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**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 traveller.

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

Dawkins refers to signalling 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 of 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 Johannes 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 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% of the successes. He suggests that empirical-then-theoretical research has accounted for 80% 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-term 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 advice 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

¹Chomsky 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).

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 1 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 than 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 firm. In terms of a reward system this means that tier 1 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 "arbitrage" 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.

²I 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!

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 (1995), 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

³The 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 that this is the end result of a successful effort by Yazaki to reduce significantly the length and weight of the wiring.

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 corporation, 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.

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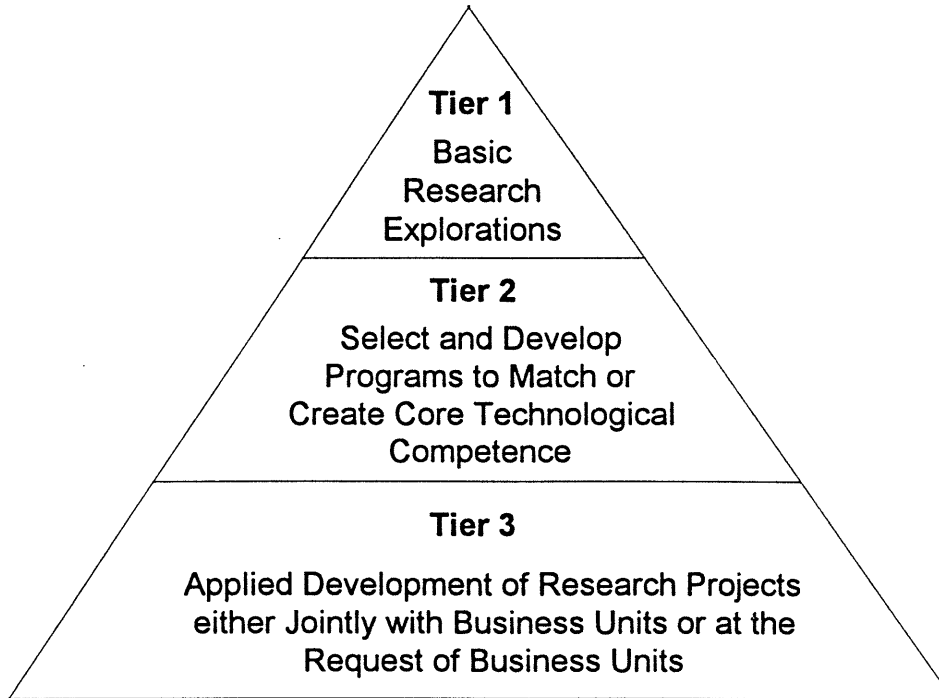


Figure 1. Tiers of R&D
(from Hauser and Zettelmeyer 1996)