhaps the most prominent area of philosophical inquiry for millennia, space limitations dictate that the analysis here be restricted to certain fundamental issues regarding idealism, realism, and reification.

As detailed in Hunt (2003a, section 2.4), the philosophical view labeled idealism holds that the material world of tangible objects, such as trees and rocks, does not exist unperceived: "All reality is mental (spiritual, psychical). Matter, the physical, does not exist" (Angeles 1981, p. 120). In direct contrast, realism holds that the world of tangible objects exists unperceived: "Philosophical idealism is . . . opposed to realism and is thus the denial of the common-sense realist view that material things exist independently of being perceived" (Acton 1967, p. 110). "Reification" is inextricably associated with the concept "reality." (Both words have the common Latin root *res,* meaning "thing.") Reification implies treating an abstract concept as having a real existence in the same sense that a thing exists: It is "the fallacy of taking abstractions and regarding them as actual existing entities that are causally efficacious and ontologically prior and superior to their referents" (Angeles 1981, p. 243). Given that it is a fallacy by definition, "reification" is customarily used pejoratively in philosophy to indicate that one has somehow improperly treated an abstraction as having a "thing-like" reality. With the preceding distinctions in mind, we can now investigate the ontology of logical positivism.

Were the positivists realists? If, by "realist," we mean only the minimal position that tangible objects, such as what we call "trees" and "rocks," exist independently of our perception and labeling (i.e., they exist "out there"), then the logical positivists were realists. Recognizing that idealism always degenerates into nihilism, sophistry, and/or solipsism,[5] the positivists embraced a minimal realism called "empirical realism" (Manicas 1987, p. 247). But when philosophers today refer to their own positions or to science as "realist," they customarily mean something much stronger than the minimal belief that the existence of trees and rocks is independent of human thought.

As discussed in Chapter 9, although operating under the common rubric of "scientific realism," there are almost as many versions of realism today as there are realist philosophers. Nevertheless, all realists, either explicitly or implicitly, reject Humean skepticism with respect to the ontology of scientific theories: "The basic claim made by scientific realism . . . is that the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" (McMullin 1984, p. 26). Thus, scientific realism contends that the explanatory, predictive, and pragmatic success of a theory provides evidence for the existence of its associated entities, be they observable or unobservable, tangible or intangible, "thing-like" or "nonthing-like." The evidence is not presumed conclusive, in that "success" does not enable us to know with certainty that the entities and structure exist. For example, as discussed in section 9.5.1, the long-term success of viral theory in explaining, predicting, and solving pragmatic problems with respect to diseases provides evidence warranting our belief in the existence of the entity labeled "virus" (a tangible unobservable). Similarly, to the extent that theories in marketing, management, and consumer research incorporating latent constructs, such as "attitude," "intentions," and "beliefs" (intangible unobservables), have been successful in explaining, predicting, and solving pragmatic problems, such evidence provides warrant for believing that these psychological states of consumers and others exist independently of researchers' labeling of them. That is, the psychological states are real.

Did the positivists adopt a realist ontological view? According to how "realism" is most commonly used today in the philosophy of science (i.e., unobservables can exist and are appropriate for theory construction), the positivists were, most definitely, not realists. The positivists, guided by the views of Mach and Hume, viewed unobservables as metaphysical concepts to be strictly avoided. In fact, many philosophers of science actually use positivism's opposition to realism as its major defining characteristic: "A philosophy (of science) [is] positivist if it holds that a scientific explanation must thoroughly eschew appeal to what is in principle beyond experience. . . . By contrast, a realist holds that a valid scientific explanation can appeal to the in principle non-observable" (Manicas 1987, pp. 9–10).

So, the positivists were not realists. We turn now to the reification issue. Did the positivists engage in reification? That is, did the positivists improperly treat abstract concepts as having a real existence in the sense that a "thing" has existence? Most assuredly, they did not. Their Humean skepticism and revulsion toward metaphysics led them to insist that theories must contain only observables, that is, labels for "things."[6] In contrast, those scientists and

philosophers of science guided by scientific realism, because they contend (on the basis of "success") that some unobservables may actually exist, could, at least *potentially,* be faulted for reification. Therefore, the claim that positivist researchers engage in reification—in the philosophy of science sense of reification—is historically false. Exactly the opposite is true: researchers adhering strictly to positivism cannot engage in reification.

A second use of "reification" draws on Marxist social philosophy.[7] Marxist philosophers (e.g., Berger and Luckman 1967; Lukacs 1971) castigate contemporary social science on the basis of charges of "reification" and "commoditization." As Belk, Wallendorf, and Sherry (1989, p. 24) point out, Marxist philosophers see "a general 'drive to commoditization' in capitalist society." This "commodity fetishism" is uniquely associated with capitalism, Marxists contend, because only in capitalism does the production of commodities dominate society. Such societies necessarily, then, reify social relations. For example, the relationship between two people called "love" becomes nothing more than a commodity, a "thing" to be bought and sold. This reification extends to Western science, particularly sociology, which reifies societal institutions so that they are taken to have the causal power to determine how humans behave. Thus, Western science and Western democratic societies assume that societal institutions (e.g., corporations, the Roman Catholic Church, and "bourgeois science") have an existence separate from the people that produce them. Furthermore, and important for Marxist theory, individual human beings are assumed in Western science and societies to be powerless to change these "immutable" institutions.

From the Marxist view, then, "positivistic social science" just means "Western science" or "bourgeois science" in toto, and, therefore, it must engage in reification because this is an underlying characteristic of all aspects of Western democratic societies. Because the objective of Marxist social philosophy is to "liberate the proletariat" from the "false consciousness" that a market economy serves their interests, Marxist philosophers offer the following argument: (1) social science is dominated by positivism, a "dead" philosophy; (2) positivism implies reification; (3) reification is pernicious; therefore, (4) contemporary (bourgeois) social science is not only fundamentally misguided (in adopting a "dead" philosophy), but its reification of social relations is pernicious as well. In marketing, this appears to be Monieson's (1988) argument in favor of his "enchanted inquiry."

Did the positivists engage in reification in its Marxist sense? This seems implausible because, as historians of logical positivism point out, the rhetoric of the positivists had "explicitly Marxist overtones" (Friedman 1999, p. xiii). Indeed, Levin examines this issue and concludes:

> Whether or not positivism contributed to reification in its Marxist sense—which seems doubtful, if only because many positivists were socialists in their political moments—this sense has little to do with any position taken by positivists in the philosophy of science. (Levin 1991, p. 59)

Whether or not contemporary marketing, management, and consumer researchers engage in reification in either its philosophy of science or its Marxist sense is an issue that could

be addressed only by a detailed examination of extant research. We can say, however, that any research guided by positivism, because of its focus on "observables," *cannot* engage in reification.

Functionalism

Is functionalist research positivist? Functionalism as a distinctive research method has its origins in cultural anthropology. Although "function" and "functional analysis" are used in a variety of ways (see section 3.7), functionalism generally seeks to understand a behavior pattern or a sociocultural institution by determining the role it plays in keeping a given system in proper working order or maintaining it as a going concern. Thus, Radcliffe-Brown (1952, p. 179) states that "the *function* of any recurrent activity, such as the punishment of a crime, or a funeral ceremony is the part it plays in the social life as a whole and therefore the contribution it makes to the maintenance of the structural continuity." And Malinowski (1954, p. 132) maintains that "the functional view of culture insists therefore upon the principle that in every type of civilization, every custom, material object, idea, and belief fulfills some vital function, has some task to accomplish, represents an indispensable part within a working whole."

Not only should functionalism not be *equated* with positivist research, but the positivists were sharply critical of functional analysis and the claim that functional explanations constituted a distinctive method. For example, Nagel (1961, p. 527) points out that, although functional or "teleological" analyses are appropriate under certain circumstances in biology, such analyses do not transfer to social systems because "there is nothing comparable in this domain [i.e., society] to the generally acknowledged 'vital functions' of biology as defining attributes of living organisms." Furthermore, "proposed explanations aiming to exhibit the functions of various items in a social system in either maintaining or altering the system have no substantive content, unless the *state* that is allegedly maintained or altered is formulated more precisely than has been customary" (Nagel 1961, p. 530). Therefore, concludes Nagel, "the cognitive worth of functional explanations modeled on teleological explanations in physiology is . . . very dubious" (1961, p. 535).

Similarly, Hempel (1965b, p. 319) analyzes functional explanations and finds them often to be "covert tautologies" because "the vagueness of the qualifying clauses . . . deprives them of the status of definite empirical hypotheses that might be used for explanation or prediction." Hempel analyzes the "law of adaptation to an obvious end" as an example of functionalist laws and concludes that "the 'law' asserts nothing whatever, therefore, and cannot possibly explain any social (or other) phenomena" (1965b, p. 328). More generally, Hempel concludes that "for most of the self-regulatory phenomena that come within the purview of functional analysis, the attribution of purposes is an illegitimate transfer of the concept of purpose from its domain of significant accessibility [in physiology] to a much wider domain, where it is devoid of objective empirical import" (1965b, p. 327). In like manner, Rudner (1966, p. 88) evaluates teleological explanations and concedes

that "the term 'purpose' *may* come to figure essentially in some scientific theories and explanations." Nevertheless, when he analyzes actual functionalist explanations in anthropology, he concludes:

> However, not a single one of the myriad claims in the anthropological literature can be accepted without serious qualification. . . . All too frequently these claims [of functional explanations] may be counted as at most containing some more or less accurate *descriptions,* rather than explanations of specific phenomena, couched in or accompanied by a rhetoric that may be mistaken for explanations by the unwary. (Rudner 1966, p. 108)

Even though the positivists did not believe that functional explanations constituted a distinctive type of scientific explanation, and even though they were sharply critical of the actual output of functionalist research, they did not claim that functionalism was devoid of merit. Rather, the positivists believed that whatever merit functionalism had was to be found in the context of discovery. That is, adopting a functionalist orientation or functionalist set in approaching a research project might have heuristic value in suggesting either relationships among social phenomena or hypotheses to be tested. On the subject of scientific explanation, however, which falls into the context of justification, the positivists believed that functionalism had nothing of value to contribute. Indeed, as previously noted, they believed it was just "rhetoric that may be mistaken for explanations by the unwary" (Rudner 1966, p. 108). Functionalism, therefore, is not positivistic. If marketing, management, and consumer research are dominated by functionalism—a big if, if "functionalism" is construed in its anthropological sense (see Nagel 1961, pp. 522–34)—such inquiry is not positivist.

10.2.2 On Antipositivism: For Reason

In a section entitled "Rampant Antipositivism," Phillips (1987, p. 94) reviews the social science literature and concludes that "there have been many exaggerated claims about the evils of positivism . . . [and] many factual errors are made when researchers refer to [it]." As we have seen, there have been similar errors in the marketing, management, and consumer-research literatures. Antipositivist claims to the contrary, "positivist" does not equal "quantitative." Furthermore, the positivists did not search for causal explanations or causal linkages; they did not adopt or hold that science should adopt determinism or the machine metaphor; they did not have a realist view with respect to scientific theories; they could not possibly have been guilty of reification; and they did not argue for functionalist explanations. Phillips (1987, p. 44) also compares the views of social science antipositivists with the actual positions of the positivists and concludes that "some of the most boisterous celebrants at positivism's wake are actually more positivistic than they realize, or have more in common with the positivists than they would care to admit." As we shall see, a similar situation prevails in the marketing, management, and consumer-research literatures.

Positivism Versus Contemporary Antipositivism[8]

Although often reaching their positions by different routes, those who advocate alternatives to "positivistic social science" actually share many views with their supposed antagonists. First, the positivists explicitly embraced Humean skepticism, and, similarly, many antipositivists are strongly influenced by it (see, e.g., Anderson 1983, p. 19; 1989, p. 11; Hudson and Ozanne 1988, p. 515). Even those who (quite correctly) believe that "the term positivist is used much too loosely to be descriptive of any approach" (Calder and Tybout 1989, p. 200) embrace Humean skepticism. For example, in explaining why the "comparative" approach is superior to the "confirmatory" approach in testing theories, Sternthal, Tybout, and Calder (1987, p. 124) put forth the standard Humean position that it is impermissible to reason inductively from the successful predictions of a theory to its truth-content: "The goal of the confirmatory approach cannot be realized because theories cannot be proven." Similarly, Calder and Tybout (1987, p. 137) argue in favor of "sophisticated falsificationist methodology" by contrasting their view with the "common view of science . . . that empirical data are used to accumulate evidence for a theory until it is proven. This view of science is, of course, unacceptable . . . [because, in part] inductive proof is logically impossible."[9]

Second, antipositivist methodology shares a striking similarity to that of the positivists in the use of meaning analysis. The positivists adopted the tool of formal logic and the goal of meaning analysis to clarify the language employed in the scientific community. Contemporary antipositivists, often under the rubric of "interpretivism," explore how the language of consumers consists of "shared meanings" (Ozanne and Hudson 1989, p. 2). As Hirschman (1989b, p. ix) puts it, "The best definition I have encountered of interpretive consumer research is that presented recently by Holbrook and O'Shaughnessy (1988). They view interpretation as 'the critical analysis of a text for the purpose of determining its single or multiple meaning(s).'"

The similarities notwithstanding, contemporary antipositivists diverge from positivism on several issues. Because the positivists were sympathetic toward both philosophy and science as institutions, a major part of their program was to effect a rapprochement between them. In contrast, some contemporary antipositivists are openly hostile to the institution of science, relying on—as detailed in Hunt (2003a, Chapter 4)—the views of such bitterly antiscience authors as Feyerabend, whose works contend that "Western science has now infected the whole world like a contagious disease" (Feyerabend 1987, p. 297). In discussing the history of science, Firat (1989, pp. 95–96) asks, "Is science an institution we want to preserve?" He concludes that it may be desirable to "erase the accumulation of scientific knowledge" because "science is a political institution [that] has lost its relevance for the solution of present human problems. Thus, the scientific establishment has become anti-change and reactionary."[10] Not only would Firat's assessment of science differ from that of the positivists, the "book burning" implicit in "erasing the accumulation of scientific knowledge" would have been repugnant to them (as well as to most scholars).

A final difference between positivism and contemporary antipositivism centers on two different meanings of "meaning analysis." The English word "meaning" (and, therefore,

"meaning analysis") suffers from systematic ambiguity. It can refer to the *communicative* characteristics of a word, phrase, or "text," and, thus "meaning analysis" can imply the exploration of how such expressions serve this communicative role. However, "meaning" is also used in an evaluational sense, referring to something's importance, value, or significance. The positivists recognized this systematic ambiguity and restricted their meaning analyses to communicative meaning (Rudner 1966, pp. 75–83). Similarly, as previously mentioned, those referring to their research as "interpretivist" often define their area of inquiry as exploring communicative meaning, as do those using the label "semiotics": "[Semioticians] investigate the sign systems or codes essential to all types of communication for the latent rules that facilitate sign production and interpretive responses" (Mick 1986, p. 197). However, contemporary antipositivists, in contrast to the positivists, have not, in general, focused their meaning analyses on communicative meaning. In fact, most of their published works have actually focused on *evaluational* meaning. A prominent example is the extensive work of Belk and colleagues, which explores how things become either "sacred" or "profane," and they thereby acquire "personal meaningfulness" (Belk et al. 1989, p. 31), "transcendent meaning in their lives" (p. 32), or "significance" (p. 32) through a "process of meaning investment and divestment" (p. 2).

10.2.3 Paradigm Dominance in Marketing, Management, and Consumer Research

The preceding enables us to reexamine premises 1 and 2 in the "positivism is dead" argument introduced at the beginning of section 10.2. To this point, our analysis reveals not only that "positivism" does not imply "quantitative," but also: (1) Mesmerized as they were by Humean skepticism, the positivists considered the concept "cause" to be metaphysical and (at best) superfluous to science. (2) Absorbed as they were with quantum mechanics, they did not contend that science should be deterministic or adopt the machine metaphor. (3) Obsessed as they were with restricting scientific theories to nonmetaphysical observables, the positivists did not adopt a "realist view" with respect to scientific theories. And (4) they could not possibly have been guilty of reification. Therefore, premise 2 is false. Furthermore, if antipositivist writers are correct that *marketing, management,* and *consumer research* are dominated by the search for causality, by the machine metaphor, by reification, and by the realist view with respect to unobservables, then such research is "antipositivist," or, more accurately, "nonpositivist."[11] Thus, premise 1 is also false. In conclusion, the argument underlying much of the debate has been woefully uninformed: Contemporary marketing, management, social science, and consumer research are neither motivated by the "positivistic metaphysic" nor, most assuredly, "dominated by logical positivism."

10.2.4 The Dominance of Positivism: A Postmodern View

Firat discusses the major themes of "postmodern culture," and, drawing on the Marxist social philosophy of Baudrillard, advocates adopting "hyperreality":

> A second major reason for the attacks on science is clarified in the postmodernist critique and analysis of culture. . . . Currently, the most powerful articulator of this recognition [that social reality is produced or constructed] in the postmodernist literature seems to be Baudrillard. . . . His theory articulates the creation of the culture of "hyperreality," a reality that is constructed based on signifiers that become separated from their original referents and "free floating," [to] which, then, are attached new meanings. Thus, new symbols are created as well as a new reality independent of the original referents or foundations. Hyperreality is produced when these new symbols are accepted as real and so acted upon by the social actors. (Firat 1989, p. 94)

On the basis of postmodernism, then, one might attempt to sanction the use of the term "positivism" stripped from its "original referents" (what the positivists actually believed in and advocated) and use it to refer to any set of beliefs or methods one chooses. The only restraint is rhetorical success, that is, whether this new "constructed reality" is "accepted as real and so acted upon by the social actors." Thus construed, "positivism" becomes a pejorative term of rhetorical abuse that can effectively stifle discussion and critique. If one's work is subjected to critical evaluation, rather than defending the research on its merits, one can shut down all discussion by retorting: "You're just using outmoded positivistic criteria to evaluate my post-positivistic, postmodern study." Phillips (1987, p. 94) reviews the debate in the social sciences and concludes that many writers do appear to be socially constructing a hyperreality: "Without suggesting that those who make the errors are deliberately dishonest, it seems as if the word 'positivism' arouses such negative feelings that it is often used in a blanket way to condemn any position at all that the writer disagrees with, irrespective of how positivistic that position really is." Likewise, Friedman (1999, p. xiii) finds logical positivism to be, essentially, an "intellectual scapegoat" for uninformed scholars.

A major problem with the use of "positivism" as a socially constructed hyperreality dominating marketing, management, social science, and consumer research is that it violates the integrity of history. Lavin and Archdeacon (1989, p. 62) review the advantages of historical method and discuss its two "central imperatives": "Respect the integrity of the past and call attention to what was unique about it." No one disputes that the positivists were real people in an identifiable historical period and that they shared some common views. One may argue that some of their views were correct and others incorrect, or some well reasoned and others poorly thought out. One may further argue that some of the consequences of positivism were good for science and society and others were bad for both. Nevertheless, historical integrity and respect for those who can no longer speak for themselves obligate us (at the very least) to criticize the positivists for the beliefs they actually held and the views they actually espoused.

The article by Van Eijkelenburg (1995) represents an example of the contemporary postmodernist approach. He reviews the version of the "positivism is dead" argument in Hunt (1994c) and concludes that he sees "no reason to accept this criticism" (Van Eijkelenburg 1995, p. 209) of the argument because "it does not make sense . . . to condemn a specific usage of words by arguing that it does not conform to the meaning

originally attached to the term" (1995, p. 209). As I replied in Hunt (1995a), I regret that Van Eijkelenburg "sees no reason" to maintain the integrity of history with respect to "positivism." Indeed, I see at least three reasons.

First, the purpose of using language is to communicate with others. Although "positive" is a common English word, the word "positivism" is a term of art from philosophy, just as "Marxism" and "Nazism" are terms of art from political economy. If scholars use the term "positivism" in methodological discussions with the idiosyncratic disclaimer, "of course, when I use 'positivism' I do not refer at all to the positions of the logical positivists or logical empiricists," they jeopardize communication—just as would discussions that use the terms Marxism or Nazism followed by "of course by 'Marxism' I do not refer to its traditional sense related to a certain nineteenth-century economist" or "by 'Nazism' I do not refer to its traditional sense related to certain Germans during the 1920s, 1930s, and 1940s." Communication, I argue, is important in academic discourse. Is it, or should it not be, in postmodernist discourse?

Second, if one chooses to use the term "positivist" idiosyncratically and then alerts readers that one is doing so with a disclaimer, then readers will know that the "positivism is dead" argument for qualitative methods has lost all probative force. Note carefully that the persuasiveness of the argument depends crucially on premise 3 (i.e., positivism in its traditional, philosophical sense is dead or discredited). Probative force, I argue, is important in academic discourse. Is it, or should it not be, in postmodernist discourse?

Third, if writers know that premise 2 is false in the argument (i.e., that positivism is not the same thing as quantitative methods, and so forth), and if at the same time they know that their readers will *not* know this fact, then it is sophistry—if not prevarication—to use the "positivism is dead" argument without the idiosyncratic disclaimer. Although sophistry and prevarication are widely practiced in many areas of human interaction, I argue that they are inappropriate in academic discourse. Are they not, or should they not be, unacceptable in postmodernist discourse?

10.2.5 Logical Empiricism as the Dominant Paradigm

If logical positivism is not the dominant paradigm in marketing, management, social science, and consumer research, what is? Some might be tempted to conclude that, if not logical positivism, then surely logical empiricism must dominate such research. However, there is as much confusion in the philosophy debates about logical empiricism as there is about logical positivism.

Confusions About Logical Empiricism

Arndt (1985, p. 16) claims that, in logical empiricism, "marketing systems are viewed as being equilibrium-seeking," and that "the real world is considered essentially as harmonious and conflict-free in the long run." However, there is nothing in logical empiricism that implies that marketing systems are necessarily "equilibrium-seeking." Similarly, there is nothing in logical empiricism that implies that the world must be considered

"essentially as harmonious and conflict-free." It is true that some researchers choose to treat marketing systems as equilibrium-seeking, and it is true that some researchers either choose to treat the world as conflict-free or choose not to have conflict as a central theme of their research. (Indeed, relationship marketing focuses on cooperation, rather than conflict [Berry and Parasuraman 1991; Christopher, Payne, and Ballantyne 2002; Grönroos 2000; Gummesson 2002a, 2002b; Hunt, Arnett, and Madhavaram 2006; Hunt and Morgan 1994; Morgan and Hunt 1994; Sheth and Parvatiyar 1995b].) However, there is nothing in the nature of logical empiricism that suggests, implies, demands, or impels researchers to deemphasize conflict.

Arndt (1985, p. 12) also claims that the influence of logical empiricism has resulted in marketing being an "applied discipline concerned with the improvement of management practice and research methodology." However, logical empiricism's emphasis was on the positive, that is, "what is." In contrast, marketing management's emphasis is on the normative, that is, what one "ought to do." Therefore, it is uninformed, if not absurd, to claim that logical empiricism leads to an emphasis on the "improvement of management practice."

Contrasted with Arndt (1985), consider Dholakia's (1988, p. 13) claim that the emphasis on marketing management has actually *prevented* the discipline from being dominated by logical empiricism. As to this claim, readers should note that it is obviously the case that the marketing discipline has historically been interested in problems concerning the improvement of marketing management. Furthermore, a good case can be made that the marketing discipline should be more concerned with societal, or "macro," issues concerning marketing. Moreover, as discussed in Chapter 1, an exclusive focus on marketing management (the profit/micro/normative dimensions in the "three dichotomies model") may retard the development of theories that explain and predict marketing phenomena (the profit/micro/positive dimensions). Thus, to focus exclusively on marketing *management* may retard the growth of marketing *science.* Therefore, Dholakia's claim that marketing's emphasis on managerial issues has thwarted the dominance of logical empiricism makes sense *only* if one equates "logical empiricism" with "science." However, such an equating is not just false, but also an equating that is specifically denied by Dholakia and other antipositivists, relativists, and postmodernists. Such arguments, therefore, are incoherent.

As another example of a common error concerning logical empiricism, Anderson (1983, p. 19) states: "logical empiricism is characterized by the inductive statistical method. On this view, science begins with observation, and its theories are ultimately justified by the accumulation of further observations." As an illustration, Anderson cites and discusses the well-known PIMS studies. Although it is true that empirical testing and observation play significant roles in logical empiricism, it is not true that logical empiricism implies "the inductive statistical method," where "science begins with observation." That is, whereas the classical empiricism of Bacon (see Hunt 2003a, section 2.3.1) viewed science as inducing generalizations from observations, logical empiricism (because it separated the context of discovery from the context of justification) did not propose a method for generating hypotheses, laws, and theories. Rather, it focused exclusively on the justification of hypotheses, laws, and theories.

Does Logical Empiricism Dominate?

Logical empiricism was a research program that emerged directly from the logical positivist movement. Indeed, prominent logical empiricists, such as Rudolph Carnap (1934) and Herbert Feigl (1969), had themselves been logical positivists. The major characteristic distinguishing logical empiricism from logical positivism was the substitution of the "testability principle" for the "verifiability principle." Because they accepted Humean skepticism with respect to induction, the positivists came to realize that the generalized nature of scientific laws rendered them "unverifiable," that is, incapable of being proved conclusively true. Therefore, Carnap (1936) replaced the verifiability principle with the "testability principle," which requires that all statements must be observationally "testable" to be "cognitively meaningful" (for them to have done otherwise would have implied the absurd claim that scientific laws were meaningless). However, what is most important for our purposes here is that the logical empiricists continued to embrace Humean skepticism, continued to reject realism with regard to theories, laws, and explanations, and continued to consider causality to be a metaphysical concept that (at best) was superfluous to science. Therefore, on the very same grounds as for logical positivism, we may conclude with great assurance that logical empiricism does not dominate marketing, management, social science, and consumer research.

The issue of the dominant paradigm in marketing, management, and consumer research can be addressed best by first returning to the work of Kuhn (see Hunt 2003a, section 4.2.1), whose *Structure of Scientific Revolutions* (1962) originated the view that each mature science has a dominant paradigm, which consists of (1) a knowledge content (i.e., theories, laws, concepts, symbolic generalizations, and "exemplars"), (2) a methodology (i.e., procedures by which knowledge is to be generated), and (3) an epistemology (i.e., criteria for evaluating knowledge-claims). The construct "dominant paradigm" assisted Kuhn in his efforts to explain, among other things, why mature sciences progressed and others (e.g., the social sciences) did not. For Kuhn, each "mature" science has a dominant paradigm, and, rather than engage in endless, unproductive disputation on methodological and epistemological issues, researchers spend their time making progress by fleshing the dominant paradigm out through "puzzle solving."

Like almost all the major views in *Structure,* that of the "dominant paradigm" or *Weltanschauung* has been totally discredited. For example, Laudan (1977, pp. 74, 151) reviews the history of science and concludes, "Virtually every major period in the history of science is characterized both by the co-existence of numerous competing paradigms, with none exerting hegemony over the field, and by the persistent and continuous manner in which the foundational assumptions of every paradigm are debated within the scientific community. . . . Kuhn can point to no major science in which paradigm monopoly has been the rule, nor in which foundational debate has been absent."

If paradigm dominance has been found to be absent in the historical development of even mature sciences, one would hardly expect to find a dominant paradigm in marketing, management, and consumer research, areas of inquiry whose systematic study is only several decades old. Indeed, as pointed out by numerous observers, the history of

marketing, management, and consumer research is best characterized by the open, often indiscriminate, borrowing of disparate methods and theories from everywhere.

10.2.6 Conclusion: For Reason

Because premises 1 and 2 in the "positivism is dead" argument are false, the "standard argument" (Hunt 2002a) for qualitative methods is false. It is no wonder, then, that participants in the philosophy debates have complained of "misses" (i.e., misconceptions, misunderstandings, misrepresentations, and mischaracterizations). The rhetoric of positivism bashing, so common in discussions justifying qualitative studies, actually degenerates into nothing more than simply the bashing of contemporary marketing, management, and consumer research. Indeed, the term "positivism" in the social science literature has become just a convenient term of abuse. As philosophers of science have noted, this way of using "positivism":

> is not a mere terminological confusion. It is so tendentiously inaccurate that *positivist* . . . becomes a term of abuse. . . . In reality, logical positivism was the most self-critical movement in the history of philosophy. Every major objection to positivism was proposed by positivists themselves or associates at work on problems set by positivism, all in the scientific spirit of seeking truth. It is particularly unfortunate that the technical failure of particular positivist doctrines is so often used . . . to cover an attack on clarity and science itself. (Levin 1991, pp. 63–64)

Because it is so well known that positivism's technical failures have resulted in its being discredited or even "dead" in the philosophy of science, attacking contemporary marketing, management, and consumer research by labeling it "positivist" has, no doubt, been rhetorically successful in gaining converts to qualitative research. But there is a price to pay when academic communities justify historically false argumentation on the grounds of rhetorical success.

If qualitative researchers knowingly (and *knowingly* is key here) justify their methods by using rhetoric that violates the integrity of the past and constitutes sophistry or prevarication, this prompts *users* of qualitative research to wonder: Are the results reported in qualitative studies *also* untrustworthy? Academic integrity is worth safeguarding. Rhetoric has consequences. Communities of academic researchers have fiduciary responsibilities to their colleagues, to other academics, to students, and to society at large. The price paid for false rhetoric is the potential destruction of trust, both (1) among academics and (2) between academics and each of their client publics. This price, I suggest, is too high—it is also a price that it is unnecessary to pay.

The rhetoric of positivism bashing is, I argue, unnecessary for justifying qualitative methods. That is, it is unnecessary for qualitative researchers to discredit quantitative research in order to justify their own studies. Disciplinary change does not require the kinds of techniques associated with Kuhnian revolutions, that is, destroying one's (perceived) adversary. Qualitative research can "stand on its own two feet." Furthermore, we should

recall that a Kuhnian *rival* paradigm is one—like Copernicus vs. Ptolemy—that makes conflicting knowledge-claims, and, therefore, requires *choice.* In this philosophy of science sense of "rival," qualitative and quantitative methods are not even rivals. They are not, or at least should not be, *adversaries.* Rather, sometimes qualitative studies add to what we know from quantitative research, and sometimes it is just the reverse. Therefore, rather than *rivals,* qualitative studies *complement* quantitative research.

Is it not time for recognizing the complementarity of qualitative and quantitative methods? Is it not time for recognizing our responsibility for producing trustworthy knowledge, whether it be from qualitative *or* qualitative methods? Is it not time for advocates of both qualitative and quantitative methods to move on? Is it not time for a truce between qualitative and quantitative researchers or, at the minimum, a rhetorical "cease-fire"? I argue for a "yes" on all these questions.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. In contrast to Anderson (1983), who holds that logical empiricism is characterized by the "inductive statistical method," Arndt contends that the opposite is true: "A corollary of this view [empiricism] is that the hypothetico-deductive method of the unified science is elevated into being the only acceptable scientific approach" (1985, p. 9). Arndt also states that logical empiricism assumes: "Marketing relations have a concrete, real existence and systemic character producing regularities in marketing behavior." Furthermore, "marketing systems are viewed as being equilibrium-seeking," and "the real world is considered essentially as conflict-free" (1985, p. 12). Evaluate these descriptions of logical empiricism. If you conclude that Arndt is not talking about logical empiricism, then what is he talking about? What set of circumstances has resulted in Arndt's making these kinds of assertions?
2. "Many critics claim to attack positivistic science. However, because most of science is not 'positivistic' in any true sense of the term, such critics are actually attacking science itself." Evaluate this thesis.
3. Paul Meehl, at a conference on social science, states the following:

> It was agreed that logical positivism and strict operationism won't wash. . . . The last remaining defender of anything like logical positivism was Gustav Bergman, who ceased to do so by the late 1940's. Why, then the continued attack on logical positivism and its American behaviorists' near-synonym "operationalism"? My answer to this is unsettling but, I think, correct. Our conference on social science came about partly because of widespread dissatisfaction about the state of the art, and we have always been more introspective methodologically than the physicists and biologists, who engage in this kind of thing only under revolutionary circumstances. My perhaps cynical diagnosis is that one reason the conference members spent needless time repeating that logical positivism and simplistic operationalism are incorrect views of

> scientific knowledge is that this relieves scientific guilt feelings or inferiority feelings about our disciplines. It is as if somebody said, "Well, maybe clinical psychology isn't up to the standards of historical geology or medical genetics, let alone theoretical physics; but we need not be so fussy about our concepts and empirical support for them because logical positivism, which was so stringent on that score, is a nefarious doctrine and we are no longer bound by it." (Meehl, in Fiske and Shweder 1986, pp. 315–16)

Evaluate the thesis of Meehl. To what extent does it or does it not apply to marketing?

4. What is the most appropriate philosophy to guide marketing? Marketing science? Why?
5. J. Paul Peter contends: "While logical positivism is the dominant philosophical force in consumer research, it has been eschewed by philosophers since the late 1960's" (Peter 1983, p. 384. Evaluate. In what way or ways has it been eschewed?
6. In the first long quote in section 10.1.1 is the sentence: "On this view, science seeks to discover 'the truth' by the objective methods of observation, test, and experiment" (Anderson 1983, p. 26). In section 10.1.4, there is the sentence: "In order to 'save' relativism, weak-form relativists have to destroy it." Why are there quotation marks around "the truth" in the first quote and "saves" in the second? Should there be? Why?
7. Evaluate the view, stated at the end of the chapter, that there should be a "truce" between qualitative and quantitative researchers. Also, note that the preceding sentence has "truce" in quotation marks, but in the text, there are none. Why?

NOTES

1. Hunt (1976a, 1976b) assumed that "causal structures" was implied by "uniformities and regularities." I now make "causal structures" explicit, to avoid ambiguity.

2. The belief that contemporary social science, marketing, and consumer research are dominated by positivism is a consensus position (Anderson 1986, p. 155; Arndt 1985, p. 11; Belk, Sherry, and Wallendorf 1988, p. 467; Deshpandé 1983, p. 101; Firat 1989, p. 93; Hirschman 1986, p. 237; Hirschman 1989a, p. 209; Holbrook, Bell, and Grayson 1989, p. 35; Holbrook and O'Shaughnessy 1988, p. 402; Hudson and Ozanne 1988, p. 508; Lavin and Archdeacon 1989, p. 60; Lutz 1989, p. 3; Mick 1986, p. 207; Ozanne and Hudson 1989, p. 1; Peter and Olson 1983, pp. 111, 118; Thompson, Locander, and Pollio 1989, p. 134; Venkatesh 1989, p. 101).

3. See also the "standard argument" discussion in Hunt (1994b, 2002a, section 2.3.3).

4. The arguments in this section follow those developed in Hunt (1989a, 1989b, 1991a, 1991b, 1994b, 1994c, 1995a).

5. Nihilism is "the theory that nothing is knowable. All knowledge is illusory, worthless, meaningless, relative, and insignificant." Sophistry holds that "victory in argumentation at whatever cost, outwitting opponents, is the sole aim of disputation, no matter how bad the argument." Solipsism is "the theory that no reality exists other than one's self" (Angeles 1981, pp. 188, 265, 266).

6. For the positivists, as detailed in Hunt (2003a, sections 3.2 and 3.3), theories could also contain "theoretical terms," but all such terms must be explicitly and completely definable through "corre-

spondence rules" to "observables." Therefore, a "theoretical term" is a shorthand way of talking about a collection of observable terms and *nothing more.* Most especially, a theoretical term does not refer to anything unobservable.

7. I am indebted to Michael E. Levin of the Department of Philosophy, City College, City University of New York, for pointing out the Marxist view of reification. I hasten to add, however, that Levin does not subscribe to this view.

8. For the limited purpose of this section, "antipositivist" seems a more appropriate label than either "qualitative" or "interpretivist." For example, many writers believe that "interpretivism" does not properly characterize their views. They rightly point out that all research necessarily involves interpretation. The fundamental issue is whether all research is *just* interpretation. Those (mis)labeled as "positivist" insist that their inquiry touches base with a reality that is outside their paradigm or theory. Antipositivists at times seem to claim that all research, including by implication their own, is linguistically encapsulated and paradigm bounded. That is, it is *just* interpretation, nothing else.

9. For a discussion of the implications of using "proof" as a criterion for "acceptable" science, see Hunt (2003a, sections 3.4 and 5.1).

10. Here, Firat is clearly referring to all science, not just social science, or, even more narrowly, "positivistic" social science.

11. For the record, I believe that causality and the realist view with respect to unobservables are prominent—but not dominant—in social science research. I reject the charges of determinism, the adoption of the machine metaphor, and reification.

11

ON TRUTH AND MARKETING RESEARCH

A general respect for truth is all that is needed for society to be free.
—Michael Polanyi

Consistent with the views of the sixteenth- and seventeenth-century founders of modern science, all the major schools of thought in philosophy of science in the first six decades of the twentieth century held the pursuit of truth in high regard, including the classical realism of G.E. Moore and Bertrand Russell, the pragmaticism of Charles Sanders Peirce, the logical positivism of Moritz Schlick and Otto Neurath, the logical empiricism of Carl Hempel and Ernst Nagel, and the critical rationalism (falsificationism) of Karl Popper. Though differing greatly in numerous respects, all these philosophical "isms" held that it is possible for science to develop genuine knowledge, or truth, about the world. As detailed in Hunt (2003a, Chapter 4), this "traditional image of science" was challenged dramatically in 1962 by Thomas Kuhn's *Structure of Scientific Revolutions.* In *Structure,* Kuhn came to the same conclusion about truth as did Protagoras in his debate with Socrates (see Hunt 2003a, section 2.1.3) some twenty-five centuries earlier:

> One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. . . . There is, I think, no theory-independent way to reconstruct phrases like "really there"; the notion of a match between the ontology of a theory and its "real" counterpart in nature seems to me illusive in principle. (Kuhn 1970b, p. 206)

Relying heavily on the works of Kuhn and Feyerabend, advocates of relativism and postmodernism in marketing, management, social science, and consumer research have raised significant issues regarding the role of truth as either an objective or a regulative ideal in marketing research. This chapter analyzes the "truth controversy" in marketing's philosophy debates: what role, if any, should *truth* play in marketing research? This chapter argues *against* the view that truth should be abandoned and *for* the view that truth is, and ought to be, an important objective and regulative ideal.

Prior to the advent of relativism (and approaches such as postmodernism), truth was considered to be an overriding, central goal of marketing theory and research. For example:

> When confronted with any theory, ask the basic question: *Is the theory true?* Less succinctly, to what extent is the theory isomorphic with reality? Is the real world actually constructed as the theory suggests, or is it not? (Hunt 1976a, p. 130; italics in original)

Marketing's traditional view has been derided as a "fairytale" version of research that is "outdated" (Peter and Olson 1983, pp. 122–23). The fairytale version, it is argued, should be replaced by relativistic/constructionist truth, which leads some relativists to claim: "Truth is a subjective evaluation that cannot be properly inferred outside of the context provided by the theory" (Peter and Olson 1983, p. 119). Other relativists, particularly advocates of "critical relativism, rather than arguing that relativistic truth should just replace the traditional view, disdain any role at all for truth. Indeed, they urge its abandonment: "I have made it quite clear that 'truth' plays no role in the ontology of critical relativism" (Anderson 1988a, p. 4). Therefore, "the foregoing has demonstrated that 'truth' is an inappropriate objective for science, and that consumer research will do well to abandon such a quixotic idea" (Anderson 1988b, p. 405).

Should "relativistic truth" be accepted? Is the pursuit of truth an inappropriate goal for marketing theory and research? Does critical relativism, with its abandonment of truth, provide the most appropriate philosophical foundations for marketing theory and research? This chapter explores these questions by (1) examining the nature of truth, (2) discussing the scientific realist approach to truth, (3) evaluating the argument for relativistic truth, (4) exploring whether truth should be abandoned in marketing theory and research, (5) showing how those opposing truth in marketing commit the "philosophers' fallacy," (6) discussing the implications of the differences between TRUTH and truth, (7) exploring the relationships among truth, reality relativism, and idealism, and (8) arguing for truth on the basis of trust.[1]

11.1 THE NATURE OF TRUTH

In viewing marketing science as a truth-seeking enterprise, we conceptualize *truth* as not an entity, but an *attribute.* It is an attribute of both beliefs and linguistic expressions. For example, it is an attribute of such linguistic expressions as those denoted by the labels "theories," "laws," "hypotheses," and "propositions." The correspondence theory of truth provides that the truth-content of an expression is the extent to which *what* the expression refers *to* does, in fact, correspond with reality. Although versions of the correspondence theory of truth can be traced to Aristotle (Agazzi 1989), the defense of correspondence theory has come to be associated closely with its articulation in formal logic by Tarski (1956). Using Tarski's famous example, the sentence "snow is white" is true *iff* (if and only if) *snow is white.* In this Tarskian "biconditional," what is within quotes refers to a linguistic expression, and what is italicized refers to an aspect of reality that is independent

of the language used to form the linguistic expression. That is, "A statement (proposition, belief, . . .) is true if and only if what the statement says to be the case actually is the case" (Alston 1996, p. 6).

Briefly, as interpreted by scientific realism, the correspondence theory of truth holds that an assertion such as "gas flames are hot" is true if there is in fact something like a real entity corresponding to "gas flames" that has the corresponding attribute, "hot." In contrast, the coherence theory of truth holds that an assertion is true if it follows from, is consistent with, or coheres with another statement or system of statements that is believed to be true. That is, because "all flames are hot" is true, so is "gas flames are hot." Many relativist and postmodernist writers adopt what is called the "consensus theory of truth" (e.g., Lincoln and Guba 1985), which holds that there can be no objective criterion of truth. If the consensus of a group of people is that an assertion is true, then it is true. For example, if a group believes that gas flames are not hot, then it is true that they are not hot, and nothing more can be said on the subject. That is, there is no objective truth to any assertion independent of group beliefs.

A marketing example from Hunt (1992a) illustrates the correspondence theory of truth. Suppose a marketer wants to test two, genuinely rival, explanatory theories (i.e., two theories having conflicting knowledge-claims). Suppose further that both theories entail the concept "intentions to buy," which is measured on a scale from "1" equals "definitely will not purchase" to "10" equals "definitely will purchase." After being asked to mark the box that "best describes your beliefs," a subject puts a checkmark on the questionnaire. The coder of the questionnaire then reports, "The subject checked the ninth box."

For the correspondence theory of truth, "The subject checked the ninth box" linguistic expression is true if and only if the subject actually checked the ninth box. Furthermore, when the example is interpreted by scientific realism, the subject checking box 9 may (if our measurement theory is good) be an indicator (albeit, a fallible indicator) of the subject's actual intentions to buy. For the consensus theory of truth, "the subject checked the ninth box" is true if the coder believes (or claims to believe that) the subject checked the ninth box, irrespective of the subject's actual behavior. How, then, does scientific realism approach truth?

11.2 TRUTH AND SCIENTIFIC REALISM

The preceding section introduced the correspondence theory of truth. This section develops a scientific realist model of truth that focuses on the successes and failures of empirical tests. To articulate the model, we use an example from consumer research.[2] Heath and Chatterjee (1995) review the effects of decoys on consumer choice sets. A decoy is an option introduced into a choice set that causes preference reversals between two other options in the set. These preference reversals are referred to as "attraction effects theory," and they contradict standard economic models of individual choice, which customarily assume all preferences to be independent of irrelevant alternatives.

Heath and Chatterjee (1995) identify five theoretical issues and assess the extent to which ninety-five empirical tests (found in fifteen articles) support the five hypotheses

of attraction effects theory. One hypothesis they examine is: "Decoys will reduce shares of lower quality competitors more than they will reduce shares of higher quality competitors" (p. 270). They report that the fifteen articles they review contain ninety-two tests in which product attribute quality is the independent variable and competitor product market share is the dependent variable (see their Table 1). Of the ninety-two tests, "decoys reduced shares of lower-quality competitors 50 percent of the time (18/36) but reduced shares of higher-quality competitors only 11 percent of the time (6/56)" (p. 277). Comparing each combination of the higher-quality product share reduction (change in the C^d variable in their Table 1, where $n = 56$) and lower-quality product share reduction (change in the C^d variable in their Table 1, $n = 36$), 1,568 of 2,016 (78 percent) possible comparisons support the quality hypothesis, 406 of 2,016 (20 percent) are counter to the hypothesis, and 42 of 2,016 (2 percent) of the possible comparisons show no statistically significant differences. We use the findings of Heath and Chatterjee (1995) on the product attribute-quality hypothesis, in particular, and the implications of their findings for attraction effects theory, in general, as continuing examples to illustrate the scientific realist model of truth.

11.2.1 A Scientific Realist Model of Truth

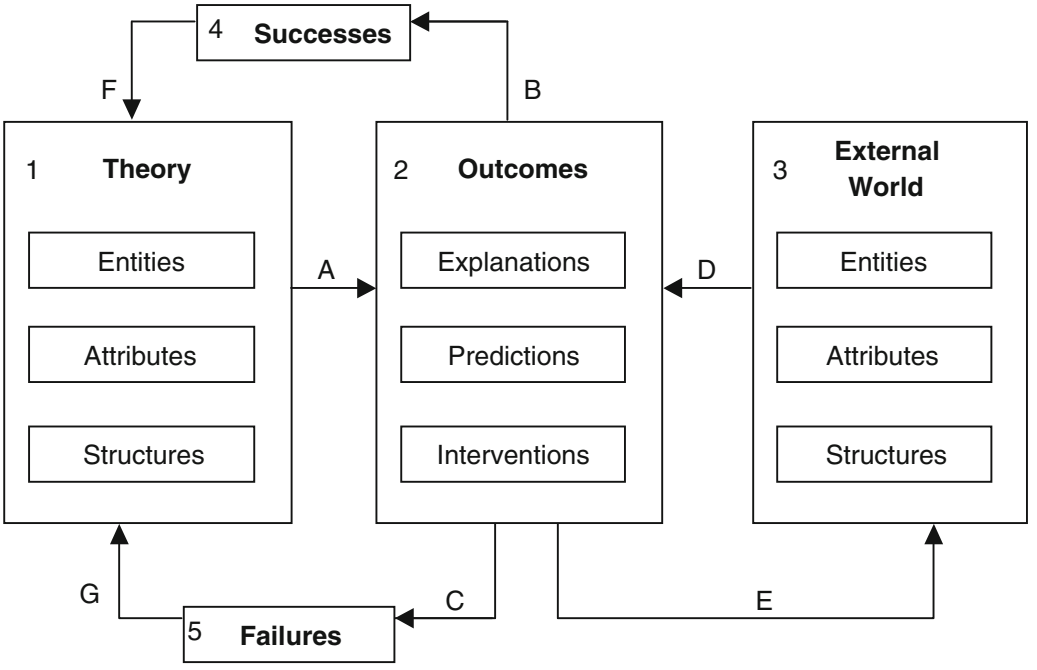
Recall that scientific realism, in viewing marketing research as a truth-seeking enterprise, conceptualizes *truth* as not an entity, but an *attribute.* It is an attribute of both beliefs and linguistic expressions. For example, it is an attribute of such linguistic expressions as those denoted by the labels "theories," "laws," "propositions," and "hypotheses." Also recall that the inductive realism tenet of scientific realism maintains that the long-run success of a theory gives reason to believe that something like the entities and structure postulated by the theory actually exists. Figure 11.1 is a model that explicates the meaning of "something like theory *X* is likely true" and "something like theory *X* is likely false" in the scientific realism approach to science.

Successes, Failures, and the External World

Assume that box 1 in Figure 11.1 contains the linguistic expression denoted by "attraction effects theory." Attraction effects theory posits entities (e.g., the entities labeled "goods," "brands," and "people"), attributes of entities (i.e., the identifiable characteristics or properties of goods, brands, and people—e.g., loss aversion, higher quality, lower quality, and market share), and structures (e.g., the proposition that there is a negative relationship between the attribute-quality level of a firm's product and the product's resistance to attacks from competitors' decoys). The theory (i.e., attraction effects theory) posits that the entities, attributes, and structures referred to in box 1 exist in the world external to the theory (i.e., box 3). That is, the linguistic expressions that constitute the theory in box 1 are *about* the world in box 3.

Path A, from box 1 to box 2, shows that some theory (e.g., attraction effects theory) has certain implications or outcomes. That is, the theory can be used to explain some

Figure 11.1 **Truth and Scientific Realism: Successes, Failures, and the External World**



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phenomena. For example, why did fewer consumers purchase the decoy product when the competitor product was higher quality? They did so because "in real world markets, price discounts move consumers from lowerquality to higher-quality brands more than from higher-quality to lower-quality brands . . . [and] loss aversion, the tendency for losses to be more unpleasant than equivalent gains are pleasant, appears to be greater for quality than price" (Heath and Chatterjee 1995, p. 270). The theory can also be used to predict some phenomena (e.g., "Decoys will reduce shares of lower-quality competitors more than they will reduce shares of higher-quality competitors"). The theory can also be used to suggest interventions (e.g., to attack competitors' brands, use viable—not dominated—decoys).

Paths B and C show that the theory's outcomes are sometimes successful (box 4) and sometimes not (box 5). For example, as to explanations, some competitors that have lower-quality product attributes lose more market share when decoys are present than competitors with higher-quality product attributes do (which would constitute explanatory successes), but other competitors with lower-quality product attributes lose less market share when decoys are present than competitors with higher-quality product attributes do (which would constitute explanatory failures). As to *predictions,* recall that of the ninety-two tests reported on by Heath and Chatterjee (1995) with product attribute quality level as the independent variable, 78 percent of the comparisons (of the 2,016 higher-quality product versus lower-quality product share changes in the C^d variables in their Table 1)

were successes (box 4) and 20 percent were failures (box 5). As to *interventions,* at times the suggestions of attraction effects theory are right (e.g., sometimes the viable decoys have stronger effects on competitors' market shares), and at times the suggestions are wrong (e.g., sometimes the viable decoys have weaker effects on competitors' market shares). Both the successes and failures are impacted by the entities, attributes, and structures that exist in the external world (box 3), as shown by path D. In turn, the outcomes in box 2 impact (by way of path E) the entities, attributes, and structures in box 3 (e.g., when managers in firms interpret the outcomes in box 2 as supporting the truth of attraction effects theory, and this belief then guides their future patterns of behavior).

What, then, is the import of a high or low proportion of successes (box 4), and a low or high proportion of failures (box 5)? Paths F and G represent inferences from a theory's successes and failures to the truth-content and falsity-content of a theory. For scientific realism, a high proportion of successes, relative to failures, gives reason to believe that something like the entities, attributes, and structures posited by the theory in box 1 (e.g., the hypotheses of attraction effects theory) actually exist in the world external to the theory (i.e., they exist in box 3). That is, we infer that something like the theory posited in box 1 is likely true. The "something like," then, equates with attraction effects theory being "approximately true" or "having truth content." From a high proportion of failures, relative to successes, we infer that something like the theory (e.g., again, attraction effects theory) is likely false. In a sense, paths F and G depict a "weighing" of evidence, as Bunge (1967b, p. 319) so aptly puts it. However, scientific realism, as a theory of science, does not imply that "a high proportion of successes, relative to failures" means "true with probability 'p.'" Likewise, it does not imply that "a high proportion of failures, relative to successes" means "false with probability 'p.'" Indeed, most scientific realists are highly skeptical of efforts that attempt to apply the logic of probability to the weighing of evidence involved in the empirical testing of theories.

