The Role of Mathematical Models in the Study of Product Development

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Reengineering and reorganizing new product processes and structures is an unending endeavor. . . .

Robert G. Cooper and Elko J. Kleinschmidt (1995), "Benchmarking the Firm's Critical Success Factors in New Product Development"

The world can doubtless never be well known by theory: practice is absolutely necessary; but surely it is of great use to a young man, before he sets out for that country, full of mazes, windings, and turnings, to have at least a general map of it, made by some experienced traveler.

Lord Chesterfield (1749), *The Letters of the Earl of Chesterfield to His Son*

A junior faculty member came to me seeking advice on how to earn tenure. He had gone to the formal modelers who suggested that he collect some data, run a few regressions, and knock out a few empirical papers. Then he would have breathing room for the (clearly) more difficult theoretical papers. The empiricists also gave him excellent career advice. They suggested he write down a few equations, take some derivatives, and publish a few quick theoretical papers. That would give him the breathing room to do the (clearly) more difficult empirical papers. They are both right and they are both wrong.

Personally I was never able to set forth a theory without spending time in the field. It's amazing how much insight one can obtain from a manager who is facing a difficult (and scientifically interesting) problem. Nor was I ever able to make sense of field observations without spending considerable time developing an underlying theory to explain both the expected and the unexpected results. All too often the field observations gave anomalous results that challenged many an a priori expectation. Only after many false starts did theories crystallize and obvious answers become obvious.

I have been given the opportunity today to reflect upon my attempts to study product development. I have chosen to begin this paper with two quotes. Because the Converse announcement cites the work that I have done with Glen Urban on new product development, I have chosen the first quote to epitomize the challenge and excitement of product development. We have made progress, but the road is never ending. Perhaps the future will be one of continuing improvement, but I am hopeful someone will use globalization, information ubiquity, or today's astounding computer power to effect a paradigm shift in the

way we develop new products. The second quote illustrates the interplay of experience and conceptual models. Neither approach is effective without the other.

I have chosen to focus this essay on the second theme rather than the first. I need not convince you of the importance of product development. We all accept that it is critical to growth and profitability. Nor do I need to convince you of the challenges that remain in the study of product development. They are many and varied. On the other hand it is rare that I am given the opportunity to muse upon the methods by which I study product development. I take that opportunity here.

This essay is neither prescriptive nor evangelical. I describe here only what has worked for me. I have found eclecticism productive, but I am happy to acknowledge that the concentration of effort is, for some, a more effective strategy.

Problem-Driven Theory and Theory-Driven Solutions

Experience alone, without theory, teaches management nothing about what to do to improve quality and competitive position, nor how to do it.

W. Edwards Deming (1982), *Out of the Crisis*

This project began with a simple question.

Robert Axelrod (1984), *The Evolution of Cooperation*

I have read many essays by marketing scholars. Some argue that marketing is a science-, others that it is an application of other social sciences. Some say simply that "we solve problems." For example, Bob Klein of Applied Marketing Science, Inc. sees his company's core competence as using marketing science to sell "solved problems." Gary Lilien of Pennsylvania State University has coined the term "marketing engineering" to reflect the use of marketing science to solve real problems. My own approach has been one of engineering science—the study of phenomena and methods that enable us to solve relevant problems.

In 1984 Robert Axelrod published his influential book on the evolution of cooperation. This text, and a paper with William Hamilton, introduced a new paradigm of thought that has influenced scientists in fields as diverse as biology, political science, economics, and marketing. Prof. Axelrod began with a simple question drawn from his experience in political science. "When should people cooperate?" He asked scientists in a variety of fields to submit their solutions and played them one against the other in a simple tournament. Surprisingly, strategies that resulted from very sophisticated (but not empirically-driven) theory were beaten by simple strategies drawn from experience. Had he simply described the outcomes, the tournament would have had little impact. However, faced with unexpected results, Axelrod reinterpreted game theory and proposed that we examine properties of strategies rather than strategies and examine only those properties that have survived evolution. He then completed

the loop and used the new theory to re-examine both social and natural phenomena. Even the influential ethologist, Richard

Dawkins, acknowledges the impact of Axelrod's work. Axelrod succeeded because his theory was problem-driven and, subsequently, his solutions were theory-driven.

Personally, I have found it much easier to formulate theories if I understand the problem. My work on defensive strategies with Steve Shugan (1983) was driven by the observation that, in the late 1970s, new-product pretest market models such as Assessor were used more often by incumbents than by the pioneers. Steve and I spent many an hour trying to understand how incumbents used this information, what new information they needed, and how we might collect that information. The paper as published contains no empirical data, but it was the result of field experience.

Subsequently the theory led to an engineering model (with Steve Gaskin's help, 1984). The model enhanced the effectiveness of pretest market models and led to valuable managerial insights. The application was made possible by the theory.

Many papers on defensive strategy have been written since. Some have confirmed our initial model, some of have extended it, and some have challenged it. In parallel the empirical applications have strengthened the model. The model has been "matricized" and "logitized" to account for the heterogeneity of consumer perceptions; practitioners have added brand-specific constants to account for inertia and unmeasured variables; and competitive effects have been internalized. Over the last 15 years it has been the interplay of data and theory that has enabled the model to survive.

I can cite many personal examples such as my work with Birger Wernerfelt and Duncan Simester (1994) where we studied customer satisfaction systems at a variety of firms in order to understand why firms would measure customer satisfaction in the first place. The theories in that paper, which drew upon published work in agency theory, led us to a different perspective on the use of customer satisfaction. Another example is a theory of how consumers search for information. This research evolved from an attempt (with Glen Urban, John Roberts, and Bruce Weinberg) to build a prelaunch forecasting system for General Motors.

In each case the theory was driven by the problem and the solution was driven by the theory. It was hard to say where one started and the other ended.

Throughout the history of science there are many great examples of problem-driven theory. For example, Louis Pasteur's was attempting to help French wine growers to keep their wine from souring when he discovered Pasteurization and, subsequently, the germ theory of disease. In turn, the germ theory of disease led to many great advances in medical science. Even the Panama Canal owes its success, in part, to the efforts of Walter Reed to wipe out Yellow Fever among the workers. However, not all great problems lead to productive theory. Sir Isaac, Newton spend considerable effort on alchemy and the transmutation of metals. We have yet to find an economical way to turn lead into gold.

The Revolution Came

In this essay I hope to persuade you that the revolution is coming. It will be resisted, but it will come. My thesis is not normative, but predictive.

John Hauser (1985), "The Coming Revolution in Marketing Theory"

In 1984 the Harvard Business School held a colloquium on the coming impact of the information age. We all made predictions and many of them came or are coming true (for example read Robert Buzzell's opening description of the office of 1995). By drawing an analogy to Kuhn's (1970) history of science, I felt that the explosion of marketing data would lead to the growth of mathematical theory in marketing. I felt that this would change the paradigms in many areas of marketing thought.

In 1996 it is common to see papers using formal mathematical methods to address marketing problems. And, there have been some major successes. I cite here two. There are many others.

In the early 1980s two teams were formulating theories to guide the study of marketing channels—the Carnegie team of Richard Staelin and Timothy McGuire (1982) and the Chicago team of Abel Jeuland and Steven Shugan (1983). At the time there was an extensive literature describing channel behavior, documenting how power and dependency relationships form, and suggesting how one might manage channel conflict. Both teams were aware of this literature. However, each team, in its own way, asked the more fundamental question of whether the structure of the channel was the underlying force that led to conflict. The answer, that we now accept, is "yes, structure is extremely important." Among other things, the Jeuland and Shugan paper highlighted why it is difficult to coordinate a channel and the McGuire and Staelin paper highlighted why the order of decision making is important. Although both groups were influenced by the economic theory of the time, but both groups drew upon their understanding of channel phenomena to develop a marketing theory. These theories, and their subsequent progeny, are now taught routinely in MBA programs and have made it into the standard texts. More importantly they have directed subsequent scientific investigation and have led to real managerial insights. Today's papers use more complicated mathematics to extend the early work, but the ideas began their germination with these papers.

Also in the early 1980s, an MIT team of John Little and Peter Guadagni (1983) were working on a new set of methodologies to describe and predict consumer response to package-good marketing strategies. The explosion in data made possible by coordinated supermarket scanners compelled this development, but Guadagni and Little took an approach that was far from obvious. Rather than continuing the tradition of aggregate models, these authors developed a series of models that were based on the behavior of individual families. In developing their models they made a critical decision that later proved prophetic—in addition to control variables they included a family-specific variable, called "loyalty," which changed over time (non-stationarity) and included

the effects of family differences (heterogeneity) and past purchases (state dependence). The model has held up well. It's been improved with new methods, such as probit analysis, and the effects of non-stationarity, heterogeneity, and state-dependence have been studied with increasingly sophisticated methods. But the basic ideas remain. Now that the models are well-accepted and well-calibrated researchers are able to model the effects of competition (endogeneity) to the extent that they are not confounded by non-stationarity, heterogeneity, and state-dependence. The most promising approach is by a team of Northwestern University researchers (Dipak Jain, Mohanbir Sawhney, and their students) who, with a paradigm shift driven by their application to high-definition television, are combining direct measures of competitive reaction with revealed preference estimates of consumer behavior.