Returning to the study by Heath and Chatterjee (1995) on the hypotheses of attraction effects theory, note that the authors frame their conclusions consistent with scientific realism. They find that, as an independent variable, higher-quality product attributes seemed to fare best: "The meta-analysis and experiment add evidence to three lines of research suggesting that it is generally easier to increase share of higher-quality than lower-quality brands" (p. 282). Furthermore, their findings regarding higher-quality product attributes showed that: "Decoys increase shares of higher-quality targets more . . . than they increased shares of lower-quality targets" (p. 274). Moreover, when they, as scientific realism puts it, "weighed" the total empirical evidence, they concluded:

> The asymmetric decoy effects . . . parallel three other asymmetries: (1) asymmetric price competition across lower-quality and higher-quality brands in real-world markets . . . (2) greater loss aversion to quality than to price . . . and (3) compromise brands drawing a larger share from lower-quality than higher-quality competitions. . . . These disparate research streams converge on the same conclusion: It is generally easier to increase share of higher-quality brands than lower-quality brands (1995, p. 282).

Therefore, Heath and Chatterjee (1995) disagree with Allenby and Rossi's claim (1991) that income effects can explain the attraction effects. Indeed, for them, the empirical evidence to date shows that "the fact that the populations would have differed on many dimensions makes it difficult to know exactly which factors mediated the effect" (Heath and Chatterjee 1995, p. 282).

11.2.2 Truth Is Not an Entity

Readers should note that truth is an attribute of beliefs and linguistic expressions, it is not an entity in the external world (i.e., truth is not in box 3 in Figure 11.1). Therefore, *truth* is not an entity that researchers do (or can) study. To treat truth as an entity in box 3 is to engage in reification, that is, "to postulate as an entity fallaciously" (Levin 1991, p. 57). For example, with regard to truth, Anderson (1988, p. 404) asks, "Indeed, how would we know truth even if we held it in our hands?" His query is (one suspects) meant to be taken as just an instance of colorful, relativist rhetoric. Nevertheless, his reification of truth vividly illustrates the conceptual danger of treating an unobservable, intangible concept, such as truth, as if it referred to an observable, tangible object, such as an apple. By wrongly leading us to believe that truth could be held in our hands, his reification of truth, absurdly, leads us to inquire how we could recognize it with our eyes.

A common accusation by relativists is that those holding that science should seek true theories must also reify truth. For example, Zinkhan and Hirschheim (1992, p. 83) maintain that those who hold truth in high regard must assume that "there is an immutable truth out there which scientists can study." However, they provide no quotations of realists or any other evidence of instances of reification—and for good reason: it is likely that no such evidence exists. Truth for scientific realists is not an entity for study, let alone, an "immutable" entity.

11.2.3 Consistent with Marketing Science Practice

The scientific realist model of truth in Figure 11.1 seems consistent with good marketing science practice, as evidenced by the kind of arguments advanced in the review of attraction effects theory by Heath and Chatterjee (1995). This is unsurprising, for many philosophers of science, as well as most philosophically oriented social science researchers, believe that only some version of realism can explain the actual workings of science without reducing it to a shameful charade. For example, because no rational person searches for the characteristics of a nonexisting entity, what other than the warranted belief that the entity labeled "brands" exists could motivate the search for whether (or not) *decoy* brands prompt consumers to adopt the target's or competitor's brands in choice sets? Are we to believe that researchers say to themselves: "Even though I do not believe in the existence of decoy brands, I shall pretend they exist and then pretend to investigate whether (or not) such decoy brands have an impact on the decision to adopt the target's or competitor's brands, whose existence I also shall pretend to believe in?" Although disingenuous activities do take place in science, scientific realism maintains

that the totality of research in the physical sciences, social sciences, and marketing is not best described as an elaborate charade.

11.2.4 Inconsistent with Logical Positivism, Logical Empiricism, and Falsificationism

The scientific realist model of truth in Figure 11.1 is inconsistent with logical positivism and logical empiricism. Even though both the logical positivists and the logical empiricists held truth in high regard, both were under the spell of Humean skepticism with respect to induction (Stove 1982). Therefore, both refused to countenance the real existence of entities that were, in principle, unobservable. Because, for the logical empiricists, only observables are real, the concept of "brands" must be simply a shorthand way of talking about a collection of observable entities. That is, the logical empiricists made a sharp distinction between "theoretical terms" and "observation terms," with only the latter referring directly to some aspect of the world. Theoretical terms would have to be given meaning by being defined through "correspondence rules" with observation terms. However, this posed a major problem for the logical empiricists: the problem of theoretical dispensability. Called the "theoretician's dilemma" by Hempel (1965d), the first half of the dilemma is: if all theoretical terms can be defined through correspondence with observation terms, and if the purpose of science is to determine relationships among observation terms, then theoretical terms are unnecessary in science. The second half of the dilemma is: if theoretical terms *cannot* be defined through correspondence with observation terms, then theoretical terms are meaningless and, *surely,* are unnecessary in science.

For scientific realism, the "theoretician's dilemma" is no dilemma at all. Scientific realism dismisses the theoretical term/observation term dichotomy as a false dichotomy. That is, scientific realism acknowledges that all the terms in a theory are, properly speaking, "theoretical terms." The expression "theoretical term" means nothing more than "a term in a theory." For scientific realism, some terms in a theory may denote something more observable, more detectable, more easily measurable than other terms. In fact, some terms may denote in principle, nothing, observable at all. However, all the terms in a theory (excepting, of course, mathematical and logical terms) can legitimately claim to denote the existence of some entity, such claims being based on (1) the senses (classical realism) and/or (2) the success of a theory (inductive realism).

Also, the model in Figure 11.1 is inconsistent with strict falsificationism. As were the logical empiricists, Popper (1972) was strongly influenced by Humean skepticism (Stove 1982). For Popper (1972, pp. 86, 88; italics added), "I regard Hume's formulation and treatment of the logical problem of induction . . . as a flawless gem . . . a gem of priceless value . . . a simple, straightforward, logical refutation of any claim that induction could be a valid argument, *or a justifiable way of reasoning.*" Thus, Popper (1972), by claiming that all *positive* results of a theory test are irrelevant to science (not a "justifiable way of reasoning"), fell into a form of irrationalism (Stove 1982). In terms of Figure 1, falsificationism would maintain that, whereas the inferences of path F are not a justifiable way of reasoning, the inferences of path G are justifiable. The scientific realist model of

truth—in accord with the actual practice of science—maintains that both paths F and G are defensible.

11.2.5 Not with Certainty

Neither the correspondence theory of truth nor the realist interpretation of correspondence theory, as shown in Figure 11.1, equates "truth" with "truth with certainty." As the realist Siegel (1983, p. 82) puts it, "To claim that a scientific proposition is true is not to claim that it is certain; rather, it is to claim that the world is as the proposition says it is." And the realist Groff argues:

> The conclusion that truth cannot be known with certainty to have been achieved . . . follows from an appreciation of the reality that there is no extracognitive standpoint from which to assess the relationship between scientific propositions and that to which they refer. . . . The metatheoretical implications of this situation, however, should not be thought to be terribly grave. Indeed, the metatheory of truth that I have proposed is nothing other than a statement of what it means to be a fallibilist about knowledge. For how are we to understand fallibilism, except as the belief that the norm of truth may never be conclusively determined to have been met? (Groff 2000, pp. 428–29)

In *Philosophy of the Social Sciences*, Fox (1994) evaluates the confusions in the relativist writings of a philosopher named Raven:

> It seems to be from the *fallibility* of theories that Raven infers we cannot say what the world is really like; for if our accounts were mediated through a theory that was *certainly true,* they would surely be of what the world is really like. Here again, an anti-realist argument seems to depend on a demand for certainty. If this is the reason, it is confused. That something is infallibly known implies that it is true, but not vice versa; that our descriptions are fallible . . . does not imply that they are not what the world is really like. (Fox 1994, p. 507)

There is a confusion similar to Raven's in the marketing literature. Thus, relativism maintains:

> Unless science can know with certainty what truly is the real world, it is impossible to judge *the extent to which* knowledge claims truly represent or correspond to that world. In other words, without independent knowledge of a standard (i.e., what reality truly is), how can scientists know how close they are to reaching the standard? (Peter 1992, p. 73)

The relativist argument, however, represents a classic example of what Harré has labeled the "philosophers' fallacy of high redefinition" (Harré 1986, p. 4). This is the fallacy—

discussed in detail in section 11.5—of defining a concept (e.g., scientific knowledge, truth, falsity, objectivity, or progress) in such a manner (e.g., must be "known with certainty" or "known with probability 'p'") that the concept cannot be realized and then lapsing into relativism, nihilism, or skepticism. Note that Peter (1992) claims that if we cannot "know with certainty what truly is the world," then we cannot know anything about the world. How, we might ask, could relativists *know* that we can never know anything?

The traditional view in marketing theory is that the search for true theories must be distinguished from knowing truth with certainty. As Hunt (1983b, p. 372) put it, "The concept 'certainty' belongs to theology, not science." The relativist argument provides no reason either to (1) abandon the traditional view of truth or (2) equate truth with certainty.

11.2.6 Not Equal to Pragmatic Success

Critics of scientific realism often maintain that, for realism, truth and pragmatic success are equated. As an example from postmodern marketing:

> The issues of "truth" and pragmatic success, however, are [for postmodernism] neither mutually implicative nor inexorably intertwined. . . . The [scientific realism] narrative equation of truth = pragmatic success is one that focuses attention only on the technocratic dimensions of science: that is, the power of science to provide technologies for "controlling" natural and/or social phenomena. (Thompson 1993, p. 331)

However, scientific realism does not maintain that truth and pragmatic success are (a) mutually implicative, (b) inexorably intertwined, or (c) equated. Indeed, Thompson's postmodernism is wrong on four counts. First, note that scientific realism maintains that the long-run success (in terms of explanations, predictions, and interventions) of a theory gives reason to believe that something like the entities and structure posited by the theory actually exists. Therefore, scientific realism, which stresses the fallibility of science, maintains that success *gives reason* for believing a theory has truth-content; it does not claim that success and truth-content are "mutually implicative." Second, *gives reason that something like* implies the fallibility of the existence inference; it does not make the claim of "inexorably intertwined." Third, scientific realism infers truth content, that is, *something like,* from success; it does *equate* the two in the manner of "truth = pragmatic success." (Equating truth with pragmatic success is—see Hunt (2003a) section 3.6.1—a characteristic of the pragmatic theory of truth, not the correspondence theory.) Fourth, for scientific realism, success has three dimensions, successful (1) explanations, (2) predictions, and (3) interventions. Therefore, contra postmodernism, scientific realism does *not* "focus attention *only* on the technocratic dimensions of science" (Thompson 1993, p. 331; italics added).

In conclusion, scientific realism does not equate truth with pragmatic success. The cause of postmodern marketing, it would seem, is served poorly by such, obviously

erroneous, analyses. Griffin (1988) demonstrates that it is possible for postmodern discourse on philosophy of science to be informed by reason. What is lacking in marketing's postmodernism is the will to do so, not the way to do so.

11.3 RELATIVISTIC TRUTH

Several writers argue for a relativistic view of truth to be accepted. The various arguments for relativistic truth may be reconstructed (or partially formalized) as follows:

The Relativistic Truth ("RT") Argument

RT1. All research takes place within paradigms (or conceptual frameworks, research traditions, and so forth).
RT2. Paradigms are "highly encapsulated" (Anderson 1986, p. 158) and "context dependent" (Peter and Olson 1983, p. 119).
RT3. Rational choices across rival paradigms, as Kuhn and Feyerabend have shown, are impossible due to their "incommensurability" (Anderson 1989, p. 19; Ozanne and Hudson 1989, p. 1; Peter 1992, p. 76).
RT4. Therefore, "Truth is a subjective evaluation that cannot be inferred outside of the context [e.g., paradigm or research tradition] provided by the theory" (Peter and Olson 1983, p. 119).

Note that the RT argument draws heavily on the concept of incommensurability in the works of Kuhn and Feyerabend. As detailed in Hunt (2003a, section 4.2), Kuhn and Feyerabend (at various times) offered three different interpretations of what it meant for paradigms to be incommensurable: (1) the *meaning-variance* view that scientific terms change meaning between paradigms, (2) the *radical translation* view that in some meaningful way the terms in one paradigm cannot be translated into the language of its rivals, and (3) the *incomparability* view that rival paradigms cannot be meaningfully compared. Which interpretation(s) do marketing's relativists adopt? Here, two examples illuminate this question.

Ozanne and Hudson (1989, p. 1) compare "interpretivist" research with what they (misleadingly, see Chapter 10) call "positivist" research and conclude that "the knowledge outputs of these two approaches are incommensurable." The evidence for their conclusion comes from their examination of Bower's approach to studying emotion. For Bower, emotion is "a physiological, internal state that is not dependent on the surrounding context," compared with Denzin's "interpretivist approach," in which "emotions are defined as self-feelings, they are feelings of and for one's self" (Ozanne and Hudson 1989, p. 4). Thus, their claim of incommensurability is based on the assertion (which for the present purposes we shall assume to be true) that, for the two researchers, "it was clear that what was perceived to be the phenomenon of emotion changed when investigated" (1989, p. 14). Ozanne and Hudson then buttress their incommensurability claim by citing Shapiro's (1973) work. They report that Shapiro had attempted to integrate "the data

of a more interpretive methodology and the data of a more positivist methodology," and she had concluded that her problems "were the result of her measuring different things" (Ozanne and Hudson 1989, p. 6).

As a second marketing example of (claimed) incommensurability, Anderson (1989) compares the work of Tybout, Sternthal, and Calder (1983) on "multiple request effectiveness" with that of ethnomethodologists, such as Heritage (1984). Anderson argues that the approach of Tybout, Sternthal, and Calder is a kind of "psychological instrumentalism," in which "man is often a psychological dope whose verbal and motor behavior result from the operation of an autonomous central nervous system" (Anderson 1989, p. 21). In contrast, "while ethnomethodologists see man performing his mundane everyday activities in a largely unreflective fashion, there is always the possibility of choice" (ibid.). More important for our purposes, however, he concludes that the "'psychological' and ethnomethodological approaches to this phenomenon are largely incommensurable [because,] while it can be seen that both perspectives 'cover' or 'save' multiple requests, it should also be clear that they do so by completely redefining the subject matter of the problem" (ibid.).

It is clear that both the works of Ozanne and Hudson (1989) and Anderson (1989) are using the meaning-variance interpretation of incommensurability. That is, for Ozanne and Hudson, though Bower and Denzin were using the same term (i.e., emotion), they were not referring to, or measuring, the same phenomenon. Similarly, for Anderson, though Tybout, Sternthal, and Calder and the ethnomethodologists would discuss "multiple requests," the topic would be redefined when investigated. Do the examples of Anderson, Ozanne, and Hudson constitute a nontrivial, interesting, or meaningful kind of incommensurability? Obviously, the (presumed) fact that Bower and Denzin mean very different things when using the same term poses significant problems for them in communicating with each other. Also obviously, Tybout, Sternthal, and Calder might have difficulty communicating with ethnomethodologists. But in what meaningful way does this communication problem imply that the knowledge outputs of these approaches are *incommensurable?*

The concept of incommensurability comes from the classic examples of Kuhn, such as the rival knowledge-claims of Ptolemy and Copernicus: one paradigm claimed that the Sun revolved around the Earth, and its rival claimed that the Earth revolved around the Sun. Clearly, these are two *rival,* or competing, claims. Choice is required. If Ptolemy is right, Copernicus is wrong, and vice versa. The problems between Ptolemy and Copernicus were not, simply, as a result of their using common terms (e.g., "Earth," "Sun," and "revolve") in different ways. In the examples of Anderson and Ozanne and Hudson, however, the researchers are using the same labels (i.e., "emotion" and "multiple requests") to mean very different things. Therefore, the knowledge-claims of Bower and Denzin, as well as those of Tybout, Sternthal, Calder, and the ethnomethodologists, though *different,* cannot be considered *rival,* or *competing.* No choice is required. Therefore, their claims are not *incommensurable* in any meaningful epistemic sense.[3]

In conclusion, the argument for relativistic truth fails because a key premise (RT3) is false. We turn now to the arguments advanced by critical relativists that truth should be abandoned.

11.4 CRITICAL RELATIVISM AND TRUTH

Doppelt's (1978/1982) "moderate" relativism and Laudan's (1987) reticulated model of scientific rationality—see Hunt (2003a, section 5.2)—form the philosophical foundations for marketing's "critical relativism" (Anderson 1986, 1988a, 1988b), with its requirement that truth is inappropriate and should be abandoned. Critical relativists point out that many different cognitive aims have "figured prominently in the history of natural and social science" (Anderson 1986, p. 159). Critical relativism entails "axiological relativism" (cognitive value relativism) because: "Whether those aims are themselves worthy of pursuit will be judged differently by various research programs. However, no 'independent arbiter' of the merits of an axiology can exist as long as the axiology is neither utopian nor inconsistent with the practices of the program" (Anderson 1988a, p. 134).

Critical relativism never attempts "to discriminate genuine from nongenuine knowledge. The bottom-line claim of critical relativism is that some programs deliver on certain axiologies, and others deliver on different aims and objectives" (Anderson 1988a, p. 134). Truth ("genuine knowledge") and falsity ("nongenuine knowledge") are thus absent from the lexicon of critical relativism. Not only is truth absent in critical relativism, but it "is an inappropriate objective for science," and marketing and consumer behavior researchers would "do well to abandon [it]" (Anderson 1988b, p. 405). Critical relativism's case against truth stems from two arguments, the argument from the falsity of realism and the argument from utopianism.

11.4.1 The Falsity of Realism Argument

The claim that realism is false is based on an analysis of "convergent" realism and "motivational" realism. Because the arguments are similar, we focus here on convergent realism. As critical relativists explain it:

> In a nutshell, the main tenets of convergent realism would include the following assertions: 1) "mature" scientific theories are approximately true, 2) the concepts in these theories "genuinely refer" (that is, there really are things in the world that correspond to these concepts), 3) successive theories in a domain will retain the ontology of their predecessors, 4) truly referential theories will be successful, and, conversely, 5) "successful" theories will contain central terms that genuinely refer. (Anderson 1988b, p. 403)

Critical relativists conclude that the theory of convergent realism is false because "we can easily produce historical evidence from the so-called 'hard-sciences' that demonstrates that the fourth and fifth assertions are false" (Anderson 1988b, pp. 403–4). The empirical evidence includes the fact that the atomic theory of the eighteenth century was "singularly unsuccessful" but we now believe it to be genuinely referential. In contrast, such theories as the phlogistic theory of chemistry were "successful in their day" but are now "thought by scientists to be non-referring" (p. 404). Therefore, critical relativists

maintain, because assertions 4 and 5 of the theory of convergent realism "are false," and because "the cognitive aim of 'truth' is linked ineluctably with realism" (p. 403), then truth is an "inappropriate objective for science," and we should abandon it (p. 405).

On the Falsity of Realism Argument: For Reason

Our analysis of the argument from the falsity of realism does not examine the historical episodes used as evidence for the falsity of convergent realism.[4] Instead, it focuses on the structure of the argument itself. Skipper and Hyman (1987, p. 60) point out that many of the scholarly works in marketing are "argument-centered," containing "nothing resembling a rigorous proof, yet the conclusion apparently 'stands to reason' or 'is intuitively obvious' given the premises." Our question here is: Does it stand to reason that marketing should abandon truth because convergent realism is false? Clearly, the answer must be "no."

First, note that scientific realism is a theory of science. It is totally incoherent to claim that truth should be abandoned as a goal for all theories because a particular theory of science (i.e., convergent realism) is false. The claim that the assertions of realism are "false" is unintelligible without the presumption that, under different circumstances, the assertions could have been true. Thus, critical relativism uses the concepts "truth" and "falsity" in the very argument that purportedly demonstrates that truth is inappropriate for science. Such an argument fails even minimal standards for coherence. It makes no sense.

Second, if it is true that the assertions of realism are *false,* as critical relativism maintains, then truth plays a very definite role in critical relativism, which (ironically) constitutes evidence for truth having a role in both critical relativism and science. Another way of stating the preceding analysis is that critical relativism is self-refuting: The argument for critical relativism contains its own refutation. As shown in Hunt (2003a, section 4.1), relativists for over 2,000 years have been attempting to develop a nontrivial, interesting version of relativism that would not be self-refuting. Starting with Socrates versus Protagoras, all attempts have failed (Siegel 1986, 1988). Given two millennia of repeated failures, the fact that marketing's version of relativism is also self-refuting is not surprising.

Demonstrations that relativism is self-refuting are often counterargued on the basis that relativists "simply argue for their positions by employing the intellectual resources that are sanctioned by the 'scientific culture' of the present age and/or by attempting to change the evaluative criteria, aims, or methods of contemporary intellectual discourse" (Anderson 1986, p. 157). Applying this line of reasoning here would mean the following: (1) Though critical relativism contends that truth should be abandoned in science, because (2) most marketing researchers are "country bumpkins" (Calder and Tybout 1989, p. 203), who believe in the value of truth, it is therefore (3) appropriate to rely on truth to demonstrate that truth is inappropriate.[5] Not only is this standard counterargument of relativism an example of disingenuous argumentation, it is also unavailable here on other grounds. In particular, critical relativism explicitly adopts the norm of "reflexivity"

(Anderson 1986, p. 157), which implies that the criteria proposed by critical relativists to explain and understand science must also be applied to critical relativism itself. Therefore, if critical relativism claims that truth is inappropriate for science, reflexivity requires that truth must be inappropriate for critical relativism. Consequently, critical relativism cannot *coherently* claim that any analysis demonstrates a theory to be *false.*

The issue here is not simply a "slip of the pen." It is not that critical relativism uses the words "true" and/or "false." Everyone acknowledges that by a suitable selection of euphemisms and surrogates (e.g., "consistent/inconsistent," "accords with/does not accord with"), careful relativists can avoid the use of the words "true" and "false." It would be insulting to critical relativists for marketing to interpret critical relativism's total cognitive content to be such trivial semantics. Rather, critical relativism is obviously making a substantive claim that the meanings that "stand behind" the terms "truth" and "falsity" are inappropriate for science and should be abandoned. And that claim, as has been shown, is incoherent—it makes no sense.

11.4.2 Reticulational Philosophy and Truth

Critical relativism is grounded primarily in Laudan's work, particularly his reticulated model of scientific rationality (see Hunt 2003a, section 5.2). Therefore, further light can be shed on the argument from the falsity of realism by examining Laudan's philosophy, which, for convenience, we refer to as "reticulational philosophy."

Reticulational philosophy's perspective on truth has its origin in Laudan's (1973) article on the "self-corrective" thesis of Charles Sanders Peirce. However, Laudan's 1977 book, *Progress and Its Problems* (hereafter *P&P*) has an attack on truth as its major focus. *P&P* attempts to replace the search for truth with the search for maximum problem solving as the overriding goal of science: "It has often been suggested in this essay that the solution of a maximum number of empirical problems, and the generation of a minimum number of conceptual problems and anomalies is the central aim of science" (Laudan 1977, p. 111). The history of science and of the philosophy of science reveal, for reticulational philosophy, the hopelessness of the search for truth: "Attempts to show that the methods of science guarantee it is true, probable, progressive, or highly confirmed knowledge—attempts which have an almost continuous ancestry from Aristotle to our own time—have generally failed, raising a distinct presumption that scientific theories are neither true, nor probable, nor progressive, nor highly confirmed" (Laudan 1977, p. 2).[6] Furthermore, the search for truth is utopian, and utopian goals are undesirable: "We apparently do not have any way of knowing for sure (or even with some confidence) that science is true, or probable, or that it is getting closer to the truth. Such aims are *utopian* in the literal sense that we can never know whether they are being achieved" (p. 127). Thus, "determinations of truth and falsity are irrelevant to the acceptability or the pursuitability of theories and research traditions" (1977, p. 120).

In the article "A Confutation of Convergent Realism," reticulational philosophy links modern versions of realism with the search for truth: "If the realist is going to make his case for CER (Convergent Epistemological Realism), it seems that it will have to hinge on approximate truth" (Laudan 1981, p. 29). The article argues that history shows the theory of

convergent realism to be false: "The realist's claim that we should expect referring theories to be empirically successful is simply false" (1981, p. 24). Furthermore, "S2 [a thesis of convergent realism] is so patently false that it is difficult to imagine that the realists need to be committed to it" (1981, p. 25). And, "I shall assume that Putnam and Watkins [two realist philosophers] mean that 'most of the time (or perhaps in most of the important cases) successor theories contain predecessor theories as limiting cases.' So construed, the claim is patently false" (1981, p. 39). Finally, "the realist's strictures on cummulativity are as ill-advised normatively as they are false historically" (1981, p. 43).

The case against truth as a goal for science comes to full flower in *Science and Values* (Laudan 1984). Whereas *P&P* argued that maximum problem solving was the "central aim" of science (Laudan 1977, p. 111), *Science and Values* maintains that there is no overriding goal for science at all: "There is simply no escape from the fact that determinations of progress must be relativized to a certain set of ends, and that there is no uniquely appropriate set of those ends" (Laudan 1984, p. 66). However, not all goals are appropriate for scientific communities. First, the goals or "cognitive values" must not be "utopian" (1984, p. 52). As in *P&P,* Laudan claims that truth is utopian, and, therefore, truth as a goal "cannot be rationally propounded" (1984, p. 53). Second, goals can be evaluated on the basis of "reconciling theory and practice." This congruency criterion allows us to criticize a scientist or a scientific community that claims to be pursuing one goal, their explicit goal, but whose actions and actual choices imply the pursuit of some other implicit goal. Scientific rationality, honesty, and the justified fear of the reprobation of one's peers require goal congruency:

> When we find ourselves in a situation where there is a tension between our explicit aims and those implicit in our actions and judgments, we are naturally under significant pressure to change one or the other, or both. On pain of being charged with inconsistency (not to mention hypocrisy, dishonesty, etc.), the rational person, confronted with a conflict between the goals he professes and the goals that appear to inform his actions, will attempt to bring the two into line with each other. (Laudan 1984, p. 55)

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Like all of Laudan's writings, his arguments against truth are meticulously crafted, engagingly written, and "come alive" with their many historical examples and anecdotes. Unlike the writings of many contemporary philosophers of science, even those who have not mastered modern logic (which would include most of us in marketing) can follow his arguments. Unfortunately, though his case against truth is brilliantly developed, it is fundamentally flawed.

The works of many philosophers suggest that any philosophy abandoning the goal of truth must choose ultimately between incoherence and irrelevance (e.g., Newton-Smith 1981; Watkins 1984). Consider, for example, the societal debate on whether the theory of scientific creationism should be taught in public schools. Defenders of scientific creationism claim that it is a genuine scientific theory and should (at least) be taught in

addition to evolutionary theory. Others claim that scientific creationism is basically a religious theory and (at the minimum) oppose laws that would require it to be taught in public schools. In 1982, a U.S. District Court struck down the Arkansas law requiring scientific creationism to be taught in public schools.[7] The court concluded that scientific creationism was principally a religious theory and not science, which agreed with the many traditional, realist philosophers of science who testified at the trial.

A philosophy relevant to the scientific creationism debate might take the traditional view of pointing out some significant differences among science, nonscience, and religion, and on those grounds argue for or against scientific creationism (see section 10.1). Alternatively, a relevant philosophy might opt for a second traditional view that the empirical evidence is strongly in favor of the truth or falsity of either evolutionary theory or scientific creationism (see Figure 11.1). However, as just discussed, the truth-falsity option is closed to reticulational philosophy. As for the first option, "it is probably fair to say that there is no demarcation line between science and nonscience, or between science and pseudoscience, which would win assent from a majority of philosophers. Nor is there one which should win acceptance from philosophers or anyone else" (Laudan 1983, p. 112). Thus, reticulational philosophy seems to be faced with the choice of incoherence or irrelevance on this societal issue. And it chose incoherence.

In a widely discussed article, Laudan (1982) applied reticulational philosophy to the societal issue of scientific creationism. Though he concluded, "the verdict itself is probably to be commended," it was "reached for all the wrong reasons and by a chain of argument which is hopelessly suspect" (Laudan 1982, p. 16). How should the federal court have justified its ruling? Laudan argues that "to make the inter-linked claims that creationism is neither falsifiable nor testable is to assert that creationism makes no empirical assertions whatever. That is surely *false*" (1982, p. 16; italics added). He then details many of the assertions and claims of creationism and states, "In brief these claims are testable, they have been tested and they have failed those tests" (1982, p. 16). He concludes: "Indeed, if any doctrine in the history of science *has ever been falsified,* it is the set of claims associated with 'creation-science'" (1982, p. 17; italics in original).[8]

As the preceding discussion demonstrates, reticulational philosophy is incoherent. To claim that "determinations of truth and falsity are irrelevant to science" and nevertheless claim that the theory of scientific creationism has "been falsified" by science is incoherent. Indeed reticulational philosophy violates, in the most egregious manner, its own "noncongruency" criterion. Hence, marketing's critical relativism and Laudan's reticulational philosophy (on which critical relativism is anchored) are both unintelligible. In fact, a strong, *prima facie* case can be made that all philosophies that seek to abandon the pursuit of truth in discussions about science will inexorably generate unintelligible, incoherent discourse, or face irrelevancy, or both.

11.4.3 Truth and "Utopianism"

Critical relativism (1) defines truth as "that which is unequivocally the case," (2) refers to truth as a "utopian" goal, (3) insists that utopian goals are "inappropriate" for science, and

(4) concludes that truth should be "abandoned" (Anderson 1988b, pp. 404–5). One might ask: Why could science not choose to pursue a utopian goal? Because, critical relativism claims, "to adopt a goal with the feature that we can conceive of no actions that would be apt to promote it, or a goal whose realization we could not recognize even if we had achieved it, is surely a mark of unreasonableness and irrationality" (p. 404). Setting aside the philosophers' fallacy—see section 11.5—involved in defining "true" as "unequivocally the case," we now address a second question: If truth is utopian and utopian goals are to be strictly avoided, what, then, might be an acceptable goal for marketing science? Unfortunately, the reticulational philosophy on which critical relativism is based provides no guidance as to which goals will pass the utopian criterion; *Science and Values* (Laudan 1984) provides not a single example of an acceptable goal for science. However, as "early" reticulational philosophy argued powerfully that maximum problem solving was the *central* aim of science (Laudan 1977), it might serve as an example.

On Utopianism: For Reason

Basically, *P&P* (Laudan 1977) proposed a counting and weighting procedure to demonstrate the rationality of science and the fact that it was making progress toward the goal of maximum problem solving. *P&P* contended that "the workability of the problem-solving model is its greatest virtue" (Laudan 1977, p. 127). This counting and weighting procedure has been evaluated by Kordig (1980), Krips (1980), Leplin (1981), McMullin (1979), Musgrave (1979), and Sarkar (1981). They point out that applying the model in actual scientific practice would require an extraordinarily complex procedure. Neither Laudan in *P&P,* nor anyone else since, has actually employed this complex procedure; all commentators have concluded that it is manifestly unworkable. In fact, we can be stronger yet in our claim. The counting and weighting procedure that was claimed to be the central aim of science in 1977 is utopian by the very same criterion later employed to dismiss the aim of truth in science. So, we have the highly curious situation of a goal for science slipping from the (claimed) central aim of all inquiry, according to 1977 reticulational philosophy, to an impermissible aim that would have to be abandoned on the basis of the utopian criterion and 1984 reticulational philosophy.

How would other potential aims for science stand up to the utopian criterion? For example, how about falsifiability, parsimony, explanatory power, fruitfulness, mathematical elegance, and so forth? If each were defined in "high redefinition" fashion (we must know "unequivocally" that a theory is falsifiable, or has maximum explanatory power, or has the greatest fruitfulness), then all these aims would be utopian and impermissible. But this conclusion, again, is nihilism and would not be countenanced by anyone who wants to talk meaningfully about science. (And critical relativism has—most appropriately—expressly adopted the goal of being non-nihilistic [Anderson 1986].) Therefore, we should examine more closely why the utopian criterion fails.

First, Stern (1989a) has suggested that in evaluating the rhetorical force of argumentation in marketing, both the denotative and connotative meanings of terms must be examined. Using this procedure enables us to recognize that the choice of the word "utopian" has "loaded the semantical dice." To see this, compare the meaning of "utopian goal"

with that of "visionary goal." Both are denotative synonyms (Morehead 1985), implying an aim that is probably unrealizable, yet "utopian" connotes images such as impractical, hopeless, foolish, or quixotic, whereas "visionary" connotes lofty, exalted, or highly desirable. However, if critical relativists had advocated the abandonment of all *visionary* goals, a criterion that would have had the same denotative meaning as the abandonment of utopian goals, the proposal would, for most scholars, have lacked initial plausibility.

Second, critical relativism confuses the short-run, tangible, realizable objectives of a societal institution with its long-run aims, regulative ideals, or mission. Consider higher education as a societal institution. Though it has many "realizable" objectives (e.g., increasing the number of student credit hours, increasing the number of volumes in the library, decreasing the heating bill, and so forth), no one would claim that these realizable objectives constitute the *mission* of higher education. Or, consider "the law" as a societal institution. Historically, the pursuit of justice has served as a regulative ideal for our legal system. Obviously, a mission such as justice cannot always be "cashed out" in a set of completely unambiguous practices and procedures. Yet, does this imply that the visionary ideal of justice should be abandoned? How about "utopian" personal goals, such as honesty, fairness, ethics, and morality? Should all these, as Feyerabend (1975) implies, be abandoned as well? On the contrary, rather than advocating that utopian (visionary) goals should be abandoned, a better case can be made that the overriding goals worthy of pursuit, both in personal relations and in science, are likely to be utopian (visionary).

11.5 THE PHILOSOPHERS' FALLACY REVISITED

The works of Adler (1985) and Harré (1986) can help us understand how so many scholars, in both philosophy and marketing, generate philosophies that produce unintelligible discourse. Adler details ten key mistakes that have plagued philosophy for centuries, one of which is to define "knowledge" in such an exacting and circumscribed manner that knowledge becomes impossible for anyone to attain, and to conclude, therefore, that all knowledge claims are "mere opinion." As mentioned in section 11.2.5, Harré (1986, p. 4) addresses the same fallacy, which he labels the "philosophers' fallacy": the "fallacy of high redefinition." Basically, the philosophers' fallacy is to take a meaningful and useful term (e.g., "truth," "knowledge," "progress," "objectivity," or "science") and subject it to such a "high redefinition" that the term no longer can be applied to anything. For example, if "knowledge" and "truth" must be known with certainty, it becomes easy to demonstrate that truth and knowledge do not exist. Harré shows that much of the irrationalism found in the works of Kuhn and Feyerabend (as detailed by Stove 1982) have their origins in the philosophers' fallacy.

To consider Kuhn and the philosophers' fallacy, Kuhn claims that he "always meant" for "incommensurability" to be defined as equivalent to a logical or mathematical proof (Kuhn 1970a, p. 260). Finding no such *proofs* available to empirical science, his philosophy collapsed into relativism and irrationalism. Similarly, Feyerabend sought "absolutely binding principles" (Feyerabend 1970, p. 21) and "general rules that would cover all cases" in relation to the concepts "truth," "reason," and "morality" (Feyerabend 1978b, p.

117). Finding no such universal principles for guaranteeing the accomplishment of these aims, he characterizes truth, reason, and morality as "abstract monsters," and his relativism counsels us to allow them to "wither away" (Feyerabend 1975, p. 180). In short, if one cannot know with certainty (high redefinition) that one can apply a concept (truth, reason, morality), it should be abandoned. Such nihilism, Harré maintains, is "absurd" (Harré 1986, p. 62).

At least in part, critical relativism and the reticulational philosophy on which it is based produce unintelligible discourse on science because of the philosophers' fallacy. For example, "If 'truth' is properly defined as that which is unequivocally the case, then *there can be no criterion* for absolute truth. Indeed, how would we know truth even if we held it in our hands?" (Anderson 1988b, p. 404; italics in original). Note that truth should be "properly" defined as that which is "unequivocally the case." With such a high redefinition of truth, the slide into incoherence and irrationalism is to be expected. Similarly, critical relativism demands a "universal demarcation criterion" to justify distinguishing among science, nonscience, and pseudoscience (Anderson 1989). Then, if no *universal* criterion can be supplied, "science is whatever society chooses to call a science" (Anderson 1983, p. 26). But such a set of beliefs implies nihilism. That is, one cannot distinguish between astronomy and astrology (Feyerabend 1975), medical science and palmistry (see Chapter 10), or, as shown here, science and religion.

11.6 Truth AND TRUTH

Scientific realism distinguishes between truth and TRUTH, that is, between "this proposition is true" and "I know with certainty this proposition is true." In like manner, it distinguishes between "science is objective" and "science has the objectivity of a God's-eye view," that is, between "objectivity and OBJECTIVITY." The philosophy debates in marketing often fail to make these distinctions. For example, Peter sees no distinction between "to know" and "to know with certainty," because "Surely, 'to claim that the world *is* as the proposition says it *is*' appears to suggest that the world is *unequivocally* or *certainly* as the proposition says it is" (Peter 1992, p. 72). (With unintended irony, we should note, Peter begins his assertion with "Surely.") What, then, are the epistemological differences between "truth" and "TRUTH"?

Table 11.1 displays a continuum of perspectives on truth, a label for the epistemological position underlying the perspective, and a series of exemplars.[9] For example, starting from the right, dogmatism claims to have found TRUTH. Dogmatists not only know that truth is findable but they have found the one and only truth, unequivocally, certainly, or surely. Furthermore, their TRUTH is not to be questioned. In the philosophy of science, both vulgar absolutism and scientism are exemplars, where the former refers to a set of beliefs and a set of principles for generating beliefs based on a unique privileged framework that produces incorrigible truth (Siegel 1987, p. 162), and the latter refers to "the unwarranted idolization of science as the sole authority of truth and source of knowledge" (Angeles 1981, p. 251). Exemplars of dogmatism in political philosophy include Nazism and Marxism-Leninism.

Table 11.1

The Truth Continuum

Dogmatic skepticism (TRUTH)	Humean skepticism (truth)	Fallibilism (truth)	Dogmatism (TRUTH)
Academic skepticism	Logical positivism	Scientific realism	Vulgar absolutism
Solipsistic skepticism	Logical empiricism	Critical pluralism	Scientism
Relativism	Critical rationalism	Naturalism	Fundamentalism
Idealism	Falsificationism		Theocracy
Subjectivism	Instrumentalism		Marxism-Leninism
Constructionism			Nazism
Deconstructionism			Fascism
Deconstructive postmodernism*			
Neo-Marxism			
Critical theory			
Radical feminism*			

Source: Hunt (2002c). Reprinted by permission of author.

*Note that it is only *deconstructive* postmodernism and *radical* feminism that fall into the dogmatic skepticism category.

At the extreme left of Table 11.1, we find the TRUTH of dogmatic skepticism. "Skepticism" comes from the Greek *skeptesthai,* which means "to examine," "to look carefully about," and the Greek *skeptikos,* which means "thoughtful" and "curious." Therefore, skepticism can be a healthy attitude of suspending doubt, pending thoughtful, reflective examination. However, dogmatic skepticism claims to have incorrigibly, certainly, surely found the one and only TRUTH, that is, there is no truth to be found. Both academic skepticism, the belief that "there is but one thing one can know, namely that one can know nothing else" (Watkins 1984, p. 3), and solipsistic skepticism, the position that all one can know is "(a) that one exists and (b) that one is having certain ideas" (Angeles 1981, p. 262) are philosophy of science examples of dogmatic skepticism.

It seems that marketing versions of relativism have embraced academic and solipsistic skepticism, and their one TRUTH: Nothing is knowable (because of incommensurability, the theory-ladenness of observation, and so forth). Curiously, however, throughout marketing's philosophy debates we find assertions such as "objectivity is *impossible,*" "scientists do not *discover* anything about the world," "intellectualization in marketing is *inexorable,*" "the results of research based on hypothetico-deductive method are *illusory,*" "reality is *all* mental and perceptual," and so on. Unlike the knowledge claims found in traditional marketing research (where, quite properly, cautions of the "do not overgeneralize" variety abound), the claims of relativists, postmodernists, and others are customarily put forth with bold certitude, great emphasis, and total lack of equivocation. As Calder and Tybout (1989, p. 203) question, how can relativist writers *know* such claims? If the absence of a "God's-eye view" supposedly defeats even highly qualified stochastic propositions or tendency laws (see section 6.3 on tendency laws), what unique privileged position justifies the certitude of relativists' sweeping generalizations? The answer is, of course, there can be none: relativism is best described as dogmatic skepticism.

Between the extremes in Table 11.1 lie the acceptance of "truth" by those embracing Humean skepticism and fallibilism. In the philosophy of science, both the logical positivists and logical empiricists embraced Humean skepticism, whereas scientific realists reject it. Although science can exist within a positivist framework (as the "Copenhagen" interpretation of quantum mechanics and behaviorist psychology attest), most scientific disciplines and their research programs require a much bolder ontology than positivist "observables" (Levin 1991; Manicas 1987). Indeed, many science scholars (e.g., Fay 1988) contend that, positivist rhetoric notwithstanding, even those researchers and research programs that *claim* to be "positivist" are mostly realist. One reason is that the ontology of scientific realism has heuristic value for generating new hypotheses and theories, whereas positivism and the "constructive empiricism" of Van Fraassen (1980) are heuristically impotent (Levin 1991). At any point in time science can be interpreted in a positivist manner. But, to move forward, science (at least most of science) seems to require realism.[10]

11.6.1 Postmodernism and Dogmatic Skepticism

It must be emphasized that, though radical feminism and postmodernism are grouped under dogmatic skepticism, not all researchers who refer to themselves as feminists and postmodernists embrace extreme skepticism.[11] For example, there are splinter groups within the postmodernist research tradition who neither disdain truth nor embrace postmodern, "methodological babble" (Calder and Tybout 1989), or "epistobabble" (Coyne 1982).

Consider the "constructive postmodernism" of Griffin (1988). Griffin notes that "the term *postmodern* is used in a confusing variety of ways." For him, "deconstructive or eliminative postmodernist . . . thought issues [or results] in relativism, even nihilism" (Griffin 1988, p. x). In contrast, his "constructive postmodernism" endorses the "notion of truth as correspondence, . . . [in that] science can lead to ways of thinking about the world that can increasingly approximate to patterns and structures genuinely characteristic of nature" (Griffin 1988, p. 29).

Although Griffin rejects "scientism" as dogmatic (see Table 11.1), his "constructive or revisionary postmodernism involves a new unity of scientific, ethical, aesthetic, and religious institutions" (Griffin 1988, p. x). This new unity requires retaining not just "science's concern for truth," but also "the principle of noncontradiction" (1988, p. 30). Under this principle, incoherent arguments must be rejected:

> If two statements contradict each other, both cannot be true. . . . Accordingly, science must aim for coherence between all its propositions and between its propositions and all those that are inevitably presupposed in human practice and thought in general. (Obtaining such coherence is indeed the primary method of checking for correspondence.) (Griffin 1988, p. 30)

Constructive postmodernism, alas, is a minority view; deconstructive postmodernism (in marketing, management, social science, and consumer research) appears to be the majority view (e.g., Thompson 1993).

11.6.2 On Marketing and Noncontradiction: For Reason

In marketing's philosophy debates, the principle of noncontradiction is often held in low regard. Indeed, it is considered to be just a technical nicety. For example, Zinkhan and Hirschheim report that Hunt (1990b) "castigates critical relativism for using the very concepts that it purports to stand against, viz., truth and falsity . . . [and] showing that this latest version of relativism is still self-refuting and, moreover, incoherent and unintelligible" (Zinkhan and Hirschheim 1992, p. 81). For them, incoherence and unintelligibility are not problems. Rather:

> The problem seems to be with the notion of "truth." Any attempt to dispense with that notion typically incurs the wrath of the realist community. Hunt clearly cannot accept its abandonment . . . one must question whether, in the social domain, it makes sense to focus on the notion of truth as *it is essentially a social construction.* (Zinkhan and Hirschheim 1992, pp. 81–82; italics added)

In contrast, I argue, the problem is not anyone's "wrath," nor (as discussed in the next section) is the solution to regard truth as "essentially a social construction." The problem is that arguments for one's proposed philosophy, including arguments proposing relativism, should make sense. And *making sense,* being coherent and abiding by the principle of noncontradiction, is—as even the version of postmodernism argued for by Griffin (1988) agrees—a minimum desideratum for any academic discourse, including marketing. Similarly, the solution, I argue, is not to accept the reality relativism—see Hunt (2003a, section 4.1.3)—implicit in regarding truth as "essentially a social construction." Instead, the solution is to (1) develop coherent arguments and (2) adopt a philosophy that makes sense of (and for) science. One such philosophy is scientific realism. There may be others.

11.7 TRUTH, REALITY RELATIVISM, AND IDEALISM

Scientific realism embraces the classical realist view—see section 9.3—that the external world exists unperceived. Indeed, the reality of the external world, shown as box 3 in Figure 11.1, is posited to impact the outcomes (i.e., box 2) of theories (i.e., box 1) by path D. This section explores how marketing's relativism addresses the reality of the external world. Peter (1992) provides a detailed exposition of the relativist/constructionist view of reality. We shall review and then evaluate his exposition.

11.7.1 Relativistic Reality

Peter (1992) develops a model he calls the "Relativist/Constructionist View of Reality" (see his 1992, Figure 2, p. 74). In this model, "uninterpreted reality" is shown with a solid arrow as impacting on three constructs: (1) "scientist's worldview," (2) "research paradigm," and (3) "mental interpretation of reality." In turn, these three constructs

influence science's "public construction of reality," which then provides feedback to "uninterpreted reality."

What is "uninterpreted reality," and to what extent is the solid arrow from this construct the same thing as path D in the realist model in our own Figure 11.1? Peter (1992, p. 73) answers: "the relativistic view has no problem with the possibility of an external world that is independent of the scientist." So far, so good. However, readers might wonder why the qualifier "possibility." Elsewhere in the same article, Peter urges marketers to research problems like "starving third world countries, the delivery of a standard of living to the poor and homeless, the misuse of drugs, or the spread of AIDS" (1992, p. 78). So, why does Peter imply that it is only a "possibility" that third world countries, the poor and homeless, drugs, and AIDS exist "independent of the scientist"? Also in the same article, Peter confidently speculates that the motivations of those philosophers of science who have examined the concept of incommensurability (and found it wanting) resulted from their being "threatened." Indeed, the philosophers feared that incommensurability "then and now . . . drastically reduces the importance of philosophy of science in society" (1992, p. 77). Given such confidence, why is it only a "possibility" that, "then and now," such philosophers actually have motivations (fearful or not) that are independent of Peter?

Peter (1992) continues his exposition by arguing for relativistic reality. Compared with scientific realism, "the difference in the relativistic perspective [compared with the realist view] is that *no interpretation of that world can be made independently* of human sensations, perceptions, information processing, feelings, and actions" (Peter 1992, p. 74; italics in original). I quote at length how Peter argues for relativistic reality, that is, how science socially constructs reality:

> A construction of reality is not equivalent to uninterpreted reality. The reason is that the meanings given to phenomena are humanly constructed and bounded by cultural, historical, and theoretical limitations . . . there are clearly differences in the naming and meaning of even simple objects across languages and culture. The meanings of more complicated terms, such as "attitude," "brand loyalty," or "brand equity," differ across theories and research paradigms, even within the same culture and language. . . .
>
> An important point of this discussion is that, even for simple objects, the labels placed on them and the meanings given to them are human constructions in the form of language, mathematics, or other symbols; *they are not the objects themselves.* However, concepts are easily confused with the phenomena they are intended to represent. . . . In sum, there is no theory-independent way to know what an object is or, indeed, whether it is an object.
>
> Similarly, empirical data are not equivalent to uninterpreted reality. Empirical data are constructed by scientists through processes such as measurement and sampling; *they are not the phenomena themselves.* Measures cannot be constructed meaningfully unless one has at least an implicit theory of what is to be measured, that is, an interpretation to guide measure development. The meaning of data derived from measures also depends entirely on the interpretations scientists give to them concerning what

> was measured, how well, and from what theoretical perspective. . . . Hence, empirical data are not independent criteria for judging the closeness of a theory to reality because they are entirely dependent on theory for their meaning and interpretation. Without a theory that describes, among other things, what concepts are putatively measured, empirical data are merely numbers.
>
> Human constructions in the form of language, mathematics, and other symbols are the "reality" scientists analyze, evaluate, debate, and compare with their own beliefs. As stated previously, these constructions of reality are in no sense equivalent to uninterpreted reality nor is there any method to evaluate whether they truly capture uninterpreted reality. (Peter 1992, p. 75; italics in original)

11.7.2 On Relativistic Reality: For Reason

Our evaluation begins with the claim that the labels we use to identify objects "are not the objects themselves," though they "are easily confused with the phenomena they are intended to represent." Furthermore, we evaluate the claim that the meanings of labels such as "apples," "trees," "star," and "planet" depend on "theory," which implies: "In sum, there is no theory-independent way to know what an object is or, indeed, if it is an object."

While it is true that words, the labels we use to refer to objects, are not themselves the objects we intend to represent, Peter presents no evidence of any such confusion in the scientific realism he is attacking. Furthermore, while the meanings of words often depend significantly on context, and sometimes the context might legitimately be referred to as a "theory," it is a *non sequitur* that "there is no theory-independent way to know what an object is or indeed, if it is an object." First, it is incorrect that accurate human recognition (perception) of all objects depends on language. We may safely assume (since we are here) that our ancestors, long before the development of language, recognized lions, tigers, and bears as (dangerous) objects. Second, when humans started communicating with each other through the use of language (e.g., English), their use of "lions," "tigers," and "bears" to refer to lions, tigers, and bears required nothing that might be properly referred to as a "theory." Third, even if we—sloppily and inappropriately—extend the word "theory" to mean "a consensus as to the use of terms in a specific language community," lions, and tigers, and bears would be unchanged. Indeed, they would still be dangerous, even if we labeled them (for example) "bunnies," "fawns," and "kittens."

In conclusion, contra Peter, even in the absence of "theory" we can recognize many objects as objects and know what they are. Equally important, "labels" and "theory" neither create genuine objects nor change their characteristics. Why does Peter feel the need to deny the preceding? As we shall see, one possible reason was hinted at when he admitted that the world external to the researcher is only a "possibility."

To evaluate relativistic reality, let us return to the example in section 11.1. The example involved a marketer who wants to test two, genuinely rival, explanatory theories (i.e., two theories having conflicting knowledge-claims). Recall that both theories entail the concept "intentions to buy," which is measured on a scale from "1" equals "definitely

will not purchase" to "10" equals "definitely will purchase." After being asked to mark the box that "best describes your beliefs," a subject puts a checkmark on the questionnaire. The coder of the questionnaire then reports, "The subject checked box 9." For the correspondence theory of truth, "The subject checked box 9" is true, if and only if, the subject actually checked box 9. Furthermore, when the example is interpreted by scientific realism, the subject checking box 9 may (if our measurement theory is good) be an indicator (albeit, a fallible indicator) of the subject's actual intentions to buy.

How does Peter's relativism, in his words, "account for" (Peter 1992, p. 97) the "9"? Peter states, "empirical data are not equivalent to uninterpreted reality" (1992, p. 75). Now, no one would deny that "9" is not the *same* as the reality to which it is intended to relate or refer, that is, the subjects' genuine intentions to buy. In short, we will all agree with Peter that "they are not the phenomena themselves" (1992, p. 75). However, Peter continues, because one needs "an interpretation to guide measure development," then "the meaning of data derived from measures also depends entirely on the interpretations scientists give to them concerning what was measured, how well, and from what theoretical perspective" (1992, p. 75). We should note carefully his words, "depends entirely." Later in the same paragraph he states that empirical data are "entirely dependent on theory," and empirical data are "merely numbers without a theory." In the next paragraph he states that data "are in no sense equivalent to uninterpreted reality."

Why does Peter say "depends entirely" and not "depends"? Why does he say "merely numbers" and not just "numbers"? Why does he say that data are "in no sense equivalent," rather than "are not equivalent"? Three possibilities are apparent. First, if he meant only "the same as," his view reduces to a banal triviality. Just as no physician believes that "106" on a fever thermometer is the *same as* a child's high fever, it would be the height of condescension for Peter to claim that his fellow marketers are such silly, confused, "country bumpkins" (Calder and Tybout 1989, p. 203) that they believe that "9" is the *same as* a high intention to buy. Second, he cannot mean that (just as "106" on a fever thermometer is associated with, is an indicant of, or corresponds to a child's high fever) the "9" (if our measurement theory is good, like that underlying a fever thermometer) is associated with, is an indicant of, or corresponds to a consumer's intentions to buy. In short, he cannot mean that the greater the subject's intentions to buy, the higher the number on our scale. He cannot mean that the sentence "the subject checked box 9" is true if the subject indeed checked box 9. Why "cannot"? Because this view is the realist view being argued against, an example of the very correspondence theory of truth claimed to be a "fairytale" by Peter and Olson (1983, p. 122) and referred to as "naive" by Zinkham and Hirschheim (1992, p. 81).

There appears to be one and only one interpretation left—and an unfortunate one it is. Peter must mean that the meaning of empirical data, for example, the "9," comes only from the researcher's theory and not at all from any "uninterpreted reality." For example, the meaning of "9," its information value, has nothing to do, *can* have nothing to do, with whatever prompted the subject to check "9." And, because Peter does not restrict his discussion to just the social sciences—indeed, he contends that the relativist view reflects that of modern physics (Peter and Olson 1983, p. 120)—the "106" on a fever

thermometer is just a "created reality" and has nothing, can have nothing, to do with a child's fever (what he calls "uninterpreted reality" and realists call "the world external to the researcher"). The preceding appears to be the only nontrivial explanation available as to Peter's use of "entirely" with respect to interpretations of data, his use of "merely" with respect to numbers, and his use of "in no sense equivalent" with respect to the external world. In short, though Peter admits the "possibility" of an external world that is independent of the scientist on his page 73, by page 75, even this slim, remote chance has vanished. Similarly, what Peter calls "uninterpreted reality" in his Figure 1 on page 74, and shows by means of an arrow as impacting on or influencing the process of science, is erased on page 75. Otherwise, Peter would state that the meaning of the data derived from measures depends in part on the researcher's theory and in part on something external to the theory, for example, the subjects. Why does Peter do this? Because relativism/constructionism requires it.

If relativists acknowledge that the meaning of data is influenced by *both* scientists' theories and a world external to their theories, then they would have to acknowledge that it is at least possible for scientists' theories to "touch base" with some reality external to the theorist. Furthermore, acknowledging that theories can touch base with some external reality would then imply that some theories might accomplish this task better, more accurately, more faithfully, more genuinely, than others. But this would imply that it is at least possible, just possible, that some of our theories may be false, and others may be true, or approximately true, or closer to the truth than others. And this, of course, is what relativism/constructionism denies. Therefore, the external world vanishes in Peter's relativism. All philosophies based on the idealist view (see Hunt 2003a, section 2.4) that the external world does not exist unperceived degenerate into nihilism. And Peter's relativism, as acknowledged by Olson (1981, p. 14), embraces idealism. Thus, it degenerates to nihilism.

11.8 FOR TRUTH

The preceding shows that arguments *against* truth are uninformed by reason. We now turn to arguing *for* truth. Our argument is based on trust. What is trust? Trust exists when one has confidence in another's reliability and integrity (Morgan and Hunt 1994; Moorman, Deshpandé, and Zaltman 1993). In turn, the confidence of the trust*ing* party in the trust*worthy* party's reliability, and integrity is associated with the belief that the trustworthy party has attributes such as consistent, competent, honest, fair, responsible, helpful, and benevolent (Altman and Taylor 1973; Dwyer and LaGace 1986; Larzelere and Huston 1980; Rotter 1971). The importance of trust is recognized across disciplines and research traditions.

In organizational behavior, the study of "norms of trust" is considered a characteristic that distinguishes management theory from organizational economics (Barney 1990; Donaldson 1990). In communications, a key construct has been source credibility, originally defined by Hovland et al. (1953) as trust of the speaker by the listener. In social policy, Fukuyama (1995) argues that social trust positively affects the wealth of nations.

In economics, Arrow (1972) stresses how trust increases productivity. In competition theory, Hunt (2000b, pp. 235–37) explicates how resource-advantage theory can explain the relationships among trust, competitive advantage, and wealth.

In the marketing of services, Berry and Parasuraman (1991, p. 144) find: "Customer-company relationships require trust." Indeed, they contend, "Effective services marketing depends on the management of trust because the customer typically must buy a service before experiencing it" (1991, p. 107). In marketing education, Huff, Cooper, and Jones (2002) find that trust has consequences important to the success of student project groups. In strategic alliances, Sherman (1992, p. 78) concludes that "the biggest stumbling block to the success of alliances is the lack of trust." In retailing, Berry (1993, p. 1) stresses that "trust is the basis for loyalty," and Ganesan (1994) finds trust as influencing a retailer's long-term orientation. In relationship marketing, Morgan and Hunt (1994) find that trust promotes cooperation, increases the likelihood that conflict will be of the functional kind, and decreases uncertainty. In international marketing, Hewett and Bearden (2001) find that a multinational's subsidiary having trust in the headquarters' marketing function will increase the subsidiary's acquiescence to headquarters' direction. In the brand equity area, Chaudhuri and Holbrook (2001) find brand trust to impact purchase loyalty and attitudinal loyalty.

In short, trust is a key concept in many different literatures. What, then, are the relationships among trust, science, realism, and ethics?

11.8.1 Trust, Science, Realism, and Ethics

Zaltman and Moorman (1988) explore the factors determining whether marketing managers actually use the research generated by marketing research departments. The key factor, they find, is trust: "Perhaps the single most important factor affecting the use of research is the presence or absence of trust" (1988, p. 16). Indeed, a major requirement for developing and maintaining trust is "being a truth teller" (p. 20). Thus, truth and trust are interrelated.

Mainstream philosophy of science views trust as a key construct for understanding the dynamics of scientific disciplines. Trust is essential in science (indeed, in all disciplines) because scientific knowledge is a shared form of knowledge; it is shared with its clients. The clients of commercial marketing researchers are limited in general to the organizations that purchase the research. However, as discussed in Chapter 2, the clients of academic marketing research include not only marketing practitioners, but also students, government officials, consumers, other academicians, and members of the general public (Monroe et al. 1988). In essence, all researchers who share their research with clients state implicitly: "Trust me." Thus, science and trust are interrelated.

One consequence of the importance of trust in science is for those whose research projects are guided by philosophies maintaining that the research does not "touch base"—path D in Figure 11.1—with a reality external to the researcher's own linguistically encapsulated theory, or paradigm, or research tradition, or worldview. Such philosophies provide no grounds for the client trusting the knowledge claims of the researchers. Thus,

philosophies such as reality relativism, constructionism, critical relativism, and deconstructive postmodernism that abandon truth are not only self-refuting for their philosophical advocates, but also self-defeating for practicing researchers who might adopt them at the "workbench" level.

Trust and Ethics

Studies indicate that a difficult ethical problem facing marketing researchers is "misinterpreting the results of a research project with the objective of supporting a predetermined personal or corporate point of view" (Hunt, Chonko, and Wilcox 1984, p. 312). Because such biases would destroy trust, marketing associations, (e.g., the American Marketing Association, the Academy of Marketing Science, the European Marketing Academy, and the Society for Marketing Advances) are paying more attention to marketing's codes of ethics. It has been long recognized that one of the major distinguishing characteristics that separates professions from other vocations is that all true professions maintain a degree of self-control by means of formal and/or informal codes of ethics. An underlying tenet of all such codes is that the true professional, when interacting with clients of any kind, is not guided totally by self-interest. For example, when people go to physicians, they have a right to expect that their physicians will not adopt methods of treatment based solely on which method will best serve the physicians' interests. Because of the disparity in knowledge of diseases and their respective treatments, the social compact between laypeople and their physicians requires a significant element of *trust.* Many philosophers of science are coming to realize that both *trust* and *ethics* are interrelated keys to understanding scientific communities.

Rom Harré has been at the forefront of those philosophers advocating the importance of, in his terms, "moral order" in science. Avoiding the philosophers' fallacy, Harré (1986) defines scientific knowledge as "trustworthy knowledge," rather than truth with certainty: "Science is not a logically coherent body of knowledge in the strict, unforgiving sense of the philosophers' high redefinition, but a cluster of material and cognitive practices, carried on within a distinctive moral order, whose main characteristic is the trust that obtains among its members and [the trust that] should obtain between that community and the larger lay community with which it is interdependent" (1986, p. 6). What, for him, is trust? "To trust someone is to be able to rely on them in the matter in question. . . . Scientists believe that things personally unknown to them are as *another scientist* says they are." However, "trust is not maintained by telling each other only literal truths. Under that constraint the members of the community would perforce remain forever silent. It is enough that they tell each other what they honestly believe to be the truth" (1986, p. 12). In this regard, Harré is claiming that the moral order of science implies, among other things, the avoidance of sophistry and deception, as well as outright fraud.

Harré points out that trust in all societies is most often role-related: "it is because the trusted one is in the role of parent, guardian, policeman, research supervisor, and so on, that the trust is there until something happens to upset it" (Harré 1986, p. 21). Therefore, scientists in their role as researchers producing trustworthy belief are required by their

peers and by the lay community to maintain a moral order. This moral order is necessary, Harré argues, because researchers are involved in producing "practically reliable scientific knowledge." This "reliance might be existential, concerning what there is or what might be, or it might be practical, concerning what can and cannot be done, or both. The moral quality of the product comes through clearly in the kind of outrage felt by the [scientific] community at the disclosure of scientific fraud" (Harré 1986, p. 13). Harré (1986, p. 26) asks: "Is scientific method . . . and scientific morality, the fiduciary act of committing oneself to make one's scientific utterances fiduciary acts, the best way to discipline a community which exists to find out about the natural world"? Harré answers this question affirmatively on the basis that science is committed to referential realism. This realism holds that "existence is prior to theory, and that while no ontologies for science could be absolute, nevertheless, ontologies (realized in referential practices) are always, at any moment, less revisable than their associated belief-systems. . . . On this view, truth and falsity migrate from the epistemology of science to the morality of its human community" (1986, p. 6). For Harré, any view of science that claims that scientific knowledge is "constructed" or "created" by the scientific community independent of some external reality is to be rejected on moral grounds. He summarizes his position as follows:

> Science has a special status, not because it is a sure way of producing truths and avoiding falsehood, but because it is a communal practice of a community with a remarkable and rigid morality at the heart of which is a commitment that the products of this community shall be trustworthy. . . . Science is not just a cluster of material and cognitive practices, but is a moral achievement as well. . . . Antirealism, which, like it or not, seeps out into the lay world as antiscience, is not only false, but *morally obnoxious.* (Harré 1986, p. 7; italics added)

As members of the marketing academic profession, we have numerous clients for marketing knowledge, our product. Concerning marketing knowledge, its development, and dissemination, does the *trust* that these constituencies have *in* us impose certain special responsibilities *on* us? If so, what is the nature of these responsibilities, and what does it imply about the most appropriate philosophy to guide marketing science? Philosophies based on reality relativism, constructionism, critical relativism, and deconstructive postmodernism would seem to be unlikely candidates for inspiring trust. Most assuredly, no philosophy of research can *guarantee* trustworthy knowledge. Nevertheless, researchers can find comfort in the fact that there exist philosophies of science—such as scientific realism—that, at the minimum, are not antithetical to truth and its surrogate, trustworthy knowledge, and, at the maximum, may (fallibly) yield knowledge that is truly worthy of others' trust.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Define and differentiate among the following theories of truth: pragmatic, correspondence, coherence, and consensus. How does the consensus theory of truth fail

to address the issue of the pragmatic success of science? What is the relationship between the consensus theory of truth and reality relativism? Should marketing adopt the consensus theory? Why, or why not? Should marketing adopt the correspondence theory? Why, or why not?

2. Both boxes 1 and 3 in Figure 11.1 contain entities, attributes, and structures. Is Figure 11.1 not, therefore, a tautology or at least redundant? Does Figure 11.1 relate to empirical testing? If so, how? If not, why not?
3. If truth does not *equal* pragmatic success, how does (or does not) pragmatic success relate to truth? Could a theory be characterized as having high explanatory and predictive success, yet have low pragmatic success?
4. In Orwell's *1984,* the society of Big Brother maintained: "Reality is inside the skull . . . you must get rid of the 19th century ideas about the laws of nature. We make the laws of nature" (Orwell 1949, p. 168). Furthermore, the society exercised control of its citizens through "doublethink," which was the practice of holding "two contradictory beliefs in one's mind simultaneously, and accepting both of them" (p. 215). Doublethink was necessary in order "to deny the existence of objective reality and all the while to take account of the reality one desires" (p. 216). Furthermore, it was necessary to destroy empirical science:

   > The empirical method of thought, on which all of the scientific achievements of the past were founded, is opposed to [our] most fundamental principles. [Therefore,] in Newspeak there is no word for "science." (Orwell 1949, p. 194)

   Are reality relativism and constructionism consistent or inconsistent with the society in *1984?* How? How does the principle of noncontradiction relate or not relate to *1984?* How does the issue of self-refutation relate or not relate? Find an example of "doublethink" in marketing and/or society. Analyze the example.
5. How does Laudan reach the conclusion that "truth" and "falsity" are "irrelevant" to science? Why has Laudan fallen victim to the "philosophers' fallacy" with respect to truth?
6. Evaluate the thesis of Laudan that "truth" is irrelevant to science, and, at the same time, science should "treat theories as if they were true" for purposes of "practical action" and "testing theories."
7. How does the dogmatic skepticism in Table 11.1 differ from dogmatism? How does each relate to trust? Which of the two, dogmatism or dogmatic skepticism, is more prevalent in marketing? Why?
8. How are trust, science, realism, and ethics related? How does truth relate to each?

NOTES

1. This chapter draws heavily on Hunt (1990b, 1992a, 2005; Hunt and Hansen 2010, forthcoming).

2. I thank Jared Hansen for his contribution in developing this example. For an example that uses transaction cost economics, see Hunt (2005).

3. Even Feyerabend, whom Anderson (1986, p. 156) describes as "one of the most radical of contemporary relativists," concedes that incommensurability is a "rare event" and "is a difficulty for philosophers, not scientists" (Feyerabend 1987, p. 81).

4. See, for example, McMullin (1984) for an evaluation of this issue and whether Laudan's "convergent realism" is a straw man.

5. Calder and Tybout (1989) in the passage quoted are complaining—quite appropriately, in my judgment—that relativist writers seem often to view nonrelativist scholars as "country bumpkins."

6. The quote of Laudan is sometimes referred to as the "pessimistic induction" thesis. See Psillos (1996).

7. The case is *McClean vs. Arkansas Board of Education,* 529 F. Supp. 1255 (E.D. Arkansas 1982).

8. See Reisch (1998) for a good discussion of the problems confronting those who wish to deny that science differs from nonscience in any substantive way, but who wish to deny the right of those advocating creation science to have their view taught in public schools.

9. Table 11.1 is a modified version of Table 1 in Hunt (1992a). Since its original publication, numerous writers have suggested that feminism and postmodernism should be added to the table as examples of dogmatic skepticism. In contrast, the view here is that only radical feminism and deconstructive postmodernism embrace dogmatic skepticism.

10. The exception might be quantum mechanics.

11. In addition, not all ethnographic researchers are dogmatic skeptics. For example, Stewart, an ethnographic methodologist maintains: "Statistics-oriented researchers and ethnographers share an ultimate epistemic value. Whether or not they define themselves as 'scientists,' they both adhere to the fundamental purpose of science: to try to learn the truth about the world" (Stewart 1998, p. 12).

12

ON OBJECTIVITY AND MARKETING RESEARCH

> The heart of the matter is that, while it is one thing to say that certain notions are "the socio-historical constructs of a particular time," it is quite another to add that they are no more than "the socio-historical constructs of a particular time." It cannot be right to proceed from a premise stating only that this is that, direct to the richer conclusion that this is merely that, that this is nothing but that, that this is that and nothing else.
>
> *—Antony Flew, "The Debunker's Fallacy" (1985)*

This chapter analyzes the "objectivity controversy" in marketing's philosophical debates, that is, the question: can marketing research be objective? More generally, it addresses the question: can *any* research be objective? Many writers answer "no." As Rescher (1997, p. 25) has observed, "objectivity is not exactly popular in the present era." In the area of history, for example, Novick (1988, p. 6) reviews works (e.g., Kuhn 1962) in philosophy and concludes that "to say of a work of history that it is or isn't objective is to make an empty observation: to say something neither interesting nor useful."

This chapter begins by evaluating the arguments that have led so many relativists, social constructionists, postmodernists, feminists, historicists, subjectivists, and humanists to deny the possibility of objectivity in marketing research.[1] It then puts forth the "positive case" for pursuing objectivity. Like the case for truth, the positive case for objectivity focuses on trust. We conclude by developing a scientific realist model of empirical testing and using the model to analyze issues related to the objectivity controversy in marketing.

12.1 THE NATURE OF OBJECTIVITY

The philosophy of science distinguishes ontological objectivity from epistemic objectivity. The former refers to whether the entities in a theory that are claimed to exist actually exist independently of someone's perception of them. The latter refers to certain aspects of how one justifies knowledge-claims. Specifically, epistemic objectivity:

> relates to the appropriateness of *claims or contentions,* addressing the question of whether a claim is impersonally and generically cogent rather than personal and

> idiosyncratic—whether it holds not just for me (egocentric subjectivity) or for some of us (parochial subjectivity) but for all of us (impersonal or interpersonal objectivity). . . . Objectivity in this sense has to do not with the *subject matter* of a claim but with its *justification.* It pivots on the way the claim is substantiated and supported—namely without the introduction of any personal biases or otherwise distortive individual idiosyncrasies, preferences, predilections, etc. (Rescher 1997, p. 4; italics in original)

Thus, the *pursuit* of objectivity relates to efforts at minimizing bias of some kind (or kinds) in research findings. As to the objectivity of research methods, for example, bias is thought to be reduced in experiments and survey research by using control groups and placebos, by checking for nonresponse bias (Armstrong and Overton 1977; Hunt 1990a), by checking for same-source bias (Podsakoff and Organ 1986; Crampton and Wagner 1994), and by separating the measurement model analysis from the test of the structural model in structural equation modeling (Anderson and Gerbing 1988). As to the objectivity of ethnographic methods, Stewart (1998) argues for objectivity as a goal. For him, the specificity of the research circumstances of ethnographic research implies that strict replicability (i.e., the reproduction of identical results by other researchers) is not "humanly possible" (Stewart 1998, p. 30). Nevertheless, objectivity can (and should) be sought through bias minimization. For Stewart, the questions to ask of any ethnography include:

> To what extent, and in what ways, were the results affected by the peculiarities of researchers, insiders, or their interactions? How well were their biases, and the reactivity of insiders to researchers, minimized or offset by the use of research method? . . . Have the context and results been sufficiently specified that the findings could potentially be disconfirmed in a follow-up study? Replicability is not a precondition for scientific findings; intersubjective testability is. (Stewart 1998, pp. 30–31)

As to the objectivity of marketing research, for example, consider the issue of salesperson performance. Churchill and colleagues (1985, p. 117) conducted a meta-analysis of the determinants of salesperson performance and concluded: "Enduring personal characteristics such as aptitude variables and personal/physical traits do have some relationship to performance, but not as much as those characteristics which are 'influenceable' through increased training and experience or more effective company policies and procedures (e.g., skill levels, role perceptions, and motivation)." This knowledge-claim prompts three questions: (1) Is the claim objective? (2) In general, are any such knowledge-claims in marketing research objective? (3) More generally yet, should the marketing research community even pursue the ideal of objectivity? Marketing's traditional view on these questions is summarized as follows:

> Scientific knowledge, in which theories, laws, and explanations are primal, must be objective in the sense that its truth content must be intersubjectively certifiable.

> Requiring that theories, laws and explanations be empirically testable ensures that they will be intersubjectively certifiable since different (but reasonably competent) investigators with differing attitudes, opinions, and beliefs will be able to make observations and conduct experiments to ascertain their truth content. (Hunt 1976b, p. 27)

Participants in marketing's philosophy debates question or deny the possibility of objective marketing research. For example, some caution, "Scientific inquiry may not deserve the objectivity that we tend to assume to be true of it" (Sauer, Nighswonger, and Zaltman 1982, p. 19). Going much further, Peter and Olson (1983, pp. 119–20) not only caution that the "aura of objectivity has been steadily eroding for years across all sciences, including physics," but conclude that "science is subjective," and marketers, therefore, should adopt relativism and constructionism. Going further yet, Peter (1983, p. 385) examines the "received view on theories," contends that "leading philosophers no longer espouse this approach," puts forth what he calls "a more current understanding of the philosophy of science literature," and concludes, albeit incoherently, "objectivity is an illusion."[2] Even more strongly, Mick (1986, p. 207) advocates semiotics for marketing research because, among other reasons, "objectivity is impossible." Similarly, Fullerton (1986, p. 433) endorses historicism, claiming, "Researcher objectivity and intersubjective certifiability are chimeras—they cannot be achieved." Likewise, Hirschman (1986, p. 249) points out that "the humanist approach denies the possibility of discovering objective truth."

Therefore, in marketing's philosophy debates, the three questions posed concerning the possible objectivity of the claim of Churchill and colleagues (1985) and others in marketing are uninformed queries. If objectivity is a "chimera," an "illusion," or "impossible," its pursuit and all claims to its accomplishment are, to say the very least, naive. If all inquiry is subjective, then so is the specific assertion of Churchill and his associates, as well as *all* marketing research.

At the outset of our analysis of objectivity, three preliminary issues warrant attention. First, many marketers believe that the relativist/subjectivist claims that objectivity in research is impossible and/or undesirable are so specious that they do not merit analysis. Some maintain, for example, that relativists and subjectivists do not intend for their views on objectivity to be taken seriously by anyone. Indeed, it is claimed, relativists *themselves* do not believe in their arguments. In contrast, I maintain that (1) the cogency of arguments against objectivity are such that they fully merit counterarguments, (2) it is patronizing to dismiss relativist/subjectivist arguments as being unbelieved by their proponents, (3) when one is condescending toward relativist claims, one is engaging in impolite academic discourse, and (4) ideas (including, perhaps especially, wrong-headed ideas) have consequences. Therefore, relativist/subjectivist claims should be treated not with condescending dismissal, but with both seriousness and civility.

Second, skillful rhetoricians recognize that many philosophies, when applied at the "workbench" level, are implausible or even bizarre. Nonetheless, these philosophies can be made to seem reasonable or commendable when argued in the abstract. Relativism, subjectivism, and constructionism, I maintain, are such philosophies (as well as Marx-

ist utopianism—as the twentieth century reminds us). Therefore, each argument against objectivity is illustrated here by use of Churchill and his associates' claim (hereafter, simply "CW's claim") concerning salesperson performance as a continuing example at the "workbench" level. Lest there be any misunderstanding, however, this chapter is not critical of Churchill and his associates, their article, or their article's scholarship. CW's claim is used only to illustrate the implications of relativism/subjectivism at the level of actual marketing research practice.

The third preliminary issue is that many marketers confuse *objectivity* with *objectivism.* Because the confusion in marketing, management, social science, and philosophy is so widespread, it is worth discussing in detail.

12.1.1 Objectivity and Objectivism

Many philosophers attack what they call "objectivism." For example, Bernstein contrasts relativism not with absolutism (its usual opposite) but with objectivism, which he defines as:

> the basic conviction that there is or must be some permanent, ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality, goodness, or rightness. . . . Objectivism is closely related to foundationalism and the search for an Archimedean point. (Bernstein 1983, p. 8)

Similarly, Lakoff and Johnson argue against "objectivism," which they maintain is the belief that:

> There is an objective reality, and we can say things that are objectively, absolutely, and unconditionally true and false about it. . . . Science provides us with a methodology that allows us to rise above our subjective limitations and to achieve understanding from a universally valid and unbiased point of view. Science can ultimately give a correct, definitive, and general account of reality, and, through its methodology, it is constantly progressing toward that goal. (Lakoff and Johnson 1980, p. 187)

For anti-objectivists, seeking knowledge that is absolutely true, universally valid, absolutely correct, definitive, known with certainty, or known from a unique privileged position is not just impossible, it is also morally undesirable. Such objectivist views "inevitably turn into vulgar or sophisticated forms of ethnocentricism" (Bernstein 1983, p. 19).[3] Thus, Sapire (1988, p. 497) asks: "Is it possible to believe that science as it has developed mainly in the West reigns supreme among the systems intended to provide knowledge of the world, yet not be committed to derogatory views about other cultures and societies"? And he answers: "Orthodoxy . . . [holds that] all such comparisons are invidious and are to be avoided."

Stemming from the belief that objectiv*ism* is impossible (because knowledge with certainty is beyond human capability) and morally undesirable (because believing in the

knowledge-claims and method of science is necessarily ethnocentric), one might argue that objectiv*ity* is impossible and its pursuit undesirable. This appears to be Holt's (1991) argument. Wallendorf and Belk (1989) have argued that interpretivist researchers should use (and report the use of) checks, audits, triangulation, and purposive sampling as a means for minimizing bias. That is, these procedures, for Wallendorf and Belk, are meant to increase objectivity. Holt (1991, pp. 60–61) argues against these procedures on the grounds that they constitute adopting the "objectivist evaluative banner" of "positivist-inclined researchers" and "Western ways of thinking."[4] Similarly, Thompson (1991, pp. 63–64) cites Bernstein (1983) as showing that the pursuit of "objective knowledge" is misguided because it stems from "objectivism" and "foundationalism," which reflect a "Western view of knowledge."

On Objectivism: For Reason

Several observations illuminate the objectivism controversy and its implications for marketing and objectivity. First, though many philosophers in the past, and perhaps some in the present, subscribe to the foundationalist search for infallible knowledge and a "god's-eye view" of absolute certainty, marketing's scientific realism advocates fallibilism. *Pursuing* objectivity (minimizing known sources of bias) differs from the claim that one has attained OBJECTIVITY (maintaining that one has eliminated *all* bias). Hence, to the extent that Bernstein's "objectivism" and "foundationalism" are problems at all, they seem (like paradigm incommensurability) to be problems for philosophy and some philosophers, not for science and most scientists.

Second, holding the claims and method of science in high regard is anything but ethnocentric. Without such Eastern contributions as the Arabic numeral system, science might never have arisen anywhere, let alone in the West.[5] Third, for Western humanists and intellectuals to denigrate science because it originated in their own cultures not only commits the genetic fallacy, but is perversely ethnophobic as well, for science, no matter where it originated, now belongs to all of humanity—just as does the Arabic numerical system.[6] Fourth, what Bernstein calls "objectivism"—because it is confused with *objectivity*—is better characterized as "vulgar absolutism" (Siegel 1987, p. 163). Using "objectivism" in the manner of Beach is recommended for marketing because it more accurately describes the practice of science and its goal of objectivity:

> Objectivism: The thesis that there exists a systematic method of reasoning and a coordinate set of beliefs embodying its principles, which, despite the vicissitudes of social psychological conditioning, are accessible to knowledge and are capable of sustaining a dynamic, self-correcting belief system. These principles may contain errors or half-truths, and they may never attain a fixed and final form. Yet, insofar as (a) their consistency is publicly verifiable, (b) their development is rational, and (c) their truth-content is demonstrably greater than that of rival contenders, they do constitute reliable criteria by which to evaluate subsidiary beliefs and hypotheses. (Beach 1984, p. 14)

In conclusion, though objectivism in the sense of vulgar absolutism may be impossible, this does not imply that objectivity is impossible. Nor should a paranoid fear of ethnocentrism deter marketing from pursuing objectivity. Bernstein, on whom many writers in marketing's philosophy debates rely, was confused on both counts. As Schwandt explains:

> The confusion over the meaning of *objectivity* as a regulative ideal has arisen from [Bernstein's] confounding it with the notion of *objectivism* and juxtaposing the latter with *relativism.* Recognizing that [Bernstein's] objectivism is a bankrupt notion in no way entails rejecting the bid to be objective, neither does it follow that we must slide down the slippery slope of relativism. (Schwandt 1990, p. 270; italics in original)

The preceding sheds light on the claim by Zinkhan and Hirschheim (1992, p. 82)—who are professed *advocates* of scientific realism, no less—that "The realist position in the social domain is untenable." That is, after arguing in the first part of their article that marketers "in the social domain" should adopt scientific realism because such concepts as "memory" and "desires" have "causal powers," Zinkhan and Hirschheim (hereafter ZH) then claim that their own position is "untenable." ZH's position is, of course, incoherent.

To understand the source of the incoherency of ZH's position, we must first ask: What is the realist ontological position? Manicas (1987, pp. 9–10), the realist philosopher on whom ZH base their version of scientific realism, states: "A philosophy (of science) [is] positivist if it holds that a scientific explanation must eschew appeal to what is in principle beyond experience . . . by contrast, a realist holds that a valid scientific explanation can appeal to the in principle non-observable." It is the posited existence of unobservable, intangible entities in the "social domain" that ZH's realism claims will have "causal powers." For example, ZH use the Blair and Zinkhan (1984) study as a prototypical example of applying their realism to health-care marketing, and they claim that, in this study, "desire was the generative mechanism producing adherence" (Zinkhan and Hirschheim 1992, p. 86). As Manicas (1987) and Bhaskar (1979) point out, using concepts such as "desire" in scientific explanation is a major feature that distinguishes the ontology of scientific realism from that of logical positivism.

ZH can maintain, as do the logical positivists in the past and relativists (see Anderson 1989, p. 14) in the present, that "the realist ontological position, in the social domain, is untenable" (Zinkhan and Hirschheim 1992, p. 82). Alternatively, they can maintain that scientific realism, whether the realism of Manicas or some other version, is "more appropriate for describing marketing phenomena" (Zinkhan and Hirschheim 1992, p. 84). However, they cannot maintain coherently both positions simultaneously. Failing a test for coherence is not just some philosophical technicality, as ZH imply. Incoherent arguments make no sense, and *making sense* is a fundamental desideratum of academic discourse.

What led ZH to believe that their own, their *own,* realism's ontology was untenable?

They cite Berger and Luckmann (1967) and Sayers (1987) as support for their claim that the "realist ontological position" requires the "certainty" of an "absolute viewpoint," or a "god's-eye or no-eye view." However, ZH are twice wrong. First, as discussed in section 9.3, the realist ontological position does not require certainty. Indeed, it specifically adopts fallibilism. Neither realism, nor science, nor truth, nor objectivity requires the certainty of a god's-eye view. Only when truth (with a small "t") is turned into TRUTH, and objectivity (with a small "o") is turned into OBJECTIVITY, by the philosophers' fallacy of high redefinition (or by other means) do truth and objectivity imply the certainty of a god's-eye view.

Second, Sayers (1987), whom ZH cite for authority, was not even discussing the realist ontological position. Indeed, it is instructive to review what Sayers was actually discussing. Because Wittgenstein "seemed to advocate . . . what has been called cultural relativism . . . an often despised thesis . . . and justifiably so" (Sayers 1987, p. 134), what did Wittgenstein's arguments imply? Specifically, did they imply that Wittgenstein's views also supported the relativist "equivalence postulate" (Sayers 1987, p. 135). That is, does Wittengenstein support the view of Barnes and Bloor that "For the relativist there is no sense attached to the idea that some standards are really rational as distinct from merely accepted as such. . . . Hence the relativist conclusion that they are to be explained in the same way" (Barnes and Bloor 1982, pp. 27–28). Sayers argues that Wittgenstein, in fact, "does not grant equal status to other belief systems—he does not accept the equivalence postulate of the strong thesis" (Sayers 1987, p. 142). Wittgenstein's views actually imply that there is no *need* for a "god's-eye view," because "the lack of some ultimate standard of rationality . . . is a mere bogeyman" (1987, p. 142). Indeed, for Sayers:

> That we exempt some beliefs from doubt, that we ground our other beliefs on these, that we use them as the context in which we argue or disagree on other matters, and that they are products of a particular social environment which varies from group to group, is no reason to adopt relativism or seek to overcome it [by a futile search for a god's-eye view]. This is the lesson to be learned from Wittgenstein. (Sayers 1987, p. 145)

In short, Sayers's work parallels the argument here: The absence of TRUTH and OBJECTIVITY (a God's-eye view) constitutes a bogeyman and no grounds for rejecting realism, adopting relativism, or abandoning the pursuit of truth and objectivity. Though ZH embrace the very bogeyman that Sayers (and Wittgenstein) warn against, marketing need not.

With the three preliminary issues out of the way, we begin our analysis of objectivity with the work of the logical empiricists and falsificationists. These philosophers addressed the issue of objectivity in the contexts of both the natural and the social sciences.

12.2 LOGICAL EMPIRICISM, FALSIFICATIONISM, AND OBJECTIVITY

The logical empiricists, led by Carnap (e.g., 1956), Hempel (e.g., 1965b), and Nagel (e.g., 1961), believed that all cognitively meaningful statements must be either empirically test-

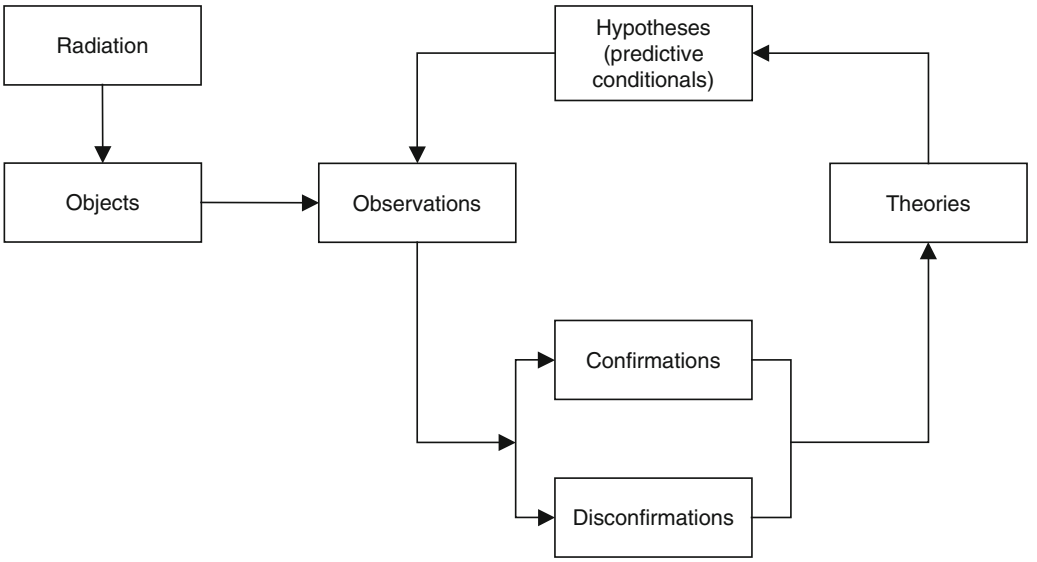
able (i.e., true or false by observational tests) or else purely analytical (i.e., true or false by definition). For science, all other statements would be impermissible, metaphysical, "empty talk." Therefore, theories (i.e., *groups* of statements) would have to be empirically testable, which could be guaranteed by segmenting unambiguously each theory's terms into (1) logical and mathematical terms, (2) theoretical terms, and (3) observation terms. The theoretical (unobservable) terms were not to be construed as genuinely *referring.* That is, every theoretical term was to be tied rigorously to a unique set of observation terms through a series of definitions (i.e., correspondence rules). Contra scientific realism, because the relationship between a theoretical term and its associated observable terms was purely analytical, a theoretical term was just a "shorthand" way of referring to a unique collection of observation terms.

For the logical empiricists, objectivity in science presupposed the existence of a nonproblematical, theory-free, observation language. Just as a child learns the meaning of the word "chair" ostensively—by adults pointing to examples of objects to which "chair" refers—the observation language of science, its observation terms, was thought to be purely observable, presuppositionless, or theory-free. Thus, observation terms (measures and data) would be a function only of the sensations that result from the retinal stimulation of the eye brought about by light reflecting from tangible objects. In short: observation terms = *f*(sensations).

For the logical empiricists, "Science strives for objectivity in the sense that its statements are to be capable of public tests with results that do not vary essentially with the tester" (Hempel 1970, p. 695). For the falsificationists, "the objectivity of scientific statements lies in the fact that they can be inter-subjectively tested" (Popper 1959, p. 44). These intersubjective, public tests, for both the logical empiricists and falsificationists, presumed a theory of the process of empirical testing. Figure 12.1 shows a path diagram of the process theory implicit in the logical empiricist and falsificationist view (hereafter, LEF view) of empirical testing. It should be stressed that Figure 12.1 is, intentionally, a parsimonious representation of the LEF view; only key concepts and their interrelationships are presented.

Starting from the right, the LEF view of empirical testing proposed that the scientist has a reasonably well-articulated theory (or theories) to be tested, and, from the theory, hypotheses (or predictive conditionals) are deduced. These hypotheses, roughly speaking, state: "If theory *T* is true, then, after conducting this test procedure, we should expect to observe phenomenon *K*." If the test procedure confirms the hypothesis, this confirmation or verification lends inductive empirical support to the truth of theory *T.* The LEF view maintained that if phenomenon *K* is not observed, then the disconfirmed hypothesis would imply that at least some aspect of theory *T* is false. Popper's (1959, 1963) falsificationism, relying on *modus tollens,* focused on the allegedly unique role of disconfirmations in the empirical testing process, in particular, and the development of science, in general (as in his position that science progresses by "bold conjectures and refutations"). In contrast, the logical empiricists focused on (see Hunt 2003a, section 3.3.4) the qualitative problem of when a positive instance of a generalization counts as a confirmation (which resulted in discussions

Figure 12.1 **The Logical Empiricist and Falsificationist (LEF) Model of Empirical Testing**



Source: Adapted from Hunt (1992a).

Note: Read—Radiation reflected from objects results in observations that are used to test our theories.

such as the paradox of the ravens) and the quantitative problem of how much confirmation a confirming instance of observational evidence provides a theory (i.e., they focused on a theory of inductive confirmation).

Starting from the left side of the model, the LEF view assumed that there were both objects and radiation in the world that existed independent of human perception. When radiation in the visible spectrum is reflected from objects in the world, such objects are directly observable and describable in a physicalistic language, in which each term directly refers to material things and their observable properties (Suppe 1977b). Thus, according to the LEF view, the objectivity of the empirical testing process relies on, among other things, scientists having at their disposal a nonproblematic, physical-thing, observation language. In this language, the meanings of the concepts were to be determined ostensively, that is, by "pointing at." As originally conceived by Schlick (1934, pp. 223, 226), such a foundation was intended to make science "absolutely certain," with "absolute fixed points," and an "unshakable point of contact between knowledge and reality."

The LEF theory of the process of empirical testing has much to recommend it. By assuming that radiation and objects exist in the world independent of perception, it denied idealism, which is "The theory that (a) the knower and the thing known do not have independent existence; all knowledge is knowledge of our conscious states and processes, and not of any external world; (b) that which is known is created by the human mind; matter is not real" (Angeles 1981, p. 121). Because idealism reduces empirical testing,

and, indeed, all of science to a charade, denying idealism seems reasonable. Furthermore, observation, deduction, and induction play important roles in empirical testing.

A major issue for the logical empiricists was whether their view of empirical testing and objectivity, as represented by the LEF model, precluded the social sciences from being *science.* That is, is it the case that the social sciences are inherently subjective?

12.2.1 Are the Social Sciences Inherently Subjective?

Ever since the social science disciplines emerged in the latter half of the nineteenth century, a central concern has been whether social science, like natural science, could produce objective knowledge. In the 1940s and 1950s, the logical empiricists entered the debate over the alleged subjectivity of social science. The debate is summarized in Ernest Nagel's (1961, pp. 473–85) classic work, *The Structure of Science.* Nagel's analysis begins with a discussion of the "subjective categories" argument of Weber (1947) and others. According to this argument, social science cannot be objective because (1) the subject matter of social science—in contrast to that of natural science—involves purposive human action directed at attaining various human ends. Therefore, (2) because the language used to explain these actions includes such terms as "motives" and "goals" that refer (if they refer to anything at all) to the private, unobservable, radically subjective experiences of individuals, then (3) social science must rely on such nonobjective techniques of inquiry as Weber's *Verstehen* (i.e., empathic understanding).

Nagel (1961) counterargues by first pointing out that many of the explanatory categories of the social sciences are objective—for example, the publicly observable environmental factors that influence human action. Therefore, knowledge-claims relying on such factors are also objective. Second, concepts such as motives and goals can be accessed through subjects' introspective reports, and these verbal responses can be regarded as publicly observable, objective data for grounding objective knowledge. Third, though researchers' projecting themselves empathically into their subjects may be helpful in producing hypotheses in the context of discovery, such claims could rightfully constitute scientific knowledge only by later verification in the context of justification. Such verification must be on the basis of evidence obtained from observing physical phenomena, human behavior, or people's verbal responses.

Nagel (1961) next identifies three different versions of Weber's "value-bias" argument against objectivity. These arguments are: (1) Because researchers' selection of topics reflects their values, what constitutes a desirable social order will inevitably bias their claims as to what kind of social order currently exists. (2) Because fact and value are fused together in social language (e.g., "murder" not only describes a behavior, i.e., to kill, but also evaluates that behavior, i.e., to *wrongfully* kill), social science's value judgments cannot possibly be eliminated. (3) The time during which researchers lived (see this chapter's epigraph) and their social class, as argued in historical relativism and Marxism, bias social science.

Nagel (1961) counters by agreeing that the importance of a research topic is a value "bias," but then contends that this innocuous bias also occurs in natural science. He

continues by agreeing that many, ostensibly objective, social science analyses are in fact disguised recommendations for social policy, but then urges social scientists to state their value assumptions fully, and he counsels their scholarly communities to adopt natural science's self-corrective mechanisms. These procedures, though neither infallible nor complete, he argues, can progressively diminish the effects of individual researchers' bias. Nagel then agrees that much social science discourse fuses fact and value, but maintains that careful scholarship can distinguish between statements that characterize states of affairs (i.e., positive) and those that appraise those states (i.e., normative). Finally, Nagel argues that "social class determines knowledge" can be justified by Marxists only if they themselves have transcended their own social class. (How else could they know this?) But if Marxists can transcend their own social class and attain a "unique privileged position" to justify "social class determines knowledge," then why cannot other social scientists in a similar manner transcend their social class? Thus, Nagel argues that Marxism is self-refuting. As to whether the social sciences can be objective, Nagel concludes:

> it is not easy in most areas of inquiry to prevent our likes, aversions, hopes, and fears from coloring our conclusions. It has taken centuries of effort to develop habits and techniques of investigation which help safeguard inquiries in the natural sciences against the intrusion of irrelevant personal factors; and even in these disciplines the protection those procedures give is *neither infallible nor complete.* The problem is undoubtedly more acute in the study of human affairs, and the difficulties it creates for achieving reliable knowledge in the social sciences must be admitted. . . .
>
> In brief, the various reasons we have been examining for the intrinsic impossibility of securing objective (i.e., value-free and unbiased) conclusions in the social sciences do not establish what they purport to establish, even though in some instances they direct attention to undoubtedly important practical difficulties frequently encountered in these disciplines. (Nagel 1961, pp. 488, 502; italics added)

The preceding review prompts five observations. First, Nagel stresses the fallibility of science. He, a prominent logical empiricist, did not advocate the "god's-eye view" of Bernstein's *objectivism.* Second, all the debate's participants presumed that natural science was objective, which to them meant "value-free" and "unbiased," and that this objectivity resulted, at least in part, from theory-free observation. For example, Nagel locates the value neutrality that is "pervasive in the natural sciences" (Nagel 1961, p. 485) in its "purely observable data" and "publicly observable subject matter" (1961, p. 474). Indeed, even his opponents ground the objectivity of natural science in its "presuppositionless investigation of empirical data" (Weber 1947, p. 76).

Third, some marketers ground their claim that objectivity is impossible by using arguments paralleling those in the Nagel-Weber debate. These include the discussions of "value-free" knowledge by Hirschman (1986), historicism by Fullerton (1986), and *Verstehen* by Hudson and Ozanne (1988) and Holbrook and O'Shaughnessy (1988). Fourth, these marketers do not provide, nor do they even attempt to provide, rebuttals

to Nagel's well-known and forceful analysis. Fifth, most participants in marketing's philosophy debates reject a major underlying premise of the Nagel-Weber debate: the premise that natural science is objective. These writers claim, ostensibly "in line with modern philosophical positions" (Peter and Olson 1989, p. 26), that is, in line with their historical relativism, that objectivity is impossible in *all* the sciences.

Before addressing the historical relativist view that objectivity is impossible in all science, we inquire: How would the logical empiricists have evaluated the objectivity of CW's knowledge-claim concerning salesperson performance? In reviewing CW's evidence, they would have paid special attention to the studies' measures, and, no doubt, would have criticized the measures of variables such as "aptitude," "motivation," and "role perceptions." These variables, which in the marketing literature are customarily presumed to be real latent traits that imply "reflective" measures (Howell 1987), would have violated the logical empiricists' belief that all theoretical terms are only arbitrary labels given to "collections of observables." For the logical empiricists, only a kind of "formative" measure (Howell 1987; Diamantopoulos and Winklhofer 2001; Howell, Brevik, and Wilcox 2007) is permissible. That is, even though Nagel accepted introspective reports from respondents as objective data, he admonished researchers never to consider such reports as "statements about private psychic states of the subjects" (Nagel 1961, p. 477). In our terms, such reports were never to be thought of as being reflective indicants of real, latent traits. Because CW's claim relies on studies employing reflective measures of hypothesized latent traits, its objectivity would have been questioned with much "clucking of tongues" by the logical empiricists.

How would the historical relativists regard the objectivity of CW's claim? As we shall see, they would dismiss all claims to its objectivity as "impossible" (Peter and Olson 1989, p. 26).

12.3 HISTORICAL RELATIVISM AND OBJECTIVITY

Discussions of Kuhn's (1962) "new image of science" dominated philosophy in the 1960s (see Hunt 2003a, section 4.2.1). Despite the fact that all the major positions in his *The Structure of Scientific Revolutions* were thoroughly discredited decades ago (Suppe 1977b), the book continues to be highly influential in marketing, management, social science, and consumer research. Much of *Structure* concerning the objectivity of science stems from Hanson's (1958) work on perceptual processes and scientific observation. Drawing on the psychology of perception, Hanson contended that the logical empiricists' assumption that science has access to theory-free data was false because experimental evidence from the psychology of illusions shows that what scientists "see" is determined by their theories and prior expectations. Indeed, a person:

> must learn some physics before he can see what the physicist sees . . . the infant and the layman can see: They are not blind. But they cannot see what the physicist sees; they are blind to what he sees . . . seeing is "a theory-laden" undertaking. (Hanson 1958, pp. 17, 19)

For Hanson, therefore, choices between rival theories cannot be made objectively because—in reference to whether the Sun revolves around the Earth (Ptolemy) or the Earth revolves around the Sun (Copernicus)—assuming that "Kepler and Tycho see the same thing at dawn just because their eyes are similarly affected is an elementary mistake" (Hanson 1958, p. 8). In this view, scientific observation = *f*(interpretation + sensations). However, anyone who thinks interpretation is anything other than *the* determining factor for scientific observation is making, for Hanson, an "elementary mistake."

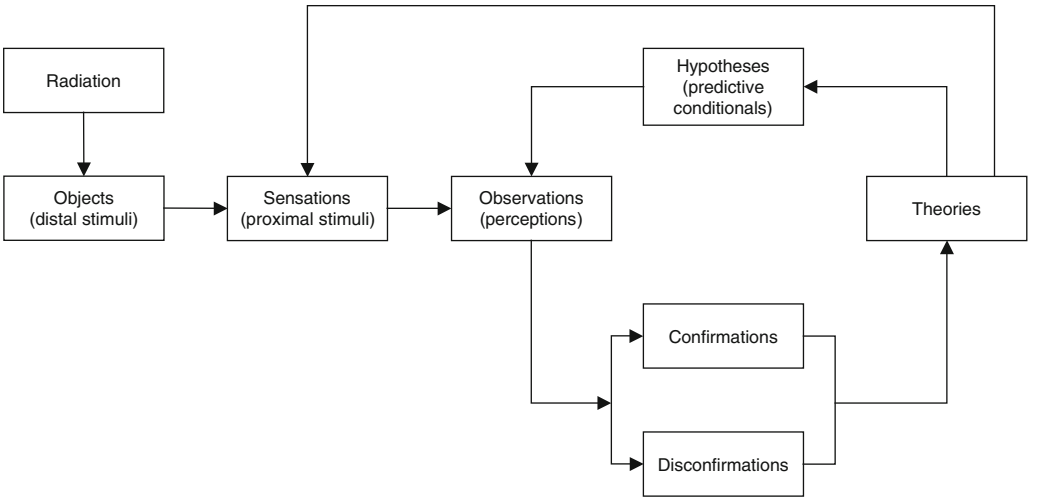
Extending Hanson's work, Kuhn (1962)—see Hunt (2003a), especially section 4.2.1—concluded that researchers' "paradigms" or "worldviews" guided the interpretive part of scientific observation and determined what researchers "saw." Therefore, collecting data for empirical testing could not adjudicate objectively any dispute among researchers who hold different paradigms or worldviews (because their respective paradigms would contaminate the data to guarantee each paradigm's truth). In short, (1) paradigms determine observations (observations are theory-laden) and (2) are incommensurable (there exists no theory-free means, no "privileged position," no "higher authority," no "god's-eye view," for adjudicating paradigm disputes). Therefore, (3) objectivity in science is impossible. For example, even today we are not warranted in claiming that Ptolemy was wrong and Copernicus right; we can say only that current believers in the Copernican paradigm embrace the results of the most recent paradigm shift.

Relying on the Gestalt theory of perception and laboratory experiments on reversible (duck/rabbit) figures, Kuhn (1962, p. 111) states, "What were ducks in the scientist's world before the revolution are rabbits afterwards." Scientific revolutions, like Gestalt shifts, cannot be accomplished by reasoned discourse about objective evidence (because data are theory-laden and paradigms are incommensurable), but must be produced by mass persuasion—much like religious conversions.

Kuhnian relativism dovetailed neatly with the Sapir-Whorf thesis of linguistic relativism. Sapir (1949; Mandelbaum 1949) and Whorf (1956), after studying American Indian languages and contrasting them with English, concluded that each language structures thought in such a manner that it determines the reality that speakers of that language perceive. One perceives only the reality that is permitted or required by one's language. Therefore, according to linguistic relativism, the interpretation inherent in scientific observation is theory-laden by each scientist's language, which makes objectivity impossible across languages.

Figure 12.2 displays a model of the process theory implied by the historical relativist view. It differs from the LEF model, shown in Figure 12.1, by interpreting scientific "observations" to be equivalent to "perceptions" and by adding a new concept, "sensations" (or, in the terms of the psychology of perception, "proximal stimuli"), and a path from "theories" to "sensations." The theories-to-sensations path implies the cognitive process of interpreting sensations by means of theories. By acknowledging that the proximal stimuli of retinal images, sensations, are always interpreted in the process of empirical testing, the model seems to fit what we know about perceptual processes better than the

Figure 12.2 **The Historical Relativist Model of Empirical Testing**



Source: Adapted from Hunt (1992a).