I expect the revolution in theory to continue and that it will be driven by researchers attempting to solve the challenges of complex products, global markets, global supply chains, instantaneous information, abundant information, and electronic markets. However, I do not believe, nor have I ever believed, that theory alone will consummate the revolution.

Why I Both Love and Hate Theory

I find the prospect (of signaling theories) rather worrying, because it means that theories of almost limitless craziness can no longer be ruled out on common sense grounds. If we observe an animal doing something really silly, like standing on its head instead on running away from a lion, it may be doing it in order to show off to a female. It may even be showing off to the lion, 'I am such a high-quality animal you would be wasting your time trying to catch me."

Richard Dawkins (1976), *The Selfish Gene* (from 1989 update notes)

But no matter how crazy I think it something is, natural selection may have other ideas.

Richard Dawkins (1976), *The Selfish Gene* (from 1989 update notes)

Theory is a two-edged sword. On one hand it provides a parsimonious chronicle of observations, a shared language (and values), and tremendous insight into practical problems. On the other hand it is tempting to put too much faith in a theory's assertions even if they conflict with our experience.

We must, at all times, remember that a theory is but a model, an abstraction of the real world. Those who introduced the theory had as their purpose to explain a set of observations that could not be otherwise explained. Or, which could not be explained with the same parsimony. It is likely that they made certain simplifications ignoring some phenomena to concentrate on those that were critical to their needs. They may have made arbitrary decisions (this variable, this function, that measure); there may have been other details that were just as reasonable. A theory is reasonable if it provides insight and fits the data reasonably well. But theory is not gospel.

Dawkins refers to the signaling theories that were developed in the early 1970s by ethologists (e.g., Zahavi 1975) and by economists (e.g., Spence 1973). In each case one party knows something important that the other does not. In Dawkins' case a gazelle knows that it is difficult to catch but the lion does not. The gazelle seemingly puts its life at risk by jumping in front of the lion to demonstrate its strength and stamina. If the lion recognizes the signal, the lion prefers to chase another (weaker) gazelle. Furthermore, because signaling is costly to a gazelle, the equilibrium strategy for all gazelles is to signal honestly. In marketing these concepts have been applied to pricing, promotion, advertising, and other marketing actions. (In fact, Dawkins uses the word "advertising" to describe his gazelles.)

However, the natural selection analogy also provides caution. First, all animals do not signal—there are other evolutionary mechanisms that enhance an animal's survival probabilities. Second, even when signaling might be an explanation, there may be more to the story. Birds cry out to members of their flock that danger is approaching. At first this appears to be a pure signaling model. But the acoustic properties of the alarm calls of birds are such that a predator would have difficulty locating the alarm-giving bird. There are other, better, explanations for bird alarms including the argument that the alarm-giving bird is better off if the flock flies off together (thus reducing the odds of being singled out). See Dawkins (1989, p. 168-171). Third, the effectiveness of the signaling argument (for gazelles) depends upon the strategies that one allows the gazelle to adopt. One must allow "a choice from a continuous range of strategies" (Dawkins 1989, p. 312).

What we can draw from the natural selection analogy is that signaling theories might or might not apply to marketing phenomena. Firms might advertise ("burn money in public") simply as a signal that they have much at stake and it is in their best interests to provide a high quality product. On the other hand, firms might find that advertising makes customers aware of products, communicates information about product attributes, and/or creates a positive image for the brand. Signaling theory provides one possible explanation, but it may not be the only explanation nor the most compelling.

Dawkins' first quote cautions that almost any observation is consistent with a signaling theory. His second quote cautions that we can should not rule out arbitrarily potential signaling explanations. Rather we must re-examine all explanations both from the perspective of common sense and from the consistency of signaling with other facts relevant to the phenomena. Occam's razor is a puissant tool.

Dawkins refers to signaling theories, but the same cautions apply to almost all theories. In the past twenty years I have used or proposed many a theory. I cringe at the thought that these theories would be used without checking their consistency with real phenomena.

My other love-hate relationship with theory concerns the "stylized fact." I have found the stylized fact to be a very powerful mechanism. Stylized facts allow one to abstract the essential features of complex phenomena so that the

phenomena might be modeled. But stylized facts are not true universally, nor do they tell the entire story. As someone once said, "the plural of anecdote is not data." A good theoretician sees one example, abstracts a stylized fact, and produces a model to explain that fact. This is a valuable exercise in hypothesis generation. If the next steps include testing the universality of the stylized fact and testing the completeness of the explanation, then I am comfortable. But, alas, I have seen many examples where either the stylized fact proves to be a special case or the abstraction misses relevant phenomena. Unfortunately the sociology of the field appears to be such that these stylized-fact papers are quoted as if they were an empirical demonstration of the veracity of the phenomena. The stylized fact and the explanation take on the role of universal truths and become grounds for rejecting any paper that challenges them. My only defense has been to attempt to read the original papers and decide for myself.

In the end theory illuminates empirical research, but early on I found that I could not be an effective researcher if I only developed theories.

Do the Returns from Field Research Justify the Investment?

In confronting the enormous complexity of human behavior, the investigator has two choices. he can severely simplify the phenomena under study and base all his conclusions on this simplified model. Or he can attempt to grapple with all the complexities simultaneously, hoping for an inspired solution. Each approach has its limitations, the first one suffering from sterility and the second from hopelessness.

Philip Kotler in the Foreword to Green and Wind (1973) *Multiattribute Decisions in Marketing.*

Every reader in Spaceland will easily understand that my mysterious Guest was speaking the language of truth and even of simplicity. But to me, proficient though I was in Flatland Mathematics, it was by no means a simple matter.

Edwin A. Abbot (1884), *Flatland*

In a recent essay on research traditions in marketing Hermann Simon (1994) of Johnannes Gutenberg University writes "Over the last decade, we have experienced an increasing estrangement of academic research from business practice." In the same collection of essays, Andrew Ehrenberg (1994) of the South Bank Business School in London of writes "Much of the weightier research literature in marketing can be characterized as (theoretical-in-isolation)." He suggests that while the bulk of attention has been focused on theory it has accounted for no more than 20 percent of the successes. He suggests that empirical-then-theoretical research has accounted for 80 percent of the successes. More recently, Patrick Barwise (1995) of the London Business School opines "the field treats hypothetico-deductive research—T before E—as virtually the only true path. This places it at odds with all the natural sciences." Simon, Ehrenberg, and Barwise are but three of the many critics calling for more empirical research.

I agree with the need for empirical research, but I am not so pessimistic as these critics. I feel that there are many excellent empirical researchers in marketing. I have chosen not to provide an enumeration for fear of omission. However, I do note that every one of today's Converse Award winners and discussants has spend substantial time in the field and that every one has made substantial contributions to practice. And, they are certainly not alone.

Theoretical research has its limits. There will always be propositions that are unprovable from a finite set of axioms. Gödel's theorem establishes that this is true even for the axioms of ordinary integer arithmetic. It must certainly be true for the axiomization of complex social systems. In fact, prior to Gödel's theorem "it was tacitly assumed that each sector of mathematical thought could be supplied with a set of axioms sufficient for developing systematically the endless totality of true propositions about the given area of inquiry" (Nagel and Newman 1958). Gödel established that no matter how complex a set of axioms seems to be, one can always establish a proposition that can neither be proved nor disproved by the axioms. Thus, no matter how we struggle to explain marketing phenomena with simple axioms we must always return to the field to observe additional phenomena and, hence, establish new axioms for further work.

For example, many marketing models attempt to model the equilibrium among actions-by the firm, its competitors, and consumers. In most cases more than one equilibrium is possible; sometimes infinitely many. A common approach to equilibrium selection is to establish more and more logical rules that define rationality. Another approach is to study real systems. I suspect that ten years from now the latter will have proven to be the most productive.

Empirical research is productive, but not everyone does empirical research. I certainly do not wish to argue that everyone should do empirical research. Philip Kotler's quote tells us that field research is difficult. The world is a messy place. Managers do not always say what they mean nor do what they say. Managers may choose successful strategies by instinct or by luck. However, they are almost always willing to talk to researchers and they always provide the raw material from which insight might be secured.

Field research is time-consuming. It is easy to make the case for the long-term contribution of empirical research. But how about the short-ten-n value to the researcher who is facing a tenure decision in a few years? Does the investment justify the opportunity cost?

I recall an incident two summers ago. I had just interviewed the Chief Executive Officer at a large research-intensive firm. The purpose of the interview was to determine how he managed R&D. As I left I asked him if there was any one question to which he needed an answer. He said, "How do I protect R&D budgets from my business unit managers?" So I asked him what would happen to the stock price if the business unit managers had their way. He said, "It will go up, of course." I left shaking my head. Didn't he understand the efficient market theory?

It was over a year later before I fully understood his answers and how they relate to the challenges of establishing a credible value for basic research. What

he really was trying to say was that the long-term value of the firm would go down if he cut basic research but he had not yet solved the metrics problems. He needed a measure of research productivity upon which to reward business unit managers so that their incentives for investment in basic research were compatible with the firm. He also needed a measure which would communicate accurately the value of basic research to the stockholders. Without such a measure it was rational for them to be skeptical that the money was well-spent. In many ways his challenges were the similar to those universities face when evaluating faculty research.

This datum is typical. Field research may not provide immediate value and the value may not be for the immediate topic. Field research is, in many ways, cumulative. The best way to reap the value of field research is to maintain a variety of interests and be vigilant to synergies between experiences. For example, when I examine the work of my colleague Abbie Griffin I see the tremendous concurrence between her research on quality function deployment, communication among new product teams, measures of new product effectiveness, cycle time reduction, and improved customer measurement. Each topic has led to insights into other investigative areas (as well as enhanced classroom effectiveness).

In my own career I have found that empirical research has provided a significant return on investment and that the return has fully justified any opportunity cost. But if I were to give one piece of advise to a beginning assistant professor, I would advise him or her to begin field research early so that he or she might reap the cumulative rewards.

The Research Triangle (or Why We Need Both)

. . . factual and theoretical novelty are (closely) intertwined . . . in the sciences fact and theory,
discovery and invention, are not categorically and permanently distinct, ...

Thomas S. Kuhn (1970), *The Structure of Scientific Revolutions*

By performing painstaking technical analyses of the sentences ordinary people accept as part of their mother tongue, Chomsky and other linguists developed theories of the mental grammars underlying people's knowledge of particular languages and of the Universal Grammar underlying the particular grammars.

Steven Pinker (1994), *The Language Instinct: How the Mind Creates Language*

I have argued that theories come from the crucible of empirical experience and that empirical research is improved with theory. I think that this duality generalizes. Certainly, Thomas Kuhn in his history of scientific revolutions believes that they are intertwined. Similarly, Steven Pinker, in his description of the Chomskian breakthroughs, argues that one of the most cited theoretical developments emerged from detailed field observations of real people speaking

living languages.[1]

In the past two years my colleagues and I at the International Center for Research on the Management of Technology have been studying how corporations evaluate and manage their research and development investments (R&D). One simple, but powerful, observation is that R&D is structured into three tiers as illustrated by the conceptual diagram in Figure 1.

Tier 1 is basic research explorations. Activities in tier 1 focus on new science and new technology and are rarely tied directly to market outcomes. At the other end of the spectrum, tier 3 focuses on applied research projects with business units. Research in this tier uses science and technology to solve practical problems and to develop new products. Tier 2 functions as a bridge by selecting and developing research programs that match (or create) core technological competence. The system functions such that tier 2 selects those explorations (theories) that address applied problems and encourages the development of explorations based on the needs of the business units (empirical applications). Thus we see a duality in corporate R&D as well as academic research.

In Figure 1 tier 1 represents the smallest effort while tier 3 represents the largest effort in terms of people and other resources. In university research I suspect that the triangle might be inverted with greatest emphasis on basic research, but I am not sure. (One might also argue that the research university places equal emphasis on basic and applied research because research can only be effective through a combination of rigor and relevance.)

In practice the tiers of R&D are managed and evaluated differently. The value metrics and management issues vary in emphasis depending upon the tier. Florian Zettelmeyer and I (1996) have recently completed a formal paper describing what we have learned by studying the tiers of R&D. In this essay I summarize qualitatively some of the results from that paper and take a leap of faith by attempting to interpret the implications for academic research. I begin with tier 3, applied research.

Tier 3. We found that tier 3 research projects could and should be evaluated by business units. Business units are asked to pay for tier 3 R&D, but subsidies are necessary to align business unit (managers) incentives with those of the firm. Specifically, these subsidies account for time preference, risk preference, and research scope. By time and risk preference we recognize that business unit managers are often more short-term oriented and more risk averse than the firm. By research scope we refer to the phenomenon that most applied projects lead to methods and technologies that benefit many projects in a variety of business units The scope of benefits to the firm is well beyond the benefits to the business unit that funded the project. We also found that firms recognize the option value of research—that is, many subsequent investments are contingent upon the outcomes of initial investments. With tier 3 R&D the firm buys the option to invest further if and only if that further investment is justified. In fact, some firms are considering formal "options" theory.

The analogy for academic research is that we can value some components of applied research by its impact on practice. However, in calculating that value

we must recognize implications beyond the initial applied research. There may be synergies to other applied research projects and/or to new theoretical breakthroughs. In academia we must also provide mechanisms that encourage researchers to take risks and to focus on the long-term. The analogy to a research subsidy might be that we "overvalue" the successful completion of risky, long-term inquiries. Perhaps, like industry, we should recognize that a researcher sometimes succeeds by determining which areas are not worth further investment. Aggregates (the department, the school, the field) should encourage a variety of research projects and recognize that some projects are valuable if only to maintain an option for further investigation.

Tier 2. In tier 2 R&D we found a tension between rewards based on market outcomes and rewards based on effort indicators. To understand this tension, consider how tier 2 performs its functions. R&D managers told us that tier 2 succeeds if it selects the right programs. The amount of effort allocated to the research program was important, but not as important as getting the programs right. Tier 2 would first select a program, second allocate enough effort to determine the magnitude of the program's applicability to the firm, and third undertake research to advance the program.

Because tier 2 managers and researchers select programs before the scope and value are known, there is considerable uncertainty in the choice. (They usually have some idea of the expected benefits, but the variance in benefits is immense.) Because tier 2 makes its program decisions well in advance of tier 3 projects, any difference in time valuation between tier 2 managers and the firm implies a large difference in the valuation of tier 2 projects. If market outcomes (sales, profit, percent of revenue due to new products, customer satisfaction, etc.) weigh heavily in the valuation of tier 2 programs, then risk aversion or short-termism take their toll. Risk aversion and short-termism cause tier 2 managers (and researchers) to reject falsely some programs and to avoid high benefit programs that are long-term and risky. In our paper we illustrate that many programs can fall into these false-rejection and false-selection regions.

To minimize the impact of risk aversion and short-termism the firm would like to avoid an emphasis on market outcomes. However, the firm can not avoid placing some weight on market outcomes because, if there is no weight, then there is little incentive for tier 2 managers to choose high-benefit programs. The net implication appears to be that, to incent the proper choice of research programs, tier 2 research programs should be judged on market outcomes, but the weight on that measure should be small.

But tier 2 does more than just choose research programs. Tier 2 managers and researchers must be given the right incentives to induce them to allocate the right amount of resources to the program. This incentive problem is a standard agency theory problem; the suggested strategy is to weight market outcomes highly. Hence, the tension—the choice of research programs requires a small weight on market outcomes but the allocation of research effort requires a large weight. Corporations finesse this problem by looking for metrics that correlate with research effort, but do not depend heavily on market outcomes. If these

metrics induce less risk for the researchers and can be observed well in advance of market outcomes, so much the better. These metrics are the metrics with which we in academia are well familiar—publications, citations, patents, citations to patents, and peer review. Tier 2 research is judged with a small, but not insignificant, weight on market outcomes and a higher weight on publications, citations, patents, citations to patents, and peer review.

I make the obvious analogy to academic research. Publications, citations, and peer review are not so bad. (Patents are rare in marketing science research.) By evaluating faculty on these metrics we provide incentives to allocate the "optimal" research effort. However, we must also place some weight, albeit a smaller weight, on market outcomes. The recent trend towards placing higher values on teaching performance is just one manifestation of this need for market-outcome metrics. We should consider the relevancy and scope of faculty research. Industry impact should be encouraged and rewarded. I have seen no systematic study of the tenure-review processes at business schools, but the trends at M.I.T. are consistent with these interpretations.

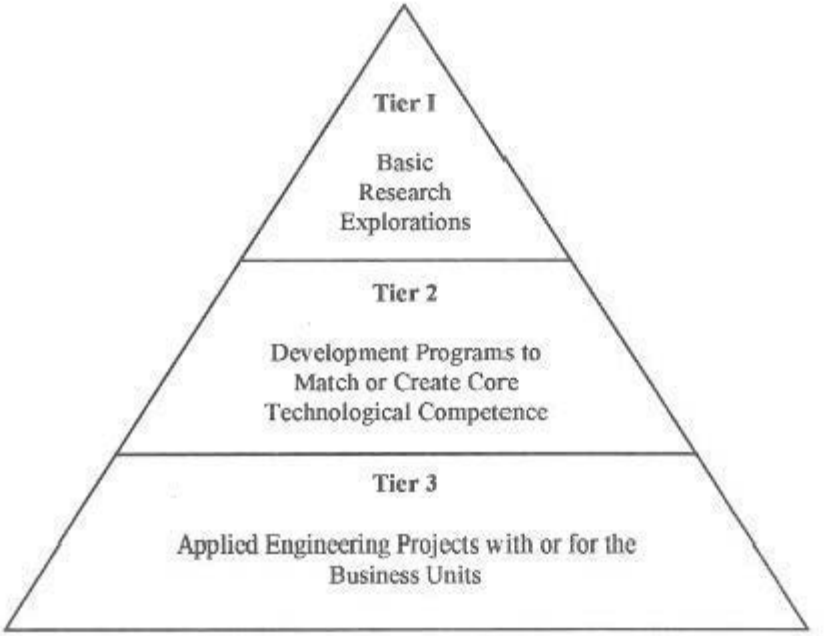
Tier 1. This is the tier that is probably closest to the heart of most faculty researchers. Tier 1 is even further from market outcomes than tier 2, hence publications, citations, and peer review are even more critical. But we can learn two additional lessons from corporate R&D -portfolio management and research spillovers.