Note: Read—The observations used to test our theories are perceptions and result from sensations (proximal stimuli) informed by the theories being tested.

simpler LEF model. Yet, it does so at a price. As the path from "theories" to "sensations" implies, because science's observations are not neutral to its theories, empirical testing, apparently, cannot adjudicate impartially the disputes concerning rival theories. According to Churchland (1979, p. 41), "Perceptual judgments cannot provide a conceptually neutral level of factual information against which competing theories can always be effectively tested."

Embedded within the historical relativism of Kuhn and his followers are five intertwined arguments that purportedly defeat all claims to objectivity. Table 12.1 presents the arguments, cites the works of philosophers of science that ground them, and identifies the marketing articles in which they are used to support the view that marketing research cannot be objective. For example, Peter and Olson (1983, p. 120) claim that "science is subjective" because researchers cannot provide "objective, unbiased representations of the real world" that are "independent of any theory." They claim:

> It is impossible to collect data that are "theory neutral," since at least some implicit theory is needed to create measures and attach a meaning to them. . . . [W]e know that well-trained researchers can construct empirical data and results to support or refute almost any theory without violating "accepted standards" of research in a field. In fact, we have pointed out that the creation of empirical data and empirical results is a process that is controlled by the researcher. . . . [E]mpirical data and research do not and cannot "test" hypotheses or theories. Rather they typically provide demonstrations of the researcher's predilections or skill at post hoc rationalization. (Peter and Olson 1989, pp. 26–27)

Similarly, for Hudson and Ozanne (1988, p. 515), "observations are value-laden, theory-laden and interpreted." Likewise, Mick (1986, pp. 198, 207), after favorably discussing the linguistic relativism of Sapir (1949) and Whorf (1956), asserts, "Objectivity is impossible: theories precede facts and interpretation precedes perception." Therefore, for Mick, researchers are misguided when they believe "what we are studying is 'out there' [in the real world] rather than 'in here' [in the mind]."

Returning to CW's salesperson-performance knowledge-claim, relativism would hold that it cannot be objective because all the studies underlying the meta-analysis were theory-laden. That is, the findings were determined by the researchers' theories, perceptual processes, worldviews, paradigms, or language. In short, objectivity in marketing research is impossible because it is a "rigged game" (Olson 1987, p. 388). Indeed, Greenwood (1990, p. 553) points out that relativist views on objectivity have become dogmas, for "many contemporary philosophers of science appear to accept them uncritically." Though the impossibility of objectivity is accepted uncritically by many philosophers of science, marketers, management scholars, consumer researchers, and social scientists, this uncritical acceptance is unwarranted—as we shall now argue.

12.4 FOR OBJECTIVITY: THE "NEGATIVE CASE"

Table 12.1 summarizes the five primary arguments that ostensibly imply the impossibility of objective marketing research. The counterarguments discussed here provide the "negative case" for objectivity. That is, they show that the arguments *against* objectivity fail. The "positive case" for objectivity is made in section 12.5. How scientific realism addresses objectivity is then shown in section 12.6, which develops a scientific realist theory of empirical testing. Readers should note that Table 12.1 reveals that an impressively large number of distinguished scholars question (or deny) the possibility of objectivity in marketing research. Their views merit serious appraisal, not condescending dismissal.

12.4.1 Linguistic Relativism

The first argument maintains that the language of a culture determines the reality its members will see. For example, CW are members of the English-language community, and, hence, the fact that they speak and think in English determines what they observe. Consequently, their knowledge-claim cannot be objective, as it could not hold for other language communities. To think otherwise, so the argument goes, betrays simply the "ethnocentrism" of speakers of English in believing they have "privileged access" to the world.

If the thesis of linguistic relativism were true, objective inquiry across cultures (languages) would indeed be problematic. Fortunately, a host of studies on a large number of different languages strongly implies that language does not determine perceived reality. The works on perceptions of color are typical.

Heider and Oliver (1972) compared speakers of English with the Dani of West New Guinea in their ability to see different colors. After subjects had been exposed to a color chip

Table 12.1

Objectivity and Marketing Research

Arguments against objectivity	Philosophy sources[a]	Marketing sources[b]
1. Objectivity is impossible because the language of a culture determines the reality that members of that culture see.	Sapir (1949) Whorf (1956)	Moorman (1984, p. 53), Rexeisen (1984, p. 329), Mick (1986, p. 198), Ozanne and Hudson (1989, p. 7)
2. Objectivity is impossible because the paradigms that researchers hold are incommensurable.	Kuhn (1962) Feyerabend (1975)	Anderson (1983, p. 22), Anderson (1986, p. 158), Hudson and Ozanne (1988, p. 508), Ozanne and Hudson (1989, p. 1)
3. Objectivity is impossible because theories are underdetermined by facts.	Kuhn (1962) Feyerabend (1975) Goodman (1978)	Sauer, Nighswonger, and Zaltman (1982, p. 19), Anderson (1983, pp. 22, 26), Rexeisen (1984, p. 330), Hudson and Ozanne (1988, p. 515), Anderson (1989, p. 11), Ozanne and Hudson (1989, p. 2), Peter (1991, p. 535)
4. Objectivity is impossible because the psychology of perception informs us that a theory-free observation language is impossible.	Hanson (1958) Kuhn (1962) Feyerabend (1975) Goodman (1978) Churchland (1988)	Anderson (1983, pp. 20, 26), Peter and Olson (1983, p. 188), Peter (1983, p. 385), Rexeisen (1984, p. 329), Mick (1986, p. 207), Holbrook (1986, p. 238), Jaworski and MacInnis (1987, p. 163), Peter and Dacin (1991, p. 279)
5. Objectivity is impossible because all epistemically significant observations are theory-laden.	Kuhn (1962) Feyerabend (1975) Brown (1977)	Sauer, Nighswonger, and Zaltman (1982, p. 380), Anderson (1983, pp. 20, 26), Peter and Olson (1983, pp. 121–122), Mick (1986, p. 207), Jaworski and MacInnis (1987, p. 164), Olson (1987, p. 388), Hudson and Ozanne (1988, pp. 515, 518), Holbrook and O'Shaughnessy (1988, p. 401), Firat (1989, p. 95), Peter and Olson (1989, p. 26), Thompson (1990, p. 29), Peter and Dacin (1991, p. 280)

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[a]These philosophical and historical sources contain the foundational arguments that have led many writers to conclude that objectivity in science is impossible or problematic.

[b]These marketing sources either claim that objectivity is impossible on the basis of the arguments in the first column or imply that objectivity is impossible or problematic by citing and discussing the sources in the second column.

for five seconds, the chip was removed and they were asked to pick out the color from an array of forty color chips. Because the Dani language divides the entire color spectrum into only two categories (roughly, "light" and "dark"), if the position that language determines reality were correct, the Dani should differ greatly from speakers of English in their ability to match color chips (in this case, the Dani language would *limit* their perception). Heider and Oliver found virtually no differences at all; the cognitive maps of the color spectrum of the Dani and Americans were almost identical.

Similarly, Berlin and Kay's (1969) classic work on languages and human color perception, instead of finding evidence for linguistic relativism, actually found support for linguistic universalism. As just two of their many universals, Berlin and Kay found that languages having only three color terms always contain a term for red and those with four terms always contain a term for either green or yellow—but never both.[7] Steinfatt reviews the extensive literature on linguistic relativism and concludes, "The differences between languages are not to be found in what *can* be said, but in what it is *relatively easy to say*" (1989, p. 63; italics in original). Philosophers of science, using analytical (rather than empirical) methods, come to the same conclusion (Levin 1979).

A kind of linguistic relativism seems to underlie the proposed "rhetorical turn" in social science (Simons 1989), as advocated in economics by McCloskey (1985) and in marketing by Sherry (1991). For example, Sherry (1990) cites current literature as showing that "objectivity is not generally possible in statistics" and concludes that the "rhetoric of inquiry replaces logic of inquiry in postmodern epistemology" because "the theory-ladenness of scientific observation ensures that all of consumer research is an interpretive task" (Sherry 1990, p. 551). However, Sherry's conclusion notwithstanding, a deeper interpretation of the philosophical foundations of the "rhetorical turn" is that rhetorical analysis *presupposes* objectivity. As pointed out by Keith and Cherwitz (1989), themselves scholars of rhetoric, if rhetorical analysis is anything, it is the exploration of persuasive communication among human beings. Communication may be either successful (persuasive) or unsuccessful (unpersuasive). Therefore:

> to deny objective status to the other person in a communicative situation . . . would amount to talking to oneself . . . [and] since it is clearly the case that the vast majority of the time we understand each other well enough to get along there must be something objective about language that permits it to be a medium of exchange. (Keith and Cherwitz 1989, p. 202)

Keith and Cherwitz show the clear implications of the objective status of language for both rhetoricians and other researchers. If successful communications, both rhetorical and nonrhetorical, "entail an objective status for language and its users, it makes little sense to deny an equivalent objective status to the objects . . . of scientific inquiry." Therefore, "not only do rhetorical views of language and communication not defeat objectivity, they actually entail it" (Keith and Cherwitz 1989, p. 203).[8]

In conclusion, the thesis of linguistic relativism (in any form that would pose a threat to the objectivity of science) is simply false. In fact, in a misguided effort to avoid

"ethnocentricism," advocates of linguistic relativism actually embrace an extreme, if not bizarre, nihilism—for it is a truism that different language communities do, at least sometimes, successfully communicate. As put succinctly by Dennett (1981, p. 18), "the faculty of communication would not gain ground in evolution unless it was the faculty of transmitting true beliefs." Similarly, Jacobs (1989, pp. 79, 80) points out: "Awareness of propositions is not seen as a condition of their truth or validity. . . . Many people have never heard of botulinus or arsenic, while many others have; and the knowledge claim that botulinus and arsenic are lethal applies equally and indiscriminately to both groups."

So it is in marketing. Though knowing English is necessary for understanding CW's claim, thinking and communicating in English does not determine its truth or falsity for either speakers or nonspeakers of English. Language differences inhibit communications; they do not doom objectivity.

12.4.2 Paradigms Are Incommensurable

The second argument contends that objectivity is impossible because all knowledge-claims are embedded in paradigms that are incommensurable. Of all the concepts that Kuhn introduced, none has been investigated more thoroughly than paradigm incommensurability, and no one has yet developed an interpretation of it that would pose a meaningful threat to the objectivity of science. Because the topic has been extensively discussed in Hunt (2003a, section 4.2.1), our treatment here of the three interpretations of paradigm incommensurability (the meaning-variance, radical translation, and incomparability views) is brief.

The early meaning-variance view held that scientific terms change meaning from paradigm to paradigm—for example, Newtonian "mass" differs from Einsteinian "mass"—and was critiqued by Shapere (1964, 1966), Scheffler (1967), and Kordig (1971). The incomparability view that rival paradigms simply cannot be compared meaningfully was also critiqued by Scheffler, Shapere, and Kordig, and later by Laudan (1976) and Putnam (1981). When Kuhn, in his postscript to the 1970 edition of *Structure* (1970b, pp. 200–204), shifted to the radical translation interpretation, which suggested that the actual terms involved in one paradigm cannot be translated into the language of its rivals, it was analyzed by Kitcher (1978), Moberg (1979), and Levin (1979).

Cumulatively, the critiques were so conclusive that Kuhn (1976) virtually abandoned the concept of paradigm incommensurability. His final position was that all he ever meant to say was that choices among rival theories could not be made on the basis of a mathematical algorithm or deductive proof—a view nonproblematical for objectivity. In his own work on the history of quantum mechanics, Kuhn (1978) uses neither the word "paradigm" nor the expression "paradigm incommensurability." (Kuhn ceased to be a "Kuhnian.") Similarly, Feyerabend's (1987b, p. 81) later works conceded that incommensurability is not a problem for science but "is a difficulty for philosophers."

The key to understanding the incommensurability debate is to keep two points in mind. First, the very claim that two paradigms are incommensurable must imply that one can compare them—and, indeed, has compared them (Davidson 1973). (Otherwise, how could

one know they are incommensurable?) Thus, it is simply incoherent—it makes no sense—to extensively discuss, compare, and contrast different so-called paradigms in marketing and then to claim that they are incommensurable because they are "noncomparable" (Anderson 1989, p. 21). Second, for incommensurability to thwart our ability to choose objectively between two paradigms implies that they are *rival.* That is, they must make conflicting knowledge claims that require *choice* (as did Ptolemy and Copernicus). Most unfortunately, some marketers have blurred the distinction between (1) paradigms that are *rival* and (2) paradigms that are simply *different* (e.g., Ozanne and Hudson 1989).

As to the objectivity of CW's claim, for paradigm incommensurability to pose a threat, one would have to put forth a rival paradigm that not only resulted in a conflicting conclusion, but also resulted in a situation in which the choice could not be made on the basis of objective evidence. It is easy to find different paradigms in marketing if one uses a suitably loose interpretation of the word "paradigm," but no one has yet put forth different paradigms that (1) make conflicting knowledge-claims (and, thus, are rival) and (2) are in any meaningful sense incommensurable (objective choice is impossible). Until someone does so, we can only conclude that paradigm incommensurability poses no threat to the objectivity of CW's claim or to any others in marketing research.

12.4.3 Facts Underdetermine Theories

The third argument against objectivity contends that our theories about the world are underdetermined by our facts; no conceivable number of facts proves conclusively a theory's truth. Because theories contain lawlike generalizations (e.g., generalizations such as Newton's laws), even though the predictions of theories are validated every time they are tested empirically (the facts for 200 years supported Newton), new facts could invalidate the theory at some future time in some domain (e.g., tests on subatomic particles after 1900). Therefore, objectivity is impossible. In marketing, for example, there may well be other theories that could account for the same evidence (facts) marshaled by CW to support their claim, and the success of past empirical tests does not necessarily imply the success of future tests. Therefore, CW's claim, so the argument goes, cannot be objective.

Humean skepticism—as detailed extensively in Hunt (2003a, section 3.3.4)—which underlies the "facts underdetermine theories" argument, maintains that, though genuine knowledge of the external world can be had through observation, only deductive logic is permissible (Watkins 1984). As Calder, Phillips, and Tybout (1983, p. 113) put it, "An inductive argument . . . has no basis in logic."[9] Therefore, we cannot know the truth-content of any of our theories about the world because any process that reasons to the truth of a theory from its successful predictions is inductive and, therefore, improper.

Note that Hume's "problem of induction" presupposes the following standard for knowledge claims: one must not claim "I know" unless one knows with the certainty of deductive logic. The logical empiricists thought Humean skepticism was problematic for science, and they tried to solve it by developing a system of inductive logic (Carnap 1950). Popper (1972, p. 88), claiming that induction is not "a justifiable way of reasoning,"

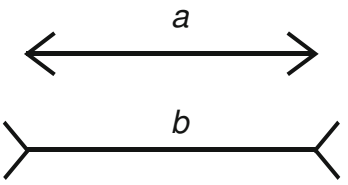
also accepted Humean skepticism as potentially defeating objectivity in science, and he attempted to locate the objectivity of science in its ability to falsify theories deductively, rather than confirm them inductively. Kuhn reveals the source of his own flirtation with relativism by explaining that "I make no claim to have solved the problem of induction" (1977, p. 332). Similarly, even some defenders of objectivity in marketing have been influenced by Humean skepticism (e.g., Anderson and Gerbing 1988; Calder, Phillips, and Tybout 1983; Calder and Tybout 1989; Sternthal, Tybout, and Calder 1987).

Scientists and science differ from philosophers and philosophy in many ways. Since the Enlightenment, one striking difference has been that many philosophers have embraced "foundationalism," which holds that (1) all knowledge-claims have foundations and (2) the foundations of science must be known with certainty (McMullin 1985). Thus, Schlick (1934, pp. 223, 226), the founder of logical positivism, believed scientific knowledge must be "absolutely certain," with "absolute fixed points" and an "unshakable point of contact between knowledge and reality." For Hume and countless philosophers thereafter, "to know" meant to know with the certainty of the deductive logic in geometry and mathematics. However, most practicing scientists have seldom viewed Hume's "problem" of induction as seriously thwarting knowledge-producing inquiry. Most assuredly, all knowledge-claims have foundations, and some claims have firmer grounds than others. But restricting "knowing" to "knowing with certainty" is not just being prudently conservative or cautious. Rather, because it denies even the possibility that we can learn or *know* on the grounds of accumulated experience, such a restriction amounts to nothing less than nihilism.

The evolutionary success of humankind as perceiving and thinking beings implies the possibility of learning from experience (and strongly suggests its realization). Therefore, marketing science should reject foundationalism and Humean skepticism, while embracing fallibilism: "In science all knowledge claims are tentative, subject to revision on the basis of new evidence. The concept 'certainty' belongs to theology, not science" (Hunt 1983b, p. 372). Similarly, Suppe (1977b, p. 726) urges philosophers of science to adopt fallibilism, because it "appears to accord most closely with the actual means whereby science evaluates putative knowledge claims in the attempt to undergo the objective growth in scientific knowledge."

The preceding discussion warrants three conclusions. First, objectivity in marketing research is not doomed by Hume's problem of induction (the fact that facts underdetermine theories), except to those who insist misguidedly that one can never know without knowing with certainty. In marketing, though we do not know the truth of CW's claim with certainty, we nevertheless have good reasons for believing CW, and for acting on the basis of that belief. Second, because scientific realism specifically rejects Humean skepticism in its adoption of "inductive realism" (see Chapter 9), scientific realism provides grounds for objectivity in marketing research. Third, Bunge's (1967b, p. 324) metaphor of "weighing the evidence" is most appropriate for marketing. Because empirical tests do not imply certainty, the community of marketing researchers can provide its clients with no *more* than a reasoned "weighing" of the evidence. As fiduciary agents, we should provide no *less.*

Figure 12.3 **The Müller-Lyre Illusion**



12.4.4 The Psychology of Perception

The fourth argument claims that, because the psychology of perception implies that our cognitively held theories determine what researchers perceive, theory-neutral observations in science are impossible. That is, because researchers see what their theories tell them is there, scientific observation is theory-laden. In short, as shown by the path from theories to sensations in Figure 12.2, scientific observation = perception = *f*(sensations interpreted by theories). In marketing, therefore, because the perceptions (observations) of the researchers on which CW's claim relied were "laden" by their theories (or worldviews, paradigms, etc.), CW's claim is not objective.

At the outset, note the implausibility on *a priori* grounds of the psychology of perception argument. If researchers' theories determined in any strong manner what they perceived, or *saw,* would they not always (as Peter and Olson [1989] maintain) find strong support for their theories? If so, why are most correlation coefficients so small in social science? Why do goodness-of-fit indexes in structural equation modeling usually reject the very models researchers are proposing? That is, if researchers' theories determine perception, why are there so many disappointing surprises. Indeed, why are there surprises at all? Scheffler (1982, p. 44) points out that to accept the "theories determine perceptions" view is "absurdly, to deny the common phenomena of surprise, shock, and astonishment, as well as the reorientations of belief consequent upon them." Nonetheless, the philosophy debates in marketing claim that the psychology of perception tells us just that.

Are researchers' perceptions "laden" with cognitively held theories? Fodor (1984, 1988), a philosopher of psychology, explores the psychological grounds for the argument, and he points out that all supporters of the theory-ladenness of observation rely on experiments from the psychology of perception, such as the Müller-Lyre figures (see Figure 12.3).

The Müller-Lyre illusion is, of course, that, though line *b* is perceived to be longer, it is in fact exactly the same length as line *a.* Thus, perception, it is argued, necessarily involves interpretation by our theories of the world. As Kuhn (1962, p. 113) put it, "The rich experimental literature [in psychology] . . . makes one suspect that something like a paradigm is prerequisite to perception itself." However, Fodor points out that Kuhn and his legion of followers completely misinterpreted—and most continue to misinterpret—the findings of perceptual psychology.

Consider when students in a psychology course find out that the Müller-Lyre lines are an illusion or when others (the reader?) are told the truth about the lines' lengths. Even

armed with this piece of knowledge, this background "theory" about the world, people (you?) still *perceive* line *b* to be longer than *a.* How can this be so if our cognitively held theories determine perception? As an everyday example, consider the little words on the bottom of the mirror on the passenger's side of our cars: "Objects in mirror are closer than they appear."[10] Even after we read (and believe) the cautionary message, the cars behind us still *look* deceptively distant. Fodor points out, "All the standard perceptual illusions exhibit this curiously refractory character: knowing that they are illusions doesn't make them go away" (Fodor 1984, p. 34). Though perceptual psychology implies that perception = *f*(interpreted sensations), it manifestly does not imply that perception = *f*(sensations interpreted by cognitive theories):

> [The psychology of perception does not tell us that perception is] saturated with cognition through and through. On the contrary, it suggests just the reverse: that how the world looks can be peculiarly unaffected by how one knows it to be. . . . Because the way one sees the world is largely independent of one's theoretical attachments, it is possible to see that the predictions—even of theories that one likes a lot—aren't coming out. . . . [T]herefore belief in the best [of] science is rational because it is objective. (Fodor 1984, p. 34)

Not only does a proper reading of the implications of perceptual psychology for objectivity yield exactly the opposite of Kuhn's conclusion, but so do informed interpretations of Kuhn's duck/rabbit and "sunrise" examples. Ambiguous, "reversible figure" drawings from Gestalt psychology do not imply that theory determines perception. Theorizing that an ambiguous drawing can be seen as either a duck or a rabbit does not enable one to effect a Gestalt switch, nor does believing that the task can be accomplished so enable. Indeed, many subjects see only one figure, despite their firm convictions that it is possible to see two. Similarly, to Kuhn and Hanson's (now famous) question as to whether Kepler and Brahe "saw" the same thing at dawn, we should answer "yes." Both *saw,* or perceived, a "sunrise." Kepler perceived a "sunrise" and, on the basis of his cognitively held theory of the cosmos (Copernicus), believed correctly that it was an illusion. Brahe perceived a "sunrise" and, on the basis of his cognitively held theory of the cosmos (Ptolemy), believed incorrectly that it was true. Why do neither paradigms, nor theories, nor beliefs, nor even firm convictions determine or "laden" perception? It is because there is a "part of observation . . . [that is] bottom-up and in some important sense theory neutral" (Raftopoulos 2001, p. S188). That is, the human brain is not "programmed" for "ladening perception." Work in neurobiology on the brain's "wiring" reveals the absence of efferent nerves connecting higher brain centers (wherein reside our theories) with our perceptual mechanisms:

> We do not seem to understand any general and widespread class of cases in which higher brain centers appear to alter the character of empirical information. . . . As we know it, the wiring of the brain does not seem to suggest, either very strongly or otherwise, a role for beliefs or theories in perception. (Gilman 1991, p. 499)

The preceding discussion does not imply that the results of scientific observations—that is, "measures" or "data"—do not rely on human perception. Indeed, the accuracy or veridicality of perception is essential to science.[11] On a continuum from total veridicality to complete illusion, where does human perception lie? Evolutionary theory suggests that it is substantially veridical. The fact of human species survival implies that early humans were capable of distinguishing veridically (at least more often than not) alligators from logs, solid earth from quicksand, tigers from domestic cats, wolves from dogs, and human friend from human foe. In fact, the success of human evolution gives us confidence in the veridicality of perception unless our cognitively held theories of the world warn us of an illusion.[12]

As to marketing, the psychology of perception poses no threat to the objectivity of CW's claim. On the contrary, the psychology of perception explains how human perception enabled the researchers on whom CW relied to be objective, if in fact they were. The extraordinary recalcitrance of human perception to researchers' theories of the world enables them to strive for (and, thus, perhaps attain) objective knowledge about that world.

12.4.5 Epistemically Significant Observations

The fifth argument contends that all epistemically significant observation in science is theory-laden, which makes objectivity impossible. This fifth, much more sophisticated, argument must be distinguished carefully from the fourth.[13] Kuhn's mistake, so argument 5 goes, was to deny that researchers observe or see the same things. He erred by denying that human perception allows medical researchers to see the same nine inches of mercury in a cylindrical tube, physicists to see the same nine-degree deflection of a needle on a meter, social science researchers to see the same nine checkmarks on a questionnaire, or marketers to see the same box "9" (see section 11.7.2) checked on an intentions-to-buy scale. What Kuhn should have argued, so the argument goes, is that such observations are not epistemically significant in research. To be epistemically significant and play their designated role in empirical testing, such "percepts," or "raw" observations must be interpreted by cognitive theories. For example, nine inches of mercury means ninety degrees centigrade, a nine-degree needle deflection means ninety volts, nine checkmarks on a questionnaire mean a score of ninety on a brand attitude scale, and a checkmark in box "9" means a high inclination to buy. It is not perceptual psychology that informs us that observation ("measures" or "data") is theory-laden, it is the undisputed, actual practice of science itself. In brief, epistemically significant observation = *f*(observations or "percepts" interpreted by theory). Therefore, because all epistemically significant observation in research is theory-laden, objectivity is impossible.

As to marketing and CW's claim, advocates of the theory-ladenness of epistemically significant observation would point out that it was only through the application of theory that checkmarks on questionnaires became measures of "aptitude," "motivation," and "role perceptions." Therefore, all the studies on which CW relied were theory-laden, which defeats the objectivity of CW's claim.

Shapere (1982) and Greenwood (1990) point out two crucial mistakes in the theory-

laden argument that prevent it from being compelling, or even moderately persuasive. First, advocates of the theory-laden argument fail to distinguish between two very different kinds of theories that are involved in empirical testing. On the one hand, there are theories that specify relationships among our concepts. These explanatory theories are the ones we test empirically. For example, CW compare the explanatory theory salesperson performance = *f*(skills, role perceptions, and motivation) with its rival salesperson performance = *f*(personal and physical traits). On the other hand, testing CW's explanatory theories required accessing a great amount of background information, or what we will call "measurement theory."[14] Just as studying cells in biology presumes measurement theory related to the use of a microscope, studying sales performance, motivation, and so on requires theories related to questionnaires, Likert scales, factor analysis, and so forth. Quite clearly, testing CW's explanatory theories presumed a great amount of measurement theory. Therefore, also unquestionably, epistemically significant observations ("data") in science are not theory-free.

However, the theory-informity of data by measurement theories does not doom objectivity. Advocates of theory-ladenness have failed to identify the characteristics of an observation language that are necessary for objectivity—their second critical error. Recall that empiricist philosophers of science (see section 12.2) thought that objectivity required a theory-free observation language. Current philosophy of science now recognizes that objectivity requires a theory-neutral language, not a theory-free one. Our data, measures, or observations need not be theory-free, but only neutral. Neutral to what? Neutral to the theory or theories being tested. Our measurement theories must not presume the truth of our explanatory theories; they must not "beg the question." In multiple regression terms, one must not have the same "thing" on both sides of the equal sign. In structural equation modeling terms, our measurement model must not guarantee our structural model.

For CW's claim, do the measurement theories bias the analysis toward finding that aptitude, skills, and motivation are more important than personal characteristics in explaining sales performance? If so, such theory-informity compromises objectivity. If not, then objectivity is not threatened. Obviously, it is within our capabilities to examine CW's measures for such threats to objectivity. Furthermore, good researchers do precisely that.

The preceding discussion not only warrants that the theory-informity of epistemically significant observation does not make objective research impossible, but, much more strongly (surprisingly?), it implies that (measurement) theory-informity actually helps ensure objectivity:

> [Science] learns how to observe nature, and its ability to observe increases with increasing knowledge. . . . In the process of acquiring knowledge, we also learn how to learn about it, by learning (among other things) what constitutes information and how to obtain it—that is, how to observe the entities we have found to exist, and the processes we have found to occur. (Shapere 1982, pp. 513–14)

As our measurement theories progress, our epistemically significant observations improve and, thus, the theory-informity of observation helps ensure research objectivity.

Kepler and Brahe's problem was that their measurement technology was primitive (in comparison with ours). Since then, fortunately, science has progressed in both its explanatory and measurement theories. Only after the development of x-ray diffraction techniques, a new "measurement theory," could researchers confirm the double helix structure of DNA by making epistemically significant observations (Greenwood 1990). In marketing, objectivity has been furthered by the introduction and development of multidimensional scaling (Green and Carmone 1969), conjoint analysis (Green and Rao 1971), true score measurement theory (Churchill 1979), causal modeling procedures (Bagozzi 1980), and item response theory (Singh, Howell, and Rhoads 1990). By such theories, marketing's "theory-laden" research becomes more objective, not less. As Trout (1998, p. 113) puts it, "at least some of the quantitative methods and practices of science reliably detect and identify some of the central posited entities, processes, states, properties, and events in the social and behavioral sciences." So it is in marketing.

12.5 FOR OBJECTIVITY: THE POSITIVE CASE

Should the marketing research community pursue the ideal of objectivity? The preceding discussion establishes the negative case that there is nothing in modern philosophy of science or perceptual psychology that makes objectivity either impossible or undesirable.[15] We now explore the positive case for objectivity. At the outset, we acknowledge that, because a community's regulative ideals are fundamental values, objectivity cannot be guaranteed or even known conclusively to be achieved. To restrict a community's goals, as discussed in section 11.4.3, only to those that can be conclusively achieved or guaranteed would be, misguidedly, to treat the regulative ideals of a community on a par with its tangible, short-run objectives.

When should any community of inquirers pursue the ideal of objectivity? Our argument here parallels our argument for truth in section 11.8. Both Scheffler (1982) and Harré (1986) maintain that the ideal of objectivity is related to the extent to which a community is relied on or trusted by others. Harré even develops his argument with the aid of marketing language. Almost as though he had read Levitt's (1960) "Marketing Myopia," Harré (1986, p. 12) contends that scientific communities are "in the business of producing practically reliable scientific knowledge." He asks, "Why are the products of the scientific community marketable"? (1986, p. 13), and he answers, "It can be nothing but the quality of the goods" (p. 15). Indeed, "scientific knowledge is itself defined in moral terms. It is that knowledge upon which one can rely" (p. 20). Trust, the belief that one can rely confidently on others, sometimes arises out of direct personal experience. However, people often do not rely on others on the basis of direct knowledge, but because of others' roles in their communities (e.g., "professor" and "market researcher"). As to the marketing research community, the corporate clients of commercial marketing researchers, most assuredly, rely on them. Equally assuredly, students, academicians, practitioners, government agencies, and consumers at times rely on the output provided by academic marketing researchers (Monroe et al. 1988). Hence, the marketing research community, encompassing both commercial and academic researchers, has a moral, professional obligation to pursue objectivity.

What does the pursuit of objectivity imply? For Scheffler, objectivity requires the "commitment to fair controls over assertion" (1982, p. 2). For Harré, it requires the community to exercise "quality control over its products" (1986, p. 12). In short, communities producing knowledge that will be (and can be) relied on by others must have a set of norms to maintain quality control over assertion. Harré aptly points out a minimum norm: "For there to be public reliability, something must exist independently of whomsoever first found it" (1986, p. 12). The minimum norm, therefore, for a community's knowledge-claims to be relied on is for the community to reject reality relativism and constructionism. Because such doctrines specifically state that their knowledge-claims do not "touch base" with any reality other than that "constructed" by the researchers themselves, such doctrines self-defeat even the potential for being relied on by outside clients (see section 11.7.2).

It is important to recognize that the norms a community develops to control assertion in no way imply that such assertions are value-free or theory-free. Indeed, the clients of the marketing research community have the right to expect, nay insist, that our assertions be thoroughly "laden" with many values, one such being the ideal of objectivity. Similarly, clients have a right to expect our assertions to be informed, well-informed, by our theories—our very *best* theories. However, the norms that control assertion in the marketing research community must strike a reasoned and reasonable balance between being overly restrictive and overly permissive. For example, the norms, as implemented through such mechanisms as the peer review process, must not be so permissive that "anything goes" in assertion, for such a stance would be destructive of reliability and trust. (Indeed, if "anything goes," peer review is unnecessary.) If the norms are so restrictive that "nothing goes," the situation is equally undesirable. Avoiding error by remaining forever silent is not an acceptable option. What are the current norms that control assertion in marketing research? Where along the continuum between "anything goes" and "nothing goes" do these norms lie? Should these norms be modified? If so, how? These are questions worth investigating—objectively, that is.

12.6 A REALIST THEORY OF EMPIRICAL TESTING

Section 12.2 developed path diagrams of the process of empirical testing held by the logical empiricists and falsificationists (Figure 12.1) and historical relativists (Figure 12.2).[16] These models were used to analyze the five major arguments against objectivity: (1) linguistic relativism, (2) paradigms are incommensurable, (3) facts underdetermine theories, (4) the psychology of perception, and (5) epistemically significant observations. The positive case for objectivity was then advanced. This case, as is the case for truth, is grounded in trust.

It must be admitted, however, that the arguments in section 12.4 are unlikely to be probative for many researchers. Consider the views of Smith and Deemer:

> within Anglo-American philosophical circles, a good case can be made that it was Hanson (1958) and then, most definitely, Kuhn (1962) who brought the problem-

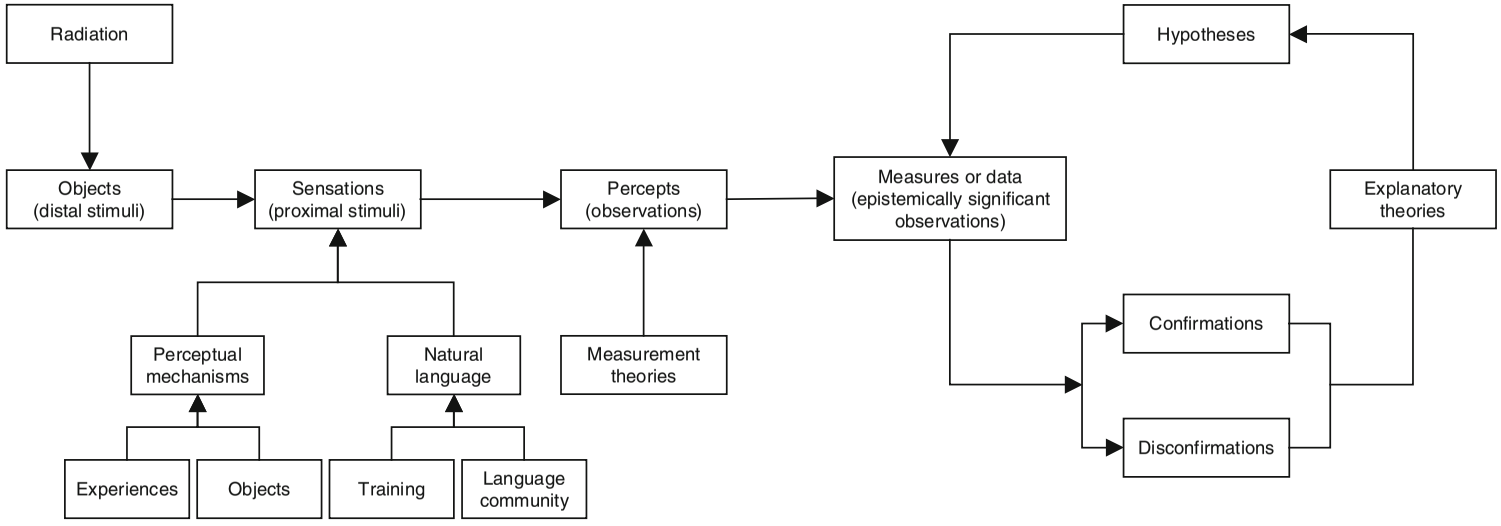
> atic associated with the dualism of subject and object to the forefront. . . . By the mid-to-late 1980s, the work of numerous people made it apparent that any claim of or hope for "theory-free knowledge" was untenable on all counts. . . . There is no possibility of theory-free observation and knowledge, the duality of subject and object is untenable, no special epistemic privilege can be attached to any particular method or set of methods, and we cannot have the kind of objective access to an external, extralinguistic referent that would allow us to adjudicate from among different knowledge claims. (Smith and Deemer 2000, p. 879)

Now, Smith and Deemer are respected, serious education researchers, not members of some fringe minority. Furthermore, readers should note that their claims appear not in an obscure, ideological journal, but in the highly regarded *Handbook of Qualitative Research,* edited by Denzin and Lincoln (2000). Readers should also note that their position that "we *cannot* have . . . objective access to an extralinguistic referent" (2000, p. 879; italics added) is viewed as so obviously correct in mainstream qualitative research that no contrary view is even worth presenting in the over 1,000-page *Handbook.* However, Smith and Deemer's position that objectivity is impossible puts them in a quandary. They realize that "the charge of relativism as anything goes does not make sense, [and] if we no longer can speak of the absolute without embarrassment, then we must realize we cannot speak of 'anything goes' without embarrassment" (Smith and Deemer 2000, p. 895). Therefore, many mainstream qualitative researchers (and others) seek an alternative to the extremes of dogmatism and dogmatic skepticism shown in Table 11.1. The purpose of this section is to resolve Smith and Deemer's quandary by developing such an alternative. The alternative's aim is the development of a scientific realist theory of empirical testing that addresses the various challenges related to objectivity. As with section 12.2, I use a path diagram of empirical testing to explicate the theory.

12.6.1 The Realist Model

Figure 12.4 displays the model of the proposed realist theory of empirical testing, which not only adopts and extends the insights of Fodor, Greenwood, and others, as discussed in section 12.2, but also retains the valuable aspects of the logical empiricalist falsificationist, and historical relativist views. First, what was labeled "observations" in Figure 12.2 is split into (a) "percepts," the immediate results of visual processing, that is, the results of only perceptual discrimination and recognition, and (b) "data" or "measures," the results of percepts informed by theories, that is, epistemically significant observations. Second, what was lumped together under "theories" in Figure 12.2 is now split into "explanatory theories," "measurement theories," "perceptual mechanisms," and "natural language." Explanatory theories are the theories being tested by a process involving (among other things) observation reports, whereas measurement theories are the ontological and other theories that are assumed (explicitly or implicitly) in the process of testing the explanatory theories under investigation. Percepts, the immediate output of visual processing (e.g., the checkmarks on the Stanford-Binet test), become data (e.g.,

Figure 12.4 **The Scientific Realist Model of Empirical Testing**



Source: Hunt (1992a). Reprinted by permission of the American Marketing Association.

Note: Read—From upper left, radiation reflected from objects (distal stimuli) results in sensations (the proximal stimuli of retinal images). From lower left, our experiences with objects in the world we inhabit contribute to developing our perceptual mechanisms' ability to recognize patterns of proximal stimuli, and our language community trains us to use our natural language to label the recognized patterns. Therefore, our percepts result from sensations or proximal stimuli being informed by our perceptual mechanisms and labeled by our natural language. Our data, measures, or epistemically significant observations result from percepts being informed by our measurement theories. These data can be used objectively to confirm or disconfirm hypotheses, and, by implication, objectively test the explanatory theories, if the explanatory theories being tested do not prejudge the test by biasing the measurement theories.

such epistemically significant observations as "IQ of 90") only after being interpreted with the aid of measurement theories.

A major deficiency of the historical relativist view is that it not only blurs the important distinction between explanatory and measurement theories, but also confuses "sensations are theory-laden" with (1) sensations are informed by perceptual *mechanisms* and (2) patterns of sensations are labeled by natural language. Even Fodor (1984, p. 39) blurs important distinctions when he states that perceptual mechanisms must have access to "a grammar," which he refers to as a "background theory." Contrary to both Fodor and the historical relativist view, the proposed theory maintains that percepts do not result from sensations, or proximal stimuli, being informed by theory. Rather, they result from perceptual mechanisms interpreting proximal stimuli in conjunction with the interpreter's natural language. Percepts do not require theories, but (1) mechanisms that can perceptually discriminate among proximal stimuli (e.g., the proximal stimuli resulting from light reflected from such disparate distal stimuli as ducks and cyclotrons), (2) mechanisms that can recognize *patterns* of proximal stimuli associated with distal objects (this pattern results from a duck, that from a cyclotron), and (3) a set of *conventions* for labeling the recognized patterns (my natural language community calls these patterns of proximal stimuli "ducks" and those "cyclotrons").

In contrast, at the minimum, *theories* of or about the world are cognitive constructions that purport to explain some aspect(s) of the world. Because no *mechanism* is a cognitive construction, no mechanism can be a theory of or about the world—a perceptual mechanism is no exception. Similarly, because a *convention* is "any commonly agreed upon statement whose truth is based not upon the way things are in nature but upon that agreement itself" (Angeles 1981, p. 49), no set of conventions (alone) can purport to explain any aspect of the world. Therefore, no set of labeling conventions is a theory of or about the world; the set of labels used to designate certain percepts (in, e.g., empirical tests of theories) by speakers of any natural language (e.g., English) is no exception.

Although the capability to process proximal stimuli is inborn universally to healthy humans, experience and training (as shown in Figure 12.4) play major roles in its development. We learn to recognize ducks and cyclotrons by *experiencing,* in the particular world we inhabit, ducks and cyclotrons (or representations thereof). We are then *trained* by members of our language community to label patterns of proximal stimuli associated with distal objects in our world, that is, ducks are "ducks," and cyclotrons are "cyclotrons." The perceptual recognition, *seeing,* of distal objects, however, does not imply understanding them. To see objects successfully, that is, to visually recognize cyclotrons and label them "cyclotrons," is often an important precursor to understanding them. Indeed, a premise of this section is precisely the view that path diagrams, a heuristic enabling the reader to see depictions of the views, can contribute to understanding the debate over objectivity. However, visual processing is neither a necessary precursor for understanding (consider the blind) nor is it sufficient (consider a small child). To accurately perceive, that is, see, recognize, and label the—in Greenwood's (1990) terminology—inten*tional* objects of observation (e.g., these are checkmarks) implies nothing about—again, in Greenwood's terms—the inten*sional* contents of observation. Specifically, it implies nothing about

whether one understands the purpose, functioning, characteristics, or nature of the perceived intentional objects (e.g., "these checkmarks on the Stanford-Binet test imply an IQ of 90").

A major problem has been that advocates of historical relativism have indiscriminately used "seeing" as a metaphor for "understanding" (Scheffler 1986, p. 268). For example, Hanson (1958, p. 59) states: "A blind man cannot see how a timepiece is designed, or what distinguishes it from other clocks. Still, he may see that, if it is a clock at all, it will embody certain dynamical properties; and may explain the action to his young apprentice." Clearly, the second "see" in the quote is used completely as a metaphor for "understand." In most discourse, even scholarly discourse, using "see" metaphorically is nonproblematic. However, Hanson (1958) and others purport to establish that laboratory experiments from the psychology of perception (where "seeing" implies visual processing) inform us that scientific observation in empirical testing is laden with the explanatory theories being tested. Any such discourse must scrupulously preserve the distinction between "seeing" and "understanding."[17] Otherwise, as historical relativist discussions tellingly reveal, confusion reigns.

The proposed theory contends that, although percepts (the immediate output of perceptual processing) are formed from proximal stimuli, they are not proximal stimuli interpreted by our explanatory theories of the world. Contra Figure 12.2, it is simply not the case that small children do not or cannot see what the physicist sees. Indeed, small children with a modicum of worldly experience and language training, none of which involving what are (or ought to be) called "explanatory theories," can see (perceptually discriminate, recognize, and label) ducks as "ducks" and cyclotrons as "cyclotrons." What the small child cannot do, without extensive experience or training, is understand ducks and cyclotrons in the manner of the zoologist and physicist, respectively. Such accomplishments, the intensional contents of scientific observation, require percepts that have been informed, very well-informed, by a host of complex theories about the world.

The distinction here between percepts and data differs from the position advocated by Greenwood (1991). Indeed even though he distinguishes between exploratory theories and explanatory theories, and between the intentional object of observation and the intensional contents of observation, he does not distinguish between percepts and epistemically significant observation. In advocating a realist approach to social psychology, Greenwood states:

> It is simply not true for example that our perception of trees and tables is unmediated by theoretical interpretation, any more than our perception of planets and anxiety. In order to observe that there are trees or pigeons present, we need to employ intersubjectively-agreed exploratory theories about how trees and pigeons manifest themselves to our "naked" eyes and ears. (Greenwood 1991, p. 109)

Contrary to Greenwood, psychologists do not perceive—see—anxiety, for it is precisely the kind of concept whose presence is suggested by perceiving checkmarks on questionnaires (or other behavioral or physiological indicators) and interpreting them by

some measurement theory. The percept of a checkmark requires no measurement theory. Nor does perceiving tables and chairs. Recognizing that checkmarks, trees, and tables are checkmarks, trees, and tables and using the labels "checkmarks," "trees," and "tables" does not require—contra Greenwood—an "intersubjectively-agreed exploratory theory." What does it require? As shown in Figure 12.4, in addition to radiation, objects, proximal stimuli, perceptual mechanisms, and experiences with checkmarks, trees, and tables, the recognition process requires only the learning of an intersubjectively agreed-upon set of conventions for the application of one's natural language. This set of conventions does not constitute, and should not be confused with, an explanatory theory about the world. To repeat a prior point, no set of conventions, by themselves, can constitute a theory about the world. Because the truth content of a set of conventions is "based not upon the way things are in nature" (Angeles 1981, p. 49), a set of labeling conventions is not a theory about the world. This is not to say that one may not have theories about, as Greenwood puts it, "how trees and pigeons manifest themselves" to us. It is to say that such theories are not required to perceive trees and pigeons.

Why did Greenwood not distinguish between percepts and data? It appears that he associated any such distinction with the foundationalist notion of direct perception, which implies for him that percepts would have to be "immune from error" (Greenwood 1991, p. 109). However, no such immunity is implied by the proposed theory, for our percepts may be nonveridical. But the absence of such immunity does not vitiate the important distinction between percepts and data.

12.6.2 Müller-Lyer Revisited

To illustrate how the proposed theory, modeled in Figure 12.4, can be employed, let us reexamine Fodor's (1984) analysis of the Müller-Lyer illusion. As discussed in section 12.4.4, the illusion results in the percept that line *b looks* longer than line *a.* Is this percept veridical? Fodor concludes it is not, but his contention is elliptical—it skips over the underlying rationale. By explicating Fodor's (implied) rationale, we can further explicate the proposed theory of empirical testing. The rationale may be framed as two relatively trivial, rival theories: (1) All lines that look the same length *are* the same length, and (2) some lines that look longer than others are not so in fact, and line *b* in the figure labeled "Müller-Lyer" is such a case. What is required to test the rival explanatory theories are some measures, that is, epistemically significant observations. Here, the problem is easy. We place a tape measure or equivalent device first on line *a,* then on *b,* and observationally compare them. The resulting percepts are that both lines *a* and *b* extend to the identical hash mark on the tape. Therefore, relying on the measurement theory underlying our use of tape measures, these percepts become epistemically significant observations, and we conclude that lines *a* and *b* are of equal lengths. This empirical test would tend to support theory 2 and undermine theory 1.

For most people, the preceding empirical test would be dispositive for the Müller-Lyer problem. But why is this the case? It is because we have confidence in our measures, that is, our percepts informed by our cognitively held measurement theories. In short, we have more confidence in the veracity of the measurement theory underlying the tape measure,

and, thus, in our theoretically informed percepts (lines *a* and *b* are equally long when measured) than we do in the veridicality of our nontheoretically informed percept (line *b* looks longer than *a*). Therefore, we allow our percepts informed by our measurement theories (our "data") to overrule our nontheoretically informed percepts. Could we be wrong? Could the real illusion be that lines *a* and *b* in Müller-Lyer are equally long? It is (remotely) possible. Our tape measure theory could be faulty (as well as, of course, the procedures used in conducting the test). Empirical testing does not "buy us" certainty—as the experience of physics with Newtonian mechanics reminds us (or should remind us). Humility is a virtue everywhere; science is no exception.

Is empirical testing objective? Do we believe *objectively* that lines *a* and *b* are the same length? There are several threats to objectivity implicit in the theory underlying Figure 12.4, and each will be addressed separately.

12.6.3 Threats to Objectivity

What is required for objectivity? Unfortunately, advocates of Figure 12.2, including historical relativists and others, often confuse objectivity with omniscience. Agreeing with Shapere (1982), the proposed theory contends that what is required for objectivity in any particular empirical test is not an observation language that is omnisciently—"god's-eye"—neutral to all theories. Rather, what is required for objectivity is an observation language that is neutral to the theories being tested. Therefore, in terms of Figure 12.4, the first threat to objectivity would be the existence of a direct path from the explanatory theories being tested to the measurement theories informing our percepts. "Direct path" means that a particular explanatory theory informs our measurement theories in such a manner as to bias a test of it in its favor (over a rival). If such a path exists, the test is not objective. The existence of such a path in each case of empirical testing is a contingent issue.

In our Müller-Lyer empirical test, it seems obvious that our measurement theories do not prejudge the case; our tape measure and our procedure for applying it can be assumed to be unbiased. Clearly, other cases may not be so obvious. Also clear, however, is that *all* measurement theories in *all* cases of empirical testing do not necessarily prejudge the veracity of a particular theory over its rivals, as advocates of historical relativism contend. All empirical tests do not beg the question being investigated. Moreover, the potential biasing effects of explanatory theories on measurement theories is a contingent issue that can be addressed specifically by the researchers and those appraising their research. For example, the "two-step" procedure in structural equation modeling advocated by Anderson and Gerbing (1988) attempts to minimize the potential bias of explanatory theories on measurement theories.

A second threat to objectivity would be a direct path in Figure 12.4 from the explanatory theories to be tested to perceptual mechanisms, as advocates of historical relativism claim is implied by the psychology of perception. As discussed in section 12.4.4, the proposed theory claims that no path is there. That is, our percepts, what we literally see, are not determined by our explanatory theories of the world. To generate data, epistemically significant observations, our percepts must be informed by our theories. But this does

not mean our percepts are determined by our theories. Because Kuhn (1962), Churchland (1988), and others rely so heavily on the "inverting lenses" experiments in psychology to justify their opposing contention, reviewing these experiments is warranted.

Briefly, the inverting lenses experiments involve fitting normal humans with lenses that have the effect of inverting the orientation of all visual information accessible to the brain. The world, therefore, looks "upside down" (Kottenhoff 1957). However, when subjects fitted with the lenses are forced by practical necessity to interact with common objects, in about a week the subjects' perceptual mechanisms adjust to the lenses and the illusion of the world being upside down is claimed to fade away. The subjects now claim to see the world "right-side up."

Do the inverting lenses experiments not show that there is a path from our explanatory theories of the world to our perceptual mechanisms? No. As the proposed theory grants, our experiences with objects in our world (i.e., subjects with inverting lenses interacting with objects) unquestionably help develop our perceptual mechanisms (and, indirectly, help us form percepts from our proximal stimuli). However, the import of the inverting lenses experiments is not that our explanatory theories of the world shape our perceptual mechanisms. The import is that our perceptual mechanisms—marvelous as they are—are sufficiently adaptive that we could learn to function in other worlds by experiencing them, even worlds where we would have to reach down for objects that look up, and vice versa. What historical relativism advocates need to show, as Fodor's discussion of this issue reveals, is not that worldly experiences help shape our percepts, but "that you can somehow reshape the perceptual field by learning physics" (Fodor 1988, p. 194). He knows of no examples that have been shown of this kind of reshaping, and he "strongly suspect[s] that's because there aren't any" (1988, p. 194). I also strongly suspect this. An example from psychology will demonstrate explicitly why we are unlikely ever to be provided examples of the required kind of perceptual reshaping.