Tier 1 is managed for its research portfolio. The value to the firm of a tier I research portfolio is the value of the best outcomes, not the average outcomes. To maximize the maximum value, firms manage their tier 1 portfolio for high variance and for negative correlation among projects. For academic research this implies we should be eclectic in our approaches, take risks, and be tolerant of approaches that are different that the ones we favor. Avoiding false rejection should be a high priority for academic research. A journal can survive false acceptance, but I am not sure the field can survive the false rejection of ideas.

Tier 1 is also managed to take advantage of research spillovers. By a research spillover I mean research that is done at another firm or in another industry which, if recognized by the recipient firm, can solve a critical research problem. Two characteristics of research spillovers are important. First, the impact of research spillovers is significant and, second, the more a firm invests in its own research the better it is able to take advantage of spillovers. While the direct effect of competitive R&D is negative (when competitors spend more they improve their products and this hurts you), the indirect effect through spillovers is positive (when competitors spend more you get more research spillovers). In fact, for large firms Jaffe (1986) suggests that the spillover effect of competitive R&D might actually be larger than the direct competitive effect. Spillovers are also important within a firm because research in one discipline (e.g., biology) provides value to another discipline (e.g., pharmacology). See Henderson and Cockburn (1994).

The importance of research spillovers suggests that firms should encourage tier 1 researchers to take advantage of potential ideas that originate outside the

FIGURE 1
Tiers of R&D



firm. In terms of a reward system this means that tier I should reward researchers both for ideas that they originate and for ideas they bring to the firm from other sources.

However, this, too, provides a tension. Because basic research is so removed from market outcomes it is extremely difficult to evaluate people. Hence, to retain and support proven researchers, many firms attempt to identify the best people and institute "research fellow" systems that are not unlike university tenure systems. It is tempting to identify the "best" people by their original research rather than by spillover identification. We have analyzed this situation with simple agency theory models. Our results suggest that a focus on original research leads directly to (1) "not invented here (NIH)" attitudes, (2) research empires of too many internal projects, and (3) fewer total ideas available to the firm.

Academic tenure does reward past performance and helps to retain and support proven researchers. However, we must be careful that our reward system does not to institute an NIH bias. We should reward and encourage "arbi-

trage" from other fields and from other researchers (with appropriate attribution).[2] We are all better off when we learn from one another.

I am also persuaded by Henderson and Cockburn's research on interdisciplinary spillovers. They suggest that there are economies of scale to concentration (enough critical mass in a discipline) but economies of scope across disciplines. My interpretation is that we benefit from a multiplicity of perspectives and approaches in the marketing sciences. An ideal department should have critical mass in a variety of disciplines and in a variety of application domains.

Emerging Topics in Marketing Science (Product Development)

. . . no final account can be given in the precise logical form of valid mathematical demonstrations.

Ernest Nagel and James R. Newman (1958), *Gödel's Proof*

It is clear that there is no unique method or formula for (the) discovery. . .

Frank M. Bass and Jerry Wind (I 995), in *Marketing Science*

Throughout the past twenty years we have seen tremendous advances in research on product development. Product development is now more efficient and effective. We listen to the customer earlier in the process and we know how to ask the right questions. We analyze the data with powerful methods driven by advances in stochastic models, scaling methods, conjoint analysis, pretest markets, and prelaunch forecasting. We make recommendations based -on optimization methods, (gaming) models of competitive response, and agency theory. We know about quality tools, concurrent engineering, cross functional teams, design for manufacture and assembly, computer-aided design, rapid prototyping, supply chain management, and information acceleration. We have advanced the state-of-the-art in segmentation, differentiation, advertising, and promotion. Fewer products fail, fewer resources are spent on failed products, and successful products are better-designed. As a field we can take pride in these accomplishments.

However, I agree with the opening quote by Cooper and Kleinschmidt that product development is an ongoing challenge. All of the methods that I have mentioned from stochastic models to game theory are now required in most Ph.D. programs and have even made their way into MBA programs. Tomorrow's product-development researchers will have to know all of these methods and know them well. This will be their ticket of entry. There will be many advances in these methods, but I believe that the true paradigm shifts will begin from field-based problems. The best way to identify emerging topics and to define "hot" research areas is to look to practice. We must not rely on our current models (nor treat them as doctrine). Rather we may have to discard our current paradigms and adopt new ones.

I am not so fool hardy as to predict all of the challenges, but I am aware of a few. The area of metrics is clearly important. People respond to what is measured. Product developers are creative people. They respond creatively to metrics and incentive systems. With the right incentive systems they act in the firm's best interests, but the wrong incentive systems lead to counterproductive behavior. Griffin and Page (1995) and Griffin (1995) have demonstrated these phenomena for both product-development success metrics and for product development cycle-time metrics. I hope that I have convinced you that it is true for R&D metrics. However, the study of metrics is more than a simple agency-theory problem. Real product-development teams are complex and multi-faceted, product development is a complex task, and product development takes place in a complex environment. It is difficult to isolate the effect of any one metric or for any one actor and the long-term effects (feedback loops) may differ from the direct effects. Today's agency theory is a powerful paradigm, but we may need a new paradigm to make significant progress. Hopefully, such a complex-team agency theory will emerge.

Design complexity is another important topic. Today's products are complex and growing more complex. The design of the Boeing 777 required 100 million design decisions. Even in an automobile there are over 2-3 kilometers of wiring connecting an extensive network of sensors, switches, motors, and computers.[3] Even seemingly simple products such as kitchen appliances now contain integrated circuits that allow them to react to user needs and to monitor usage (and their own reliability). There are clear challenges in managing use and reuse of parts, the hierarchical structure of teams, the architectures that define product platforms, and many of the other issues driven by complexity. Such themes may seem closer to engineering than marketing, but, in practice, these roles are being merged. Perhaps they should be merged in academia as well.

A third topic is the explosion of information. The Internet is just one demonstration of what is happening as more information is made available to more people. Communication has always proven critical to product development (Allen 1978). Information technology has made it feasible for remote team members to play active roles in cross-functional product-development teams. Technologies make it possible to monitor consumer usage and to communicate more easily with existing consumers. New media enable consumers to obtain data more easily on product performance, availability, and price. Such reduced information-search costs might lead to larger consideration sets which, in turn, will affect competitive structures. Software "agents," or other intermediaries, may emerge to serve consumers and/or manufacturers? This will affect the distribution and supply systems. Even our own education systems will be changed by "distance learning." To participate in these revolutions academia must study and plan for the structural changes induced by the information revolution.

There are other trends, including globalization of competition and demand, cradle-to-grave product planning, the need for environmental planning, virtual prototyping, virtual-customer decision support systems, and the virtual corpo-

ration, but I am confident that we will make progress on them all. I have always been optimistic about the ultimate impact of academic research and I remain so today.

Some Thanks

I would like to close this essay with some thanks to my colleagues throughout the years. I began my academic career as an engineer working on dial-a-ride bus systems. (In fact, my first paper was on routing algorithms.) Despite our best efforts, ridership was low on an experimental system. As the most junior person in algorithm development, the task fell to me to complete a market survey to find out why. We surveyed consumers, found a fundamental flaw in the objective function, changed the algorithm, and ridership improved dramatically. A little marketing research did more for that project than many long hours at a computer terminal! I was impressed and I never looked back. I went to John Little (then head of the Operations Research Center at M.I.T.), he introduced me to Glen Urban, and so began a long career in marketing.

For the past twenty years I—have gone to John and Glen for advice and it has always proven valuable. I have collaborated with Glen on many a paper and two books and, in each case, I have enjoyed the experience, learned valuable lessons, and have come to appreciate his insight, creativity, and capabilities. I have co-authored but one paper with John, but that comes no where near indicating my debt to him.

I have asked two of my former students, now recognized researchers, to comment today. I have enjoyed working with each and can not begin to express what I have learned from them. I want also to thank my other co-authors (in alphabetical order) Jon Bohlmann, Roberta Chicos, Don Clausing, Josh Eliashberg, Pete Fader, Steve Gaskin, Phil Johnson, Bob Klein, Frank Koppelman, Leonard Lodish, John Roberts, Bill Qualls, Duncan Simester, Patricia Simmie, Peter Stopher, Derby Swanson, Alice Tybout, Bruce Weinberg, Birger Wernerfelt, Nigel Wilson, Ken Wisniewski, and Florian Zettelmeyer. I wish that I had the space to write an essay about each one. And these people are but a small fraction of the colleagues who have influenced and supported me and to whom I wish to express my thanks.

Endnotes

[1] Chomsky is one of the ten most cited writers in the humanities, right up there with Shakespeare, the Bible, Aristotle, Plato, and Freud. See Pinker (1994).

[2] I find it curious that I am best known outside of marketing for an article (with Don Clausing) on the "House of Quality." It has sold over 128,000 reprints. In that article Don and I simply described an emerging product development practice. That's a research spillover from which I have benefitted!

[3] The aircraft example is due to Warren Seering of M.I.T. Of those 100 million,

only 100,000 were "hard" in the sense that the rest followed from the initial 100,000. But 100,000 design decisions is still an immense engineering challenge. The automobile wiring example is due to Mr. Takahiro Oikawa of Yazaki Corporation. Mr. Oikawa points out this is the end result of a successful effort by Yazaki to reduce significantly the length and weight of the wiring.

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You Are What You Measure

Comments on John Hauser and the Converse Award

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"When you can measure what you are speaking about, and express it in numbers, you know something about it: But when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind."

William Thompson, Lord Kelvin

Since John started his paper with a quote, I thought it would be appropriate to start this comment off in the same manner. This particular quote epitomizes two themes in John's paper on which I want to expand, making these themes slightly more explicit and providing anecdotal evidence of just how important they are.

I am not used to John waxing philosophical—usually he waxes equational. The change in persona represented by writing from this non-traditional (for John) viewpoint is a symptom of a more general theme in John's paper suggesting the need for a breadth of perspectives in investigating marketing problems. This is indeed the model that John's body of research represents. John's 86 publications span theory and application, including synthesizing the literature and generating hypotheses, developing and validating new methods, modeling and then testing both behavior and theory. He has practiced what he preaches.

As important as the discussion of theory versus application as research topics is to the field, so is maintaining the breadth of the knowledge bases upon which we build our theories and apply them. One of the best aspects of moving to Chicago in 1989 was the breadth of perspective represented by the research of the people there. The group had economic, econometric, and statistical modelers, game theorists, consumer behaviorists, and now a field researcher. Seminars ranged across the various sub-fields, both those given by our own faculty, as well as the invited seminars and job talks. Someone always is knowledgeable about the topic, the theory, the methods; others ask interesting questions based on their different perspectives of the discipline. Our seminars are lively but more important, we learn from them. They are forums for cross-fertilizing the knowledge of a set of very capable people and providing the potential to move

individual research projects forward in unique directions—directions most interesting when suggested by others of different marketing orientations. When, for example, the consumer group is at ACR, seminar is less interesting because one of the perspectives, and one set of questions, is missing.

Finding faculty with multiple rigorously-trained bases of knowledge is a bit difficult and unusual, particularly in young faculty. So obtaining a broad research knowledge base across a school's marketing faculty requires hiring people from the different discipline bases. As advantageous as having a diverse research base at a university is, it can be a difficult situation to manage. It can make consensual hiring decisions to obtain a global optimum rather interesting.

The predicament in maintaining a cross-discipline research base is much akin to what happens in product development in firms, actually. Successful product development requires inputs of people from diverse backgrounds: marketing, engineering and manufacturing. They are not a naturally harmonious group. They speak different languages, use different logic structures and have different goals. New products are more successful when the inputs into it from each functional group is balanced, but achieving this balance requires taking pro-active steps to manage inherently different people. Such is also the case in academia.

The faculty of most marketing departments "morphs" over time. Senior faculty are recruited to more and from less advantageous positions, junior faculty get tenure elsewhere and new junior faculty fill in the ranks. However, some of the schools which seem to produce the most interesting body of research through the years are those which have found ways to encourage, support and manage a set of diverse talents from multiple knowledge bases working on a plethora of problem types with different research approaches. The tensions created are even broader than John intimates when discussing the theory versus application issue, but the potential rewards for moving the field forward by integrating across sub-disciplines are large.

The content of the quote touches explicitly upon the second thread which runs through John' paper, his research, and the research of several of the other past and current Converse Award Winners. That is the importance of learning to operationalize and measure the constructs of marketing research—especially the "squishy" constructs. Testing theory or models depends upon being able to usefully quantify the constructs and obtain enough data for statistical testing.

The following example is disguised, but did occur. Some time ago a Ph.D. candidate came to Chicago for a job talk. The student was from a well-regarded school and advisor, had clearly been rigorously trained in modeling and was even working on an interesting problem on the linkage between customer wait-times, price levels, and firm performance. The paper showed no evidence that the student intended to test the model, so of course that was one of the questions they were asked in seminar. The student had established banks, where customers stand in line for tellers, as a good potential data source. The way they proposed to operationalize "wait-time" was to count the number of teller-stations at each bank. When one of our students pointed out that wait time depended upon not just the number of open stations, but also on the amount of time/

station used per customer and the flow rate of customers into the system, the applicant replied, "But I can count the number of teller stations." Had the applicant been able to answer the question well, they might have gotten the job offer.

Model-testing is frequently limited by data availability. It always seems like the data you really want to test something are just not easily available. Over the decades, more data allowing for more advanced operationalizations of constructs slowly have become more available, which in turn has allowed the testing of significant new models in some aspects of marketing research. Researchers can approach solving the data availability problem in two ways. They can mold their research to develop theories and models which can be tested with data which are currently readily available. Or, they can take the road of developing models which require first developing new operationalizations of constructs, and then obtaining the new data. The skills necessary to create good operationalizations (tightly linked to the construct and easily measurable) are different from the skills used in creating good models. People unskilled in creating new measures will be limited to working with currently-available data sets.

The availability of scanner data has made an enormous body of household-level research now feasible. Gone are the days of single-category, limited time period analyses. Indeed, scanner data are contributing significantly to developing new knowledge in our field because of both the breadth and depth of information available. The extent of the information which is available ensures that this will be a rich source of new testable models for the foreseeable future. Indeed, many young researchers are finding that developing models based on scanner data provides them with a solid launch into the field.

Unfortunately over 60 percent of U.S. spending is business-to-business, where there are no scanner data. The many differences between consumer and business markets mean that there are enormous opportunities to start bringing the modeling sophistication of consumer markets into the business-to-business arena, if we can just come up with data. Research on the special qualities of services suffers from the same data limitations.

Lack of measure definition and lack of data have never stopped those driven to test models. For applying conjoint analysis, "relative utilities" were developed. Len Lodish and John Little developed mechanisms to quantify managerial judgments used in sales force allocation models. Pre-test marketing models had to come up with methods for quantifying trial and repeat purchase. All these models developed new measures which were used either as the input to the model or as output from the model. All these models are used in industry. Being able to measure is a necessary condition to testing models. Models need to be developed with an eye to being testable. Measurable constructs limit the models which are testable. You are what you can measure.

Actually, a third issue which deserves comment hit me as I was writing. Part of the reason John's body of research is so impressive originates a bit upstream in the academic genealogy. It has to do with "incubator" organizations. One of the most powerful mechanisms MIT has had for driving home the impor-

tance of the interplay between theory and application and for producing academics who appreciated and worked comfortably in both was Marketing Decision Sciences (MDS), and its successor manifestations in IRI and M/A/R/C. This special projects group competed for and completed bona fide marketing research projects for industry. It also provided a pilot facility for testing the veracity of new methods in an industrial environment—if you couldn't do it and sell it, the theory was great but the implementation of the method left a bit to be desired. ASSESSOR and DEFENDER were both commercialized from MDS. Voice of the Customer was piloted out of IRI. A spin-off from IRI was created to continue commercializing VOC. Because of the academic origins of the group, funding for other research was available. On the practical side, MDS provided Ph.D. students with enough supplemental income so they could finish the program at MIT without going too deeply into debt. More importantly, it connected MIT's students to practice.

MIT is not alone in developing this kind of industry-academic structure to encourage linking the theory and application. Chicago's micromarketing project with Dominic's and other grocery retailers and producers has been enormously successful in producing students and professors who are highly sought after. The work out of Wharton on conjoint analysis is also based on strong industry-academic relationships, which have earned the school a strong success record. There are many other examples of these joint efforts, such as Dipak Jain's recent work with the developers of high definition television (HDTV).

These "incubator" programs provide professors and students with the opportunity of testing theories, the challenge of operationalizing constructs into measurable variables and drive home the need to develop methods which provide value to users and which can be implemented in the real world of business. Not all research is appropriate for these forums—nor should it be. But maintaining mechanisms which provide the opportunity to test theory and methods in the field extends an institution's potential research portfolio. The implication from the long-term success of MIT, Chicago, Wharton, and a number of other schools which promote industry-academic interplay is that promoting these programs is worth the effort required to make them work. And they do take effort.

On a personal note, I've enjoyed working with John over the last decade. His attitudes about the breadth of what constitutes research allowed me to put together a somewhat non-traditional thesis, although it had some very traditional aspects. Although the thesis was a bit risky because the topic and overall goals were unusual, the work was fascinating, in part because there were lots of different research methods which could be used in investigating various aspects of the problem. It was a lot of work, but it was not a boring problem. I had fun. Unfortunately, not many people can say they had fun doing their Ph.D.