Consider two psychologists, *A* and *B*. Psychologist *A* strongly adheres to the theory that intelligence is determined primarily by heredity, and *B* believes, equally strongly, in the absolute primacy of the environment. In terms of the proposed theory, what would be required for there to be a path of influence from "theories" to "perceptual mechanisms"? What would be required for "reshaping the perceptual field" in a manner that *A*'s percepts would differ from *B*'s in potential empirical tests of their respective theories? We should recall that it is checkmarks on, say, the Stanford-Binet test form that constitute instances of percepts in the theory. Therefore, psychologist *A* would have to be so influenced by the genetic theory that, when compared with *B,* he/she would either not see the same checkmarks or see the checkmarks to be in different locations on the test. Even if *A* and *B* are ideologues of the most extreme kind, it is difficult to imagine them having percepts that will differ on account of paths from their explanatory theories to their perceptual mechanisms.

Therefore, it is no wonder that no examples of perceptual reshaping of the required kind have been offered, nor should we expect any. What have been offered, and in abundance, are such examples as "IQ tests do not measure intelligence," or "IQ tests are culturally biased," or "the study did not properly control for all environmental factors." But these are not examples of *theories* determining *percepts.* These are all examples of the poten-

tial biasing effects of (in the proposed theory's terms) different measurement theories in the formation of data from percepts. No one denies (or should deny) that measurement theories can potentially bias empirical tests and compromise objectivity.

What is denied, and should be denied, is that (1) our measurement theories *necessarily* compromise objectivity by begging the question and (2) our explanatory theories of the world compromise objectivity by *determining* our percepts. Indeed, as our explanation of the Müller-Lyer illusion illustrates (i.e., arrowheads pointing inward or outward affect perceptions of distances and lengths), instead of our perceptual mechanisms employing explanatory theories, we employ explanatory theories to help us explain and understand our perceptual mechanisms.

Recall that, contrary to Greenwood (1990), the theory argues that applying natural language—an intersubjectively agreed upon set of conventions—to what our perceptual mechanisms recognize does not constitute applying an explanatory theory. Therefore, the conventions of natural language are not a direct threat to objectivity. Nevertheless, there could be a potential threat to objectivity if there were a direct path from the explanatory theories being tested to natural language and then a second path from natural language to perceptual mechanisms. In effect, these two paths would mean that the language within which the explanatory theories are stated (e.g., English) determines not just the labels that communities affix to the proximal stimuli that their perceptual mechanisms recognize, but that each language determines what members of a language community can or cannot see. If such were the case, then an objective choice between an explanatory theory stated in, say, English could not be made vis-à-vis a rival stated in French, because English determined what English speakers saw and French determined what French speakers saw. In essence, this is the Sapir-Whorf thesis of linguistic relativism. As discussed in section 12.4.1, however, the thesis of linguistic relativism is false: The evidence supports the view that the two paths do not exist and, therefore, do not constitute a threat to objectivity.

12.6.4 Implications for Marketing and Social Science

The proposed theory provides practical guidance for marketers and social scientists in their appraisals of the objectivity of empirical research. In particular, the theory implies that the following question should always be explored: Do the measurement theories that inform the percepts and, thereby, generate data in a particular empirical test bias the test for or against one theory over its rival? Also, as to the philosophy debates, the implication of the proposed realist theory of empirical testing is that those marketers and social scientists using "theory-ladenness of observation" as a justification for their preferred research approach must abandon historical relativism or find alternative justificatory arguments—for the premise that *no* research can be objective is false.

More generally, the implications of the proposed theory for other debates in marketing, social science, and philosophy are noteworthy. Consider Murphy's (1990) discussion of the ongoing debate pitting advocates of realism against those (she describes) as favoring "constructive empiricism," the "strong program" in sociology, "postmodernism," and Bernstein's (1983) "mitigated relativism."[18] She claims: "The debate over scientific or critical realism

is characterized by confusion" (Murphy 1990, p. 291). Indeed, the participants "talk past one another" (p. 292). They do so even though "modern realists accept the same facts about science as do the sociologists," including "the underdetermination of theory by data; the theory-ladenness of data; [and] the effects upon perception of prior belief" (p. 299). What accounts for the confusion? It stems from, Murphy claims, the fact that the participants "operate with different 'paradigms' of rationality" (p. 299). Therefore, she recommends, "the best solution to the realist debate is to simply drop the issue" (p. 302).

We can agree with Murphy that a major factor contributing to the confused character of the debate in marketing, social science, and philosophy over realism has been the fact that both realists and antirealists have accepted the same (or at least similar) premises concerning the "problem" of theory-ladenness and perception. For example, Wylie argues for what she calls "mitigated objectivism" because, in part, it is "not the case that data are entirely plastic, that they are so theory-permeated that facts can be constituted at will in whatever form a contextually appealing theory requires" (Wylie 1989, p. 16). However, in the same paragraph Wylie agrees that "the vagaries of research practice make us aware that we very largely see or understand what our background knowledge and theoretical commitments prepare us to see—and that we can see things differently or even *see different things* when these presuppositions change" (1989, p. 16; italics added). By "see different things" because of "theoretical commitments," Wylie accepts one of the major premises that led Bernstein (1983) to his "mitigated relativism." That Wylie accepts Bernstein's foundational premise on perception is implied when she specifically distinguishes between "see things differently" and "see different things." Once one agrees that theories make us "see different things," the realist theory of empirical testing argues, one abandons a powerful resource for arguing against relativism, both "mitigated" and otherwise.

In conclusion, contra Murphy (1990), the proposed theory of empirical testing implies that the "best solution" to the "realist debate" in marketing, social science, and philosophy is not to drop the issue. Rather, realists should stop accepting the same foundational premises or "facts about science" as the antirealists. First, realists should reject the "fact" that "believing is seeing" and start from the premise that the theory-independence of percepts enables science to test objectively its theories. Second, realists should reject the "fact" that the theory-ladenness of data based on observation is problematic for science, and start from the premise that the theory-informity of data based on observation is what promotes the objectivity of science. And third, realists should reject the "fact" that the foundations of science must be immune from error and start from the premise that the best evidence is that percepts are substantially veridical and scientific data are substantially trustworthy. That is, starting the debate using the proposed theory, I suggest, may lead toward a resolution of differences, rather than to a confusion of issues. At least, such resolution is a possibility—and the hope, for marketing, for social science, and for philosophy of science.

12.7 FOR A COMMENCEMENT

The controversies in marketing theory that have been analyzed in Chapters 9–12 continue unabated, and their respective debates continue to evolve. Many of the philosophy debates

have been less productive than need be the case. A major reason is that they have been uninformed by the philosophy of science, uninformed by history, and uninformed by reason. The purpose of Section 3 of this book has not been to close the debates, but to argue for the commencement of an informed, reasoned approach to marketing's philosophical controversies. The clients of marketing research trust us to deliver our best-informed, best-reasoned efforts. Although they can ask no more from us, they surely deserve no less.

Several decades ago, Philip Kotler and Sidney Levy (1969a) proposed that the issue facing nonprofit organizations was not whether they were going to *do* marketing; the issue was whether nonprofit organizations were going to do it poorly, or do it well. Likewise for marketing, the issue is not whether we are going to *do* philosophy, for doing philosophy cannot be avoided: Some philosophical position or framework underlies every single controversy on every single issue in every single area of marketing theory and research. The issue is not whether marketing should do philosophy, it is whether we shall do it poorly, or do it well. If the preceding analyses of the controversies in marketing's philosophy debates help marketing do it well, its goal will have been achieved. Again, that is the hope.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. What are the differences between objectivity and objectivism, between pursuing objectivity and attaining objectivity, and between "personal bias" and "methodological bias"? Is personal bias a more (or less) serious problem, compared with methodological bias, in marketing research?
2. What is sophistry? As pointed out in section 12.1, many marketers maintain that relativists and subjectivists do not intend for their views on objectivity to be taken seriously by anyone. Is it the case, therefore, that such marketers are accusing relativists and subjectivists of sophistry? What arguments could be put forth that defend the view that relativists and subjectivists are *not* engaging in sophistry?
3. Barnes and Bloor (1982, pp. 27–28) maintain that "for the relativists there is no sense attached to the idea that some standards are really rational as distinct from merely accepted as such . . . hence the relativists' conclusion that they are to be explained in the same way." What does it mean to say that "they are to be explained in the same way"? In what other way might they be "explained"?
4. What does it mean to state that "the absence of TRUTH and OBJECTIVITY constitutes a bogeyman"? How does the philosophers' fallacy of high redefinition relate to "bogeyman"? What is the difference between theory-free observation and theory-neutral observation? Why is this distinction important?
5. There are many ways to bias the results of a survey by choosing particular wordings of the questions. Develop three examples of questions that have biased wordings. Use the analysis of Nagel (1961) to discuss the different kinds of bias in the questions and how such bias could be minimized. Would it be the case that your revised wordings would be bias-free? What is the difference between *linguistic relativism* and the view that some languages are *more precise* than others?

6. There are several different uses for quotation marks. One use is to signify that a particular writer is being quoted. A second use is to signify that one is referring to a particular *word* and not to what the word refers to. Writers in marketing's philosophy debates, instead of writing truth, write "truth." Similarly, one often sees "objectivity," not objectivity. In these cases, what do the quotation marks signify? Do you agree with this use of quotation marks? When would such a use be appropriate? When would it be inappropriate? (Hint: see Stove 1982.)
7. Why does Greenwood (1990) use the phrase "theory-informity," rather than "theory-laden"? Should he?
8. Under what circumstances is quantitative research trustworthy? Under what circumstances is quantitative research not trustworthy? Is quantitative research inherently more trustworthy than qualitative research? Less trustworthy? Why?
9. Figure 12.4 has a box for "measurement theories." However, there is no box specifically labeled "research design." Should there be? If yes, where should it go? If no, why should there not be such a box?
10. If empirical testing does not "buy us" certainty, as section 12.6.2 maintains, what does it "buy us"? Is Figure 12.4 a "causal" model in the sense of structural equation modeling? If yes, why? If no, what kind of model is it?

NOTES

1. This chapter draws heavily on Hunt (1992a, 1993, 1994a).

2. "Incoherently," because the word "illusion" implies a false perception of reality. But to know that a particular perception of reality is false can be accomplished only by showing that it has been compared with a more veridical one. Therefore, the very claim "objectivity is an illusion" implies that which it rejects. That is, to be meaningful, "objectivity is an illusion" implies the existence of the very objective reality that it ostensibly contends is illusory. How else could one know that objectivity is an illusion?

3. Ethnocentrism, a major sin according to postmodernist social philosophy and a highly pejorative label in sociology, is "the belief that our ways, because they are ours, must be closer to the truth, goodness, and beauty than are the ways of others" (Shweder and Fisk, 1989, p. 99). Unfortunately, marketing's "consumer ethnocentrism" has been defined and measured as the belief that "purchasing imported products is wrong because . . . it hurts the domestic economy, causes loss of jobs, and is plainly unpatriotic" (Shimp and Sharma 1987, p. 280). Though ethnocentric consumers would support "buy American," those who "buy American" because of such factors as the fear of "loss of jobs" are not exhibiting ethnocentrism, for "loss of jobs" would be superfluous to ethnocentric consumers. (Such consumers would believe that domestic products are obviously superior to foreign-made ones.) Therefore, the CETSCALE seems closer to measuring a belief in "protectionism" than to measuring genuine "ethnocentrism." More generally, whenever marketing borrows concepts that have pejorative connotations, we have a *special* responsibility to exercise caution.

4. Holt (1991, p. 61) further argues that, because the use of audits, triangulation, and purposive sampling does "not necessarily lead to more trustworthy research" and does "not insure greater trustworthiness," researchers "should be discouraged from using them to gain added authority in the written representation of the research." Instead "interpretations should be judged on their insightfulness . . . and their ability to convince the reader, no more." Such an argument, however, self-destructs. If all procedures must *necessarily* lead to, or ensure, trustworthy findings before being in the written report of the research, then an article's method section in a research report should always be blank. On the contrary, why, except for the bogeyman fear of "objectivism" and "positivism," should researchers

avoid conducting (and reporting) audits, triangulation, and purposive sampling? Such procedures may contribute to what Holt calls "insightfulness" or even to Holt's goal of "convince the reader."

5. Though no such monolithic entity as "Western culture" exists (for the individual cultures of Western nations, as well as Eastern ones, differ greatly), there is something that might be properly labeled "Western intellectual tradition," one of whose central tenets is openness to the ideas of outsiders (Barchas 1989, p. 27; Short 1988, p. 13; Silber 1990, p. 35). Thus, "Western ethnocentrism" (believing in and advocating Western ideas *because* they are Western) is oxymoronic.

6. As an example of the genetic fallacy, "The Nazis condemned the theory of relativity because Einstein, its originator, was a Jew" (Salmon 1963, p. 12). Ethnophobia, a peculiarly Western phenomenon, is the fear or hatred of one's own culture, as exemplified by the works of relativists such as Feyerabend (1987a) and many postmodernist writings (Barzun 1991, p. 34).

7. For other studies on the universality of color perception, see Brown and Lenneberg (1954), Lantz and Stefflre (1964), and Stefflre, Castillo-Vales, and Morley (1966).

8. Similarly, Maki (1988, p. 108) explores McCloskey's advocacy of rhetoric in economics and concludes that "the acceptance of realism does not depend on the rejection of any of the major insights of the rhetorical approach to studying economics, and vice versa . . . metatheoretical realism is subscribed to by the rhetorical approach as formulated by McCloskey and Klamer."

9. Calder, Phillips, and Tybout, though embracing Humean skepticism, do not conclude that objectivity is impossible. Rather, they conclude that falsificationism is required for objectivity.

10. I thank Roy Howell, Texas Tech University, for the example.

11. How human beings learn to apply language veridically (both verbal utterances and written words) to the world we see is complex. (Lenneberg's 1967 work is considered seminal. See also MacCormac 1985, especially pp. 80–85, for a useful introduction to the semantic marker theory.) One thing is clear, however: contra Kuhn and his followers, cognitively held theories about the world, that is, beliefs about how the world is structured, do not determine perceptual recognition. Such theories or beliefs are neither necessary nor sufficient for seeing. People can identify veridically and label as a "unicorn" a picture of a horse with a horn without believing in unicorns. (Theories about what exists in the world are not necessary for perception.) Likewise, in Kuhn's duck/rabbit example, people can believe in ducks, and, indeed, believe that others can see a duck in the duck/rabbit drawing, but may never be able recognize, see, or perceive the drawing as a duck. (Theories about what exists in the world are not sufficient for perception.) Finally, though one would need to know English to apply the label "duck" to what one perceives, one need not know English to *see* Kuhn's duck. (Linguistic relativism is false.) Even Whorf's (1956) famous example (supposedly illustrating linguistic relativism) was incorrect. Rather than the Eskimos having several words for the single English word "snow," they have several words that equate with several English words, such as "blizzard," "dusting," and "avalanche" (Martin and Pullman 1991).

12. See Giere (1988) and Harré (1986) for applications of evolutionary theory to science.

13. It is unsurprising that the marketing literature conflates arguments 4 and 5. As pointed out by Shapere (1982), they are seriously conflated throughout even the philosophy of science literature. For an example, see Churchland's (1988) discussion of "perceptual plasticity" and Fodor's (1988) reply, especially his pages 197–98.

14. Greenwood (1990) uses the term "exploratory theory." However, in marketing and other social sciences, "measurement theory" seems closer to describing what Greenwood is referring to. As Greenwood points out, the background information constituting a measurement theory in one context may be an explanatory theory one desires to test in another context.

15. Even nonrelativist philosophers who are strongly opposed to scientific realism now agree (see Laudan 1990, especially pp. vii–x).

16. This section draws extensively on Hunt (1992a, 1994a).

17. Throughout this section all uses of "see" and "perceive" are meant to refer to vision or the results of vision, whereas "view" is meant to be taken as a metaphor for "framework," "model," or "theory."

18. The label "mitigated relativism" is Wylie's (1989) characterization of Bernstein's (1983) position.

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PART 4

TOWARD A GENERAL THEORY OF MARKETING

This section, *Toward a General Theory of Marketing*, argues that a theory of competition developed by Robert M. Morgan and me, labeled resource-advantage (R-A) theory, provides the foundations for, that is, it is *toward*, a general theory of marketing.[1] The section focuses on three different arguments. First, because marketing takes place within the context of competition, a general theory of *marketing* should be consistent with the most general theory of *competition*. Accordingly, because R-A theory is a general theory of competition, it is an appropriate foundation for working toward a general theory of marketing.

Second, the closest thing to a general theory of marketing today is Alderson's (1957, 1965) functionalist theory of market behavior (Hunt 1983a). Therefore, I argue that R-A theory is toward a general theory of marketing because it accommodates and extends key concepts and generalizations from Alderson's theory and integrates them into a broader theoretical framework. Third, R-A theory is a positive theory; yet much of marketing is normative. Furthermore, *is* does not imply *ought*. However, recall from section 8.2 that good positive theory can provide a foundation for good normative theory. Therefore, I argue that R-A is toward a general theory of marketing because it provides a foundation for the normative area of marketing strategy.

Chapter 13 begins the argument that resource-advantage theory is toward a general theory of marketing by, first, providing brief overview of R-A theory, before discussing how the R-A research program was developed, and then, because the foundational premises of any theory are central to understanding it, reviewing in detail the arguments for each premise of R-A theory.

Chapter 14 develops arguments that constitute the first and second grounds for maintaining that resource-advantage (R-A) theory provides the foundations for a general theory of marketing. Chapter 15 develops the third argument that R-A is toward a general theory of marketing.

NOTE

1. Sheth (2001) has argued that Hunt (2000b) should have been titled "A General Theory of Marketing" instead of "A General Theory of Competition". In contrast, the view here is that R-A theory *is* a general theory of competition, but it is only *toward* a general theory of marketing.

13

ON THE RESOURCE-ADVANTAGE (R-A) THEORY OF COMPETITION

> Science is, above all else, a reality-driven enterprise. Every active investigator is inescapably aware of this. It creates the pain as well as much of the delight of research. Reality is the overseer at one's shoulder, ready to rap one's knuckles or to spring the trap into which one has been led by overconfidence, or by a too-complacent reliance on mere surmise. Science succeeds precisely because it has accepted a bargain in which even the boldest imagination stands hostage to reality.
>
> *—Paul R. Gross and Norman Levitt (1994)*

In the spring of 1994, Robert M. Morgan and I began working on a new theory of competition. The theory came to be known as the resource-advantage (hereafter, "R-A") theory of competition, and the original article developing the foundations, structure, and implications of the theory was published in the *Journal of Marketing* in 1995 (Hunt and Morgan 1995). Since then, scores of publications have contributed to developing the theory and have used it as a theoretical foundation for explaining, predicting, and understanding phenomena. The works developing resource-advantage theory have been written by dozens of authors and cited in several thousand other articles and books.

The purpose of this chapter is to begin the argument that resource-advantage theory is toward a general theory of marketing. First, the chapter provides a brief overview of R-A theory. Second, it discusses how the R-A research program was developed. Third, because the foundational premises of R-A theory are central to understanding it, the chapter concludes by reviewing in detail the arguments for each premise.

13.1 AN OVERVIEW OF R-A THEORY

R-A theory is an evolutionary, process theory of competition that is interdisciplinary not only in the sense that it has been developed in the literatures of several different disciplines, but also in that it draws on and has affinities with numerous other theories and research traditions, including evolutionary economics, "Austrian" economics, the historical tradition, heterogeneous demand theory, differential advantage theory, industrial-organization economics, the resource-based tradition, the competence-based

Table 13.1

Foundational Premises of Resource-Advantage Theory

P_1: Demand is heterogeneous across industries, heterogeneous within industries, and dynamic.
P_2: Consumer information is imperfect and costly.
P_3: Human motivation is constrained self-interest seeking.
P_4: The firm's objective is superior financial performance.
P_5: The firm's information is imperfect and costly.
P_6: The firm's resources are financial, physical, legal, human, organizational, informational, and relational.
P_7: Resource characteristics are heterogeneous and imperfectly mobile.
P_8: The role of management is to recognize, understand, create, select, implement, and modify strategies.
P_9: Competitive dynamics are disequilibrium-provoking, with innovation endogenous.

Source: Hunt and Morgan (1997).

tradition, institutional economics, transaction cost economics, and economic sociology. However, R-A theory is not a composite of these theories and research traditions (Hunt 2000c). R-A theory is a general theory of competition that describes the *process* of competition. Figures 13.1 and 13.2 provide schematic depictions of R-A theory's key constructs, and Table 13.1 provides its foundational premises. Our overview will follow closely the theory's treatment in Hunt (2000b).

13.1.1 The Structure and Foundations of R-A Theory

Using Hodgson's (1993) taxonomy, R-A theory is an evolutionary, disequilibrium-provoking, process theory of competition in which innovation and organizational learning are endogenous; firms and consumers have imperfect information; and entrepreneurship, institutions, and public policy affect economic performance. Evolutionary theories of competition require units of selection that are (1) relatively durable, that is, they can exist, at least potentially, through long periods of time, and (2) heritable, that is, they can be transmitted to successors. For R-A theory, both firms and resources are proposed as the heritable, durable units of selection, with competition for comparative advantages in resources constituting the selection process.

At its core, R-A theory combines heterogeneous demand theory with the resource-based theory of the firm (see premises P_1, P_6, and P_7 in Table 13.1). Contrasted with perfect competition, heterogeneous demand theory views intra-industry demand as significantly heterogeneous with respect to consumers' tastes and preferences. Therefore, viewing products as bundles of attributes, different market offerings or "bundles" are required for different market segments within the same industry. Contrasted with the view that the firm is a production function that combines homogeneous, perfectly mobile "factors" of production, the resource-based view holds that the firm is a combiner of heterogeneous, imperfectly mobile entities that are labeled "resources." These heterogeneous, imperfectly mobile resources, when combined with heterogeneous demand, imply significant diversity

as to the sizes, scopes, and levels of profitability of firms within the same industry. The resource-based theory of the firm parallels, if not undergirds, what Foss (1993) calls the "competence perspective" on competition in evolutionary economics and the "capabilities" approaches of Teece and Pisano (1994) and Langlois and Robertson (1995).

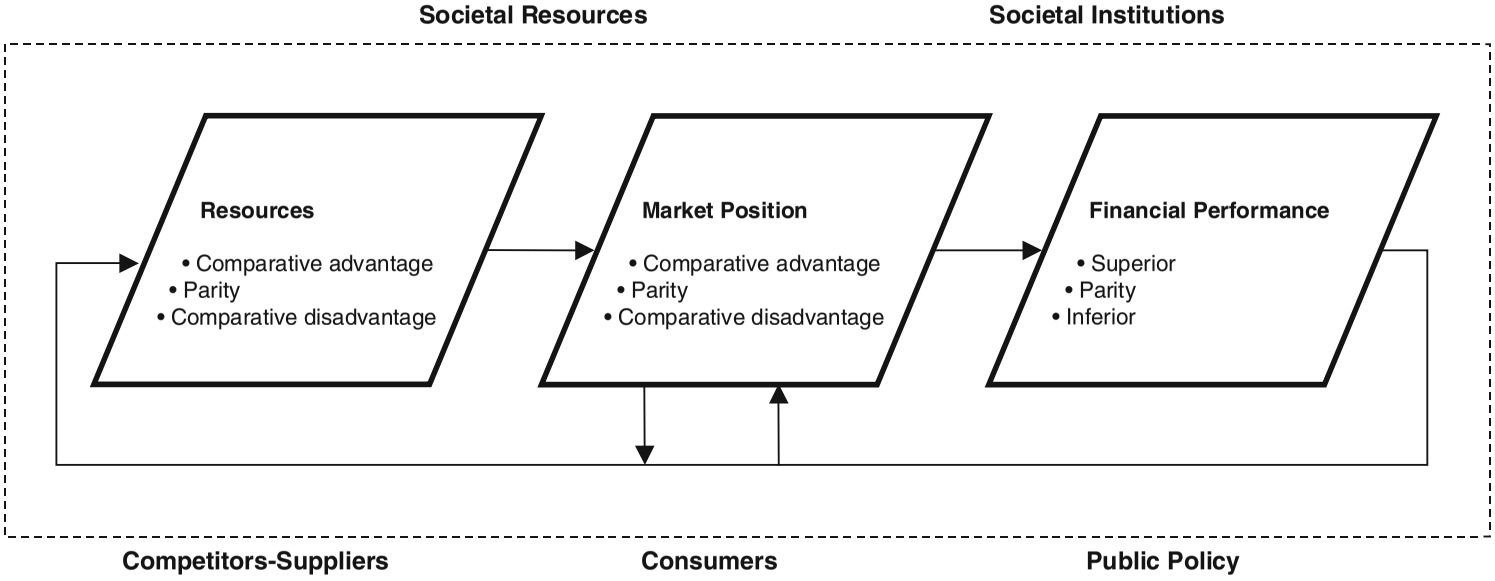
As diagrammed in Figures 13.1 and 13.2, R-A theory stresses the importance of (1) market segments, (2) heterogeneous firm resources, (3) comparative advantages/disadvantages in resources, and (4) marketplace positions of competitive advantage/disadvantage. In brief, market segments are defined as intraindustry groups of consumers whose tastes and preferences with regard to an industry's output are *relatively* homogeneous. Resources are defined as the tangible and intangible entities available to the firm that enable it to produce efficiently and/or effectively a market offering that has value for some marketing segment(s). Thus, resources are not restricted to land, labor, and capital, as in neoclassical theory. Rather, resources can be categorized as financial (e.g., cash resources, access to financial markets), physical (e.g., plant, equipment), legal (e.g., trademarks, licenses), human (e.g., skills and knowledge of individual employees), organizational (e.g., competences, controls, policies, culture), informational (e.g., knowledge from consumer and competitive intelligence), and relational (e.g., relationships with suppliers and customers).

Each firm in the marketplace will have at least some resources that are unique to it (e.g., very knowledgeable employees, efficient production processes, etc.) that could constitute a comparative advantage in resources that could lead to positions of advantage (i.e., cells 2, 3, and 6 in Figure 13.2) in the marketplace. Some of these resources are not easily copied or acquired (i.e., they are relatively immobile). Therefore, such resources (e.g., culture and processes) may be a source of long-term competitive advantage in the marketplace.

Just as international trade theory recognizes that nations have heterogeneous, immobile resources, and it focuses on the importance of comparative advantages in resources to explain the benefits of trade, R-A theory recognizes that many of the resources of firms within the same industry are significantly heterogeneous and relatively immobile. Therefore, analogous to nations, some firms will have a comparative advantage and others a comparative disadvantage in efficiently and/or effectively producing particular market offerings that have value for particular market segments.

Specifically, as shown in Figure 13.1 and further explicated in Figure 13.2, when firms have a comparative advantage in resources, they will occupy marketplace positions of competitive advantage for some market segment(s). Marketplace positions of competitive advantage then result in *superior* financial performance. Similarly, when firms have a comparative disadvantage in resources, they will occupy positions of competitive disadvantage, which will then produce *inferior* financial performance. Therefore, firms compete for comparative advantages in resources that will yield marketplace positions of competitive advantage for some market segment(s) and, thereby, superior financial performance. As Figure 13.1 shows, how well competitive processes work is significantly influenced by five environmental factors: the societal resources on which firms draw, the societal institutions that form the "rules of the game" (North 1990), the actions of competitors, the behaviors of consumers and suppliers, and public policy decisions.

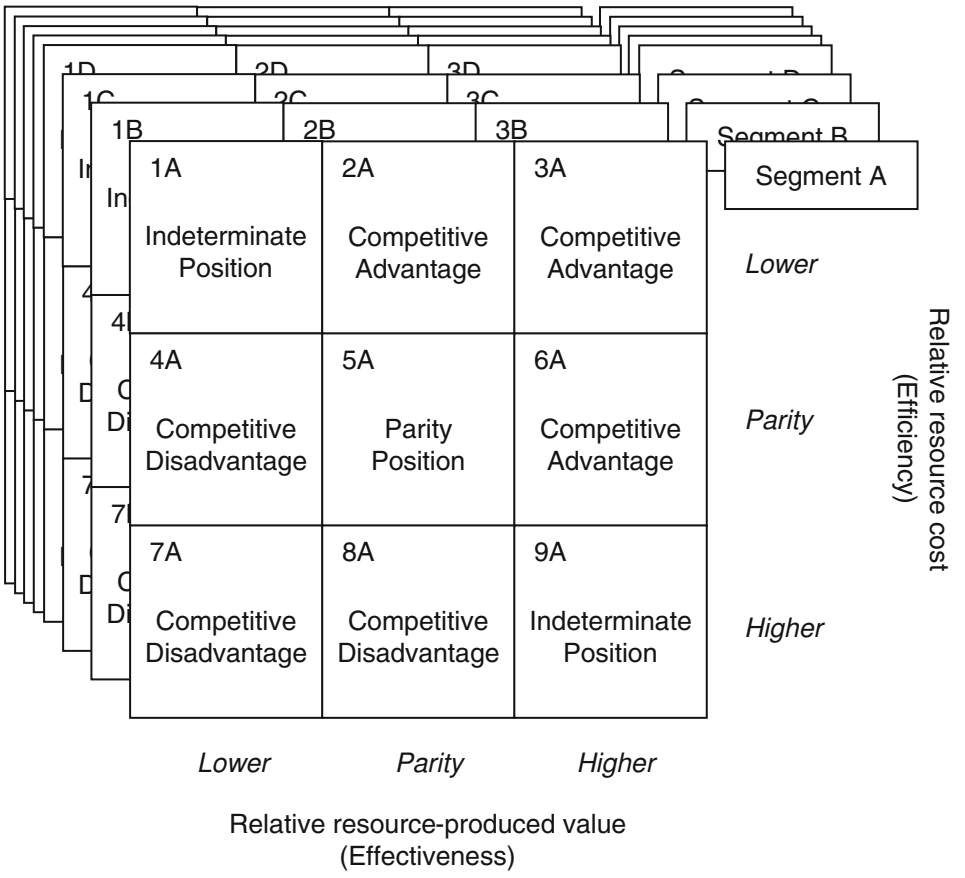
Figure 13.1 **A Schematic of Resource-Advantage Theory**



Read: Competition is the disequilibrating, ongoing process that consists of the constant struggle among firms for a comparative advantage in resources that will yield a marketplace position of competitive advantage and, thereby, superior financial performance. Firms learn through competition as a result of feedback from relative financial performance "signaling" relative market position, which, in turn, signals relative resources.

Source: Adapted from Hunt and Morgan (1997).

Figure 13.2 **Competitive Position Matrix**



Read: The marketplace position of competitive advantage identified as cell 3A in each segment results from the firm, relative to its competitors, having a resource assortment that enables it to produce an offering that (a) is perceived to be of superior value by consumers in that segment and (b) is produced at lower costs than those of rivals.

Source: Adapted from Hunt and Morgan (1997).

Note: Each competitive position matrix constitutes a different market segment (denoted as segment A, segment B, . . .).

Consistent with its Schumpeterian heritage, R-A theory places great emphasis on innovation, both proactive and reactive. The former is innovation by firms that, although motivated by the expectation of superior financial performance, is not prompted by specific competitive pressures—it is genuinely entrepreneurial in the classic sense of *entrepreneur.* In contrast, the latter is innovation that is directly prompted by the learning process of firms' competing for the patronage of market segments. Both proactive and reactive innovation contribute to the dynamism of R-A competition.

Firms (attempt to) learn in many ways—by conducting formal market research, seek-

ing out competitive intelligence, dissecting competitor's products, benchmarking, and test marketing. What R-A theory adds to extant work on learning is how the process of competition itself contributes to organizational learning. As the feedback loops in Figure 13.1 show, firms learn through competition as a result of the feedback from relative financial performance signaling relative market position, which, in turn, signals relative resources. When firms competing for a market segment learn from their inferior financial performance that they occupy positions of competitive disadvantage (cells 4, 7, and 8 in Figure 13.2), they attempt to neutralize and/or leapfrog the advantaged firm(s) by acquisition and/or innovation. That is, they attempt to acquire the same resource as the advantaged firm(s), and/or they attempt to innovate by imitating the resource, finding an equivalent resource, or finding (creating) a superior resource. Here, "superior" implies that the innovating firm's new resource enables it to surpass the previously advantaged competitor in terms of either relative costs (i.e., an *efficiency* advantage), or relative value (i.e., an *effectiveness* advantage), or both.

Firms occupying positions of competitive advantage can continue to do so if (1) they continue to reinvest in the resources that produced the competitive advantage, and (2) rivals' acquisition and innovation efforts fail. Rivals will fail (or take a long time to succeed) when an advantaged firm's resources are either protected by societal institutions such as patents or the advantage-producing resources are causally ambiguous, socially or technologically complex, tacit, or have time compression diseconomies.

Competition, then, is viewed as an evolutionary, disequilibrium-provoking process. It consists of the constant struggle among firms for comparative advantages in resources that will yield marketplace positions of competitive advantage and, thereby, superior financial performance. Once a firm's comparative advantage in resources enables it to achieve superior performance through a position of competitive advantage in some market segment(s), competitors attempt to neutralize and/or leapfrog the advantaged firm through acquisition, imitation, substitution, or major innovation. R-A theory is, therefore, inherently dynamic. Disequilibrium, not equilibrium, is the norm. In the terminology of Hodgson's (1993) taxonomy of evolutionary economic theories, R-A theory is nonconsummatory: it has no end-stage, only a never-ending process of change. The implication is that, though market-based economies are *moving,* they are not moving toward some final state, such as a Pareto-optimal, general equilibrium.

13.2 DEVELOPING THE R-A THEORY RESEARCH PROGRAM

This section discusses key articles, book chapters, and books in the development of the R-A theory research program. Two comments are worth noting concerning the publications. First, we do not discuss the scores of articles, book chapters, and books that use the theory as a theoretical foundation for research, but only those publications that focus explicitly on developing R-A theory. Second, though there are obviously more articles in the marketing literature than in other literatures, we note that R-A theory is genuinely interdisciplinary, for there are numerous articles developing the theory in the economics, management, general business, ethics, and law literatures. Although somewhat

arbitrary, we can divide the history of the research program into an introductory period, corresponding to 1995–96; a development period, which would be 1997–2000; and a research tradition period, which would be 2001 to the present. Our discussion will focus on significant events in each of the three periods.

13.2.1 The Introductory Period: 1995–1996

In the spring of 1994, Robert M. Morgan and I were reviewing some recent developments in the strategic management literature concerning "resource-based" strategy (see references in Table 14.1). In this literature, many writers were suggesting that strategy had been misguided by adopting "industry" as the central focus of strategy development. These new authors were arguing that managers should focus on developing and acquiring rare, valuable, and inimitable resources as a means for achieving "rents," that is, profits in excess of those achieved by a firm under the conditions of perfect competition. The original article that we considered writing was one that developed a new schema for categorizing the various kinds of resources. Indeed, we went so far as to prepare an outline of the structure of the proposed article.

As part of our review, we came across an article by Kathleen Conner (1991). In this article, she argued that any theory of the firm should be able to explain the reasons for the existence of firms and what limits their sizes and scopes. Furthermore, she argued that the resource-based theory of strategy, with its focus on heterogeneous, imperfectly mobile resources, constituted the beginnings of a new theory of the firm. We found her arguments to be persuasive. However, because of our background in marketing, we noticed that the new theory of the firm opened the way for possibly developing a new theory of competition. In particular, we believed, if we joined the resource-based theory of the firm with heterogeneous demand theory and Alderson's (1957, 1965) theory of differential advantage, we might be able to develop a new theory of competition.

After several months of research, we developed a manuscript on the proposed theory and targeted it to the *Journal of Marketing* (*JM*). The original submission had several key characteristics. First, it defined "resource" as those tangible or intangible entities that were available to firms that enabled them to produce, efficiently and/or effectively, market offerings that had value to any market segment. Second, it provided a set of foundational premises for the theory (see Table 13.1). Third, it provided a key diagnostic tool for understanding competitive advantage, which we labeled the "competitive position matrix" (see Figure 13.2). Fourth, it distinguished between two very different kinds of advantages. Specifically, it distinguished clearly the differences between comparative advantages in resources and marketplace positions of competitive advantage. (Often the strategy literature confounds these two types of advantages.) Furthermore, it theorized that it is comparative advantages in resources that lead to marketplace positions of competitive advantage, which, in turn, lead to superior financial performance. Fifth, it used the emerging theory and its focus on heterogeneous, imperfectly mobile resources to explain firm diversity. Sixth, it used the new theory to contribute to explaining the differences in abundance, innovation, and quality that had been observed between market-based and

command economies. Seventh, it explored the issue of whether a market-orientation can be a resource that can lead to sustained, superior, financial performance.

The original submission was reviewed by four scholars. In addition, the editor provided several pages that had a detailed list of suggestions for revising the manuscript. Many of the reviewers' comments were harbingers of three complaints that have been raised by numerous reviews of works developing R-A theory. First, a common criticism of many reviewers is that perfect competition theory is a "straw man," and we should compare R-A theory to a more robust alternative. (We will return to this complaint in the next section.) Second, reviewers often complain that we do not provide a complete literature review of all the works that have been critical of neoclassical economics over the past hundred years. Indeed, no matter how many works we cite, we always seem to leave out some reviewer's favorite critic of neoclassical economics. Partly as a response to this criticism, fully three chapters of Hunt (2000b) are devoted to other works that are either "antecedents to" or have "affinities with" R-A theory. Third, some reviewers are fundamentally hostile to market-based economies and maintain that R-A theory is too sympathetic to economic freedom. We believe that at least some writers who are harshly critical of competition and its role in market-based economies are so because (1) they presume that neoclassical theories of competition do, indeed, accurately describe the process of competition, and (2) they are reacting with hostility toward certain aspects of neoclassical theory (e.g., the self-interest maximization assumption of utility theory). We maintain that it is at least possible that some critics of market-based economies would not be so *critical* if they started from a base that included R-A theory.

After the acceptance of the Hunt and Morgan (1995) article, we knew that, because of the "silo" nature of academic disciplines, if we wanted R-A theory to be considered seriously in the areas of management and economics, it would be necessary to publish the theory in journals in these areas. As to the management area, the journal that we came upon was the *Journal of Management Inquiry,* which was specifically interested in publishing articles that were radically innovative. Believing—and being informed by reviewers—that R-A theory was, indeed, radical, a manuscript was developed that (1) reviewed the original *JM* article, (2) adopted the "R-A theory" label, (3) modified the process of competition to account for feedback effects of organizational learning (see Figure 13.1), and (4) showed how R-A theory explicates the concept of *productivity.* Specifically, the article showed how R-A theory provides a rigorous distinction between *efficiency* and *effectiveness.* Efficiency is when a firm's market offering moves upward in the marketplace position matrix (Figure 13.2). Effectiveness, in contrast, is when a firm's market offering moves horizontally and to the right in the matrix. Increased productivity, therefore, is both (1) *more* efficiently creating value and (2) efficiently creating *more* value.

The final form of the article prepared for the *Journal of Management Inquiry,* that is, Hunt (1995b), had a further, distinguishing characteristic. When we wrote Hunt and Morgan (1995), we were unaware that the standard view of neoclassicists up until the collapse of the Eastern bloc economies was that the equations of neoclassical theory provided no grounds for preferring market-based over command economies. Indeed, the standard view in the "socialist calculation debate" was that the equations of perfect

competition, when combined with general equilibrium theory, implied that planned economies should be at least as productive as market-based ones, if not more so (see, for example, Lavoie 1985). Hunt (1995b) was the first article in management or marketing to discuss the socialist calculation debate, and it argued, as did Hunt (2000b, pp. 157–75), that R-A theory can contribute to explaining and, therefore, understanding the factors that depressed the productivity of the Eastern command bloc economies when compared with their Western market-based counterparts. On this issue, neoclassicists had consistently maintained, perfect competition theory and the equations of general equilibrium theory had "proved that a Central Planning Board could impose rules upon socialist managers which allocated resources and set prices as efficiently as a capitalist society of the purest stripe and more efficiently than the capitalist communities of experience" (Lekachman 1959, pp. 396–97).

The final article in the introductory period developing the theory was Hunt and Morgan (1996). This paper resulted from a critique of R-A theory by Dickson (1996), which argued that R-A theory was not sufficiently dynamic and did not give sufficient attention to organizational learning and the phenomenon of path dependencies. Hunt and Morgan (1996) responded by showing that R-A theory is, indeed, a dynamic theory of competition. Specifically, the premise that firms are motivated by the pursuit of *superior* financial performance implies that competition in a market-based economy *must* be dynamic. Because firms always want, for example, more profits than last year, a higher return on investment than that of competitors, or better profits than some reference point, they will be motivated to develop the proactive and reactive innovations that will make competition dynamic. Furthermore, R-A theory contributes to understanding organizational learning because it shows how the feedback from financial performance causes a firm to learn crucial facts about its marketplace position and resources. Finally, we argued that, because R-A theory is an evolutionary, nonconsummatory theory of competition, it contributes to our understanding of how path dependence effects *can* occur, when such consequences of competition *do,* indeed, occur.

The three articles published in 1995 and 1996 on R-A theory provided a firm foundation for further developing and explicating the theory. We turn now to the period of development, 1997–2000.

13.2.2 The Period of Development: 1997–2000

The years 1997–2000 saw a rapid growth in the number of publications in the marketing, management/general business, and economics literatures that developed the structure and implications of R-A theory. In marketing, Hunt and Morgan (1997) addressed the issue of the relationship between perfect competition and R-A theory. We argued that R-A theory is a general theory of competition that incorporates perfect competition theory as a special, limiting case. Therefore, R-A theory preserves the cumulativity of economic science. Hunt (1999), in contrast, was the first publication that addressed the public policy implications of R-A theory. The article argued that strategy that focused on firm factors (resources) is presumptively procompetitive. After developing three tests for R-A competitiveness,

the article argues that R-A competition is prosocial because it fosters productivity and economic growth. In the management/general business area, Hunt (1998) pointed out that neoclassical theory has customarily presumed that it is the efficient allocation of scarce resources that drives productivity and economic growth. In contrast, R-A theory argues that it is resource *creation,* not allocation, that drives productivity and economic growth. Morgan and Hunt (1999) examined the role of relationship marketing in strategy and identified the kinds of resources that might be gained through relationships. Hunt and Lambe (2000) examined marketing's contribution to business strategy and argued that R-A theory integrates concepts from both marketing and nonmarketing theories of business strategy. Hunt (2000a) and Hunt (2000d) showed how R-A theory can synthesize the competence-based, evolutionary, and neoclassical theories of competition.

As documented by Nelson and Winter (1982), there is an orthodoxy in neoclassical economics that makes it very difficult for heterodox economists to find publication outlets for theories that depart from the position that all economic processes are equilibrating. However, there are some journals that will at least consider publishing articles that advocate dynamic, process-oriented theories, including the *Journal of Economic Issues,* the *Journal of Socio-Economics,* and the *Eastern Economic Journal.* In the first journal, Hunt (1997c) argued that R-A theory is an evolutionary theory of competition. Specifically, both firms and resources are argued to be the heritable, durable units of selection, and competition among firms for comparative advantages in resources is argued to be the selection process that results in the survival of the "locally fitter," not the "universally fittest." In the second journal, Hunt (1997d) explored the nature of sociopolitical institutions that influence favorably the process of R-A competition. The article argues that institutions that promote social trust promote productivity by reducing the transaction and transformational costs in R-A competition. In the third journal, Hunt (1997b) showed how R-A theory can contribute to the area of endogenous growth models in neoclassical economics. Specifically, the article argues that R-A theory, alone among theories of competition, provides a theoretical foundation for endogenous growth models.

By the close of the twentieth century, R-A theory was sufficiently developed in the various journals and academic disciplines to warrant an attempt to pull together the several strands of thought into a research monograph. Hunt (2000b) provided the vehicle for integrating the various articles. Specifically, the monograph argues that R-A theory and its foundations represent the general case of competition, and perfect competition and its foundations are a special case. Therefore, R-A theory incorporates perfect competition, explains the explanatory and predictive successes of perfect competition, and preserves the cumulativity of economic science.

A special symposium was then conducted on R-A theory, as the theory was detailed in the monograph, with commentaries provided by two marketing academics (Falkenburg 2000; Savitt 2000), an industrial-organization economist (Foss 2000), and an institutional economist (Hodgson 2000). The commentators found R-A theory to be highly provocative. Some found the theory to be "too eclectic," while others found it "not eclectic enough." Some found the theory "too incremental," while others found it "not incremental enough." Some found it to be

"too neoclassical," while others found it "not neoclassical enough." Hunt (2000c) responded to the commentators and pointed out that it is "heartening to note that none of the three commentators provides convincing argument or evidence that any claim made in the monograph is unwarranted" (p. 80). Specifically, Hunt (2000c) points out that none of the three commentators challenges a single one of the foundational premises of R-A theory.

13.2.3 The Research Tradition Period: 2001–Present

Since 2000, works continue to appear that develop the structure and implications of R-A theory. The issues addressed include antitrust policy (Hunt and Arnett 2001); business alliance success (Hunt, Lambe, and Wittman 2002); efficiency competition versus effectiveness competition (Hunt and Duhan 2002); R-A theory's philosophical foundations (Hunt 2002b); the relationships among R-A theory, cybernetic systems, and scenario planning (Morgan and Hunt 2002); "Austrian" economics (Hunt 2002d); whether R-A theory is a general theory of marketing (Hunt 2001a, 2002b; Schlegelmilch 2002; Wensley 2002); the "embeddedness" of R-A theory (Hunt and Arnett 2003); and the relationships between R-A theory and marketing strategy (Hunt 2002a; Hunt and Derozier 2004).

Also developing the structure and implications are articles that focus on: how R-A theory relates to the "service-dominant logic" (Hunt 2004); how R-A provides a foundation for market segmentation (Hunt and Arnett 2004a, 2004b); providing a review of R-A theory (Hunt and Morgan 2005); examining whether marketing success contributes to market success (Hunt and Arnett 2006a); how the view of competition adopted by the Supreme Court of the United States is consistent with R-A theory (Grengs 2006); providing an undergirding for the teaching of the service-dominant logic (Hunt and Madhavaram 2006a); how it relates to Alderson's theory (Hunt and Arnett 2006b); reforming marketing (Hunt 2006); teaching marketing strategy (Hunt and Madhavaram 2006b); whether public policy should focus on investment or dynamic competition (Hunt 2007a); developing masterful operant resources (Madhavaram and Hunt 2008); explaining alliance success (Wittmann, Hunt, and Arnett 2009); and providing a theoretical foundation for supply chain management (Hunt and Davis 2008).

Even though R-A theory is, as of this writing, less than two decades old, we feel justified in characterizing the period starting in 2001 as a *research tradition* phase. Several factors prompt us to do so. First, works developing the theory have appeared in a wide range of journals across several different academic disciplines. Thus, R-A theory appears to have "something to say" to scholars who have very different orientations. Second, the explanatory power and predictive power of the theory are well established. Indeed, the theory has increased our understanding of both micro- and macro-phenomena in marketing, management, ethics, law, and economics. Third, works using R-A theory no longer have to "start from scratch" to explain the characteristic of the theory. Scholars now simply cite the theory and move on to their own contributions. Fourth, there have been no commentaries that have pointed out fundamental flaws in the theory's structure or foundational premises. Fifth, and finally, a host of authors are using the theory as a foundation for further works of both a theoretical and empirical nature in marketing,

management, and economics. These, we argue, are characteristics of a theory's becoming the foundation for a research tradition.

We now focus on the foundations of R-A theory. These foundations have been viewed by many as extraordinarily provocative. As yet, however, there have been no successful challenges to the validity of any of the premises.

13.3 THE FOUNDATIONS OF R-A THEORY

All theories are derived from their foundational postulates, and Table 13.1 displays the core premises underlying R-A theory. Foundational, as used here, does not imply that the premises are the minimum set of axioms required for deriving theorems, but that these premises are centrally important for understanding the theory. Epistemologically, because R-A adopts scientific realism (Hunt 2002a), each premise in R-A theory—contrasted with perfect competition—is considered a candidate for empirical testing. Those found false should be replaced with ones more descriptively accurate.

As previously mentioned, a common criticism of R-A theory is that (1) perfect competition theory, its compared alternative, is a "straw man," and (2) we should compare R-A theory with a more robust alternative. However, this section will continue the tradition of contrasting R-A theory with perfect competition for four reasons. *First,* the foundational premises of perfect competition are well developed and well known. Therefore, contrasting R-A theory with perfect competition communicates efficiently and with great precision the foundations and nature of R-A theory. *Second,* because neoclassical theory argues that perfect competition is *perfect,* it continues to serve as the ideal form of competition against which all others are compared. Even many of those who have come to question perfect competition's descriptive accuracy still hold it out as an ideal form of competition. Indeed, because perfect competition underlies much public policy, especially antitrust law, perfect competition—it is typically argued—should serve as the comparison standard (see Hunt and Arnett [2001] and Grengs [2006] for more on R-A theory and antitrust).

Third, even though many scholars question perfect competition theory on numerous grounds, it dominates economics, management, and marketing textbooks. Therefore, it is the only theory of competition that most students ever see that is alleged to be socially beneficial. Discussions of such neoclassical theories as oligopolistic and monopolistic competition in almost all texts are presented (and made meaningful) as departures from the ideal of perfection. Therefore, because R-A theory argues that perfect competition is not perfect, perfect competition theory should serve as a comparison standard. *Fourth,* R-A theory is a general theory of competition. Other than neoclassical perfect competition theory, there appears to be no other rival *general* theory to use for comparison purposes (because other theories are too context specific).

Finally, R-A theory is a work in progress. Contrasting R-A theory with perfect competition constitutes an invitation to other scholars to develop rivals to R-A theory. Specifically, we have always invited scholars (in economics, management, and marketing) to identify the foundational premises of rival theories of competition and explicitly contrast them

with those of R-A theory. By doing so, we can then evaluate how and why the theories are consistent or inconsistent, saying different things or saying the same things differently, genuinely rival, or actually complementary. We again solicit rivals, but we note that, despite numerous past invitations, no rival has been offered. Indeed, no critic has ever shown deficiencies or offered revisions for any of R-A theory's premises.

This section examines the premises in Table 13.1 and follows the discussions in Hunt (2000b) and Hunt and Morgan (1995). We begin with demand.

13.3.1 Demand

For perfect competition theory, demand is (a) heterogeneous across industries, (b) homogeneous within industries, and (c) static. That is, at different configurations of price across generic product categories—for example, footwear, televisions, and automobiles—perfect competition theory allows consumers to prefer different quantities of each generic product. Within each generic product category or "industry," however, consumers' tastes and preferences are assumed to be identical and unchanging through time with respect to desired product features and characteristics. Thus, neoclassical works speak of the "demand for footwear" and the group of firms constituting the footwear "industry" are presumed to face, collectively, a downward-sloping demand curve. Each individual firm in the footwear industry, however, faces a horizontal demand curve because of the homogeneous, intra-industry demand assumption. For perfect competition, the assumptions of homogeneity of demand and supply are necessary for drawing the industry demand and supply curves required for determining the market-clearing, equilibrium price. Absent homogeneous demand, the concept of an industry-demand curve and the market-clearing price make no sense.

Demand and R-A Theory

Consistent with neoclassical theory, R-A theory accepts the premise of heterogeneous inter-industry demand. However, drawing on market segmentation theory, intra-industry demand is posited to be both substantially heterogeneous and dynamic: consumers' tastes and preferences differ greatly within a generic product category and are always changing. Heterogeneous intra-industry demand is argued to be the descriptively realistic general case of demand. That is, R-A theory posits that there are far more industries that are radically or significantly heterogeneous—for example, automobile manufacturing (NAICS #336111), women's footwear (#316213), and book publishing (#511130)—than there are relatively homogeneous, commodity-type industries—for example, corn (NAICS #111150), gold ores (#212221), and industrial sand (#212322).