Since then the support and advice have continued to flow from Cambridge to the Midwest, and a continuing stream of research is still ongoing. This is a man with whom anyone can work productively, as numerous co-authors can attest. He's accessible (I have all 6 of his phone numbers, and he's finally got e-mail file-zapping capability), he provides thoughtful and thought-provoking feed-

back in a timely manner, you can disagree with him (as long as you can prove you're right), and he's unflappable. However, as with all MIT professors (and perhaps all professors everywhere), he has his quirks. I offer the following tips for working successfully with John:

- Never schedule meetings (especially with research sites) before 10 am.
- When working jointly on a project and traveling together, don't be in charge of getting the rental car unless you too are at least 6'4" tall. He doesn't squish well.
- When a passenger in John's rental car, be prepared to test the edges of the performance limits of the car. This compulsion to test the car's limits comes from piloting too many aspects of automotive new product development. Dramamine, Valium, and Tagamet, taken simultaneously help.
- Only undertake projects you can approach with passion.
- Choose projects with sufficient risk to make you scared enough to think better and work harder than you thought you wanted to.
- Never dangle your prepositions or split your infinitives.

Last year John suggested I might find interesting a book titled *The Idea Factory: Learning to Think at MIT*, by Pepper White, a Master's in Engineering graduate from MIT. This book chronicles "the changes that take place in engineers as they learn to think" through one student's struggles to learn objective, rational, logical modes of thinking. John started his career as an MIT engineer. The school imbued him with those logical processes before he ever embarked on marketing science. However, I think it is only the combining of this engineer's education with the discipline of marketing science and the act of investigating real problems which has produced the particular mix of logico-rational and intuitive thinking which characterizes the multi-pronged way John approaches research. I would call this approach learning to think **beyond** the ways in which an engineer thinks. The utility of this mode of thinking is evidenced by the mass of publications produced and industry practices which have changed based on John's research, the awards individual publications have won, the awards John's teaching has won, and culminates in the winning of this Converse Award.

Models, Theory and Selecting Research Topics: A Discussion

Comments on John Hauser

Steven M. Shugan, University of Florida

Overview

The objective of this paper is to discuss and extend John Hauser's presentation on shifts from theoretical research to empirical research. The paper John presented, like all of his work, was rigorous, creative, well-researched and extremely thought-provoking. No one can accuse John of being a follower. In every sense, John represents a leader of the field and a personal exemplar.

John's paper focused on shifts occurring in the selection of research topics. John predicts, and somewhat encourages, slower growth in theoretical research, compared to empirical research. In no way does John suggest replacing one with the other. To the contrary, John suggests that both are necessary and synergistic. However, he does feel that empirical research, and field research in particular, demand more attention. He also feels that theoretical research has much to gain from the outcome of field research.

I find myself in almost total agreement with John. My agreement is at such a fundamental level that I feel uncomfortable even playing Devil's advocate. Therefore, I will focus may attention on putting John's remarks into a broader context. I seek to explain why research topics are changing in the hope of better understanding whether those changes are permanent. First, however, let me review John's key points.

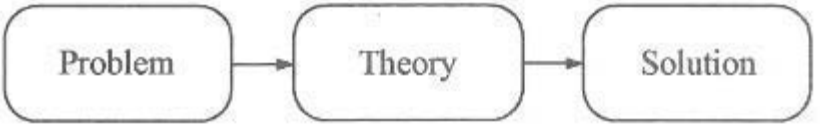
John's Key Points

John first notes that we need experience to build useful models and we need models to fully employ our experience. He frames the discussion with something resembling a chicken-and-the-egg analogy. He explains that theory must come from observation while productive observation requires some theory. Within that framework, he focuses on the source of observation

John suggests that observations might come from both data and field study. He suggests that we should talk with managers and make direct observation of

problems. This process should lead to both a better understanding of problems and better theory. He argues that marketing theory must come from understanding managerial problems. Good marketing theory comes from a deeper understanding of managerial problems. Good theory also provides solutions. Figure 1 illustrates John's paradigm.

FIGURE 1
John's Paradigm



In the process of advocating field studies, John also makes some very important points about the appropriate process for research. He notes that narrow theory can taint empirical observation. Strong beliefs can blind the researcher to the richness of the phenomenon under study. He provides several very cogent examples.

He also notes that causal observation is not field analysis. Anecdote is not the plural of datum. When done well, field research can provide the useful link between academic research and business practice. It can lead to a fundamental understanding of important researchable problems. Field research can help us better focus on improving business practice. That focus will keep our research productive.

Beyond Theoretical Versus Empirical

At this point, I would like to take a broader perspective. There are broader reasons why research topics will necessarily shift from theoretical to empirical. There are larger forces facing the selection of research topics. These forces are in the direction predicted by John Hauser (1996), but they have broader implications.

Within a broader context, we observe several changes in the market for research and the market for business education. We find, for example, a decline in the growth rate of business degree programs at leading Universities. We find fewer new business schools. Demographic shifts in the population and saturation of the business-education markets are causing declines in the number of business school applications for admission. We also find faculty retiring at older ages. Consequently, we observe a declining demand for new Ph.D. students from business schools.

The growth in demand for business schools may continue to exceed other areas, such as growth in mathematics departments. However, future growth will be far less than past growth. The growth in the demand for Ph.D.'s in business will continue to diminish. The consequence will be enhanced competition among existing business schools causing each business school to depend more on its own reputation for further growth or, at least, maintaining its current size. Fewer opportunities for public funding also will cause business schools to act more strategically. These changes will change the nature of research and the selection of research topics.

A Model of Research

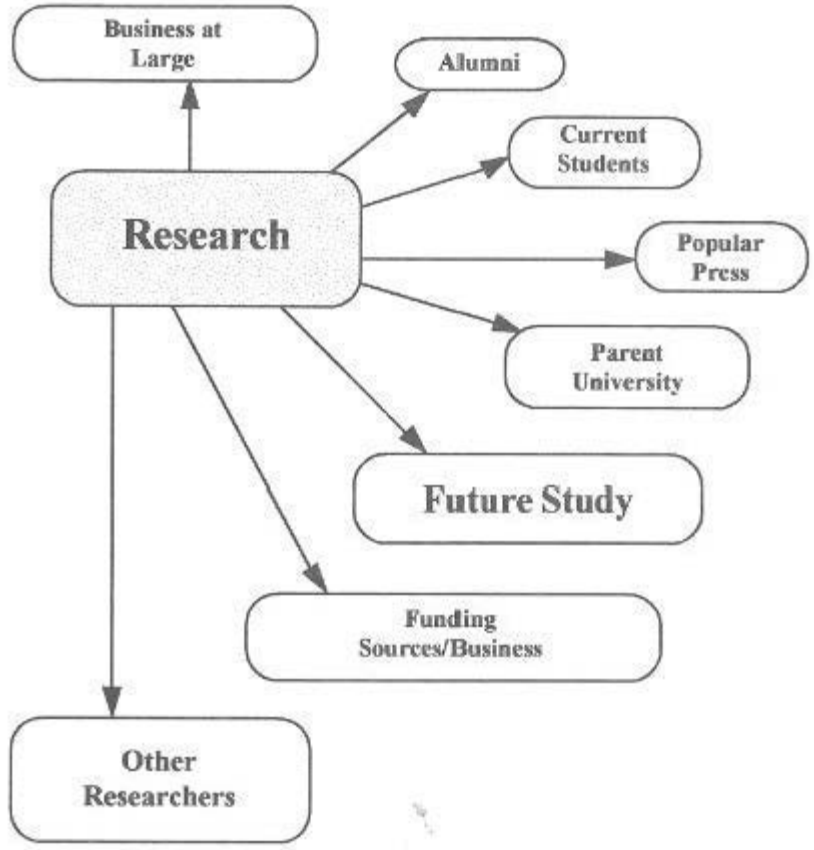
To understand the impact of these changes, let us apply the Hauser-Urban new product development methodology to the development of a research topic. Their methodology suggests attention to the market for a new product, in this case, research. Their methodology implies that when selecting a research topic, we should pay attention to the market for that topic. In other words, we should consider the markets for our research and the benefit of our research to those markets.

Figure 2 illustrates the potential markets for research. They include the business community at large which includes all business not necessarily affiliated with a particular university or the country sponsoring the research. The next market is university alumni who are interested in the reputation of their alma mater. Another market is the current student body of the university, who wants a better educational experience, enhanced job opportunities and a more reputable degree. There is also the popular press who seeks news-worthy stores associated with major changes, new views and information interesting to their audiences. A seldom overlooked market is the parent university, who uses research to evaluate faculty members and academic units. Another often overlooked market is future students who usually choose to attend a university based, in large part, on that university's reputation. An obvious market, but perhaps a smaller market for business schools, are direct funding sources who fund the university for doing research in compliance with the needs of the funding source. Finally, and perhaps the most important market, is the "other researchers" market. This market always plays a key role in evaluating research and its potential impact on the literature.

To this point in history, other researchers have been the primary market for research at business schools. This fact may bring some distress to the business community and popular press who would prefer more accessible and readily applicable research. In the past, however, writing research for other researchers was very efficient. Other researchers speak the same language or jargon, allowing less need for including definitions and background explanations. It is always more efficient to communicate with someone who "speaks the language" and already understands the prerequisite concepts.