The implication of heterogeneous, intra-industry demand is that few industry markets exist. As an example, consider footwear (NAICS #31612). R-A theory views consumers' tastes and preferences for footwear to be substantially heterogeneous and constantly changing. Furthermore, not only do consumers have imperfect information concerning footwear products that might match their tastes and preferences, but obtaining such infor-

mation is often costly in terms of both time and money. There is no "market for footwear" (NAICS #31612) or even separate, six-digit markets for women's footwear (#3161213) and men's footwear (#3161213). Even though all consumers require footwear and one can readily identify a group of firms that manufacture shoes, there is no shoe-industry market. That is, the group of firms that constitute the footwear industry do not collectively face a single, downward-sloping demand curve—for the existence of such an industry demand curve would imply homogeneous tastes and preferences.

R-A theory maintains that, to the extent that demand curves exist at all, they exist at a level of (dis)aggregation that is too fine to be an "industry." For example, even if (for purposes of argument) one considers there to be a homogeneous, men's-walking-shoe market, one certainly would not speak of the men's-walking-shoe industry. Nor would one speak of the nineteen-inch-color-television or minivan *industries.* Yet, R-A theory maintains that such market segments as these—and those smaller yet—are central for understanding competition.

The heterogeneous, intra-industry demand premise contributes to R-A theory's explanatory and predictive power. First, it implies that identifying those segments most suitable for developing market offerings should be viewed—consistent with Austrian economics—as an entrepreneurial capability that affects firm performance. Second, that intra-industry demand is substantially heterogeneous in most industries contributes to R-A theory's ability (and neoclassical theory's inability) to make the correct prediction as to the diversity in business-unit financial performance, as discussed in section 14.1.1.

13.3.2 Consumer Information

Perfect competition theory assumes that consumers have perfect and costless information about the availability, characteristics, benefits, and prices of all products in the marketplace. In contrast, drawing on Austrian economics and the works of Stigler (1961) and Nelson (1970), R-A theory posits that consumers within market segments have imperfect information about goods and services that might match their tastes and preferences. Furthermore, the costs to consumers in terms of effort, time, and money of identifying satisfactory goods and services, that is, search costs, are often considerable. Consequently, one purpose served by the legal protection of trademarks, patents, and licenses is the reduction of consumer search costs. Specifically, trademarks, licenses, and patents are societal institutions that reduce search costs by signaling the attributes of market offerings.

Consider, for example, the issue of trademarks and their relationship to competition. Specifically, are trademarks pro- or anticompetitive? Chamberlin (1933/1962) derives the implications of perfect competition theory for trademarks. He points out that the legal protection of trademarks fosters product differentiation and, therefore, a situation in which prices are higher, quantities produced are lower, excess capacity is permanent, products produced are inferior, and all factors of production are exploited (see Hunt 2000b, section 3.1.1). Therefore, for him (1933/1962, p. 270), "the protection of trademarks from infringement . . . is the protection of monopoly," and he maintains that there are no grounds by which "monopolies protected by the law of unfair competition and

of trademarks may be justified" (p. 271). Thus, the standard view in neoclassical theory became that trademarks are anticompetitive.

Consumer Information and R-A Theory

In contrast, the fact that consumers have imperfect information and often use trademarks as heuristics of quality is not a problem for R-A theory. First, because heterogeneous intra-industry demand and supply is viewed as natural by R-A theory, it is only natural that, facing imperfect information, consumers will often use trademarks as indicators of quality. Second, because a trademark is viewed as intellectual property and fully worthy of legal protection, R-A theory views firms' protecting the equity—see Aaker (1991) and Keller (1993, 1998)—in their trademarks as providing not only (1) a valuable source of information to consumers, but also (2) a powerful incentive for producers to maintain quality market offerings, and (3) a means by which manufacturers of shoddy or even defective and dangerous products can be held accountable. Third, because R-A theory rejects static-equilibrium efficiency as the appropriate welfare ideal, the heterogeneity of demand and supply does not pose a problem to be solved, but a state of nature—and a desirable one at that. Indeed, R-A theory proposes that the best way to view the role of trademarks in market-based economies is that they are quality-control and quality-enhancing institutions. (We return to the issue of trademarks in section 15.4.)

13.3.3 Human Motivation

For neoclassical theory, all human behavior is motivated by self-interest maximization. Thus, in their roles as consumers of products and owners or managers of firms, people maximize their *utility.* Etzioni (1988) shows that neoclassical theory conceptualizes utility and *utility maximization* as being (a) a pleasure utility (ethical egoism in moral philosophy terms), or (b) a tautology, or (c) a mathematical abstraction. He notes that only pleasure utility, or "P-utility," maximization is a substantive thesis that could potentially be empirically tested. Furthermore, in empirical works and public policy recommendations, P-utility is generally assumed.

Human Motivation and R-A Theory

R-A theory posits that human motivation is best viewed as constrained self-interest seeking. That is, the self-interest seeking of individuals is constrained or restrained by personal moral codes, which are, in turn, shaped or influenced by, for example, societal, professional, industry, and organizational moral codes. The concept of personal moral codes in R-A theory draws on the normative theories of ethics in moral philosophy: deontology and teleology (Beauchamp and Bowie 1988). Because deontological codes focus on specific actions or behaviors and teleological codes focus on consequences, the former stress the inherent rightness-wrongness of a behavior, and the latter emphasize the amount of good or bad embodied in a behavior's consequences.

Deontologists believe that "certain features of the act itself other than the value it brings into existence" make an action or rule right or wrong (Frankena 1963, p. 14). Moral codes based on deontology will emphasize the extent to which a behavior is consistent or inconsistent with such deontological norms as those proscribing lying, cheating, deceiving, or stealing and those prescribing honesty, fairness, justice, or fidelity. Accordingly, deontology emphasizes duties, obligations, and responsibilities to others. Teleologists, on the other hand, "believe that there is one and only one basic or ultimate right-making characteristic, namely, the comparative value (nonmoral) of what is, probably will be, or is intended to be brought into being" (Frankena 1963, p. 14).

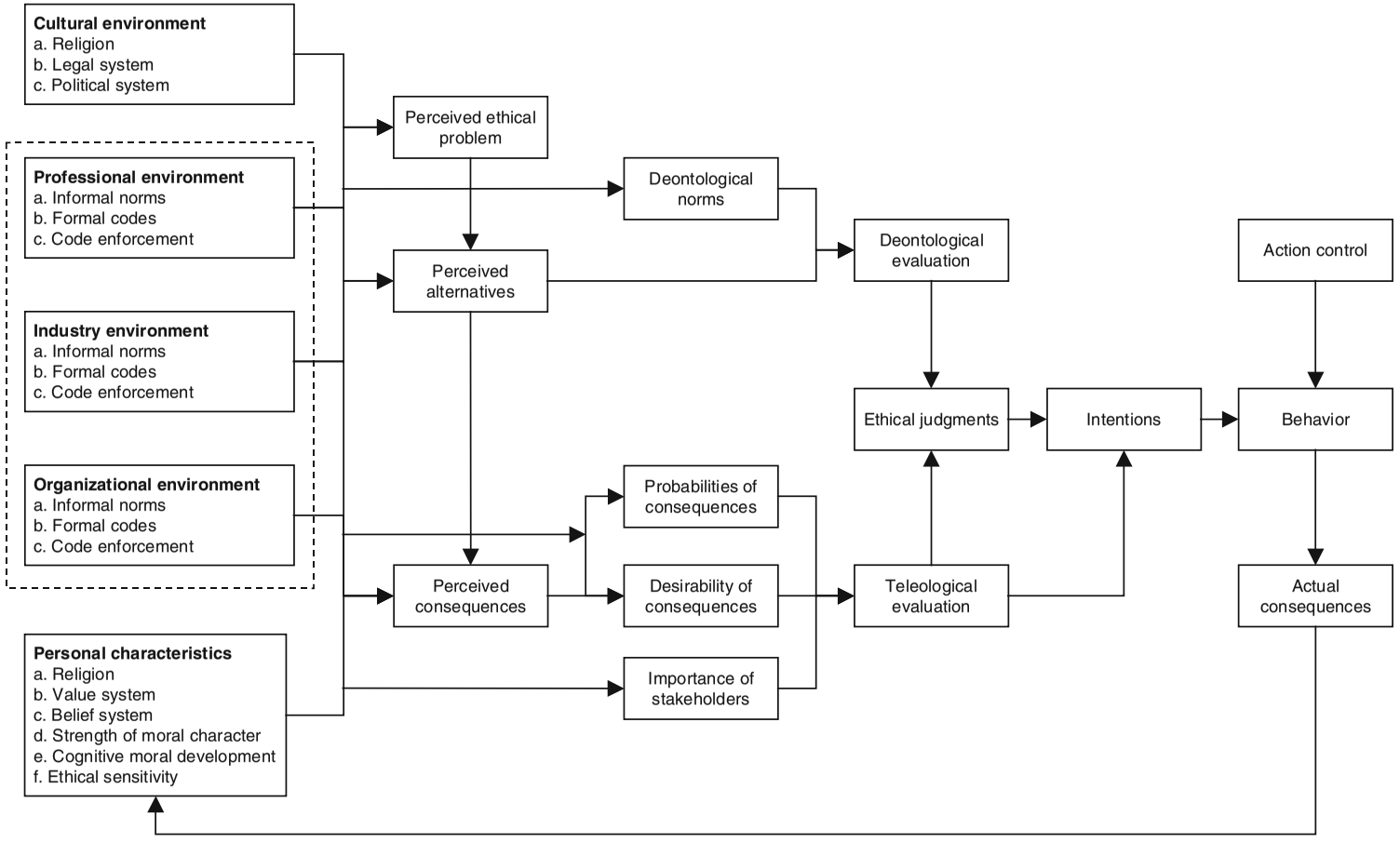
Whereas deontological moral codes must address the difficult issue of conflicting norms, those emphasizing teleological factors must grapple with which stakeholders are to be valued. Those moral codes adopting utilitarianism hold that an act is right only if it produces for all people a greater balance of good over bad consequences than other alternatives (i.e., "the greatest good for the greatest number"). Even though it focuses on consequences, because utilitarianism demands that decision makers consider an act's consequences on all stakeholders, it shares at least some common ground with deontology's emphasis on duties and responsibilities to others. In stark contrast, codes adopting ethical egoism—that is, those who adopt the substantive interpretation of utility maximizing—hold that an act is right only if the consequences of the act are most favorable for the individual decision maker. The self-interest, utility-maximizing view of ethical egoism is directly opposed by deontological ethics.

Figure 13.3 shows a model developed by Hunt and Vitell (1986, 1993) that explicates the nature of personal moral codes and shows how such codes are influenced by deontological, teleological, and environmental factors.[1] The Hunt-Vitell (HV) theory of ethics draws on deontological and teleological moral philosophy to explain (1) why people have such radically different views on the ethicality of alternative actions, and (2) why people engage in ethical/unethical behaviors. Briefly, the "triggering mechanism" of the model is the individual's perception that an activity or situation involves an ethical issue, which is followed by the perception of various alternatives or actions one might take to resolve the ethical problem. These alternatives are then evaluated both deontologically and teleologically in the "core" of the model.

For each alternative, the core of the HV model assumes that the decision maker has access to a set of deontological norms that can be applied. The deontological evaluation process, therefore, consists of applying the norms to each alternative, checking for consistency (inconsistency), and resolving the conflicts that result when not all deontological norms can be satisfied simultaneously. Each alternative is also evaluated in the core by a teleological process that combines (1) the forecasting of each behavior's consequences for various stakeholder groups, with (2) estimating the probabilities of the consequences, (3) evaluating the consequences' desirability or undesirability, and (4) assessing the importance of each stakeholder group.

For the HV model, the ethicality of an alternative, that is, *ethical judgments,* results from combining the deontological and teleological evaluations. For example, a strict deontologist would ignore totally the results of the teleological evaluation. In contrast, a

Figure 13.3 **The Hunt-Vitell Theory of Ethics**



Source: Hunt and Vitell (1986, 1993). Copyright © 1991 by Shelby D. Hunt and Scott J. Vitell.

Note: The portion of the model outside the dashed lines constitutes the general theory. The portion inside the dashed lines individuates the general model for professional and managerial contexts.

strict utilitarian would (1) ignore the deontological evaluation, (2) assign equal weights to all individual stakeholders, and (3) maximize the ratio of good consequences over the bad. Like a strict utilitarian, a strict ethical egoist would ignore the deontological evaluation. However, a strict ethical egoist would also assign zero weights to all stakeholders other than the self and maximize the ratio of good consequences over the bad for oneself. Hunt and Vitell do not theorize that individuals are (or ought to be) utilitarians, ethical egoists, or deontologists. Rather, they posit that most people in most situations evaluate the ethicality of an act on the basis of a combination of deontological and teleological considerations.

As to the nature of the personal moral codes that R-A theory posits as constraining or restraining self-interest seeking, the HV model of ethics suggests that the codes consist of (1) the deontological norms an individual applies to decision situations, (2) the rules for resolving conflicts among norms, (3) the importance weights assigned to different stakeholders, and (4) the combinatory rules for merging the deontological and teleological evaluation processes. R-A theory draws on the HV model and maintains that individuals differ greatly in their personal moral codes. Furthermore, the variance in moral codes is not a "black box" for the purpose of theory development and empirical research. Indeed, personal moral codes are shaped, but not determined, by experience and environment.

As to experience, note that ethical judgments in the HV model drives intentions and behavior. That is, in most situations, ethical judgments, intentions, and behavior are congruent. (*Guilt* occurs when teleological evaluations drive intentions and behavior in a manner inconsistent with ethical evaluation.) The HV model shows a feedback loop from behavior through actual consequences to personal characteristics. Thus, individuals learn the appropriateness of the moral codes they apply through experiencing positive and negative consequences. This learning-by-experience shapes personal moral codes.

Personal moral codes are also shaped by lifelong, vicarious learning in different environments. First, different societies have different cultures that communicate and "pass on" different moral codes. Second, within societies, different groups—for example, professional associations, industries, and organizations—communicate different moral codes to their members. Third, different families have different moral codes. The HV model views all these environmental factors as shaping—but not determining, for choices are still made as to which code to adopt—an individual's personal moral code.

Returning to the posit that human motivation is best described as self-interest seeking constrained by a personal moral code, R-A theory can account for the economic value to firms and societies of having individuals who are motivated by moral codes that emphasize deontological ethics, rather than ethical egoism.[2] In particular, when people share a moral code based primarily on deontological ethics, trust can exist, and therefore, the costs that firms and societies have that are associated with shirking, cheating, stealing, monitoring, free-riding, "hostage-taking," and opportunism in general are avoided (see Hunt 2000b, section 9.3.3). Thus, R-A theory can provide the kinds of "deeper insights" asked for by Williamson (1994, p. 85) because, not being bound to the neoclassical tradition, it can abandon the assumption of universal opportunism.

13.3.4 Firm's Objective and Information

Consistent with its assumption that humans are self-interest maximizers, perfect competition theory assumes that owner-managed firms profit maximize. (Profits are the self-interest of owners.) Furthermore, maximizing occurs under conditions of perfect and costless information about product markets, production techniques, and resource markets. In order to incorporate time and risk, the neoclassical tradition posits wealth maximization as the firm's long-term objective. That is, owner-managed firms maximize the net present value of future profits using a discount rate that accounts for the time value of money and the risk associated with an expected stream of profits.

Of course, many modern corporations, including most large firms, are not owner managed. The separation of ownership from control and management (first pointed out in Berle and Means 1932) results in situations where the self-interests of owners (i.e., the shareholders) in maximizing their wealth may conflict with managers' own personal interests. This "principal-agent" problem is addressed in the neoclassical tradition by agency theory and its "nexus of contracts" view of the firm (Fama 1980; Fama and Jensen 1983; Jensen and Meckling 1976).[3] As with transaction cost economics, agency theory assumes universal opportunism by managers. Thus, measures must be taken to prevent managers from pursuing their self-interests at the expense of shareholders. Measures commonly recommended include developing financial incentives to align managers' interests with shareholder wealth maximization; instituting tight monitoring and control systems; maintaining a high proportion of independent, outside directors on boards of directors; and avoiding "CEO duality," that is, avoiding having the same person as both chief executive officer and chairperson of the board of directors.

Firm's Objective, Information, and R-A Theory

For R-A theory, the firm's primary objective is superior financial performance, which it pursues under conditions of imperfect and often costly-to-obtain information about extant and potential market segments, competitors, suppliers, shareholders, and production technologies. Consistent with the self-interest seeking aspect of human behavior, superior financial performance is argued to be the firm's primary objective because superior rewards flow to the owners, managers, and employees of firms that produce superior financial results. These rewards include not only such financial rewards as stock dividends, capital appreciation, salaries, wages, and bonuses, but also such nonfinancial rewards as prestige and feelings of accomplishment. Because it enables firms to pursue other objectives, such as contributing to social causes or being a good citizen in the communities in which it operates, financial performance is viewed as *primary.* For-profit organizations differ from their not-for-profit cousins in that the former, but not the latter, are *for profit.* Indeed, prolonged inferior performance threatens the firm's survival and prevents the accomplishment of secondary objectives.

The "superior" in superior financial performance equates with both *more than* and *better than.* It implies that firms seek a level of financial performance exceeding that of some

referent. For example, the indicators of financial performance can be such measures as accounting profits, earnings per share, return on assets, and return on equity. The referent against which the firm's performance is compared can be the firm's own performance in a previous time period, the performance of rival firms, an industry average, or a stock-market average, among others. Both the specific measures of financial performance and the specific referents used for comparison purposes will vary somewhat from time to time, firm to firm, industry to industry, and culture to culture. That is, for R-A theory, both measures and referents are independent variables. Therefore, the theory provides a framework for investigating the role of different understandings of financial performance for managers, firms, industries, productivity, economic growth, and social welfare (e.g., Arnett and Hunt 2002).

Superior financial performance does not equate with the neoclassical concepts of "abnormal profits" or "rents" (i.e., profits differing from the average firm in a purely competitive industry in long-run equilibrium) because R-A theory views industry long-run equilibrium as a theoretical abstraction and such a rare phenomenon that the concept of "normal" profits in the neoclassical tradition cannot be an empirical referent for comparison purposes. Furthermore, the actions of firms that collectively constitute competition do not force groups of rivals to "tend toward" equilibrium. Instead, the pursuit of *superior* performance implies that the actions of competing firms are disequilibrating, not equilibrating. Indeed, consistent with Austrian economics, markets seldom if ever are in long-run equilibrium, and activities that produce turmoil in markets are societally beneficial because they are the engines of economic growth.

Positing that the firm's goal is superior financial performance ensures that R-A theory is dynamic, which accords well with the extant dynamism of competition in market-based economies. It is no accident that theories that are static equilibrium in nature assume profit or wealth maximization. But "saving the equations" through profit maximization has a price. If a firm is already making the maximum profit, why should it—absent environmental shocks—ever change its actions? For example, if a firm is maximizing profits producing a product at a certain quality level, why should it ever attempt to improve quality? If, however, firms are posited to (1) always seek *more* profits, *higher* earnings per share, and *greater* return on investment, and (2) they believe that there are always actions that can be taken to accomplish these goals, then (3) competition will be dynamic.

Nelson and Winter (1982, p. 4) maintain that "firms in our evolutionary theory . . . [are] motivated by profit and . . . search for ways to improve profits," which differs from "profit maximizing over well defined and exogenously given choice sets." Likewise, Langlois (1986, p. 252) points out that, though economic "agent[s] prefer more to less all things considered," this "differs from maximizing in any strong sense." Similarly, though R-A theory posits that firms seek superior financial performance, the general case of competition is that they do not "strong sense" maximize because managers lack the capability and information to maximize (Simon 1979).[4] That is, though firms prefer more profits to less profits, a higher return on investment to a lower return, a higher stock price to a lower stock price, more shareholder wealth to less wealth, imperfect information implies that none of these financial indicators equates with profit or wealth *maximization.*

Real firms in real economies are not presented a menu of well-defined sets of alternatives for which the problem is to choose the profit or wealth-maximizing option. Firms do indeed take actions, they do indeed take note of financial indicators, and they do indeed make causal attributions between actions and indicators. But even if—and this is a big if—managers have good reasons to claim to know that actions previously taken have led (or will lead) to increases in financial performance, they cannot know (or warrantedly claim to know) that some alternative action or set of actions (identified or not identified) would not have produced (or will not produce) even higher returns. Therefore, *superior* financial performance, not maximum performance, better describes the firm's primary objective.

In addition to informational problems, firms do not "strong sense" maximize because of the personal moral codes of owners, managers, and subordinate employees. Recall that agency theory and transaction cost economics assume self-interest maximization and universal opportunism. In terms of ethical theory, all economic agents are ethical egoists: they ignore deontological considerations, assign zero weights—see Figure 13.3—to all stakeholders other than self, and maximize the ratio of good consequences over bad.

In contrast, R-A theory posits that personal moral codes are independent variables that vary across people (and peoples). As previously noted, these moral codes entail (1) the deontological norms an individual applies to decision situations, (2) the rules for resolving conflicts among norms, (3) the importance weights assigned to different stakeholders, and (4) the combinatory rules for merging the deontological and teleological evaluation processes. Thus, R-A theory acknowledges that nonowner managers guided by ethical egoism might not profit maximize when it conflicts with their self-interests. However, by treating personal codes as independent variables, R-A theory expands the kinds of situations beyond those that can be addressed by agency theory.

Consider, for example, the case of distributors of bottled water who could easily charge double the customary price when a natural disaster shuts down a community's water supply. Some firms, guided by ethical egoism, that is, self-interest maximization, might choose to double the price. Other firms, guided by "enlightened" self-interest seeking, might choose not to double the price because they believe the long-term, net present value of doubling is less than the "goodwill value" of nondoubling. However, the personal codes of the managers of still other firms might result in their resisting the doubling of prices even though they believe the long-term, net present value of doubling is greater than the goodwill value of nondoubling. In particular, firms guided by deontological ethics might resist doubling because they believe it would constitute exploiting their customers and, hence, would be deontologically wrong. In general (and *inconsistent* with agency theory and transaction cost economics), some firms do not profit or wealth maximize in particular decision situations because such maximizing behaviors would violate (either owner or nonowner) managers' sense of rightness and wrongness. This sense of rightness and wrongness results from managers' beliefs concerning their duties and responsibilities to nonowner stakeholders; that is, it stems from their personal moral codes based on deontological ethics.

Finally, efforts to profit maximize may also be thwarted by ethical code mismatches

between managers and their subordinate employees. Suppose most of a firm's employees have moral codes stressing deontological ethics and, thus, they avoid shirking, cheating, stealing, and other opportunistic behaviors. In such a firm, the costs associated with monitoring and strong controls would be pure economic waste. If, however, the owner-manager is an ethical egoist and assumes that the employees are also ethical egoists (doesn't everyone utility maximize?), then expensive and unnecessary controls will be instituted.[5] Ironically, then, the assumption of utility maximization by managers can thwart efforts at profit maximization. Etzioni (1988, p. 257) puts it this way: "The more people accept the [P-utility maximization part of the] neoclassical paradigm as a guide for their behavior, the more their ability to sustain a market economy is undermined."

In summary, superior financial performance is argued to be the best descriptor of the firm's primary objective because (1) superior rewards flow to owners, managers, and employees of firms that produce superior rewards, and (2) the pursuit of superior financial performance ensures that R-A theory is dynamic, which makes it consistent with the observed dynamism of market-based economies. Although firms do seek superior financial performance, they are argued to *not* maximize profit or wealth because (1) imperfect information makes maximization impossible; (2) sometimes agency problems associated with ethical egoism thwart maximization; (3) sometimes firms guided by deontological ethics may choose not to maximize; and (4) sometimes ethical code mismatches between (and among) owners, managers, and subordinate employees may result in nonmaximizing behaviors.

13.3.5 Resources

For perfect competition theory, firm resources are factors of production. Two aspects of "resources are factors" are noteworthy. First, because neoclassical theory is completely mathematized, no entity can be a factor of production unless it can be represented in an equation that can be differentiated. Therefore, the customary factors are land, labor, and capital. Intangible entities, such as entrepreneurship, as Kirzner (1979, p. 187) points out, have no marginal product and cannot be a factor of production. It makes no sense to talk about the extra units of a commodity that can be produced for each additional unit of entrepreneurship.

Second, all resources are perfectly homogeneous and mobile. That is, each unit of labor and capital is identical with other units, and all units—being for sale in the factor markets—can move without restrictions among firms within and across industries. Again, labor and capital must be homogeneous to ensure that equations will be differentiable.

In addition to resources, all firms have access to a *production function,* that is, a technology that enables them to combine the factors of production to produce a product. Because of the assumption of perfect information, the production function for each firm within an industry is identical—no firm has access to a technology, capability, competence, or organizational form that is superior to those available to other firms. Because all innovation is exogenous, new technologies are given to firms by outside sources, for example, by government.

Resources and R-A Theory

Contrasted with "resources are factors," R-A theory adopts a resource-based view of firm. (See cites in Table 14.1, page 394) Specifically, R-A theory defines resources as the tangible and intangible entities available to the firm that enable it to produce efficiently and/or effectively a market offering that has value for some market segment(s). Resources are categorized as financial (e.g., cash reserves and access to financial markets), physical (e.g., plant, raw materials, and equipment), legal (e.g., trademarks and licenses), human (e.g., the skills and knowledge of individual employees, including, importantly, their entrepreneurial skills), organizational (e.g., controls, routines, cultures, and competences—including, importantly, a competence for entrepreneurship), informational (e.g., knowledge about market segments, competitors, and technology), and relational (e.g., relationships with competitors, suppliers, and customers). Each entity is a resource to the firm if, and only if, it contributes to enabling it to produce efficiently and/or effectively a market offering that has value for some market segment(s).

R-A theory posits that resources are both significantly heterogeneous across firms and imperfectly mobile. Resource heterogeneity implies that each and every firm has an assortment of resources that is at least in some ways unique. Imperfectly mobile implies that firm resources, to varying degrees, are not commonly, easily, or readily bought or sold in the marketplace (the neoclassical factor markets). Because of resource immobility, resource heterogeneity can persist through time despite attempts by firms to acquire the same resources of particularly successful competitors.

Note that resources need not be owned by the firm, but just be available to it. For example, the relationships involved in relational resources are never *owned* by firms, but only available to them for the purpose of producing value for some market segment(s). Indeed, just as there is no neoclassical market—no demand or supply curve—for "reputations," there is no market for relationships with suppliers, customers, employees, and competitors. Nonetheless, relational resources have value.

The relationships that a firm has access to become a part of what R-A theory views as organizational capital, Falkenberg (1996) calls "behavioral assets," and Gummesson (1995) refers to as "structural capital." For example, Gummesson (1995, p. 17) defines structural capital as "those resources built into the organization such as systems, procedures, contracts, and brands which are not dependent on single individuals." As he points out, there is a strong shift toward recognizing that the total value of a firm is primarily determined by what he calls "soft" assets, not inventory and equipment. Thus, the value of many organizations "cannot be correctly assessed from traditional information in the balance sheet and the cost and revenue statements of the annual report" (p. 18). Even though accounting procedures for valuing these soft assets are in their infancy, firms are beginning to recognize "the fact that the customer base and customer relationships are . . . assets, even the most important assets" (p. 18).

The work of Falkenberg (1996) provides data on just how important organizational capital or soft assets are in determining the value of a firm. Falkenberg divides a firm's resources into (1) physical assets, (2) valuable paper (e.g., cash), and (3) "behavioral assets," which

he defines as the "routines and competencies of the people involved . . . which are located not only inside, but outside the firm" (p. 4). As support for his thesis that it is behavioral assets that are the main source of wealth creation, he calculates the market-price-to-book-value ratio for numerous firms in different industries in different years. Because book value reflects only the (depreciated) value of physical assets and valuable paper, the difference is an (albeit crude) estimate of the value of a firm's behavioral assets.

Falkenberg's (1996) study finds substantial across-industry variation. For example, whereas the behavioral assets of Home Depot, Inc., are valued at 6.6 times its book value, Texaco's behavioral assets are only 2.0 times its book value. Furthermore, he finds substantial within-industry variation. For example, not only did his sample of consumer goods companies range from 0.8 (RJR Nabisco) to 15.0 (Coca Cola), but even within the petroleum industry the ratios ranged from 2.0 (Texaco) to 3.2 (Phillips Petroleum). Moreover, even across only two years' time (1993–95), the ratio for individual firms changed dramatically, both up and down. For example, whereas Apple Computer went from 3.1 in 1993 to 2.1 in 1995, IBM went from 1.1 to 2.4 during the same time period.

In short, Falkenberg's (1996) work strongly supports the view that it is organizational capital—including a firm's relational resources—that is viewed by investors as the principal determinant of its wealth-creating capacity. Furthermore, it strongly supports R-A theory's contention that important firm resources are intangible, significantly heterogeneous, and immobile. In contrast, because neoclassical theory customarily admits only capital, labor, and land to qualify as firm resources (where capital is generally construed to be such tangible assets as machinery, inventory, and buildings), such intangibles as relationships are outside the scope of the concept "resources" and are not considered as having value in the production process.

At first glance, one might believe that neoclassical theory could accommodate the concept of organizational capital by the simple expedient of permitting such intangibles as relationships to be resources. But this is problematic in the extreme. The commitment of neoclassical theory to the derivation of demand and supply curves requires that all resources be homogeneous and mobile. That is, it is only by neoclassical theory's viewing each unit of each factor of production as being obtainable in the marketplace (and identical with other units) that it can derive demand and supply curves for each factor. Why, then, could not neoclassical theory simply discard the necessity of having demand and supply curves for each factor of production? Because demand and supply curves are necessary for determining prices in static equilibrium—which is part of the neoclassical research program's "hard core" (Lakatos 1978). That is, the import of discarding the requirement that all factors of production have demand and supply curves would be that neoclassical theory would no longer be *neoclassical.*

13.3.6 Role of Management

For perfect competition theory, the role of management is limited, to say the least. Because firms are price takers and quantity makers, the short-term role of management is

to determine the quantity of the firm's single product to produce and to implement its standardized production function. Because all firms are profit maximizers, all firms in an industry will inexorably produce at an output rate where marginal cost equals marginal revenue (the product's market price). Therefore, because such resources as plant and equipment are relatively fixed in the short run, each firm will incur profits (or losses) depending on whether price exceeds (or is less than) the average total cost of producing the profit-maximizing quantity.

Management and R-A Theory

R-A theory, in contrast, views the role of management in the firm in a business-strategy manner. Specifically, the role of management (both owner and nonowner managers) is to recognize and understand current strategies, create new strategies, select preferred strategies, implement the strategies selected, and modify strategies through time. "Implementation," of course, encompasses the thousands of day-to-day decisions that must be made and activities that must be undertaken to manage a modern firm (of any significant size). "Recognize and understand" acknowledges that firms sometimes (often?) fail to recognize accurately their respective marketplace positions and/or fail to understand the nature of the resources that led to such positions (McGrath, MacMillan, and Venkataramen 1995; Schoemaker and Amit 1994). Indeed, many strategies emerge through time and, thus, may be implicit (Mintzberg 1987). "Create" and "select" emphasize the cognitive and innovative dimensions of firms. Therefore, the strategic choices that managers make influence performance. "Modify" emphasizes that managers learn through the process of competing and can make adjustments or abandon underperforming strategies.

All strategies (at the business-unit level) involve, at the minimum, the identification of (1) market segments, (2) appropriate market offerings, and (3) the resources required to produce the offerings. Strategies that yield positions of competitive advantage and superior financial performance will do so when they rely on those resources in which the firm has a comparative advantage over its rivals. Sustained superior financial performance occurs only when a firm's comparative advantage in resources continues to yield a position of competitive advantage despite the actions of competitors.

13.3.7 Competitive Dynamics

For neoclassical theory, all resources are variable in the long run, and each firm in each industry adjusts its resource mix (e.g., its capital/labor ratio) to minimize its cost of producing the profit-maximizing quantity. These adjustments inexorably lead to a long-run equilibrium position in which each firm produces the quantity for which market price equals long-run marginal cost, which itself equals the minimum long-run average cost. The position of long-run equilibrium is thus a "no profit" situation—firms have neither a pure profit (or "rent") nor a pure loss, only an accounting profit equal to the rate of return obtainable in other perfectly competitive industries.

Each industry stays in equilibrium until something changes in its environment. Thus, all forms of innovation are exogenous factors and represent "shocks" to which each industry responds. Therefore, rather than "strategic choices matter," the firm's environment strictly determines its performance (i.e., its profits). Pure profits or rents occur only temporarily—just long enough for equilibrium to be restored. Through time, the dynamics of market-based economies are represented as "moving" equilibria.

Because both product and factor markets are interdependent, the possibility of a general equilibrium for an entire economy arises. Walras (1874/1954) was the first to identify the system of equations that an economy would have to "solve" for general equilibrium to exist. Conceptualizing a fictitious, all-knowing "auctioneer" who "cries" prices (i.e., "bids" for all products and resources), Walras theorized that an economy characterized by perfect competition "gropes" toward general equilibrium. Schumpeter (1954, p. 242) calls the work of Walras the "Magna Carta of economic theory." Indeed, precisely specifying and successfully analyzing the "Walrasian equations" is considered to be the crowning achievement of twentieth-century economics—as Nobel prizes to Kenneth Arrow in 1972 and Gerard Debreu in 1983 attest.

The welfare economics literature investigates the conditions prevailing at the position of Walrasian general equilibrium. If—and only if—all industries in an economy are perfectly competitive, then at general equilibrium, every firm in every industry has the optimum-size plant and operates it at the point of minimum cost. Furthermore, every resource or "factor" employed is allocated to its most productive use and receives the value of its marginal product. Moreover, the distribution of products produced is Pareto-optimal at general equilibrium because the price of each product (reflecting what consumers are willing to pay for an additional unit) and its marginal cost (the extra resource costs that society must pay for an additional unit) will be exactly equal. Therefore, the adjective "perfect" is taken literally in neoclassical theory: Perfect competition is *perfect,* the ideal form of competition. All other forms of competition are departures from perfection, that is, "imperfect."

Competitive Dynamics and R-A Theory

In contrast, for R-A theory, competition is an evolutionary process in which the actions of firms are disequilibrium provoking. In this process, innovation is endogenous. Instead of the firm's environment, particularly the structure of its industry, strictly determining its conduct (strategy) and its performance (profits), R-A theory maintains that environmental factors only influence conduct and performance—see Figure 13.1. Relative resource heterogeneity and immobility imply that strategic choices must be made, and these choices influence performance. All firms in an industry will not adopt the same strategy—nor should they. Different resource assortments suggest targeting different market segments and/or competing against different competitors.

R-A competition is not an "imperfect" departure from perfect competition. Rather, the process of R-A competition allocates resources efficiently and, because it creates new resources, the process generates increases in productivity and produces

economic growth and wealth. Moreover, rather than R-A competition's being an imperfect departure from perfect competition, perfect competition is a special case of R-A competition.

13.4 CONCLUSION

This chapter has provided a brief overview of R-A theory, discussed how the R-A research program was developed, and, because the foundational premises of R-A theory are central to understanding it, the chapter has reviewed in detail the arguments for each premise. Chapter 14 turns to a discussion of the first two grounds for the claim that R-A theory is toward a general theory of marketing.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. "Although patents are not legal resources, a trademark and a license to produce a product granted by the patent holder are." Evaluate this position.
2. What is the difference between an *efficiency* advantage and an *effectiveness* advantage? *Ceteris paribus,* which kind of advantage is likely to last longer? Upon which kind of advantage has marketing historically focused? Why? Upon which kind of advantage has neoclassical economics focused? Why?
3. What does it mean to claim that innovation is endogenous to resource-advantage competition, but exogenous to perfect competition? Why is it important to have a theory of competition in which innovation is endogenous? Why is it the case that neoclassical economics cannot adopt the simple expedient of allowing perfect competition to have innovation be endogenous?
4. What is the difference between "profit maximization" and "superior financial performance"? Why is this difference important?
5. As Barney and Ouchi (1986, p. 374) point out, when strategy is guided by industrial-organization economics, I-O theory is "turned upside down." That is, what was considered anticompetitive and socially undesirable under neoclassical, industrial-organization economics becomes the basis for normative competitive strategy. Discuss the implications for firms and societies of adopting industry-based strategies.
6. Hunt and Morgan (1995) maintain that "market orientation" is a resource. How does the marketing concept differ (if it differs) from a firm being market oriented? Is the marketing concept a resource? Many textbooks talk about firms having adopted a "production concept" or "sales concept." Do firms actually adopt "production" or "sales" *concepts,* or do firms actually have production or sales *orientations?*
7. Is it important to distinguish between adopting "a" resource-based view of the firm—as does R-A theory—and not "the" resource-based theory of the firm? If so, why? If not, why not?

NOTES

1. The original model is in Hunt and Vitell (1986). The version reproduced here is a revision from Hunt and Vitell (1993, 2006). Discussions and tests of the model may be found in Arnett and Hunt (2002), Hunt (1990a), Hunt and Laverie (2004), Hunt and Hansen (2007), Hunt and Vitell (2005), Hunt and Vasquez-Parraga (1993), Mayo and Marks (1990), Menguc (1997), Singhapakdi and Vitell (1990, 1991), Sparks and Hunt (1998), and Vitell and Hunt (1990).

2. Etzioni (1988) refers to this view as "moderate deontology." Specifically, he argues that, rather than abandon P-utility, socioeconomics should (1) draw on deontological ethics and (2) theorize that moral commitment is a separate source of valuation. Thus, he hypothesizes that behavior is codetermined by P-utility and a moral commitment based on deontological ethics. By moderating the P-utility thesis with deontological ethics, argues Etzioni (1988, pp. 7, 8), socioeconomics can account for trust, which "is pivotal to the economy . . . as, without it, currency will not be used, saving makes no sense, and transaction costs rise precipitously."

3. See Bergen, Dutta, and Walker (1992); Davis, Schoorman, and Donaldson (1997); Eisenhardt (1989); and Perrow (1986) for reviews of agency theory.

4. However, Simon's (1979) "satisficing" differs from R-A theory's concept of superior financial performance. As Dickson (1992, p. 72) notes: "Note that this view [Dickson's view] of relentless cost management cannot be accommodated in the satisficing model by simply assuming that the firm keeps raising its efficiency aspiration levels. A firm is likely to change its aspiration levels, but they are still only minimum performance standards, often linked to management and worker reward systems. Once its aspirations levels (performance standard goals) are met, the firm that prefers more profits over less will not stop seeking ways of reducing costs. Such motivation and behavior are antithetic to satisficing because the reality is that the firm is never satisfied with its current performance."

5. Davis, Schoorman, and Donaldson (1997) discuss the differences between agency theory and stewardship theory prescriptions as to corporate governance. They develop a "prisoner's dilemma" scenario concerning the "principal's choice" of either (1) acting opportunistically or (2) acting as a steward. Similar choices are then developed for managers (see their Figure 4.1, p. 39). Among other things, they discuss the costs of what are here called ethical code mismatches.

14

COMPETITION THEORY, ALDERSON'S MARKET PROCESSES THEORY, AND R-A THEORY

> Entrepreneur: "West Texas would be a good location for growing grapes and making wine." Neoclassical economist: "Nonsense, if it were a good location, there would be wineries here."
>
> —*Anonymous*

This chapter develops arguments that constitute the first and second grounds for maintaining that resource-advantage (R-A) theory provides the foundations for a general theory of marketing.[1] First, because marketing takes place within the context of competition, a general theory of *marketing* should be consistent with the *most* general theory of *competition.* Accordingly, I argue that R-A theory is a general theory of competition, which makes it an appropriate foundation for working toward a general theory of marketing. Second, the closest thing to a general theory of marketing today is Alderson's (1957, 1965) functionalist theory of market behavior (Hunt 1983a). Therefore, I argue that R-A theory is toward a general theory of marketing because it accommodates key concepts and generalizations from Alderson's theory, extends them, and integrates them into the broader, R-A theory of competition.

14.1 R-A THEORY IS A GENERAL THEORY OF COMPETITION

Prior to the advent of resource-advantage theory, neoclassical perfect competition theory was the most general theory of competition. Other theories—for example, monopolistic competition (Chamberlin 1933/1962)—were always discussed as (1) deviations from perfect competition and (2) societally *undesirable* deviations. Accordingly, this section compares R-A theory with perfect competition theory and argues that R-A theory is the more general theory because it (1) explains more phenomena and (2) incorporates perfect competition theory as a special case. But what does "incorporates" mean?

In the philosophy of science, one theory incorporates another when the general theory can satisfactorily explain the more limited theory's explanatory and predictive successes

(Sellars 1963; Levey 1996). The classic example of incorporation, of course, is that Newtonian theory (which maintains that the acceleration of two masses increases as they approach each other) incorporates Galileo's law of descent (which assumes that acceleration is constant between two bodies). Newtonian theory, therefore, explains all the predictive successes of Galileo's law. Simply put, if d is the distance of a body from the surface of the earth and D is the radius of the earth, Galileo's law predicts well for most falling objects because the ratio d/D is—as argued in economics by Friedman (1953)—"close enough" to zero that assuming g to be constant in $S = 1/2\ gt^2$ is nonproblematic. Therefore, the foundations of Newtonian theory are such that they incorporate Galileo's law as a special case.

With "incorporation" defined, we begin with the first argument. That is, R-A theory explains more phenomena than does perfect competition theory.

14.1.1 Explanatory Power

Even though R-A theory is less than two decades old, studies have shown that, when compared with perfect competition theory, R-A theory *better* explains and predicts numerous phenomena. Specifically, R-A theory (the page numbers in this and the following paragraph refer to Hunt [2000b]; see that source for additional references) contributes to explaining firm diversity (pp. 152–55); makes the correct prediction concerning financial performance diversity (pp. 153–55); contributes to explaining observed differences in quality, innovativeness, and productivity between market-based and command-based economies (pp. 169–70); shows why competition in market-based economies is dynamic (pp. 132–33); explains how competition is a process of knowledge discovery (pp. 29–30; 145–47); contributes to explaining why social relations constitute a resource only contingently (pp. 100–102); and explains how path dependence effects can occur (pp. 149–52).

Furthermore, R-A theory predicts correctly that technological progress dominates the K/L (i.e., capital/labor) ratio in economic growth (pp. 193–94), predicts correctly that increases in economic growth cause increases in investment (pp. 194–99), predicts correctly that most of the technological progress that drives economic growth stems from the actions of profit-driven firms (pp. 199–200), predicts correctly that R-A competition can prevent the economic stagnation that results from capital deepening (pp. 200–203), contributes to explaining the growth pattern of the (former) Soviet Union (pp. 201–3), explains why formal institutions promoting property rights and economic freedom also promote economic growth (pp. 215–28), and explains why informal institutions promoting social trust also promote economic growth (pp. 235–37).

Space limitations dictate that all the preceding instances of explanatory power cannot be reviewed here. Nonetheless, we consider one example: the issue of financial performance diversity.

Financial Performance Diversity

Why are some firms more profitable than others? If, as in perfect competition theory, (1) firms are viewed as combiners of homogeneous, perfectly mobile resources by means of

a standard production function and (2) intra-industry demand is viewed as homogeneous, then the variance in financial performance across firms and their business units must result from industry factors such as collusion. This, of course, is the standard view of the structure → conduct → performance model in industrial organizational economics (Bain 1956). As Schmalensee (1985, p. 342) observes, a "central hypothesis in virtually all [neo]classical work was that increases in seller concentration tend to raise industrywide profits by facilitating collusion." Empirically, therefore, neoclassical theory predicts that "industry effects" should explain most of the variance in firms' performance, and "firm effects" should explain little, if any. In contrast, if, as in R-A theory, firms are viewed as combiners of heterogeneous, imperfectly mobile resources and intra-industry demand is viewed as heterogeneous, then "firm effects" should dominate "industry effects."

Schmalensee's (1985) study investigated the issue of industry effects versus firm effects using variance components analysis. Using the Federal Trade Commission's (FTC) line of business data and 1975 business-unit return on assets as the measure of financial performance, Schmalensee (1985, p. 349) finds industry effects to account for 19.5 percent of the variance of business-unit return on assets and corporate effects to be not significant. He concludes: "[This] supports the classical focus on industry level analysis as against the revisionist tendency to downplay industry differences." However, Rumelt (1991) pointed out that Schmalensee's use of only one year's data not only confounded stable industry effects with transient annual fluctuations but also made it impossible to separate the effects of the overall corporation from those of the individual business unit. When Rumelt (1991) supplemented Schmalensee's 1975 data with the FTC data for 1974, 1976, and 1977, he found that, whereas industry effects explained only 8 percent of the variance, corporate and business-units effects explained 2 percent and 44 percent, respectively. Therefore, he finds "total firm" effects of 46 percent (2 percent + 44 percent) to be almost six times those of industry effects.

Supporting Rumelt's findings, Roquebert, Phillips, and Westfall (1996) find industry, corporate, and business-unit effects to be 10 percent, 18 percent, and 37 percent, respectively (resulting in "total firm" effects of 18 percent + 37 percent = 55 percent). Notably, their database was much larger (over 6,800 manufacturing corporations) was from a more recent time period (1985–91), used a longer time period (seven years versus four), had a broader base (over 940 SIC, four-digit categories), and (unlike FTC data) included both small and large corporations.

Similarly, McGahan and Porter (1997) find industry, corporate, and business-unit effects for their sample of 7,003 corporations (for the time period 1982–88) to be 19 percent, 4 percent, and 32 percent, respectively (resulting in "total firm" effects of 4 percent + 32 percent = 36 percent).[2] Likewise, Mauri and Michaels (1998) use a sample of 264 single-business companies from 69 four-digit SIC code industries, for the time period 1988–92, and find industry and firm effects to be 5 percent and 30 percent, respectively. Similarly, for the time period 1978–92, Mauri and Michaels find the comparable figures to be 4 percent and 19 percent.

In summary, depending on the database used, it appears that industry effects account for 4–19 percent of the variance in performance, as measured by return on assets, and

firm effects account for 19–55 percent. That is, "firm effects" account for two to six times the variance accounted for by "industry effects." In short, industry is the "tail" of competition; the firm is the "dog."

The empirical studies on the diversity of financial performance indicate that R-A theory makes the correct prediction on the issue of financial performance diversity. Competition simply does not produce the requisite "as if" conditions, and/or the competitive circumstances in the U.S. economy are not "close enough" to the assumptions of perfect competition to enable neoclassical theory to make the correct prediction. In contrast, because R-A theory proposes that firms are *best* construed as combiners of heterogeneous, imperfectly mobile resources and intra-industry demand is *best* construed as substantially heterogeneous, then the fact that "firm effects" dominate "industry effects" is precisely what one expects. That is, the descriptively more accurate premises of R-A theory enable it to make the correct prediction.

Although R-A theory better explains and predicts numerous phenomena, there are, of course, times when perfect competition does explain and/or predict well. Why this is the case is addressed next.

14.1.2 Perfect Competition Theory and R-A Theory

The finding discussed in section 14.1.1 that firm effects (the focus of R-A theory) dominate industry effects (the focus of neoclassical theory) supports viewing the *process* identified by R-A theory in Figures 13.1 and 13.2 as the general case of the process of competition. Therefore, because a theory is derived from its assumptions, the evidence supports viewing each of R-A theory's foundational premises in Table 13.1 to be either descriptively realistic or, at least, "close enough" (Friedman 1953). Hunt (2000b) argues that the foundational premises of perfect competition are, indeed, special cases of R-A theory, and, consequently, R-A theory incorporates perfect competition. A key part of the argument is the following issue: how should the foundational premises of R-A theory in Table 13.1 be interpreted?

Note that each assumption in Table 13.1 could be viewed as an idealized state that anchors an end-point on a continuum. That is, demand (P_1) could be conceptualized as a continuum with perfect homogeneity and perfect heterogeneity as idealized anchor-points. Similar continua could be conceptualized for information and its cost (P_2 and P_5) and for the homogeneity-heterogeneity and mobility-immobility of resources (P_7). However, whereas perfect competition is customarily interpreted in the idealized, anchor-point manner, in no case is R-A theory to be interpreted as the anchor-point *opposite* perfect competition. Rather, each foundational premise of R-A theory is proposed as the descriptively realistic general case. Therefore, intra-industry demand (P_1) is to be interpreted for R-A theory as *substantially* heterogeneous. Similarly, information for both firms (P_5) and consumers (P_2) is substantially imperfect and costly. Likewise, many, but not all, resources (P_6) are substantially heterogeneous and immobile.

Consider intra-industry demand (P_1) and the case of footwear. As discussed in section 13.3.1, R-A theory views consumers' tastes and preferences for footwear, which is clas-

sified by the U.S. Census as "NAICS #31621," to be substantially heterogeneous and constantly changing. Furthermore, consumers have substantially imperfect information concerning footwear products that might match their tastes and preferences, and obtaining such information is often costly in terms of both time and money. The implication of heterogeneity is that few—if any—*industry* markets exist: there are only market segments *within* industries. There is no "market for shoes," (NAICS #31621) or even separate markets for women's shoes (#316214) and men's shoes (#316213). Even though all consumers require footwear and one can readily identify a group of firms that manufacture shoes, there is no shoe-industry *market.* That is, the firms that constitute the shoe industry do not collectively face a *single,* downward-sloping demand curve—for such an industry demand curve would imply homogeneous tastes and preferences.

For R-A theory, to the extent that demand curves exist at all, they exist at a level of (dis)aggregation that is too fine to be an "industry." For example, even if there were a men's-walking-shoe market, one certainly would not speak of the men's-walking-shoe *industry.* The fact that intra-industry demand is substantially heterogeneous in *most* industries (even at the six-digit NAICS level) contributes to R-A theory's ability (and neoclassical theory's inability) to make the correct prediction as to the diversity in business-unit financial performance. Likewise, the fact that intra-industry demand is relatively homogeneous ("close enough") in at least *some* commodity-type industries, for example, gold ores (NAICS #212221), contributes to explaining those special cases where perfect competition predicts well. Therefore, R-A theory's premise P_1 is the general case; perfect competition's premise concerning intra-industry demand is the special case.

Premise P_9, the issue of competitive dynamics, raises the following question: under what set of circumstances will the process of R-A competition (which is disequilibrium-provoking, with innovation endogenous) result in perfect competition (which is equilibrium-seeking, with innovation exogenous)? Consider the following scenario. First, assume that a set of firms producing an offering for a particular market segment within an industry has been competing according to R-A theory. Therefore, because of resource heterogeneity (say, different levels of a key competence), the firms are distributed throughout the nine marketplace positions in Figure 13.2. Some firms, because of their comparative advantage in resources, are enjoying superior returns; others, because of their comparative disadvantage in resources, have inferior returns; and still others, because of their parity resources, have parity returns (see Figure 13.1).

Next, assume that, over time, both disadvantaged and parity firms (1) gradually learn how the advantaged firms are producing their offerings more efficiently and/or effectively and (2) successfully imitate the advantaged firms by acquiring or developing the requisite resources. For example, assume that they gradually develop competences equivalent to the advantaged firms. Then assume that, even though all firms seek *superior* financial performance, no firm finds it possible to acquire, develop, or create new resources (e.g., developing a new competence) that will enable it to produce a market offering more efficiently or effectively than any other firm. That is, for some reason or set of reasons, all competition-induced innovation stops, both proactive and reactive. Consequently, all competition-induced technological change stops. Under these economic conditions, then,

the resources of all firms serving this market segment become relatively homogeneous and there will be parity resources producing parity offerings.