Beyond a common language, the research community also has a more-or-less common value system. Researchers writing for other researchers can justify

FIGURE 2
Markets for Research



a paper with a brief appeal to the existing literature. The existing literature already contains precedents, explanations and justifications for various assumptions, approaches and methods.

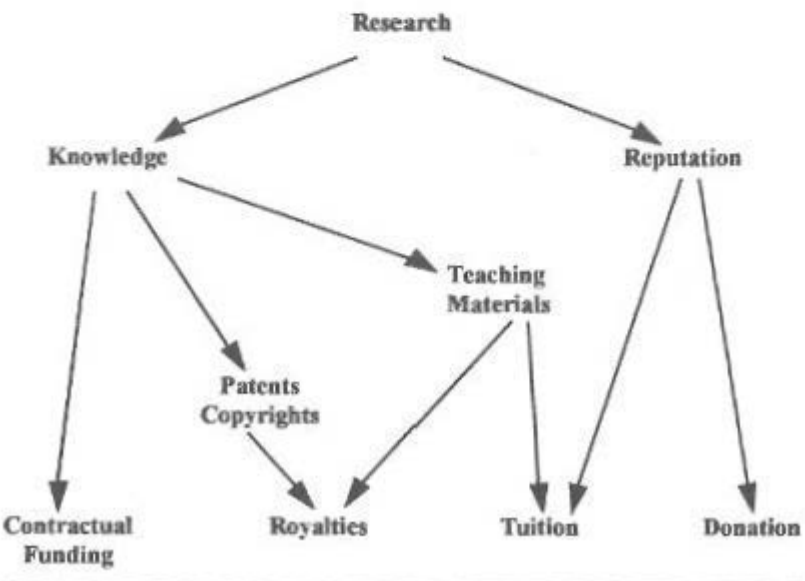
Finally, other researchers are best able to evaluate the technical and logical foundation for the research. In many cases, other researchers may value the process more than the outcome. They appreciate the quality of the arguments and the quality of the data analysis. At times, other researchers may reject a true finding because it was supported by less than rigorous justification.

In this discussion, I do not want to argue about whether this efficiency is good or bad. I do not want to argue whether the primary market for research should be other researchers. There are arguments on both sides. However, I do want to argue that this situation is changing. The focus on the "other researcher" market will continue, but other markets will become increasingly important. The changes, outlined earlier, will shift power from "other researchers" to other constituencies. To understand exactly how this shift will take place, we need to understand how these changes impact universities. Let us begin by examining the products sold by universities.

Universities produce a variety of products including education, research and service to the community. Being non-profit organizations, not all of these products need to return a profit. Some of these products may be produced for the good of society at large. However, in many cases, universities do seek some return for their investments including investments in research. Four major sources of revenue from research are funding from deliverables generated from contractual funding, royalties from patents and copyrights, tuition from students and donations. These donations come primarily from alumni and the business community.

Figure 3 presents a simple model that links research to these four sources of revenue. Figure 3 shows that research produces two quantities: knowledge and

FIGURE 3
Revenues from Research



reputation. Knowledge includes methodological advances, discoveries, inventions, a better understanding of phenomena, improvements in practice and patentable advances. It is possible that generating knowledge can also produce revenue. By creating patents and copyrights, knowledge creates revenue in the form of royalties. It directly produces revenues when the knowledge becomes the deliverable to contractual funding. Finally, by generating teaching materials, knowledge produces funding through the sale of teaching materials. It may also allow higher levels of tuition because it enhances the value of education.

The other direct product of research is reputation. Reputation includes enhancement of the image of the research-sponsoring institution, recognition by the research community, increased attractiveness of educational programs, gratification among Alumni, more attractiveness to potential faculty, and enhanced opportunities from research funding agencies. These sources of revenue generation are substantial and, perhaps, are much greater than the direct sources of revenue from knowledge. Note, however, that the impact of reputation on revenue generation is far more indirect and difficult to measure than the impact on knowledge. The ability to charge greater tuition and solicit larger donations depends on many factors beyond the reputation generated by research. Certainly, however, reputation plays a key role.

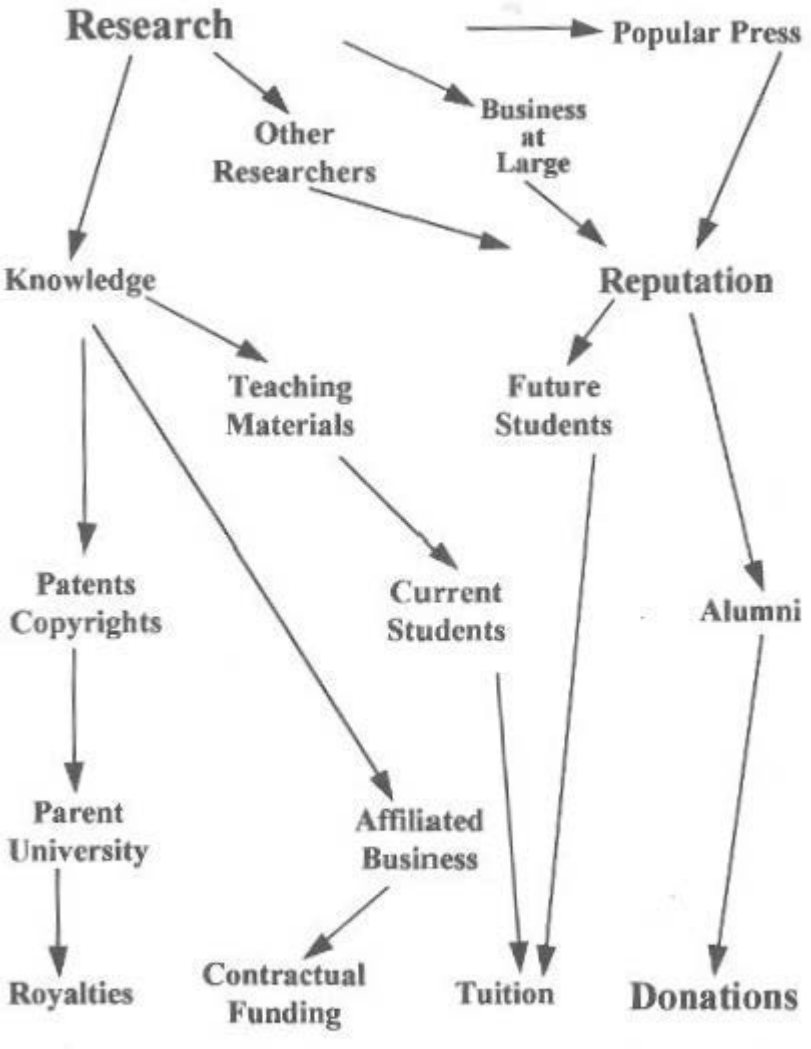
Combining the markets for research with the potential sources of revenue from research produces Figure 4.

Figure 4 is complex, but it reveals how changes in the market will impact the selection of research topics. Figure 4 illustrates that research develops reputation for a business school or university by reaching three markets: other researchers, business at large and the popular press. These three markets create reputation, which in turn, eventually leads to the ability to charge higher tuition and attract larger donations. Note that research at one university may help researchers at other universities to enhance knowledge. However, that enhancement has little impact on royalties and contractual funding for the first university. For example, when research generated by University A helps a researcher at University B obtain a contract, there is seldom any rewards, beyond reputation, to University A. Hence, advancement of knowledge without an associated enhancement of reputation, fails to generate revenues unless that knowledge generates royalties or contractual funding. Let us now use the model in Figure 4 to explore how market changes will impact the selection of research topics.

How Market Changes Impact The Selection of Research Topics

Before continuing, I must assume that incentives exist for faculty research to help the sponsoring institution. Hence, I assume that the business schools, who survive, will encourage research that enhances their goals. In other words, I assume that research topics will eventually conform, to some extent, to the best interests of the business schools that support them. This assumption implies that most research topics will eventually reflect the goals of the business school.

FIGURE 4
Research Revenues and Markets



This will be true, at least, for those business schools who prosper or, at least, survive.

Now, we could argue that it is in the best interest of business schools to encourage only that research that generates direct revenues. Indeed, some uni-

versities have made a shift in emphasis from tuition and donations to revenue from royalties and contractual funding. The success of this shift varies from university to university. It depends on both the availability of outside funding sources and the ability of faculties to generate the necessary deliverables for that funding.

Most business schools, however, have experienced limited growth in this area. One factor, perhaps, that has limited this growth is the lack of possible business school contributions to this effort. It is difficult for business schools to undertake strategies where they have no competitive advantage. In other words, the organization must be able to accomplish more as an organization than the individual employees can alone.

Unlike engineering and science departments, business schools seldom provide extensive laboratories, expensive equipment, unique instruments or rare subjects, such as medical patients. Without these assets, business schools have a limited ability to both attract high-powered consultants, and limited power to extract rents from consulting faculty. Most high-powered consults will find superior salaries and support from professional consulting firms. That faculty, who are able to raise funds from consulting, will be unwilling to incur a tax rate that exceeds the contribution of the business school. For example, suppose a faculty member engages in a $10,000 consulting project and the business school contributes $1,000 in resources to that project. In this case, it would be difficult for the business school to tax the faculty member more than $1,000.

The case would be different were the business school to supply laboratories or expensive equipment. It would also be different were the funding agency to limit funding to only faculty members at business schools. Here, the business school contribution would be high and the business school could extract a large tax. However, as stated earlier, business schools have not traditionally contributed large fixed assets to research. The rare exception may be the very prestigious business schools that provide lucrative opportunities to faculty who would otherwise be unable to obtained funded projects.

Therefore, the primary funding sources will remain tuition and donations. Both of these sources are highly related to the school's reputation. Changes in the marketplace and competition among business schools probably will not change this relationship. Students will always prefer to attend universities with better reputations and pay more for that privilege. Parents of students will continue to prefer to send their siblings to universities with better reputations. Corporations will want to send their employees to universities with better reputations and recruit new employees from those universities. Although it is possible that research itself may take a less prominent role, I feel that event is very unlikely. Research will remain the primary source of reputation for universities and business schools. It also will be the primary method of generating reputation. In sum, the reputation generated from research will always command an advantage in every market.

What will change is the source of reputation. As business education has grown, many more agents have appeared with a vested interest in evaluating

business education. As the evaluating agents change, the methods of generating reputation may change.

In the past, the "other researchers" market dominated the generation of reputation. Business at large and the popular press only echoed the opinion of researchers and the deans at research institutions. Today, in contrast, business at large and the popular press are playing a more active role in the creation of reputation.

As suggested earlier, these later markets have different values. These markets place a much greater weight on accessibility and the immediate applicability of research. The increased weight on accessibility may have a greater impact than the increased weight on applicability, but both will have a major impact.

For example, the popular press must reach a much broader market than academic journals. That objective requires simpler, shorter and more direct communication. It also requires more lively communication with a greater emphasis on relevancy. The popular press, therefore, has a greater appreciation for research that makes a simple and easily summarized statement. The popular press also has a greater appreciation for research that would be of interest to a more general audience. In marketing terms, the "popular press" market desires different benefits than the "other researcher" market.

The implication is that creating research for these other markets (the popular press and the business community at large) will become more important. Business schools must, then, place a greater emphasis on accommodating their needs. For example, there is a greater incentive to produce research that is more accessible and whose applications are easily communicated. The impact, for good or worse, will be incentives to choose different research topics.

Consider, for example, a research topic that generalizes an existing, technical but well-known published paper. The research community would probably view that topic as, not only quite acceptable, but laudable. In the "other researcher" market, extensions to the existing literature are usually considered a very appropriate line of research.

Unlike the "other researcher" market, the popular press may be less enthusiastic about this form of research. There is less "news value" to research topics that merely extend existing knowledge. Research that shows something that was previously unknown has far more news value. Replication has little news value.

Theoretical research might also have less appeal to the popular press. After all, theoretical research is merely the opinion of the researcher. Although that opinion may be expressed in the most rigorous manner, that rigor is hard to translated into easily understandable prose. The press may find little news value in research that merely provides more rigorous arguments Moreover, theoretical papers often depend on highly technical assumptions that may also have no easy English language translation.

The popular press clearly prefers empirical papers with significant new findings and, to a lesser degree, methodological papers with visible applications. Empirical papers with new findings represent real news and have empirical data

to supply credibility. Methodological papers may have less appeal, but those with visible applications are very newsworthy. The visible applications provide a nice tangible story to provide both credibility and interest for the methodology.

As stated earlier, the business community at large will also be an important influence. I distinguish between the business community at large and the affiliated business community. The affiliated business community may have narrow interests and seek funding for those interests. A bank, for example, may want a finance department to sponsor conferences to discuss, and possibly promote, a particular regulatory policy. The affiliated business community has a potentially large and direct impact on funding.

The business community at large, in contrast, may have less immediate objectives and a less immediate impact on funding. They may be interested in hiring students, educating employees or just having some contact with universities. They may also merely seek to be opinion leaders and exercise some influence over business education. Unlike the affiliated business community, the business community at large has more of an impact on reputation than on immediate funding. Their impact may be more long-term. They may increase the demand for a particular business school's students by either hiring those students or encouraging others to do so. They may encourage their employees to attend a particular business school. They may also speak favorably about a particular business school to the popular press.

I would expect the business community at large to be somewhat more receptive, than the popular press, to theoretical research. This community is more interested in solving problems than only reading interesting news. They may like creative ideas without data because they are willing to substitute their own judgment for extensive empirical support. Never-the-less, the business community probably will find empirical research more credible than theoretical research. Moreover, empirical research often is more accessible because the contribution from a substantive empirical study can be more easily explained than the contribution from either an empirical theoretical study or a methodological one.

I do not think that all empirical research is substantial, relevant, newsworthy and easily explained. Nor do I think that all theoretical research lacks credibility. It depends on many factors. I do think, however, that empirical research more often has these characteristics than theoretical research. It follows, therefore, that we should observe growth in the importance of empirical research that makes substantive contributions to business. We should observe less growth, or a decline, in both purely methodological research and theoretical research.

Challenges

Changes in the market will create other challenges beyond selection of research topics. The challenges may effect the nature of research and the public dissemination of knowledge. Let me suggest two of these challenges.

First, most universities, in some way, are funded by taxpayers. Through non-profit status, government-funding agencies and trade associations, universities enjoy a privileged position. They enjoy a privileged status because they are expected to generate many products for the public good. All of society pays some cost because all of society enjoys the benefits.

Shifts to world markets, however, may upset that balance. In the past, we could justify taxpayer subsidies to universities on the grounds that universities contributed to society at large. In the past, the business community was, in fact, the U.S. business community.

It is now less clear whether U.S. taxpayers should subsidize contributions to world-wide business. For example, should a U.S. taxpayer payer support the doctoral business education of a foreign national who returns to their country to either help overseas competitors or teach at a foreign university. Although helping the world is an admirable goal, we may have taxes from U.S. corporations funding business schools working with foreign competitors.

I suspect that the government subsidy to business education will undergo a re-evaluation as increasing numbers of foreign students attend U.S. Universities while research faculty seek to disseminate their ideas abroad.

Beyond decreased government subsidies, let us consider a second challenge to business research is the rate of increase in information or knowledge. To remain aware of advances within a field and to advance the literature, researchers must become more specialized. This specialization may conflict with their ability to generate accessible research. Never-the-less specialization is inevitable.

There are several possible solutions to this problem. John Hauser suggests field research. Here the researcher focuses on all the details of a case study and, only later, examines the generalization any findings. With field research the specialization takes the form of fewer, but more detailed observations.

Another solution is an industry focus. Researchers might become specialized in one industry and, only later, attempt to generalize findings to other industries. Although field analysis remains valuable, other sources of information about the industry are also available. For example, the trade press may become more important.

Finally, if researchers can spend less time teaching, they may be able to assimilate more knowledge without specializing. Spending less time teaching requires more efficiency or productivity. We can accomplish that productivity by coordinating research and teaching. In other words, we should teach what we research. This solution is consistent with a focus on the "popular press" and "business community" markets.

Summary and a Final Comment

This discussion suggests that business school research helps parent universities by enhancing their reputations. With a better reputation the university can charge higher tuition than without that reputation. The university can also attract larger donations with a better reputation. Hence, reputation can provide potentially

tangible benefits when a university exploits that reputation.

Research generates reputation by reaching three markets "other researchers," "the popular press" and "the business community." I have argued that the former market, that once dominated, in now sharing importance with the latter two markets. In the future, therefore, "the popular press" and "the business community" will be more important. Business schools and researchers will need to consider the values of these later two markets.

These markets do have different values. The "other researcher market" puts less emphasis on accessibility and easily understood applications than the latter two markets. This means that there will be increased incentives to provide research that is more accessible and more easily communicated. There will also be increased incentives to generate research with obvious applications.

I argued that theoretical research and purely methodology research will be at a disadvantage while substantive empirical research enjoys an advantage. Empirical research often is more accessible because the contribution from a substantive empirical study can be more easily explained than the contribution from either a theoretical study or a methodological one. It follows that more research should be substantive (i.e., providing managerial rather than methodological implications) and empirical.

I end this discussion of research topics by returning to John Hauser's talk. Although John has many talents and successes, John most certainly is qualified to discuss selection of research topics. John has an exceptional talent to select research topics that are interesting, novel, important and tractable. His track record is extraordinary. His early work on new product development, entropy measures of fit, intensity measures, defensive strategy, perceptual mapping and consumer behavior continues to have a significant impact both on current research and current management practice. His latest research on customer satisfaction, cross-functional innovation, consumer choices for new products and management of R&D shows the potential to exceed the enormous contribution of his earlier work. If John gives a presentation on a topic, you know the topic will soon be the focus of the field.

References

Hauser, John (1996), "The Role of Mathematical Models in the Study of Product Development," 1996 Paul D. Converse Symposium, May 6-8, 1996 at the University of Illinois.