Next, assume that the tastes and preferences of consumers in all other market segments served by the firms in this industry shift toward the original segment. Industry consumer demand will then become relatively homogeneous. Suppose further that consumers' tastes and preferences remain stable throughout a significant period of time and that consumers become very knowledgeable about the relative homogeneity of firms' offerings. There will then be parity resources producing parity offerings, which results in all firms' having parity marketplace positions (cell 5 in Figure 13.2).

Next, assume that firms have accurate information about competitive conditions and there are no institutional restraints preventing them from producing their market offerings in the profit-maximizing quantity. Under these economic circumstances, the industry experiences no endogenous technological change, firms become price-takers, and a static equilibrium theory of competition, such as perfect competition, applies. That is, there will be parity resources producing parity offerings, which results in parity marketplace positions and parity performance (see Figure 13.1). The industry has now become a candidate for "industry effects" to dominate "firm effects" in empirical studies, for collusion and barriers to entry to become viable explanations for any industrywide, superior financial performance, and, in general, for industry-level theoretical analyses to be appropriate.

Next, assume that the preceding process occurs in every industry in an entire economy. Then, if this set of economic circumstances persists over time, all competition-induced technological change ceases, all endogenous technological progress stops, all endogenous growth ceases, and a long-run, general equilibrium theory applies (such as Walrasian general equilibrium). In such an economy, growth comes only from exogenous sources, including those sources (e.g., government R&D or a state planning board) that might develop innovations that result in exogenous technological progress, as in neoclassical growth theory (see Hunt 2000b, pp. 179–83).

Note that the preceding analysis began with the process of R-A competition for a market segment and sketches the special economic circumstances that must prevail for the competitive process to result in a static-equilibrium situation in an industry. Among other conditions, it showed that a very important circumstance is that all endogenous innovation must stop (or be stopped). Such a stoppage might come as a result of collusion, complacency, institutional restrictions, governmental fiat, or lack of entrepreneurial competence. The analysis then sketched the special circumstances for a long-run general equilibrium to develop, and, again, it showed that all endogenous technological progress in all industries—hence, all endogenous economic growth—in an economy must cease. Therefore, the statics of perfect competition, both partial equilibrium and general equilibrium, may be viewed as a special case of the dynamics of R-A theory. R-A theory relates to perfect competition in the same way that Newtonian mechanics relates to Galileo's law: the former incorporates the latter. In this manner, R-A theory explains the predictive successes (when such successes occur) of neoclassical perfect competition theory. Therefore, R-A theory is a general theory of competition. Accordingly, the preceding supports argument one:

R-A theory is an appropriate foundation for a general theory of marketing because it is the most general theory of competition.

As noted in section 13.1, R-A theory draws on and has affinities with numerous other theories and research traditions, including evolutionary economics, "Austrian" economics, the historical tradition, heterogeneous demand theory, differential advantage theory, industrial-organization economics, the resource-based tradition, the competence-based tradition, institutional economics, transaction cost economics, and economic sociology. These theories and research traditions are shown in Table 14.1, along with representative works and affinities with R-A theory.

Table 14.1 shows that R-A theory has affinities with differential advantage theory, as exemplified by the works of Clark (1954, 1961) and Alderson (1957, 1965). We turn now to the second argument that R-A theory is toward a general theory of marketing, the "Alderson argument."

14.2 ALDERSON'S THEORY OF MARKET PROCESSES

Wroe Alderson's theoretical efforts spanned numerous marketing subjects, including theories related to pricing, advertising, consumer behavior, innovation, and retailing (Alderson 1957, 1965).[3] However, it is his functionalist theory of market processes that justified his 1965 book's subtitle, *A Functionalist Theory of Marketing.* This theory, partially formalized in Hunt, Muncy, and Ray (1981), enabled Alderson to explain how market processes can take conglomerate resources in the natural state and bring about meaningful assortments of goods in the hands of consumers. Indeed, Alderson's theory of market processes is widely acknowledged to be the theory that comes closest to being a general theory of marketing (Hunt 1983a).[4]

A key component of Alderson's theory of market processes is his theory of competition for differential advantage, which was drawn from the works of John M. Clark (1954, 1961). This theory explains the forces that motivate firms in the marketplace by positing that, in order to survive, firms compete with other firms for the patronage of households. A firm can be assured of the patronage of particular groups of households (i.e., market segments) only when members of the groups have reasons to prefer the output of the particular firm over the output of competing firms. Therefore, each firm will seek some advantages over other firms to ensure the patronage of groups of households. This process, known as "competition for differential advantage," consists of the constant struggle of firms to develop, maintain, or increase their differential advantages over other firms.

The purpose of this section is to develop the second argument for R-A theory being toward a general theory of marketing. The argument is that R-A theory is toward a general theory of marketing because it accommodates key concepts and generalizations from Alderson's theory, extends them, and integrates them into a broader theory. We first review the development of Alderson's theory of market processes, including its relationship to Clark's effective competition theory and how it influenced Alderson's functionalist theory. We then show how R-A theory accommodates, integrates, and extends Alderson's market processes theory. Therefore, we argue, R-A theory is toward a general theory of marketing.

Table 14.1

Other Research Traditions and Resource-Advantage Theory

	Research tradition	Representative works	Affinities with R-A theory
1.	Evolutionary economics	Marshall (1890) Schumpeter (1934, 1950) Alchian (1950) Nelson and Winter (1982) Langlois (1986) Dosi et al. (1988), Witt (1992) Foss (1993), Hodgson (1993)	Competition is an evolutionary, disequilbrating process. Firms have heterogeneous competences. Path dependencies can occur.
2.	Austrian economics	Mises (1920,1949) Hayek (1935,1948) Rothbard (1962) Kirzner (1979, 1982) Lachmann (1986)	Competition is a knowledge-discovery process. Markets are in disequilibrium. Entrepreneurship is important. Value is subjective. Intangibles can be resources.
3.	Heterogeneous demand theory	Chamberlin (1933) Smith (1956) Alderson (1957, 1965) McCarthy (1960) Myers (1996)	Intra-industry demand is substantially heterogeneous. Heterogeneous supply is natural. "Product" should be defined broadly.
4.	Differential advantage theory	Clark (1954, 1961) Alderson (1957, 1965)	Competition (a) is dynamic, (b) is both initiatory and defensive, and (c) involves a struggle for advantages. General equilibrium is an inappropriate welfare ideal.
5.	Historical tradition	North (1981, 1990) Chandler (1990) Landes (1998)	History "counts." Firms are entities that are historically situated in space and time. Institutions influence economic performance.
6.	Industrial-organization economics	Mason (1939) Bain (1954, 1956) Porter (1980, 1985)	Firm's objective is superior financial performance. Marketplace positions determine relative performance. Competitors, suppliers, and customers influence performance.
7.	Resource-based tradition	Penrose (1959) Lippman and Rumelt (1982) Rumelt (1984), Wernerfelt (1984) Dierickx and Cool (1989) Barney (1991, 1992) Conner (1991), Grant (1991)	Resources may be tangible or intangible. Firms are historically situated combiners of heterogeneous, imperfectly mobile resources.

(continued)

Table 14.1 *(continued)*

	Research tradition	Representative works	Affinities with R-A theory
8.	Competence-based tradition	Selznick (1957), Andrews (1971) Hofer and Schendel (1978) Hamel and Prahalad (1989, 1994a, 1994b) Prahalad and Hamel (1990, 1993) Teece and Pisano (1994) Day & Nedungadi (1994), Aaker (1995) Sanchez, Heene, and Thomas (1996) Heene and Sanchez (1997) Sanchez and Heene (1997)	Competition is disequilibrating. Competences are resources. Renewal competences prompt proactive innovation. Firms learn from competing. Firms are embedded.
9.	Institutional economics	Veblen (1899, 1904) Commons (1924, 1934) Hamilton (1932), Kapp (1976) Neale (1987), Mayhew (1987) DeGregori (1987), Ranson (1987) Hodgson (1994)	Competition is disequilibrating. "Capital" is more than just physical resources. Resources have "capabilities."
10.	Transaction cost economics	Coase (1937) Williamson (1975, 1985, 1996)	Opportunism occurs. Many resources are firm-specific. Firm-specific resources are important.
11.	Economic sociology	Parsons and Smelser (1956) Granovetter (1985, 1994) Etzioni (1988), Coleman (1990) Zukin and DiMaggio (1990) Powell and Smith-Doerr (1994) Smelser and Swedberg (1994) Scott (1995), Uzzi (1996) Fligstein (1996)	Institutions can be independent variables. Social relations may be resources. Economic systems are embedded.

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Note: This table shows the affinities that other research traditions have with R-A theory. However, as discussed in Hunt (2000c), R-A theory is not a composite of these theories and research traditions, nor was R-A theory developed from combining them. My co-author, Robert M. Morgan, and I developed the foundational premises and structure of R-A theory prior to our having detailed knowledge of most of the eleven traditions and theories (and prior to our even being *aware* of some of them). It was only *after* our initial efforts at developing the foundations and structure of R-A theory that numerous scholars alerted us that the foundations and structure of the theory had parallels in diverse research traditions. After reviewing them, we believed that academic honesty demanded that we acknowledge how the eleven theories and traditions are similar to, but not the same thing as, R-A theory. Again, R-A theory is the product of (1) a specific (and parsimonious) set of foundational premises and (2) a specific structure.

14.2.1 Developing the Theory of Market Processes

Alderson (1957, 1965), in developing his functionalist theory of market processes, drew heavily on several sources. Most notably, he was influenced by Clark's (1954, 1961) theory of effective competition. To understand Alderson, therefore, requires an understanding of Clark. Hence, we first review Clark's effective competition theory.

14.2.2 Effective Competition Theory

In the 1930s and 1940s, Clark (1940) developed the concept—not reviewed here—of "workable" competition. Later, Clark (1954, 1961) abandoned the label *workable* competition and replaced it with *effective* competition for the following reasons:

> I am shifting the emphasis from "workable" to "effective competition" because "workable" stresses mere feasibility and is consistent with the verdict that feasible forms of competition, while tolerable, are still inferior substitutes for that "pure and perfect" competition which has been so widely accepted as a normative ideal. And I have become increasingly impressed that the kind of competition we have, with all its defects—and these are serious—is better than the "pure and perfect" norm, because it makes for progress. Some departures from "pure and perfect" competition are not only inseparable from progress, but necessary to it. The theory of effective competition is dynamic theory. (Clark 1961, p. ix)

Because a dynamic theory of competition would have different standards of appraisal, Clark inquires as to the objectives that society would want competition to accomplish. He suggests that competition should provide or promote an adequate variety of products (including low-priced products, high-quality products, and new products), economic opportunity, social mobility, a productive economy, rewards to innovators, low search costs, a diffusion of the gains of progress, high and stable employment, business freedom, the elimination of process inefficiencies, and an appropriate balance of desirable and undesirable effects on individuals (Clark 1954, pp. 323–24; 1961, pp. 63, 74, 77, 78, 81, 82, 86).

Taken collectively, the desirable outputs of competition would seem to be a tall order. Yet Clark maintains that effective, dynamic competition could come tolerably close to achieving all his suggested goals. But effective, dynamic competition does not imply that firms would be price-takers, or that they would seek to maximize profits, or that competition is a struggle with only one winner (1961, p. 18). What, then, does effective competition imply? Acknowledging his "kinship" with Schumpeter's "creative destruction," competition is:

> a form of independent action by business units in pursuit of increased profits . . . by offering others inducements to deal with them, the others being free to accept the alternative inducements offered by rival business units. Active competition consists

> of a combination of (1) initiatory actions by a business unit, and (2) a complex of responses by those with whom it deals, and by rivals. (Clark 1954, p. 326)

Clark's definition of dynamic competition is remarkably compact. Indeed, his entire 1961 book is devoted to "unpacking" it.[5] A good starting point for us is his view that firms pursue increased profits rather than maximum profits.

Clark (1961, p. 9) specifically alerts readers that his "profit minded" firms are not profit maximizers. He argues that firms do not maximize profit because all firms at all times face such conditions of uncertainty as to consumers' and rivals' actions that they lack the necessary information to maximize (pp. 93, 471). He further argues that some firms at some times (1) sacrifice profits for growth (p. 96), (2) sacrifice profits in favor of community responsibilities (p. 91), and (3) sacrifice profits because of following the "morals of trade" (p. 479). By substituting "increased profits in the face of uncertainty" for the neoclassical "maximum profits in the face of perfect information," Clark makes competition dynamic. That is, the continuing pursuit of increased profits, *more* profits, prompts changes in the "inducements to deal."

When firms are successful in effecting changes in inducements targeted at specific customers, for example, by providing market offerings of higher quality or lower prices, such firms have a "differential advantage" over rivals (1954, p. 327). It is the pursuit of differential advantages over rivals that prompts the innovations that constitute "aggressive competition" (1961, p. 14). For Clark, the sum of innovations that result in differential advantages over rivals constitutes the technological progress required for a "dynamically progressive system," that is, for economic growth (1961, p. 70). Therefore, mandating the homogeneity of demand and supply—as argued for by many defenders of perfect competition theory—would necessitate the "stoppage of growth and progress, a price we should be unwilling to pay" (1961, p. 70). Indeed, "perfect competition . . . define[s] a model from which competitive progress would be ruled out; progress could come only by government fiat" (1954, p. 329).

For Clark, the innovations resulting from aggressive competition can come from small firms, as stressed by Marshall (1890/1949), or from large firms, as stressed by Schumpeter (1950). Contrasted with Schumpeter (1950), however, the innovations by firms large and small can be such that they only modestly improve quality or lower costs. Cumulatively, however, Clark points out that small innovations are important to the firm and economy. Whether an innovation is brought about by small firms or large ones, whether it is industry-shaking or only a modest improvement, "the life history of a successful innovation is a cycle. It is developed, profitably utilized, and ultimately loses its value as a source of special profit" (1961, p. 189).

An innovation loses its value to produce superior profits when it is either superseded by something better (i.e., Schumpeter's "creative destruction") or it is diffused among rivals and becomes standard practice by "defensive competition." Thus, when an innovation is diffused among rivals, it becomes—rather than a differential advantage for the originator—much like the "ante" in a poker game. Both aggressive and defensive competition are required for *effective* competition: "without initiatory moves, competition does not

begin, without defensive responses, it does not spread" (Clark 1961, p. 429). Aggressive competition creates innovations and differential advantages; defensive competition diffuses innovations and neutralizes such advantages. As to the speed of neutralization:

> If a potential innovator expects neutralization to be complete before he has recovered the costs of innovation, his incentive vanishes. . . . On the other hand, if neutralizing action were permanently blocked, the initiator would have a limited monopoly, in the sense of a permanent differential advantage. . . . The desirable case lies somewhere between too prompt and too slow neutralization. I will not call it an "optimum," because that term suggests a precision which no actual system could attain. (Clark 1954, pp. 327–28)

Clark's hope was that his dynamic theory of effective competition would provide a framework for understanding actual forms of competition and for fostering those forms most conducive to a dynamic welfare ideal. He knew, however, that "the threat of failure looms large, in that readers whose conception of theory is identified with models of determinate equilibrium are likely to decide that no theory has been produced" (1961, p. x). He was prescient, to say the least. His 1954 article and his 500-page 1961 book—neither having a single differential equation or geometrical representation—were not incorporated into mainstream economics, nor are they cited and discussed today. However, Clark's work did significantly impact Alderson's (1957, 1965) functionalist theory of market processes.

14.2.3 Alderson's Functionalist Theory of Market Processes

Alderson (1957, 1965) was strongly influenced by Chamberlin's (1933) heterogeneous demand theory and by Clark's (1954, 1961) theory of effective, dynamic competition. Also, he was impressed by Merton's (1949/1968) functionalist systems approach to theory development. Furthermore, his background in marketing, with its historical interest in groups of manufacturers, wholesalers, and retailers that form channels of distribution, pointed him toward developing a theory of marketing systems. Accordingly, his functionalist theory of market processes may be viewed as a functionalist systems approach to integrating theories of heterogeneous demand, differential advantage, and channels of distribution.

Alderson (1957, p. 16) views functionalism as "that approach to science which begins by identifying a system of action, and then tries to determine how and why it works as it does." He identifies (1) firms as the subsystems that produce goods and (2) households as the subsystems that constitute the basic consuming units. He (1965, p. 39) notes that firms evolve in a society when specialization of labor results in removing the production function for some goods from the household. Extending Chamberlin's (1933) view that intra-industry demand is substantially heterogeneous, he notes that the particular assortment of goods that is viewed as meaningful or desirable by any one household is likely to differ greatly from that of others. Thus, the macro-systems that he seeks to understand

and explain are those that involve firms taking tangible resources in their natural state and transforming them into a variety of marketplace goods. These various goods ultimately wind up as meaningful assortments of goods in the hands of particular households.

Although firms pursue profit, Alderson (1957, p. 54) maintains that they do so *as if* they had a primary goal of survival. The survival goal results from firm owners and employees believing that they can obtain more in terms of financial and nonfinancial rewards by working toward the survival of their existing firms than by acting individually or by becoming members of other firms. Firm growth, therefore, is sought because of the conviction that growth is necessary for survival (1957, pp. 103–8). In a market-based economy, however, survival depends crucially on a firm's ability to compete with other firms in seeking the patronage of specific (1) intermediate buyers and/or (2) ultimate households.

A firm can be assured of the patronage of intermediate buyers and/or groups of households only when buyers have reasons to prefer its output over that of competing firms. Therefore, each competing firm will seek some advantage over other firms to ensure the patronage of some group of either intermediate buyers or ultimate households. Citing the work of Clark (1954), Alderson labels the process "competition for differential advantage" (1957, p. 101). Indeed, "no one enters business except in the expectation of some degree of differential advantage in serving his customers, and . . . competition consists of the constant struggle to develop, maintain, or increase such advantages" (1957, p. 106). Therefore:

> The functionalist or ecological approach to competition begins with the assumption that every firm must seek and find a function in order to maintain itself in the market place. Every business firm occupies a position, which is in some respects unique. Its location, the product it sells, its operating methods, or the customers it serves tend to set it off in some degree from every other firm. Each firm competes by making the most of its individuality and its special character. It is constantly seeking to establish some competitive advantage . . . [because] an advanced method of operation is not enough if all competitors live up to the same high standards. What is important in competition is differential advantage, which can give a firm an edge over what others in the field are offering. (Alderson 1957, pp. 101–102)

Alderson (1957, pp. 184–97) identifies six bases of differential advantage for a manufacturing firm: market segmentation, selection of appeals, transvection, product improvement, process improvement, and product innovation. By market segmentation having the potential for a differential advantage, Alderson means that firms may have an advantage over competitors when they (1) identify segments of demand that competitors are not servicing (or rivals are servicing poorly) and (2) they subsequently develop market offerings that will appeal strongly to those particular segments. On the other hand, by "selection of appeals" he means that some firms can achieve advantage by the images that are conveyed to consumers through advertising and other promotional means. Similarly, by "transvection" he means an advantage in reaching a market segment through a unique channel of distribution.

The existence of a differential advantage gives the firm a position in the marketplace known as an "ecological niche" (1957, p. 56). The "core" and "fringe" of a firm's ecological niche consists of the market segments for which the firm's differential advantage is (1) ideally suited and (2) satisfactorily suited, respectively. A firm can survive attacks by competitors on its "fringe" as long as its "core" remains intact; it can survive attacks on its "core" as long as it has the will and ability to find another differential advantage and another core (1957, pp. 56–57). Therefore, given heterogeneity of demand and competition for differential advantage, heterogeneity of supply is a natural phenomenon. That is, manufacturers will respond to heterogeneity of demand by producing a variety of different goods and many variations of the same generic kind of good (1957, p. 103).

To reach households, however, manufacturing firms require market intermediaries, that is, channels of distribution. Market processes involving intermediaries are essentially "matching" processes, that is, matching segments of demand with segments of supply. In a *perfectly* heterogeneous market, each small segment of demand, that is, each household, could be satisfied by just one unique segment of supply (i.e., one firm) (Alderson 1965, p. 29). In most markets, however, there are partial homogeneities. That is, there are groups or segments of households *desiring* substantially similar products, and there are groups of firms *supplying* substantially similar products.

The major job of marketing intermediaries is to effect exchange by matching segments of demand with segments of supply. The matching process comes about as a result of a sequence of sorts and transformations (1965, p. 26). A sort is the assignment of goods, materials, or components to the appropriate facilities. A transformation is the change in the physical form of a good or its location in time or space.

With the preceding as backdrop, Alderson (1965, p. 26) can provide an answer to the question that prompted his functionalist theory. Given heterogeneity of demand, heterogeneity of supply, competition for differential advantage, and the requisite institutions (intermediaries) to effect the sorts and transformations necessary to match segments of demand with segments of supply, market processes will take resources in the natural state and bring about meaningful assortments of goods in the hands of households.

14.3 THE THEORY OF MARKET PROCESSES AND R-A THEORY

With respect to the nature of competition, both Alderson's functionalist theory and Clark's effective competition rely on the concept of competition for differential advantage. Therefore, we use the label *differential advantage theory* ("D-A theory") to refer to the combination of their respective views. This section argues that R-A theory accommodates and integrates key concepts and generalizations of D-A theory into its general theory of competition. Table 14.2 compares D-A theory and R-A theory on several attributes. As is readily apparent, R-A theory both draws on, has numerous affinities with, and extends D-A theory. Here we focus on five areas for discussion.

First, both differential advantage theory and R-A theory maintain that competition is dynamic (see #5 in Table 14.2). Indeed, they share a similar propulsion mechanism. For

Table 14.2

Differential-Advantage Theory and Resource-Advantage Theory

Differential-Advantage Theory	Resource-Advantage Theory
1. Perfect competition is not an appropriate welfare ideal.	1. Perfect competition is not an appropriate welfare ideal. However, R-A competition is desirable because it promotes resource allocation, resource creation, productivity, and economic growth.
2. Industry demand is heterogeneous.	2. Industry demand is heterogeneous (P1 in Table 13.1).
3. Competition matches segments of demand and supply.	3. R-A competition is segment by segment and matches segments of demand and supply.
4. Firm motivation is not profit maximization, but increased profits and survival.	4. Firm motivation is superior financial performance, which equates with "more than" and "better than" some referent (P4 in Table 13.1). Superior rewards to stakeholders result from firm superior performance (P3 in Table 13.1).
5. Competition is dynamic.	5. The objectives of "more than" and "better than" imply dynamic competition.
6. Markets are discrepant (products wanted, not produced; products produced, not wanted).	6. Markets are discrepant (products wanted, not produced; products produced, not wanted).
7. Competition is evolutionary, with ecological niches.	7. Competition is evolutionary, nonconsummatory, with niches. The units of the evolutionary selection are firms and resources. Competition is the selection process.
8. Firms seek differential advantages.	8. It is comparative advantages in resources that lead to marketplace positions of competitive advantage and, thereby, superior financial performance (Figures 13.1 and 13.2).
9. Competition neutralizes advantages.	9. Competition can neutralize competitors' advantages by acquisition of similar resources and/or can leapfrog competitors by reactive innovations that result in superior resources. When resources are tacit, causally ambiguous, socially or technologically complex, interconnected, or have mass efficiencies or time compression diseconomies, they are more difficult to neutralize or leapfrog.
10. Competitive actions may be aggressive or defensive.	10. Proactive and reactive innovations constitute aggressive and defensive competitive actions, respectively.
11. Firms sort (sort out, assort, allocate, and accumulate).	11. When firms sort (sort out, assort, allocate, and accumulate), they may develop sorting competences that become firm resources.

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D-A theory, the mechanism is increased profits; for R-A theory, it is the more general concept (and more completely explicated concept) of superior financial performance. That is, R-A theory proposes that the firm's primary objective of superior financial performance (P_4 in Table 13.1) is pursued under conditions of imperfect (and often costly to obtain) information about extant and potential market segments, competitors, suppliers, shareholders, and production technologies (P_5 in Table 13.1). Superior financial performance is indicated by such measures as profits, earnings per share, return on investment, and capital appreciation. Here, "superior" equates with both "more than" and "better than" (see #4 in Table 14.2). It implies that firms seek a level of financial performance exceeding that of some referent. For example, the referent can be the firm's own performance in a previous time period, the performance of rival firms, an industry average, or a stock-market average, among others. Affecting the process of competition, both the specific measure and specific referent will vary somewhat from time to time, firm to firm, industry to industry, and culture to culture (see the five environmental factors in Figure 13.1).

Firms are posited to pursue superior financial performance because superior rewards—both financial and nonfinancial—will then flow to owners, managers, and employees (consistent with the view of human motivation identified in P_3 of Table 13.1). However, superior financial performance does not equate with the neoclassical concepts of "abnormal profits" or "rents" (i.e., profits differing from the average firm in a purely competitive industry in long-run equilibrium) because R-A theory views industry long-run equilibrium as such a rare phenomenon that "normal" profits cannot be an empirical referent for comparison purposes. Furthermore, the actions of firms that collectively constitute competition do not force groups of rivals to "tend toward" equilibrium. Instead, the pursuit of *superior* performance implies that actions of competing firms are disequilibrating, not equilibrating. That is, R-A competition is necessarily dynamic because all firms cannot be superior *simultaneously.*

As a second point of similarity, neither D-A theory nor R-A theory is defended on the ground that its theory of competition represents "second best" or "workable" approximations of perfect competition. Instead, both theories deny that the equations of general equilibrium, relying as they do on perfect competition, represent the appropriate welfare ideal (see #1 in Table 14.2). For both D-A and R-A theory, the appropriate welfare ideal must accommodate, at the minimum, competition-induced technological progress. The more general R-A theory, contrasted with D-A theory, explicates in detail how R-A competition produces increases in productivity and economic growth (see Hunt 2000b).

Third, both D-A and R-A theory share the view that competition involves both initiatory and defensive actions (see #10 in Table 14.2). The "aggressive competition" and "defensive competition" of D-A theory parallel the "proactive innovation" and "reactive innovation" of R-A theory. Thus, competition-induced innovations, whether large or small, by huge corporations or solitary entrepreneurs, play a major role in both theories.

Fourth, both D-A and R-A theory share the view that competition involves the struggle among rivals for *advantages* (see #8 in Table 14.2). For D-A theory, the concept of the kinds of advantages that firms pursue are of an unspecified (or only limitedly specified) nature. For R-A theory, firms pursue two kinds of advantages: advantages in resources and advantages in marketplace position. Specifically, they pursue comparative advantages

in resources that will yield marketplace positions of competitive advantage and, thereby, superior financial performance (see Figures 13.1 and 13.2). Furthermore, R-A theory explicates the nature of resources that will make effective neutralization by rivals less likely or at least more time-consuming: when resources are imperfectly mobile, inimitable, and imperfectly substitutable, they are more likely to thwart effective neutralization (see #9 in Table 14.2). That is, when resources are tacit, causally ambiguous, socially or technologically complex, interconnected, or exhibit mass efficiencies or time-compression diseconomies, they are *less* likely to be quickly and effectively neutralized and *more* likely to produce a sustainable competitive advantage.

Finally, both D-A theory and R-A theory are developed in a natural language, that is, English. They are not developed in the language of mathematics. But R-A theory's preference for natural-language exposition should not be interpreted as being anti-equation. Rather, unlike D-A theory, the more general R-A theory—as discussed in section 14.1—is argued to be a theory of competition that incorporates perfect competition theory as a special case and, thereby, explains when the equations in the neoclassical tradition will predict accurately.

14.3.1 Conclusion on Alderson and R-A Theory

Wroe Alderson influenced considerably the development of marketing theory and practice. Indeed, his functionalist theory of marketing processes (Alderson 1957, 1965) incorporates many concepts that are integral to current marketing strategies. For example, Alderson's theory argues that both supply and demand are heterogeneous and that markets are discrepant. These concepts form the basis for market segmentation strategy. Segmentation viewed as a *strategic* option involves (1) identifying segments of demand, (2) targeting specific segments, and (3) developing specific marketing "mixes" for each targeted segment (see section 15.3.1 and Hunt and Arnett 2004a).

Alderson's differential advantage theory of competition grounds his theory of market processes, and R-A theory incorporates and extends Alderson's key concepts and generalizations. In five major ways, R-A theory draws on, has affinities with, and *extends* differential advantage (D-A) theory. Among other things, both theories: (1) maintain that competition is dynamic, (2) eschew the notion that its theory of competition represents a "second best" or "workable" approximation of perfect competition, (3) share the view that competition involves both initiatory and defensive actions by firms, (4) view competition as a constant struggle among rivals for advantages, and (5) are developed in a natural language, that is, English, rather than the language of mathematics. The affinities between the D-A and R-A theories of competition are shown in Table 14.2. Because it extends Alderson's theory of market processes, resource-advantage theory is toward general theory of marketing.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. The purpose of scientific theories is to explain and predict phenomena, thereby contributing to understanding phenomena. What should a theory of competition explain and predict? Why?

2. Clark maintains that competition should result in "a diffusion of the gains of progress." What does he mean by the "gains of progress"? To *whom* should the gains be diffused? Does R-A theory diffuse the gains of progress? If so, how? If not, should the theory be modified to accommodate the diffusion of gains?
3. Alderson (1965, p. 186) writes: "To state the essence of market segmentation more succinctly, it is the policy of selecting a group of customers with relatively homogeneous demands out of an area of consumer use which is heterogeneous in the aggregate and so deriving production economies from that selection." Evaluate. (Hint: see Figure 13.2.)
4. Neoclassical economics has historically viewed "product differentiation" as being contrary to the best interests of consumers because it establishes "monopoly." On the other hand, Alderson (1965, p. 185) states: "Chamberlin began with product differentiation and only in his later years came to realize that heterogeneity is inherent on the demand side of the market and is not necessarily created by the supplier." Why is it important to distinguish between "inherent" heterogeneity and "created" heterogeneity? Is "created heterogeneity" antisocial? Why or why not?

NOTES

1. This chapter draws extensively on Hunt (2000b, 2001a, and 2002a, Chapter 9) and Hunt and Arnett (2006b), which argue that R-A theory is a general theory of competition and toward a general theory of marketing.

2. By the time of McGahan and Porter (1997), the entire nature of the debate over firm performance had changed dramatically. Originally, advocates of industry-based strategy (e.g., Montgomery and Porter 1991) were citing Schmalensee (1985) to justify their focusing on "choosing industry" as the *key* strategic decision. After Rumelt's (1991) replication and extension of Schmalensee found firm factors to account for almost six times the variance of industry factors (46 percent vs. 8 percent), the debate shifted toward whether industry choice matters *at all.* Thus, McGahan and Porter's (1997) study, which finds that firm effects dominate industry effects by *only* 36 percent to 19 percent, is interpreted by its authors as confronting the challenge from Rumelt and others that industry, far from being *key,* does not seem to matter at all. The point to be emphasized here is that no one now claims empirical support for the neoclassical position. That is, after Rumelt's (1991) and other studies, no one argues seriously the neoclassical position that either industry is everything or industry effects dominate firm effects. Stated differently, despite the fact that Schmalensee (1985) is still cited frequently, no one arguing the neoclassical position on firm performance can now be taken seriously.

3. See Wooliscroft, Tamilia, and Shapiro (2006) for an excellent treatment and updating of Alderson's works.

4. Surveys of marketing scholars identify Alderson as the scholar who has most contributed to the development of marketing theory (Chonko and Dunne 1982). See Hunt, Muncy, and Ray (1981) for a detailed explication of Alderson's functionalist theory of market processes.

5. Throughout his 1961 book, Clark (pp. 9, 13, 18, 213) gives definitions of "competition." Although each is consistent with his 1954 work, cited here, none appears to be as complete, yet still succinct.

15

STRATEGY AND R-A THEORY

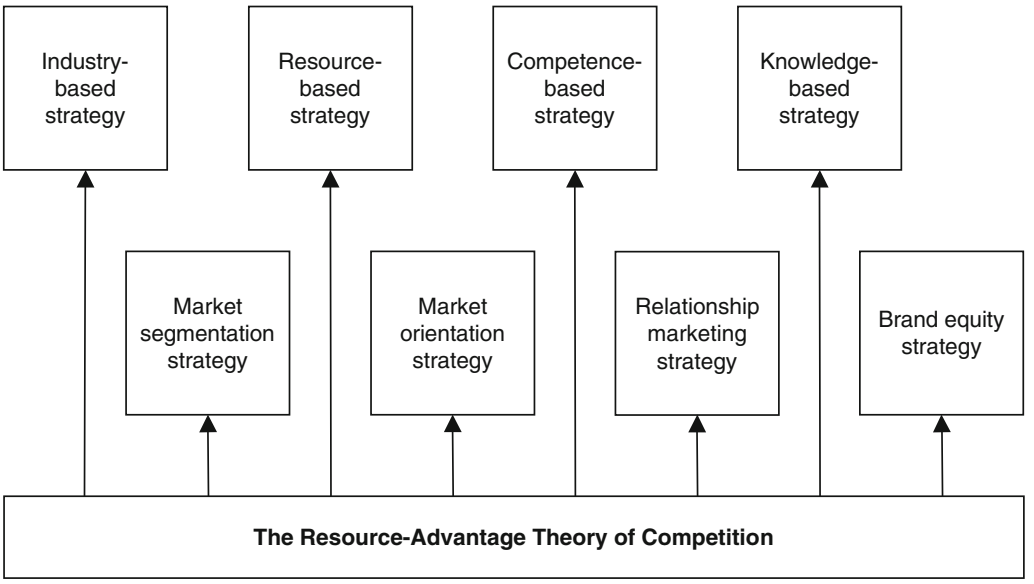
> Assume competition. . . . Good communication is economical; to place the adjective "perfect" before the noun "competition" is simply redundant. That it frames the language, hence, the discourse of mainstream economics testifies as to the dominance of the theory of perfect competition.
>
> *—Hunt (2000b, p. 1)*

Chapter 14 developed two arguments that R-A theory is toward a general theory of marketing. First, because marketing takes place within the context of competition, a general theory of *marketing* should be consistent with the most general theory of *competition.* Accordingly, because R-A theory is a general theory of competition, it is an appropriate foundation for working toward a general theory of marketing. Second, the closest thing to a general theory of marketing today is Alderson's (1957, 1965) functionalist theory of market behavior. Therefore, Chapter 14 argued that R-A theory is toward a general theory of marketing because it accommodates and extends key concepts and generalizations from Alderson's theory and integrates them into a broader theory.

Chapter 15 develops the third argument. Recall that R-A theory is a positive theory; yet much of marketing is normative. Furthermore, recall that *what is* does not imply *what ought to be.* Finally, as discussed in section 8.2, recall that good positive theory can provide a foundation for good normative theory. Therefore, I argue in this chapter that R-A theory is toward a general theory of marketing because it provides a foundation for—both research in and the teaching of—the normative area of marketing strategy. Because marketing strategy is closely related to business strategy, this chapter also shows how R-A theory provides a foundation for business strategy.

Theories of business and marketing strategy are normative imperatives. That is, they have the following general form: "In order for a firm to achieve its goals, it *should* . . ." What follows the "should" differs according to the particular theorist's school of thought. For example, one school stresses the importance of industry factors (Montgomery and Porter 1991; Porter 1980, 1985), while others stress firm-specific competences (Day and Nedungadi 1994; Hamel and Prahalad 1994a, 1994b; Prahalad and Hamel 1990; Sanchez, Heene, and Thomas 1996) and inimitable resources (Barney, 1991; Grant 1991; Wernerfelt 1984). Some schools urge firms to focus on developing their dynamic capabilities (Teece and Pisano 1994) and higher-order learning processes (Dickson 1996; Senge 1990;

Figure 15.1 **Integrating Business and Marketing Strategy**



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Sinkula, Baker, and Noordewier 1997), while others emphasize the value-creating potential of networks of relationships (Berry and Parasuraman 1991; Grönroos 1996; Gummesson, 1994; Morgan and Hunt 1994; Sheth and Parvatiyar 1995a, 1995b; Varadarajan and Cunningham 1995; Weitz and Jap 1995; Wilson 1995). Some schools advocate a market orientation (Jaworski and Kohli 1993; Slater and Narver 1994; Webster 1992, 1994), while others focus on "first mover" innovations (Kerin, Varadarajan, and Peterson 1992; Lieberman and Montgomery 1988) and brand equity (Aaker 1991; Keller 1993).

Most normative theories of *business* strategy can be grouped into one of four categories: industry-based, resource-based, competence-based, and knowledge-based. Figure 15.1 shows each of these four, as well as four specific theories of marketing strategy: market segmentation, market orientation, relationship marketing, and brand equity. As depicted in Figure 15.1, R-A theory provides a positive theoretical foundation for an *integrative* understanding of all eight normative theories of strategy. That is, because the implementation of normative strategies occurs in the context of competition, and R-A theory best describes the nature of competition in market-based economies, R-A theory can *ground* business and marketing strategy. Because it fosters an integrative understanding of business and marketing strategy, R-A theory can be used to teach these subjects. When students understand how competition *works,* they can understand which normative theory of strategy might work *well* in particular competitive contexts. Indeed, R-A theory is being used to teach business and marketing strategy in many strategy courses, and Hunt and Madhavaram (2006b) provide guidelines for doing so.

For managers, choosing wisely from among the various schools of strategic thought

requires that managers understand not just the alternative theories, but also the competitive contexts in which each normative imperative would likely work well. A strategy that is highly successful in one competitive context might fail dismally in another. Therefore, just as students' understanding of strategy is enhanced when they understand competition, managers' effective use of business and marketing strategy requires that they understand the nature of *competition.* Alternatively stated, theories of business and marketing strategy must be *grounded* in a theory of competition.

This chapter begins by succinctly reviewing the four theories of business strategy and the four theories of marketing strategy shown in Figure 15.1. Then, we show how R-A theory provides a foundation for each. The chapter concludes with an evaluation of the following macromarketing question: Does the use of strategies that focus on brand equity harm or benefit society?

15.1 BUSINESS STRATEGY

Modern business strategy traces to the works on administrative policy of Kenneth Andrews and his colleagues at Harvard (Andrews 1971, 1980, 1987; Christiansen et al. 1982; Learned et al. 1965). Viewing business strategy as the match a business makes between its internal resources and skills and the opportunities and risks created by its external environment, they developed the SWOT framework: *S*trengths, *W*eaknesses, *O*pportunities, *T*hreats. In this framework, the main task of corporate-level strategy is identifying businesses in which the firm will compete (Andrews 1971). Alternative strategies for the firm are developed through an appraisal of the opportunities and threats it faces in various markets, that is, *external* factors, and an evaluation of its strengths and weaknesses, that is, *internal* factors. Good strategies, under the SWOT framework, are those that are explicit (for effective implementation) and effect a good match or "fit." Such strategies avoid environmental threats, circumvent internal weaknesses, and exploit opportunities through the strengths or distinctive competences of the firm. Since the work of Andrews and his colleagues, research on strategy has centered on four approaches: industry-based strategy, resource-based strategy, competence-based strategy, and knowledge-based strategy. The central premises of each approach are detailed in Table 15.1.

15.1.1 Industry-Based Strategy

An "external factors" approach, the industry-based theory of strategy, as exemplified by Porter (1980, 1985), turns industrial-organization economics "upside down" (Barney and Ouchi 1986, p. 374). That is, what historically has been (and continues to be) considered anticompetitive and socially undesirable under neoclassical, industrial-organization economics forms the basis for normative competitive strategy. In this view, choosing the industries in which to compete and/or altering the structure of chosen industries to increase monopoly power should be the focus of strategy because "studies have repeatedly shown that average industry profitability is, by far, the most significant predictor of firm performance" (Montgomery and Porter 1991, pp. xiv–xv). (As shown in section

Table 15.1

Theories of Business Strategy: Central Premises

To achieve competitive advantage and, thereby, superior financial performance, firms *should* . . .

. . . for industry-based strategy:
(1) choose industries and/or modify their structures,
(2) then select one of three generic strategies (cost leadership, differentiation, and or focus),
(3) manage well the activities in their value chains.

. . . for resource-based strategy:
(1) seek resources that are valuable, rare, imperfectly mobile, inimitable, and nonsubstitutable.

. . . for competence-based strategy:
(1) identify, seek, develop, reinforce, maintain, and leverage distinctive competences.

. . . for knowledge-based strategy:
(1) systematically accumulate a storehouse of knowledge and,
(2) systematically use that knowledge to guide innovation.

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14.1.1 and further discussed in section 15.1.2, Montgomery and Porter's claim is now acknowledged to be empirically incorrect.)

Porter's (1980) "five forces" framework maintains that the profitability of a firm in an industry is determined by (1) the threat of new entrants to the industry, (2) the threat of substitute products or services, (3) the bargaining power of its suppliers, (4) the bargaining power of its customers, and (5) the intensity of rivalry among its existing competitors. Therefore, because "a firm is not a prisoner of its industry's structure" (Porter 1985, p. 7), strategy should aim at choosing the best industries (usually those that are highly concentrated) and/or altering industry structure by raising barriers to entry and increasing one's bargaining power over suppliers and customers.

After choosing industries and/or altering their structure, Porter (1980) advocates choosing one of three "generic" strategies: (1) cost leadership, (2) differentiation, or (3) focus. That is, superior performance can result from a competitive advantage brought about by a firm, relative to others in its industry, having a lower cost position, having its offering perceived industry-wide as being unique, or having a focus on one particular market segment and developing a market offering specifically tailored to it. Although it is possible to pursue successfully more than one strategy at a time (and the rewards are great for doing so), "usually a firm must make a choice among them, or it will become stuck in the middle" (Porter 1985).

After choosing one of the three generic strategies, internal factors come into play. Specifically, Porter (1985) argues that the firm should implement its strategy by managing well the activities in its "value chain," because "[t]he basic unit of competitive advantage . . . is the discrete activity" (1991, p. 102). If value is defined as "what buyers are willing to pay," then "superior value stems from offering lower prices than competitors for equivalent benefits or providing unique benefits that more than offset a higher price" (1985, p. 4).

For Porter (1985), activities in the firm's value chain are categorized as either primary

or support. Primary activities include inbound logistics, operations, outbound logistics, marketing and sales, and service. Support activities include procurement, technology development (improvement of product and process), human resource management, and firm infrastructure (e.g., general management, planning, finance). Doing these activities well improves gross margin, promotes competitive advantage, and thereby produces superior financial performance. Therefore, the fundamental strategic thesis of industry-based strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should (1) choose industries and/or modify their structure, (2) select one of three generic strategies, and (3) manage well the activities in its value chain.

15.1.2 Resource-Based Strategy

Because (1) empirical studies show that highly concentrated industries are not more profitable than their less concentrated counterparts (Buzzell, Gale, and Sutton 1975; Gale and Branch 1982; Ravenscraft, 1983), and (2) similar studies show that the industry market share-profitability relationship is spurious (Jacobson and Aaker 1985; Jacobson 1988), many business strategy theorists have questioned the focus on external factors of industry-based theory. In particular, those labeled "resource-based" theorists argue for the primacy of heterogeneous and imperfectly mobile resources.

Resource-based theory in business strategy, an "internal factors" approach, traces to the long-neglected work of Edith Penrose (1959). Avoiding the term "factor of production" because of its ambiguity, she viewed the firm as a "collection of productive resources" and pointed out: "it is never *resources* themselves that are the 'inputs' to the production process, but only the *services* that the resources can render" (pp. 24–25; italics in original). Viewing resources as bundles of possible services that an entity can provide, "It is the heterogeneity . . . of the productive services available or potentially available from its resources that gives each firm its unique character" (pp. 75, 77). Therefore, contrasted with the neoclassical notion of an *optimum* size of firm, "the expansion of firms is largely based on opportunities to use their existing productive resources more efficiently than they are being used" (p. 88).

Works drawing on Penrose (1959) to explicate the resource-based approach to business strategy include the seminal articles of Lippman and Rumelt (1982), Rumelt (1984), and Wernerfelt (1984) in the early 1980s, followed by the efforts of Dierickx and Cool (1989), Barney (1991, 1992), and Conner (1991) in the late 1980s and early 1990s. The resource-based theory of strategy maintains that resources (to varying degrees) are both significantly heterogeneous across firms and imperfectly mobile. "Resource heterogeneity" means that each and every firm has an assortment of resources that is at least in some ways unique. "Imperfectly mobile" implies that firm resources, to varying degrees, are not commonly, easily, or readily bought and sold in the marketplace (the neoclassical factor markets). Because of resource heterogeneity, some firms are more profitable than others. Because of resource immobility, resource heterogeneity can persist through time despite attempts by firms to acquire the same resources of particularly successful competitors. Therefore, the fundamental strategic imperative of the resource-based view of strategy

is that, to achieve competitive advantage and, thereby, superior financial performance, firms should seek resources that are valuable, rare, imperfectly mobile, inimitable, and nonsubstitutable.

15.1.3 Competence-Based Strategy

A second "internal factors" theory of business strategy is competence-based theory. The term "distinctive competence" traces to Selznick (1957) and was used by Andrews (1971) and his colleagues in the SWOT model to refer to what an organization could do particularly well, relative to its competitors. Stimulating the development of competence-based theory in the early 1990s were the works of Chandler (1990), Hamel and Prahalad (1989, 1994a, 1994b), Prahalad and Hamel (1990, 1993), Reed and De Fillippi (1990), Lado, Boyd, and Wright (1992), and Teece and Pisano (1994). Numerous other theoretical and empirical articles have been developing competence-based theory (Aaker 1995; Bharadwaj, Varadarajan, and Fahy 1993; Day and Nedungadi 1994; Hamel and Heene 1994; Heene and Sanchez 1997; Sanchez, Heene, and Thomas 1996; and Sanchez and Heene 1997, 2000).

Prahalad and Hamel (1990, p. 81) argue that "the firm" should be viewed as both a collection of products (i.e., strategic business units) and a collection of competences, because "in the long run, competitiveness derives from an ability to build, at lower cost and more speedily than competitors, the core competencies that spawn unanticipated products." For Hamel and Prahalad (1994a), business strategy should focus on industry foresight and competence leveraging. *Industry foresight* involves anticipating the future by asking what new types of benefits firms should provide their customers in the next five to fifteen years and what new competences should be acquired or developed to offer such benefits. *Resource-leveraging* focuses on the numerator in the productivity equation (i.e., the numerator in the value of output/cost of input equation). Specifically, they argue that too much attention in analyses of firm productivity has been devoted to resource efficiency—the denominator—and too little on resource effectiveness—the numerator.

For competence-based theorists, productivity gains and competitive advantage come through the resource-leveraging that results from "more effectively concentrating resources on key strategic goals, . . . more efficiently accumulating resources, . . . complementing resources of one type with those of another to create higher-order value, . . . conserving resources whenever possible, and . . . rapidly recovering resources by minimizing the time between expenditure and payback" (Hamel and Prahalad, 1994a, p. 160). Therefore, the fundamental strategic imperative of the competence-based view of strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should identify, seek, develop, reinforce, maintain, and leverage distinctive competences.

15.1.4 Knowledge-Based Strategy

Knowledge-based strategy maintains that organizational knowledge results in organizational innovativeness, which results in specific innovations that contribute to the long-term

success of firms.[1] Thus, recognizing the importance of innovation, Chandy and Tellis (1998) and Ettlie, Bridges, and O'Keefe (1984) argue for the pursuit of radical, rather than incremental, innovations; Henderson and Clark (1990) show how innovations in product architecture can be powerful, radical innovations; Ettlie (1983) stresses the importance of striving for both product and process innovations; and Han, Kim, and Srivastava (1998) highlight that improved performance is a result of both technical and administrative innovations. Whereas much knowledge-based strategy research has focused on issues related to understanding the *acquisition* of knowledge (Nevis, DiBella, and Gould 1995), very little attention, unfortunately, has been given to factors related to *converting* knowledge to purposeful action, such as developing or adopting particular innovations (Menon and Varadarajan 1992; Moorman 1995).

Prominent forms of knowledge-based strategies that are thought to lead to innovation include organizational learning (Huber 1991; Sinkula 1994) and absorptive capacity (Cohen and Levinthal 1990). *Organizational learning* describes the process by which organizations interact with their external environment to confirm or disconfirm their organizational norms, and, thus, potentially change the range of potential behaviors available to the firm. *Absorptive capacity* defines an organization's ability to evaluate new, external (particularly technical) information, assimilate it, and apply it to commercial ends. Therefore, the fundamental premise of knowledge-based strategy to achieve competitive advantage and, thereby, superior financial performance, is that firms should (1) systematically accumulate a storehouse of knowledge and (2) systematically use that knowledge to guide innovation.

15.2 MARKETING STRATEGY

Marketing strategy, of course, overlaps significantly with business strategy. That is, strategic decisions in the areas of product, promotion, distribution, pricing, and the sales force, though significantly developed in marketing, are frequent topics in business strategy. Therefore, this section will focus on four strategies that are, to a significant extent, distinctively *marketing* in character. As shown in Table 15.2, these strategies are market segmentation, market orientation, relationship marketing, and brand equity.

15.2.1 Market-Segmentation Strategy

Market segmentation, in its tactical sense, often refers to such things as the use of particular statistical techniques for identifying groups of potential customers who have different needs, wants, tastes, and preferences. In contrast, market-segmentation *strategy,* as used here, is a broad concept that refers to the strategic *process* that includes (1) identifying bases for segmentation, (2) using the bases to identify potential market segments, (3) developing combinations (portfolios) of segments that are strategic alternatives, (4) ascertaining the resources necessary for each strategic alternative, (5) assessing existing resources, (6) selecting an alternative that targets a particular market segment or segments, (7) securing the resources necessary for the target(s), (8) adopting positioning plans for

Table 15.2

Theories of Marketing Strategy: Central Premises

To achieve competitive advantage and, thereby, superior financial performance, firms *should* . . .

. . . for market-segmentation strategy:
(1) identify segments of industry demand, and
(2) target specific segments of demand,
(3) develop specific marketing "mixes" for each targeted market segment.

. . . for market-orientation strategy:
(1) systematically gather information on present and potential customers and competitors, and
(2) use such information in a coordinated way to guide strategy recognition, understanding, creation, selection, implementation, and modification.

. . . for relationship-marketing strategy:
(1) identify, develop, and nurture an efficiency-enhancing, effectiveness-enhancing portfolio of relationships.

. . . for brand-equity strategy:
(1) acquire, develop, nurture, and leverage an effectiveness-enhancing portfolio of high-equity brands.

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the market offerings for the segments, and (9) developing marketing mixes appropriate for each segment (Hunt and Arnett 2004a).

All market-segmentation strategies are premised on three basic assumptions. (1) Many markets are significantly, but not completely, heterogeneous regarding consumers' needs, wants, use requirements, tastes, and preferences. Therefore, many heterogeneous markets can be divided into smaller, meaningful, relatively homogeneous segments of consumers. (2) A firm's market offerings (here, including price, promotion, and channels) can often be designed to meet the needs, wants, tastes, and preferences of the relatively homogeneous segments. And (3), for many firms, a strategy of targeting specific segments can lead to competitive advantages in the marketplace and, in turn, superior financial performance.

Market-segmentation strategy is a well-accepted component of marketing strategy (Dibb 1995, 2001). Indeed, market-segmentation strategy is "one of the most widely held theories in strategic marketing" (Piercy and Morgan 1993, p. 123), is "considered one of the fundamental concepts of modern marketing" (Wind 1978, p. 317), is "the key strategic concept in marketing today" (Myers 1996, p. 4), and is one of the basic "building blocks" of marketing (Layton 2002, p. 11). The acceptance of market-segmentation strategy as a key dimension of marketing strategy traces to Chamberlin's (1933/1962) argument that intra-industry heterogeneity of demand is natural, and to Smith's (1956, p. 6) seminal article which argues: "market segmentation may be regarded as a force in the market that will not be denied." The fundamental strategic thesis of market-segmentation strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should (1) identify segments of demand, (2) target specific segments, and (3) develop specific marketing "mixes" for each targeted market segment.

15.2.2 Market-Orientation Strategy

The idea of market orientation traces to the marketing concept, which has been considered a marketing cornerstone since its articulation and development in the 1950s and 1960s. The marketing concept maintains that (a) all areas of the firm should be customer-oriented, (b) all marketing activities should be integrated, and (c) profits, not just sales, should be the firm's objective.

As conventionally interpreted, the concept's customer-orientation component—that is, knowing one's customers and developing products to satisfy their needs, wants, and desires—has been considered paramount. Historically contrasted with the production and sales orientations, the marketing concept is considered to be a philosophy of doing business that should be a major part of a successful firm's culture (Baker, Black, and Hart 1994; Wong and Saunders 1993). For Houston (1986, p. 82), it is the "optimal marketing management philosophy." For Deshpandé and Webster (1989, p. 3), "the marketing concept defines a distinct organizational culture . . . that put[s] the customer in the center of the firm's thinking about strategy and operations."

In the 1990s, the marketing concept morphed into market orientation. In this view, for Webster (1994, pp. 9, 10), even though "the customer must be put on a pedestal, standing above all others in the organization, including the owners and the managers, . . . having a customer orientation . . . is not enough. Market-driven companies also are fully aware of competitors' product offerings and capabilities and how those are viewed by customers." Furthermore, Narver and Slater (1990) and Slater and Narver (1994) characterized a market orientation as having the three components of customer orientation, competitor orientation, and interfunctional coordination. And Kohli and Jaworski (1990, p. 6) defined a market orientation as "the organizationwide *generation* of market intelligence pertaining to current and future customer needs, *dissemination* of the intelligence across departments, and organizationwide *responsiveness* to it" (italics in original). Therefore, the fundamental imperative of market-orientation strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should systematically (1) gather information on present and potential customers and competitors and (2) use such information in a coordinated way across departments to guide strategy recognition, understanding, creation, selection, implementation, and modification (Hunt and Morgan 1995). As conceptualized, readers should note that market-orientation strategy is a *form* of business strategy (i.e., a form of knowledge-based strategy).

15.2.3 Relationship-Marketing Strategy

The strategic area of relationship marketing was first defined by Berry (1983, p. 25) as "attracting, maintaining, and—in multi-service organizations—enhancing customer relationships." Since then, numerous other definitions have been offered. For example, Berry and Parasuraman (1991) propose that "relationship marketing concerns attracting, developing, and retaining customer relationships." Gummesson (1999, p. 1) proposes

that "relationship marketing (RM) is marketing seen as relationships, networks, and interaction." Grönroos (1996, p. 11) states that "relationship marketing is to identify and establish, maintain, and enhance relationships with customers and other stakeholders, at a profit, so that the objectives of all parties involved are met; and that this is done by a mutual exchange and fulfillment of promises." Also for him, relationship marketing is "marketing . . . seen as the management of customer relationships (and of relationships with suppliers, distributors, and other network partners as well as financial institutions and other parties)" (Grönroos 2000, pp. 40–41). Sheth (1994) defines relationship marketing as "the understanding, explanation, and management of the ongoing collaborative business relationship between suppliers and customers." Sheth and Parvatiyar (1995a) view relationship marketing as "attempts to involve and integrate customers, suppliers, and other infrastructural partners into a firm's developmental and marketing activities," and Morgan and Hunt (1994, p. 22) propose that "relationship marketing refers to all marketing activities directed toward establishing, developing, and maintaining successful relational exchanges."

Although the various perspectives on relationship marketing differ, one common element is that all view relationship marketing as implying that, increasingly, firms are competing through developing relatively long-term relationships with such stakeholders as customers, suppliers, employees, and competitors. Consistent with the Nordic School (Grönroos 2000; Grönroos and Gummesson 1985) and the IMP Group (Axelsson and Easton 1992; Ford 1990; Hakansson 1982), the emerging thesis seems to be: to be an effective *competitor* (in the global economy) requires one to be an effective *cooperator* (in some network) (Hunt and Morgan 1994). Indeed, for Sheth and Parvatiyar (1995a), the "purpose of relationship marketing is, therefore, to enhance marketing productivity by achieving efficiency and effectiveness."

It is important to point out that none of the previously cited authors naively maintains that a firm's efficiency and effectiveness are always enhanced by establishing relationships with all potential stakeholders. Advocates of relationship marketing recognize that firms should at times avoid developing certain relationships. As Gummesson observes, "Not all relationships are important to all companies all the time . . . some marketing is best handled as transaction marketing." Indeed, he counsels: "Establish which relationship portfolio is essential to your specific business and make sure it is handled skillfully" (1994, p. 17). Therefore, the fundamental strategic imperative of relationship-marketing strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should identify, develop, and nurture an efficiency-enhancing, effectiveness-enhancing portfolio of relationships.

15.2.4 Brand-Equity Strategy

"A brand is a distinguishing name and/or symbol (such as a logo, trademark or package design) intended to identify the goods or services of either one seller or a group of sellers, and to differentiate those goods or services from competitors" (Aaker 1991, p. 7). Keller (1993, p. 2) defines *customer-based brand equity* as "the differential effect of brand knowledge

on consumer response to the marketing of the brand," and, for him (2002, p. 153), "high brand equity occurs when the consumer has a high level of awareness and familiarity with the brand and holds some strong, favorable, and unique brand associations in memory."

For Aaker (1991, p. 15), "brand equity is a set of brand assets and liabilities linked to a brand, its name or symbol, and that add to or subtract from the value provided by a product or service to a firm and/or to that firm's customers." He groups the assets or liabilities into five categories: (1) brand loyalty, (2) name awareness, (3) perceived quality, (4) brand associations in addition to perceived quality, and (5) other brand assets such as patents, trademarks, and channel relationships. Brand strategies differ in new markets versus established markets (Carpenter and Nakomoto 2005), consumer markets versus business markets (Anderson and Carpenter (2005), and products versus services (Ostrum, Iacobucci, and Morgan 2005).

All advocates of brand-equity strategy maintain that the assets that comprise brand equity are a primary source of competitive advantage. Therefore, the fundamental thesis of brand-equity strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should acquire, develop, nurture, and leverage an effectiveness-enhancing portfolio of brands.

15.3 STRATEGY AND R-A THEORY

This section argues that resource-advantage theory is toward a general theory of marketing because it provides a positive theory that forms a foundation for the normative area of strategy. Each of the eight theories of strategy overviewed in sections 15.1 and 15.2 will be discussed, but in slightly different order. Indeed, the best place to begin is with market segmentation.[2]

15.3.1 Market-Segmentation Strategy and R-A Theory

As discussed, the fundamental strategic thesis of market segmentation is that, to achieve competitive advantage and superior financial performance, firms should (1) identify segments of industry demand, (2) target specific segments of demand, and (3) develop specific marketing "mixes" for each targeted market segment. To theoretically ground market-segmentation strategy, a positive theory of competition must meet three criteria. The theory must: (1) allow for the existence of demand heterogeneity, (2) justify why firms would choose to produce and market a variety of market offerings, and (3) explicate a mechanism by which market segmentation can lead to superior performance.[3]

Addressing criterion one, consider P_1 in Table 13.1: demand is heterogeneous across industries, heterogeneous within industries, and dynamic. "Heterogeneous within industries" implies that demand in the overwhelming majority of industries is *substantially* heterogeneous. Hence, assuming the demand for most market offerings in most industries to be homogeneous is descriptively inaccurate. While demand in a limited number of industries, for example, corn (NAICS #11115), gold ore (#212221), and industrial sand (#212322), is somewhat homogeneous, the majority of industries are more similar to the

"power tool industry" (NAICS #333991), or the "motor vehicle industry" (#236210), or the "book publishing industry" (#511130), where demand is characterized by a vast array of consumer tastes, preferences, and use requirements. As a result, companies in these industries tend to (and *should*) follow segmentation strategies.

Addressing criterion two, R-A theory's acceptance that intra-industry demand is substantially heterogeneous in most industries implies that a firm is confronted with major challenges: "how many market offerings, composed of which attributes, at what attribute levels, targeted at which market segments should it produce?" (Hunt 2000b, p. 54). R-A theory suggests that firms will deal with these challenges in different ways because each firm possesses a set of resources that is in some ways unique. Some firms' resources sets may be more consistent with a strategy of offering limited numbers of market offerings, and, therefore, they will choose to focus on a single market segment (or a few market segments) by producing fairly homogeneous market offerings. Consequently, different firms follow different marketing strategies because each believes that its resource set is better suited for its particular strategy. Therefore, R-A theory's treatment of firm resources provides an explanation for why some firms choose to produce and market numerous different market offerings, while others do not.

Addressing criterion three, consider the concept of market offering. For R-A theory, a market offering is a distinct entity that is (1) comprised of a bundle of attributes, which (2) may be tangible or intangible, objective or subjective, and that (3) may be viewed by some potential buyer(s) as a want satisfier (Hunt 2000b). Most market offerings have blends of tangible (e.g., a power drill's motor and casing) and intangible attributes (e.g., a power drill's warranty and reliability). If tangible attributes predominate, market offerings are referred to as goods; if intangibles predominate, they are services. Attributes are considered to be relatively more objective or subjective depending on the degree of uniformity across buyers as to (1) the importance weights given to different attributes, (2) the extent to which different market offerings have or do not have different attributes, and (3) the extent to which different offerings have different levels of attributes. In all cases, consumer perceptions—that is, subjective factors—are dispositive. The result is that market offerings *perceived* by consumers to be closer to their ideal constellation of attributes are, indeed, more valuable.

Now consider the nature of R-A competition. For R-A theory, as shown in Figures 13.1 and 13.2, competition consists of the constant struggle among firms for comparative advantages in resources that will yield marketplace positions of competitive advantage for some market segment(s) and, thereby, superior financial performance. Therefore, R-A theory views the basic unit of competition as market *segments.* Firms compete with each other on a segment-by-segment basis, rather than on an industry-wide basis. Market segmentation provides a mechanism by which firms can more effectively and/or efficiently use their resources (Mahajan and Jain 1978; Rangan, Moriarty, and Swartz, 1992). As Piercy and Morgan (1993, p. 124) maintain, "the logic of market segmentation suggests that designing marketing strategies around target segments allows a closer alignment between customer needs and the organization's marketplace offering, leading to increased customer satisfaction and loyalty and to building a stronger and more durable competitive position." R-A theory maintains that firms that are successful in developing market of-

ferings that provide more value to consumers in specific market segments and/or provide market offerings at a lower cost (relative to their competitors) will occupy marketplace positions of competitive advantage. In turn, positions of competitive advantage lead to superior financial performance.

Consider the competitive matrix for segment A in Figure 13.2. A firm will have a marketplace advantage in this segment if it can produce: (1) a market offering perceived as having superior value compared to rivals' marketing offerings at a lower cost than rivals (cell 3A), (2) a market offering perceived as having superior value compared to rivals' marketing offerings at the same cost as rivals (cell 6A), or (3) a market offering perceived as having value equal to rivals' marketing offerings at a lower cost than rivals (cell 2A). These positions of competitive advantage (cell 3A, cell 6A, and cell 2A) lead to superior financial performance.

In contrast, firms that fail to develop market offerings that have value for some market segment and/or do not have resource costs that are below those of rivals will occupy marketplace positions of competitive disadvantage. Specifically, a firm will have a disadvantage in a given segment if it produces (1) a market offering perceived as having lower value compared to rivals' market offerings at the same cost as rivals (cell 4A), (2) a market offering perceived as having lower value compared to rivals' market offerings at a higher cost than rivals (cell 7A), or (3) a market offering perceived as having value equal to rivals' marketing offerings at a higher cost than rivals (cell 8A). Positions of competitive disadvantage lead to inferior financial performance.

Two additional competitive positions are possible. Firms can produce: (1) a market offering perceived as having lower value compared to rivals' market offerings at a lower cost than rivals (cell 1A) or (2) a market offering perceived as having superior value compared to rivals' market offerings at a higher cost than rivals (cell 9A). In these two marketplace positions, a firm's financial performance depends on the ratio of resource-produced value (*rpv*) to resource costs (*rc*) (i.e., *rpv*/*rc*) of its market offering, compared with those of rivals. Firms with market offerings that have greater *rpv*/*rc* ratios (compared with rivals) will have superior financial performance. In contrast, firms with market offerings that have smaller *rpv*/*rc* ratios (compared with rivals) will have inferior financial performance. Therefore, R-A theory shows how market-segmentation strategies impact firm performance.

In summary, R-A theory permits the success of market-segmentation strategy. (1) The theory's foundational premises view intra-industry demand as inherently heterogeneous. (2) The theory, by means of its treatment of resources, accounts for the behaviors of firms that often choose to produce and market a variety of market offerings in the same industry. (3) The theory, by means of the marketplace position matrix, explicates the mechanism by which a market segment strategy can lead to superior financial performance.

15.3.2 Resource-Based Strategy and R-A Theory

As discussed, the fundamental imperative of resource-based strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should seek resources that are valuable, rare, imperfectly mobile, inimitable, and nonsubstitutable.

A positive theory of competition that could ground normative, resource-based strategy must (1) permit such a strategy to be successful and (2) contribute to explaining why and when (i.e., under what circumstances) such a strategy may be successful.

First, R-A theory permits resource-based strategy to be successful because it specifically adopts a resource-based view of the firm. As premise P_7 in Table 13.1 notes, firms are viewed as combiners of heterogeneous and imperfectly mobile resources—which is the fundamental tenet of the "resource-based view" (Conner 1991). Indeed, competition for R-A theory consists of the constant struggle among firms for comparative advantages in such resources.

Note, however, that R-A theory adopts "a" resource-based view of the firm, not "the" view. As discussed by Schulze (1994), many resource-based theorists view competition as an equilibrium-seeking process. Indeed, firms are often described as seeking "abnormal profits" or "economic rents," which in the neoclassical tradition imply "profits different from that of a firm in an industry characterized by perfect competition" and "profits in excess of the minimum necessary to keep a firm in business in long-run competitive equilibrium." Thus, because perfect is posited as ideal, that is, it is *perfect,* when one views competition as equilibrium-seeking and the goal of the firm as *abnormal* profits or *rents,* it is then implied that the achievement of sustained, superior financial performance by firms is detrimental to social welfare.

In contrast, R-A theory maintains that competition is dynamic and disequilibrium-provoking (see premise P_9 in Table 13.1). In a critique of resource-based strategy, Priem and Butler (2001, p. 35) argue for dynamic theory and suggest that in order for the resource-based view "to fulfill its potential in strategic management, its idea must be integrated with an environmental demand model." They point out that R-A theory's incorporation of heterogeneous demand in a dynamic theory is in the right direction. Barney (2001) replied to Priem and Butler by agreeing that a dynamic analysis using the resource-based view of the firm is important for the further development of strategic research, and he cites R-A theory as an example of an evolutionary approach that incorporates the necessary dynamics.

Also in contrast, R-A theory denies that perfect competition is the ideal competitive form. The achievement of superior financial performance—both temporary and sustained—is pro-competitive when it is consistent with and furthers the disequilibrating, ongoing process that consists of the constant struggle among firms for comparative advantages in resources that will yield marketplace positions of competitive advantage and, thereby, superior financial performance. It is anticompetitive when it is inconsistent with and thwarts this process. Therefore, R-A theory maintains that when superior financial performance results from pro-competitive ("pro" in the sense of R-A theory) factors, it contributes to social welfare because the dynamic process of R-A competition furthers productivity and economic growth through both the efficient allocation of scarce tangible resources and, more important, the creation of new tangible and intangible resources.

Specifically, the ongoing quest for superior financial performance, coupled with the fact that all firms cannot be simultaneously superior, implies not only that the process of R-A competition will allocate resources in an efficient manner, but also that both proactive and reactive innovations will be developed and will contribute to further increases in efficiency and effectiveness. Indeed, it is the process of R-A competition that provides an important mechanism for firms to learn how efficient-effective, inefficient-ineffective,

they are. (See the learning, feedback loops in Figure 13.1.) Similarly, it the quest for superior performance by firms that results in the proactive and reactive innovations that, in turn, promote the very increases in firm productivity that constitute the technological progress that results in economic growth.

As to why and when a strategy of seeking resources that are "valuable, rare, imperfectly mobile, inimitable, and nonsubstitutable" will be successful, consider the "valuable" criterion. An entity may be valuable in many ways. For example, a firm's assets may include a section of land, or a building, or a painting that has value in the marketplace (and appears in the firm's balance sheet). But what R-A theory highlights is that *marketplace* value is not the key for understanding the nature of competition. Rather, a resource is *valuable* when it contributes to a firm's ability to efficiently and/or effectively produce a marketplace offering that *has value* for some market segment or segments. And, R-A theory maintains, consumer perceptions of value are dispositive. That is, consumer perceptions are the ultimate authority as to the value of a firm's market offering.

Now consider the recommendation that valuable resources should be *rare.* Entities may be "rare" in many ways. What R-A theory highlights and emphasizes is that a valuable, "rare" resource is one that enables a firm, when competing for a market segment's patronage, to move upward and/or to the right in the marketplace position matrix (Figure 13.2). That is, valuable, *rare* resources enable firms to compete by being, relative to competitors, more efficient and/or more effective.

Now, in light of R-A theory's emphasis on proactive and reactive innovation, consider the recommendation that resources should be "inimitable and nonsubstitutable." To the list, R-A theory adds "nonsurpassable" (Hunt 1999). Firms occupying positions of competitive disadvantage (cells 4, 7, and 8 in Figure 13.2) will be motivated to engage in three forms of reactive innovation: (1) imitating the resource of an advantaged competitor, (2) finding (creating) an equivalent resource, or (3) finding (creating) a superior resource. Many authors have tended to focus on the equilibrating behavior of resource imitation and substitution. Although imitation and substitution—as emphasized by D-A theory, see section 14.2—are important forms of competitive actions, R-A theory highlights the fact that reactive innovation can also prompt disequilibrium-provoking behaviors. That is, reactive innovation in the form of finding (creating) a *superior* resource results in the innovating firm's new resource assortment enabling it to *surpass* the previously advantaged competitor in terms of either relative efficiency, or relative value, or both. By leapfrogging competitors, firms realize their objective of *superior* returns, make competition dynamic, shape their environments, and renew society. In so doing, the process of reactive innovation stimulates the kinds of major innovations described as creative destruction by Schumpeter (1950). Imitation brings parity returns; parity returns are never enough.

15.3.3 Competence-Based Strategy and R-A Theory

The fundamental imperative of competence-based strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should identify, seek, develop, reinforce, maintain, and leverage distinctive competences. Organizational com-

petences, all strategy theorists agree, have components that are significantly intangible (e.g., knowledge and skills) and are not *owned* by the firm (i.e., not capable of being *sold* by the firm, except, of course, by selling the division of the firm that houses the competence). Recall that R-A theory acknowledges that both tangible and intangible entities can be resources. Recall also that entities need not be owned by firms to be resources. Rather they need only be *available* to firms.

Premise P_6 in Table 13.1 classifies firm resources as financial, physical, legal, human, organizational, informational, and relational. For R-A theory, therefore, a firm competence is a kind of *organizational* resource. Specifically, competences are "higher order" resources that are defined as socially and/or technologically complex, interconnected, combinations of tangible basic resources (e.g., basic machinery) and intangible basic resources (e.g., specific organizational policies and procedures and the skills and knowledge of specific employees) that fit coherently together in a synergistic manner. Competences are distinct resources because they exist as distinct packages of basic resources. Because competences are causally ambiguous, tacit, complex, and highly interconnected, they are likely to be significantly heterogeneous and asymmetrically distributed across firms in the same industry. Therefore, R-A theory permits competence-based strategy to be successful.

Differences in specific competences explain why some firms are simply better than others at *doing* things (Hamel and Heene 1994; Heene and Sanchez 1997; Langlois and Robertson 1995; Sanchez and Heene 1997; Sanchez, Heene, and Thomas 1996). For example, firms can have superior entrepreneurial competences (Foss 1993), research and development competences (Roehl 1996), production competences (Prahalad and Hamel 1990), marketing competences (Conant, Mokwa, and Varadarajan 1990; Day 1992), and competitive agility competences (Nayyan and Bantel 1994).

Highlighted by R-A theory is the role of *renewal* competences, such as those described by Teece and Pisano (1994) and Teece, Pisano, and Shuen (1997) as "dynamic capabilities," by Dickson (1996) as "learning how to learn," and by Hamel and Prahalad (1994a, 1994b) as "industry foresight." Specifically, renewal competences prompt proactive innovation by enabling firms to (1) anticipate potential market segments (unmet, changing, and/or new needs, wants, and desires), (2) envision market offerings that might be attractive to such segments, and (3) foresee the need to acquire, develop, or create the required resources, including competences, to produce the envisioned market offerings. Therefore, because firms are not viewed by R-A theory as just passively responding to changing environment or looking for the best "fit" between existing resources and market "niches," it contributes to explaining why and when a firm developing a renewal competence will be successful. A strategy of developing a renewal competence will be successful (or more successful) when (1) the marketplace is turbulent, (2) competitors are "sleepy," and/or (3) the proactive innovations spawned by a renewal competence promote turbulence.

15.3.4 Industry-Based Strategy and R-A Theory

The fundamental imperative of industry-based strategy is that, to achieve competitive advantage and, therefore, superior financial performance, firms should (1) choose industries

and/or modify their structure, (2) select one of three generic strategies, and (3) manage well the activities in its value chain. Of course, as discussed, R-A theory rejects the notion that "choosing industry" is the key factor for strategy success. Indeed, empirical works on financial performance show clearly that "firm effects" dominate "industry effects" and competition is market segment by market segment. However, R-A theory does contribute to understanding industry-based strategy by showing when a strategy of expanding the firm's offerings to new segments in (1) the same industry or (2) a new industry will be successful. Such a strategy is more likely to be successful when the resources that the firm has (or can reasonably acquire or develop) are believed to be such that they enable it to produce a market offering that will occupy cells 2, 3, or 6 in Figure 13.2. That is, R-A theory highlights the role of resources in implementing a segment-based variant of industry-based strategy.

R-A theory also addresses the issue of the propriety of the recommendation that firm strategy should be directed at *altering* industry structure. As Fried and Oviatt (1989) point out, the "alter structure" recommendation is often (if not *most* often) interpreted as taking actions that will (1) drive competitors out of the marketplace in order to (2) increase industry concentration and, thereby, (3) achieve superior financial performance. Therefore, the "alter structure" recommendation is customarily interpreted as advocating predatory practices—in potential violation of antitrust law.

As shown in Figure 13.1, R-A theory views competition as "embedded" (Granovetter, 1985) within, for example, societal institutions and public policy. It is true that firms are often harmed by the actions of competitors. For example, if a firm introduces a new product at competitive prices that performs better than its rivals, then rival firms' sales and profits will likely be affected. However, R-A theory maintains that the harm to competitors is, or ought to be, a by-product of the process of competition, not the focus of competitors' actions (Arnett and Hunt 2002). The goal of R-A competition is superior financial performance, not harming competitors. Because the goal can be achieved through competing for comparative advantages in resources, success neither implies nor depends on violating norms of public policy.

Finally, consider the recommendation of industry-based strategy that firms should perform well those activities in their value chains. Unfortunately, the value *chain* metaphor has limited applicability beyond manufacturing firms. Service firms and knowledge-based firms are poorly represented by linear, input-output chains of activities. However, though R-A theory minimizes the role of value chains, it highlights the importance of value creation as a key component of strategy. Indeed, value creation is central to Figure 13.2, the marketplace position matrix. Furthermore, R-A theory provides an explanation for the claim that some firms are superior to others in performing value-creation activities: superior-performing firms in terms of value creation have a comparative advantage in resources, for example, specific competences related to specific value-producing activities.

15.3.5 Market-Orientation, Knowledge-Based Strategy, and R-A Theory

As previously pointed out, market-orientation strategy is a form of knowledge-based strategy. Given that the arguments are the same for both, our analysis will focus on market-orientation strategy.

The fundamental thesis of market-orientation (MO) strategy is that, to achieve competitive advantage and superior financial performance, firms should systematically (1) gather information on present and potential customers and competitors and (2) use such information in a coordinated way to guide strategy recognition, understanding, creation, selection, implementation, and modification. R-A theory permits MO strategy to succeed because premise P_5 in Table 13.1 assumes that the firm's information is imperfect, and premise P_6 indicates that information can be a resource. That is, the (1) systematic acquisition of information about present and potential customers and competitors and the (2) coordinated use of such information to guide strategy may contribute to the firm's ability to produce efficiently and/or effectively market offerings that have value for some market segments.

If a firm is market oriented and its competitors are not, then an MO strategy may be a resource that moves the firm's marketplace position upward and to the right in Figure 13.2. Note, however, that premise P_5 in Table 13.1 also points out that information acquisition is costly. The implication is that if implementing an MO strategy is *too* costly, then the firm's position in Figure 13.2 will shift downward toward positions of competitive disadvantage. Therefore, whether an MO strategy provides a resource that leads to a position of competitive advantage in Figure 13.2 depends on the relative value/relative cost ratio of MO implementation.

Because it consists of a synergistic combination of more basic resources (Hunt and Lambe 2000), the effective implementation of a market orientation may be viewed as an organizational competence. To implement an MO strategy, firms deploy tangible resources, such as information systems to store, analyze, and disseminate information about competitors and customers. In addition, firms use intangible resources to implement MO. That is, organizational policies must be in place to encourage MO action, and managers must have the knowledge and experience required to utilize customer and competitor information effectively.

Specifically, a market orientation may be viewed as an example of a renewal competence, as discussed in section 15.5.3. That is, a competence in MO will prompt proactive innovation by enabling firms to anticipate potential market segments, envision market offerings that might be attractive to such segments, and prompt the need to acquire, develop, or create the required resources to produce the offerings. Furthermore, a competence in MO will assist efforts at reactive innovation because it provides valuable information about existing competitors and customers.

15.3.6 Relationship-Marketing Strategy and R-A Theory

The fundamental thesis of relationship-marketing strategy is that, to achieve competitive advantage, and, thereby, superior financial performance, firms should identify, develop, and nurture a relationship portfolio. Consider what is required for a theory of competition to permit a relationship-marketing strategy to succeed. First, because relationships are intangible, the theory must permit intangibles to be resources. Second, because relationships are not owned (and, therefore, firms cannot buy and sell relationships in the "fac-

tor" markets), firm ownership must not be a criterion for an entity to be a firm resource. Third, because each relationship has unique characteristics (and, therefore, one cannot take the first derivative of any equation in which a relationship appears), unique entities must be allowed. Fourth, because (at least some) relationships involve cooperation among firms in order for them to compete, the theory must permit some relationships to be pro-competitive (and not presumptively assume all instances of cooperation to be anticompetitive *collusion*).

Now consider R-A theory with regard to its view of resources. A firm resource is any tangible or *intangible* entity *available* to the firm that enables it to produce efficiently and/or effectively a market offering that has value for some market segment(s). Therefore R-A theory satisfies criteria (1) and (2). Now recall that R-A theory views firm resources as significantly heterogeneous (premise P_7 in Table 13.1). Therefore, it satisfies criterion (3). Finally, because R-A theory assumes that (at least some) firm resources are imperfectly mobile (premise P_7), yet such resources can nonetheless enable firms to produce offerings efficiently and/or effectively, the theory satisfies criterion (4). That is, at least some cooperative relationships are *relational* resources (premise P_6), making them pro-competitive.

As discussed in Hunt (1997a), R-A theory implies that a firm should periodically conduct a strategic resource audit as a standard part of its corporate planning. The strategic resource audit should pay close attention to the competences of the organization and the role that relationships with suppliers, customers, employees, and competitors can play in enhancing the total "mix" of strategic competences. From the perspective of relationship marketing, therefore, firms should develop a relationship portfolio or mix that complements existing competences (as discussed in section 15.5.3) and enables it to occupy positions of competitive advantage, as identified in Figure 13.2. However, it is important to recognize that relationship portfolios are *developed*, not *selected.*

Because it conjures the image of being like a portfolio of stocks, Gummesson's (1999) concept of a relationship portfolio has the same systemic ambiguity as the marketing mix. The standard textbook versions of the marketing-mix concept often imply that some marketing manager sits down at a specific point in time and *selects* both a target market and a particular combination of price, product, place, and promotion that is believed to be optimal. Although this may occur on rare occasions, much more commonly these decisions are made sequentially, that is, over time. Therefore, it could well be the case that the first decision actually made was the nature of the product. Then a market segment is targeted for the product. Following that, the price, channels of distribution, and promotional programs are developed. The point is that, in contrast to standard textbook treatments, marketing mixes are most often developed over time, not selected at a point in time.

A similar ambiguity emerges in the concept of a relationship portfolio. Even more so than the marketing mix, relationship portfolios are not selected at a point in time, but developed *over* time. Indeed, good relationships take time to develop (Lambe, Spekman, and Hunt 2002). Therefore, though it is important to develop a relationship portfolio that complements existing organizational competences in an optimal manner, and it is important to strategically plan for such relationships, the relationships that compose the relationship

portfolio can be developed only over time. Though both are *portfolios,* the relationship portfolio differs dramatically from a portfolio of stocks, for it is at least possible to select a portfolio of stocks at a single point in time. Consequently, a relationship-marketing strategy will be more successful when it is a long-term strategy.

15.3.7 Brand-Equity Strategy and R-A Theory

The fundamental thesis of brand-equity strategy is that, to achieve competitive advantage and, thereby, superior financial performance, firms should acquire, develop, nurture, and leverage an effectiveness-enhancing portfolio of brands. Readers should note that brands (trademarks) can be resources under R-A theory, but only if they contribute to the firm's ability to efficiently and/or effectively produce a market offering that has value to some market segment(s). That is, the brand must *add value* to the market offering in the eyes of the market segment(s). What, then, for R-A theory, is a "high equity" brand? A high-equity brand is one that, by triggering highly favorable associations among targeted consumers, adds such value to the market offering that the resulting increase in firm effectiveness moves the market offering to the right in the marketplace position matrix (see Figure 13.2). Some brands, of course, actually *reduce* the value of the offering, as when, for example, consumers associate the brand with shoddy merchandise. In such circumstances, a brand would be characterized by R-A theory as a "contra-resource" (Hunt and Morgan 1995).

As to R-A theory's resource categories, a brand may be considered to be both a relational and legal resource. It is a *relational* resource because brand equity is a manifestation of a firm's relationship with consumers. It is a *legal* resource because trademark law prevents competitors from stealing the value of a firm's investment in developing a brand's equity. Hence, R-A theory helps inform students' appreciation of brand-equity strategy by showing that brands can be relational, legal resources that can trigger highly favorable associations among targeted customers.

15.4 BRAND-EQUITY STRATEGY AND SOCIETY

Brand-equity strategies are prominent in marketing. The preceding section showed how R-A theory can ground, provide a foundation for, managers' use of brand-equity strategies. However, does their use benefit or harm society? Many different groups argue that managers' use of brand-equity strategies is antisocial. The purpose of this section is to review the various indictments of brand-equity strategies and argue, to the contrary, that marketing's use of brand-equity strategies benefits society. We begin our analysis with a brief review of the indictment of branding.[4]

15.4.1 The Indictment of Branding

The indictment of brand marketing comes from many quarters. Here, I briefly review the attacks from antiglobalization activists, marketing academics, and equilibrium-oriented economists.

Antiglobalization Activists

Attacks on branding by antiglobalization activists have been greatly influenced by the book, *No Logo,* by Canadian journalist and social activist Naomi Klein (2000). (An Internet search for "No Logo" will yield hundreds of thousands of hits.) Klein's book attacks global brands (especially *American* global brands), and is divided into four sections: No Space, No Choice, No Jobs, and No Logo. The first section documents the pervasiveness of global brands; the second chastises global brands for replacing local alternatives; the third associates global brands with job losses in developed countries; and the fourth sets out an agenda for antiglobalization activists. For Klein, global brands exploit Third World workers (e.g., sweatshops and child labor), increase domestic unemployment, reduce domestic wages, erode workers' rights, censor the media, and debase local cultures by making them more homogeneous. She argues for boycotting global brands, disrupting shareholder meetings, filing lawsuits, and picketing trade conferences. Her hope is that, "as more people discover the brand-name secrets of the global logo, their outrage will fuel the next big political movement, a vast wave of opposition squarely targeting transnational corporations, particularly those with high name recognition" (Klein 2000, p. xviii).

Marketing Academe

In marketing academe, a major attack on brand marketing comes from Johny Johansson (2004a), who inquires: "Is American marketing morally bankrupt?" For him, "the answer is yes." His bankruptcy claim is detailed in his book, *In Your Face* (Johansson 2004b), in which he asks, "What are global marketers doing wrong?" He responds, "The answer seems to lie in their emphasis on global branding" (p. 12). Using Klein's (2000) indictment as a starting point, Johansson links together three movements: (1) antimarketing, (2) antiglobalization, and (3) anti-Americanism. He maintains, "The Americans were the main proponents of war, and they were also the main proponents of globalization. Anti-Americanism and anti-globalization seemed two sides of the same coin, and marketing surely played a common role in both movements" (2004b, p. xviii). Linking anti-Americanism with antiglobalization enables him, he maintains, to explain the fact that 121 of the brands indexed in Klein's *No Logo* were American, and only 19 were European.

Johansson (2004b) views with favor Klein's charges against American brand marketing. He also faults American marketers and what he calls the American government's "Brand America campaign" for arguing their positions with "arrogant zeal" and an "in-your-face attitude" (p. 17). He accuses American marketers of promoting "materialism and superficiality" (p. 39), and he complains that "the rate of technological innovation is so high [in America] that products are obsolete while still perfectly functional" (p. 40). Indeed, "the free market system . . . is out of whack, and our consumer paradise has turned into a quagmire of commercialism, consumption, and materialism" (p. 41). For him, "The problem with these brands is that they encourage an *American lifestyle* based on superficiality and fads, all engineered by profit-seeking marketers. It is this new consumerspace with its in-your-face marketing techniques that threatens engrained ways of life and traditional

culture" (p. 119; italics in original). Although Johansson acknowledges that "there is no gainsaying the statistical fact that the standard of living is higher with free markets" (p. 72), he maintains that American proglobalist writers fail to recognize that "in most other societies, particularly those older than America, . . . economic and social progress is much more of a zero-sum game" (p. 158). That is, for him, in most societies, one group's economic gain is another's loss, one group's progress is another's regress.

For Johansson, "In the race to the bottom [in America], marketing has, not unwittingly, played a major role" (p. 159). The "race to the bottom" in America results from its diversity: "Considering the multiracial, multi-ethnic, and multicultural mix of people inhabiting the U.S., the popular choice of the majority naturally involves a 'lowest common denominator'" (p. 159). Why the "lowest common denominator"? Because, he explains, whereas "advanced and sophisticated expressions or products" can be used in racially and ethnically homogeneous societies, in America, "to appeal to a multicultural and multi-ethnic mass market, simple statements about simple things that all can agree on are needed" (p. 159).[5] He concludes his indictment of American brand marketing by, as he puts it, trying to find grounds to "accentuate the positive" (p. 183). Alas, for him, "I would like to say there are some positive signs [in American marketing], but honestly, I don't see any" (p. 183).

Equilibrium Economics

Attacks on branding are also common in neoclassical economics. The hostility of equilibrium economics to branding stems from its reliance on perfect competition theory and the view that brands (i.e., trademarks) are anticompetitive because they promote product differentiation, which, in turn, promotes market power and monopoly. Chamberlin's (1933/1962) seminal analysis provides the standard view in equilibrium economics. He points out that the legal protection of trademarks fosters product differentiation and, therefore, a situation in which prices are higher (p. 88), quantities produced are lower (p. 88), excess capacity is permanent (p. 109), products are inferior (p. 99), and all factors of production are exploited (p. 183). Because, for him (p. 270), "the protection of trademarks from infringement . . . is the protection of monopoly," he inquires whether there are arguments by which the "monopolies protected by the law of unfair competition and of trademarks may be justified" (p. 271).

As to the rights of producers in their own names, Chamberlin (1933/1962, p. 272) first defines a trademark as "any sign, mark, symbol, word or words which indicate the origin or ownership of an article as distinguished from its quality," and he asks: "where does identification leave off and differentiation begin?" His analysis suggests that trademarks in fact stand not just as devices for "mere identification" but also signal levels of quality. Therefore, as to whether producers have intellectual property rights in their names:

> There seem to be no grounds upon which he [the producer] may justly claim such protection. Given that the consumer is equally satisfied with the goods of two sellers, the entrance of the second into the field [with the first seller's name] must be regarded as the natural flow of capital under competition to check the profits

> of the first and to adjust the supply of the commodity to the demand for it at cost. (Chamberlin 1933/1962, p. 272)

As to the interests of consumers, Chamberlin (1962) evaluates three arguments that might seem to imply that consumers actually benefit from the legal protection of trademarks: (1) trademarks stimulate variety, (2) trademarks protect consumers from deception and fraud, and (3) trademarks encourage producers to maintain the quality of their goods. As to the first, given the tradeoff between more variety and the efficiency of more competition, he argues against trademark protection because "less monopoly would be created" and "useless differentiation would be discouraged" (p. 273). As to the second and third arguments, he maintains that "equally effective" as trademark *protection* "would be a policy of permitting imitation [of a trademark] only if it were perfect, or of defining standards of quality by law" (p. 273). Whereas he believes the former is "condemned by its impracticality," the latter solution "has large possibilities, especially in the case of staples" (p. 273). Chamberlin concludes his evaluation by recommending that, if trademarks warrant legal protection at all, the protection should be limited to five years. Such protection, he argues, would sufficiently prompt innovation and:

> The wastes of advertising . . . would be reduced, for no one could afford to build up goodwill by this means, only to see it vanish through the unimpeded entrance of competitors. There would be more nearly equal returns to all producers and the elimination of sustained monopoly profits. All in all, there would be a closer approach to those beneficent results ordinarily pictured as working themselves out under "free competition." (Chamberlin 1933/1962, p. 274)

Chamberlin's analysis of trademarks graphically illustrates the power of a research tradition to frame both which phenomena are problems and which factors get considered. The fact that consumers use trademarks as heuristics that indicate quality is a problem to be solved because of neoclassical theory's exclusive focus on static-equilibrium efficiency. That is, trademarks are a problem because they contribute to product differentiation, which is itself a problem because of its inconsistency with perfect competition and the welfare implications of static equilibrium. In contrast, because property rights are outside the scope of equilibrium analysis, the moral implications of transgressing the rights that producers have in their names is outside the scope of the analysis and not even considered. Similarly, that "trademarks stimulate variety" can be dismissed with a wave of the hand because the variety so stimulated is probably *useless* differentiation. Furthermore, the goal of government is not to protect property rights, but to increase static efficiency by encouraging the imitation of successful innovators through the use of the coercive power of the state to enforce common quality standards. Such coercion, Chamberlin assures us, will be "equally effective" as the use of trademarks in consumers' search for information.

All research traditions have foundational premises. And these premises, as the neoclassical approach to branding reminds us (or should remind us), *count.*

15.4.2 For Brand-Equity Strategy

As the preceding shows, the attacks on brand marketing come from numerous and influential sources. Given the great emphasis on brand marketing in both practice and academe, one might expect to find vigorous defenses of branding in texts and journals. Such is not the case. Texts on strategic brand management discuss topics such as how to build brand equity, communicate brand attributes, and manage brand portfolios, but they devote almost no space to the role of brands in the economy or the impact of branding on society. Similarly, the academic literature is largely silent on these issues. For example, the widely cited special issue on branding in the *Journal of Marketing Research* in May 1994 contained twelve expositions of branding issues, but not a single article devoted to the role of brands in the economy or society.[6]

As Wilkie (2004) points out, prior to the shift toward marketing management in the 1950s and 1960s, texts and articles in marketing, influenced by the commodity, institutional, and functional approaches to marketing, would devote significant space to analyses of marketing systems and the impact of such systems on society. He is correct that marketing's current texts and scholarship have been remiss in not giving more attention to issues such as the role of brands in marketing systems. Although this brief section cannot address *all* the issues raised by the many critics of brand marketing, what can be offered is a start that focuses on the "anticompetitive" charge leveled at brand marketing in neoclassical economics.

Readers should recall that brands (trademarks) can be resources under R-A theory, but only if they contribute to the firm's ability to efficiently and/or effectively produce a market offering of value to some market segment(s). Also, recall that, for R-A theory, a high-equity brand is one that, by triggering highly favorable associations among targeted consumers, adds such value to the market offering that the resulting increase in firm effectiveness moves the market offering to the right in the marketplace position matrix (Figure 13.2). Some brands, of course, actually *reduce* the value of the offering, and move the brand to the left in the matrix.

The perfect competition theory on which equilibrium economics draws to analyze trademarks assumes that consumers have perfect and costless information about the availability, characteristics, benefits, and prices of all products in the marketplace. In contrast, R-A theory posits that consumers within market segments have imperfect information about goods and services that might match their tastes and preferences (Table 13.1). Furthermore, the costs to consumers in terms of the effort, time, and money in identifying satisfactory goods and services (i.e., consumers' search costs) are often considerable. Consequently, one purpose served by the legal protection of trademarks is the reduction of consumer search costs. Specifically, for R-A theory, trademarks are societal institutions that reduce search costs by signaling the attributes of market offerings.[7]

Recall that, for equilibrium economics, trademarks are a *problem* because they contribute to product differentiation, which is *itself* a problem because of its inconsistency with perfect competition and the welfare implications of static equilibrium. In contrast, the fact that consumers have imperfect information and often use trademarks as heuristics

of quality is not a *problem* for R-A theory. First, because heterogeneous, intra-industry demand and supply are viewed as natural by R-A theory, it is only natural that, facing imperfect information, consumers will often use trademarks as indicators of quality. Second, because a trademark is viewed as intellectual property and fully worthy of legal protection, R-A theory views firms' protecting the equity in their trademarks as providing not only (1) a valuable source of information to consumers, but also (2) a powerful incentive for producers to maintain quality market offerings, and (3) a means by which manufacturers of shoddy, defective, or even dangerous products can be held accountable. Third, because R-A theory rejects static-equilibrium efficiency as the appropriate welfare ideal, the heterogeneity of demand and supply does not pose a problem to be solved but a state of nature—and a desirable one at that. Indeed, R-A theory proposes that the *best* way to view the role of trademarks in market-based economies is that they are quality-control and quality-enhancing institutions. As evidence in favor of R-A theory's view of trademarks, consider the case of trademarks in the Soviet Union.

Trademarks in the Soviet Union

As Goldman (1960) recounts, the Soviet Union in its first few decades treated advertising and trademarks as capitalist institutions that, consistent with equilibrium economics, promoted inefficiency. As one might expect, with Soviet production goals set in quantitative terms, shoddy products proliferated, despite the huge inspection costs brought about by an army of inspectors. By the 1950s, Goldman points out, not only was the Soviet Union finding that advertising was an efficient means to inform consumers about products, but Soviet planners, in a desperate attempt to improve quality, made it obligatory that every plant in the Soviet Union place a "production mark" *(proizvodstennaia marka)* on all output. Goldman quotes a Soviet planner as justifying making trademarks obligatory for all plants: "This makes it easy to establish the actual producer of the product in case it is necessary to call him to account for the poor quality of his goods. For this reason, it is one of the *most effective weapons* in the battle for the quality of products" (p. 399; italics added).

But, Goldman (1960) observed, holding Soviet producers accountable for shoddy quality was not the only benefit of obligatory trademarks. He also noted that a more elaborate and attractive form of mark, a *tovarnyi znak,* while sometimes optional, is obligatory for 170 groups of goods and for all exports. Again, Goldman (1960) quotes a Soviet planner as to the quality-enhancing benefits of the "competition" resulting from mandating the use of trademarks: "Due to its originality, the trademark makes it possible for the consumer to select the good which he likes . . . this forces other firms to undertake measures to improve the quality of their own product in harmony with the demands of the consumer. Thus the trademark promotes the drive for raising the quality of production" (p. 351).

Conclusion on Brand-Equity Strategies and Society

As discussed, the experience of the Soviet Union supports R-A theory's view that consumers' use of trademarks as indicators of quality is not a problem to be solved. Instead,

trademarks are institutions that serve as important quality control and quality-enhancing devices in real economies. How important? So important that command economies *mandated* that firms use trademarks, even in those situations where all plants were supposed to produce *homogeneous* commodities. In short, trademarks and product differentiation are not problems for society to solve; they are institutions that solve societal problems.

The preceding analysis supports the reforms suggested by Wilkie (2004) and Vargo and Lusch (2004). Marketing should supplement its emphasis on the micronormative (i.e., marketing management) aspects of marketing with more research on macro-positive and macro-normative issues, with the former focusing on marketing systems as they are and the latter focusing on marketing systems as they ought to be, respectively. Using resource-advantage theory as a foundation for understanding dynamic marketing systems, this section has explored the benefits that redound to marketing systems and society when firms implement brand-equity strategies. I have argued that the best way to view the role of brands in market-based economies is that they are highly important quality control and quality-enhancing institutions. Therefore, the implementation of brand-equity strategies provides substantial benefits to market-based economies (as well as major benefits to socialist economies, a counterintuitive finding, to be sure).

15.5 A FINAL NOTE

Recall that the Three Dichotomies Model of marketing (Table 1.1 on pages 12–13 in this volume) maintains that all the problems, issues, theories, and research in marketing fall within eight cells that result from the positive/normative, micro/macro, and profit sector/nonprofit sector dichotomies. Recall also that, starting in the 1950s and 1960s, marketing shifted its emphasis toward the profit-sector, micronormative cell (cell 2). In my view, the shift was definitely warranted. However, an unintended consequence of the shift has been an extreme neglect of the study of marketing systems—to the detriment of marketing practice, marketing academe, and society. Now, many thoughtful marketers believe that a renewed emphasis on the profit sector, macro-positive, and macro-normative dimensions of marketing (cells 3 and 4) is needed. I agree; marketing needs such a shift.

As recounted in Hunt (2006), I was an assistant professor in the late 1960s and early 1970s at the University of Wisconsin, Madison. In perhaps my first year at that great institution, I recall expressing to a senior full professor the view that marketing academe has an important role to play in conducting research on marketing systems and society. His response was that such research was the province of economics. Business professors, he stated emphatically, should focus exclusively on the needs of business managers for better decision-making models. I countered by arguing that business practitioners have needs that extend well beyond the area of decision-making models. He "replied" by walking away. Marketing has paid a steep price for "walking away" from such subjects as marketing systems and society. The best interests of marketing practice, marketing academe, and society are not served by continuing to pay that price.

QUESTIONS FOR ANALYSIS AND DISCUSSION

1. Resource-advantage theory is claimed to be an interdisciplinary theory. In particular, it "draws on" and "has affinities with" numerous other theories and research traditions. What is the difference between *draws on* and *has affinities with*? Is this distinction important? If so, why? If not, why not?
2. Is it important to have a theory of competition that provides a theoretical grounding for marketing strategy? If so, why? If not, why not?
3. Find a type of marketing strategy other than market segmentation, market orientation, brand equity, and relationship marketing. Would resource-advantage theory provide a positive foundation for the strategy you identify? If so, why? If not, why not?
4. What is the difference, if any, between viewing a market orientation as an aspect of a firm's *culture* and viewing it as a kind of firm *strategy*? Discuss the pros and cons of viewing market orientation as culture versus strategy. Could it be *both* culture and strategy?
5. The purpose of scientific theories is to explain and predict phenomena, thereby contributing to understanding phenomena. What should a theory of competition explain and predict? Why?
6. "Although patents are not legal resources, a trademark and a license to produce a product granted by the patent holder are." Evaluate this position.
7. What is the difference between an *efficiency* advantage and an *effectiveness* advantage? *Ceteris paribus,* which kind of advantage is likely to last longer? Upon which kind of advantage has marketing historically focused? Why? Upon which kind of advantage has neoclassical economics focused? Why?
8. What does it mean to claim that innovation is endogenous to resource-advantage competition, but exogenous to perfect competition? Why is it important to have a theory of competition in which innovation is endogenous? Why is it the case that neoclassical economics cannot adopt the simple expedient of allowing perfect competition to have innovation be endogenous?
9. There is a *Journal of Market Segmentation,* but there is no *Journal of Product Differentiation.* Why?
10. How does the marketing concept differ from a firm's being market oriented?
11. If there ever is another edition of *Marketing Theory,* what new issues should be addressed? What issues should be deemphasized? All comments are welcome, in writing, e-mail, or otherwise.

NOTES

1. I thank Robert McDonald (Texas Tech University) for his helpful suggestions on this section.

2. See Hunt and Arnett (2004a, 2004b) for a more complete discussion of how R-A theory grounds market-segmentation strategy.

3. This section draws extensively on Hunt and Arnett (2004a).

4. This section draws extensively on Hunt (2006).

5. A reviewer of a draft of this work found Johansson's argument here to be offensive, if not insulting. He commented that many readers might be outraged, and he wondered if I perhaps had quoted Johansson inaccurately or out of context. As a check, I showed the quoted material to colleagues familiar with the source. They reread the section in the book in question and affirmed that the quotes are accurate and in context. That is, the quoted material accurately describes Johansson's argument: the "race to the bottom" in America stems from its diversity.

6. The excellent article by Low and Fullerton (1994) on the history of brands, brand management, and the brand manager system comes closest. However, the article does not position these topics within the overall marketing system or evaluate the impact of brands on the economy or society.

7. See Erdem and Swait (1998) for an informative discussion of brand equity as a signaling phenomenon.

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ABOUT THE AUTHOR

SHELBY D. HUNT is the Jerry S. Rawls and P.W. Horn Professor of Marketing at Texas Tech University, Lubbock, Texas. A past editor of the *Journal of Marketing* (1985–87), he is the author of numerous books, including *Foundations of Marketing Theory: Toward a General Theory of Marketing* (M.E. Sharpe, 2002), *Controversy in Marketing Theory: For Reason, Realism, Truth, and Objectivity* (M.E. Sharpe, 2003), and *A General Theory of Competition: Resources, Competences, Productivity, Economic Growth* (Sage Publications, 2000). One of the 250 most frequently cited researchers in economics and business (Thomson-ISI), he has written numerous articles on competitive theory, strategy, macromarketing, ethics, relationship marketing, channels of distribution, philosophy of science, and marketing theory. Three of his *Journal of Marketing* articles, "The Nature and Scope of Marketing" (1976), "General Theories and Fundamental Explananda of Marketing" (1983), and, with Robert M. Morgan, "The Comparative Advantage Theory of Competition" (1995), won the Harold H. Maynard Award for the "best article on marketing theory." The "Comparative Advantage Theory" article also won the 2004 Sheth Foundation/*Journal of Marketing* award for its "long term contributions to the field of marketing." His 1985 *Journal of Business Research* article with Lawrence B. Chonko, "Ethics and Marketing Management," received the 2000 Elsevier Science Exceptional Quality and High Scholarly Impact award. His 1989 article, "Reification and Realism in Marketing: In Defense of Reason," won the *Journal of Macromarketing* Charles C. Slater Award. His 1994, "Commitment and Trust," *Journal of Marketing* article, with Robert M. Morgan, was the most highly cited article in economics and business in the 1993–2003 decade (Thomson-ISI). For his contributions to theory and science in marketing, he received the 1986 Paul D. Converse Award from the American Marketing Association, the 1987 Outstanding Marketing Educator Award from the Academy of Marketing Science, the 1992 American Marketing Association/Richard D. Irwin Distinguished Marketing Educator Award, and the 2002 Society for Marketing Advances/Elsevier Science Distinguished Scholar Award.