銘傳大學八十六學年度管理科學研究所博士進招生考試

管理科學文獻評論　試題

請詳讀所附論文後，逐次回答下列各題：

一、　試就本篇論文的第一節 INTRODUJ10N 及第二節 THE STRUCTURING OF SCIENTIFIC CONCEPTS，分別以不超過 300 字為限，說明其主要內容。(本題佔 30 分)

二、　試就本篇論文內容提出最重要的五個主要論點，並指出其出處及您認為重要的理由。(本題佔 35 分)

三、　本篇論文之論點，如何與您的學術專長結合，以利博士研究之進行。(本題佔 35 分)

〈試題完〉

Method and Progess in Management Science*

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Abstract – A model of the management sciences is presented, detailing the roles of theory, methodology, data, and problems in scientific advance. The purpose of the model is not describe the scientific process and clarify its terminogy, so that research may be supported and performed in a way that fosters more rapid advances in worthwhile directions. A framework of "problem-driven research" is offered as a preferred altermative to theory-driven research as a basis for progress in the management sciences. The presentation draws on the insights of other writers, and uses a number of examples from the history of technology and management to illustrate and support the model.

INTRODUCTION

As persons with professional experience in both business and academia, we have become interested in the uses of management science, and have participated in some of its research developments and applications in marketing and other areas. Form these perspectives, we present a model that leads to a re-examination of "sciectific method" and the role of research policy as they bear on management science. The model reflects the unbalanced growth of science that can result from an advance in any one of the four components of scientific progress – theory, data, problems or methosology. We study the impact of such an advance on the other three components, and combine this dynamic with a classification due to Cooper [14], in order to introduce a introduce a framework we shall refer to as "problem-driven research." The purpose of the latter notion, in turn, is to propose a statement of scientific method that is appropriate for management science. The discussion leads to recommendations emphasizing: (i)the importance of structuring new problems and identifying new problem areas for reseach attention; (ii) tests of validity by reference to use as well as generalizability; and (iii) potentials for cross-connections between and within already identified areas of research. These recommendations, or at least the way we interpret them, will imply moving away from sole reliance on reproducibility of results and on theory comprised of simple “laws”[20].

A secondary, but important, function of this essay is to clarify the meaning of terms (such as “basic,” “applied,” “controlled,” and others) used to describe research projects and research methods. Academic work in management science is amply documented in journals. But the interaction of research and practice with which we are presently concerned is less well documented, especially as we reach into the history of management science developments for possible origins and meaning of terms such as “basic” vs “non-basic,” or, as some would have it, “basic” (=pure)0 vs “applied” research. The historical examples used here are necessarily selectively chosen. We have therefore drawn on our own opinions, and those of others, hoping to persuade (rather than prove to0 the reader that the model is valid.

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We open with a discussion of how new problem areas emerge in science, with special reference to management science. This discussion is intended to lay the goundwork for the role of "problems" in the unbalanced growth model of scientific progress that we present.

THE STRUCTURING OF SCIENTIFIC CONCEPTS

To a newborn infant, the world is an undifferentiated whole. The infant's first act of cognition (and its first step toward the development of language) is to make the distinctions "me"/"not-me" and "mother"/"all-things-that-are-not-mother.” Each distinction divides the world in half; each of the halves becomes a concept or an entity, and is eventually given a name (see e.g. [37]). As the nervous system develops and experience accumulates, more complex concepts are formed, and language becomes available to describe relationships among these entities and to test their validity against further experience. The complex and highly systematic rules governing the development of cognition and the nervous system are coded as genetic information. Though restrictive, these rules allow the organism to generate creative responses to novel circumstances.

Science is also a way of forming, naming and testing concepts. It, too, is governed by organized and systematic rules. These rules allow---and, indeed, encourage – reative and constructive responses to unforeseen phenomena and problems. "Science is primarily an activity of extending perception into new contexts and into new forms, and only secondarily a means of obtaining what may be called reliable knowledge" (Bohm [4], as quoted in [47]).

Figure 1 depicts the model of science just described. Within an undifferentiated continuum of stimuli (or, more mundanely, an "unstructured problem"), observation is possible, but not the systematic collection of data. (See the definition of "evidence," in its second sense, in [22, p. 2001]).It is only after observation has tentatively distinguished elements of the situation and given them names that a systematic collection of' data is possible. Examining the data leads to notions of relations between elements, which are to he represented as information. A structure has now been built that can generate testable propositions and hypotheses, by reference to suitable rules, according to which information is turned into evidence, or lack thereof. Thus, concepts alone do not constitute science. Ideas such as "competitive advantage" [34] correspond only, for example, to what we will later call "useful ideas," and what Shubik [41] has called .”conversational game theory.” Evidence, in turn, can be tacted by observation, perhaps under the controlled conditions referred to as "experiments." Figure 1 illustrates what occurs as experiments feed back to the first three stages. Here we are emphasizillg feedback to “structure,” i.e. problem identification, as well as to "theory" addressing already identified problems. Similar feedback may occur on explanation and prediction, or even on observation when inaccurate or irrelevant observations, useless data categories, or fallacious relationships are revealed. The four-element construct of the ancient Greeks provides an example of data categories that proved to be inadequate for sciectific porposes. The “earth, air, fire and water” schema could not support extensive inference and experiment. Eventually, these supposedly basic categories were supplanted by the elements in the modern periodic table. Perhaps more commonly, relationships are re-exmined. It was learned, for instance, that flies do not come from dirty rags; flies hatch from fly eggs.

It is a thesis of this paper that new problems and problem areas, arising from management practice, comprise (or should comprise) the raw materials of management science. Problem areas are “new” if analogies to known situations are not adequate for forming solutions. Because such problems are often urgent, and the key variables have not yet been identified or named, the management scientist may experience the sensation of being at sea with nothing to grab onto. It is here, in the “structuring of new problem areas,” that avenues of scientific recognition and support have often been lacking. It is our thesis that such structuring is a fundamental and integral part of management science that is not adequately attended to at present.

In the natural science that is not to surprising, especially in the theory-driven sciences such as (basic) physics. There, the re-examination of fundamental categories is a rare event [24]. Such rare paradigm shifts may also occur in amnagement science, as in the shift from economic order quantity models of inventory to just-in-time constructs. However, new problems are ubiquitous in the management of private and public sector enterprises. The shift in emphasis from efficiency to flexibility in modern high-tech manufacturing, for example, provides ample new prblems. These make the structuring activity more frequent and more in need of attention in the management sciences.

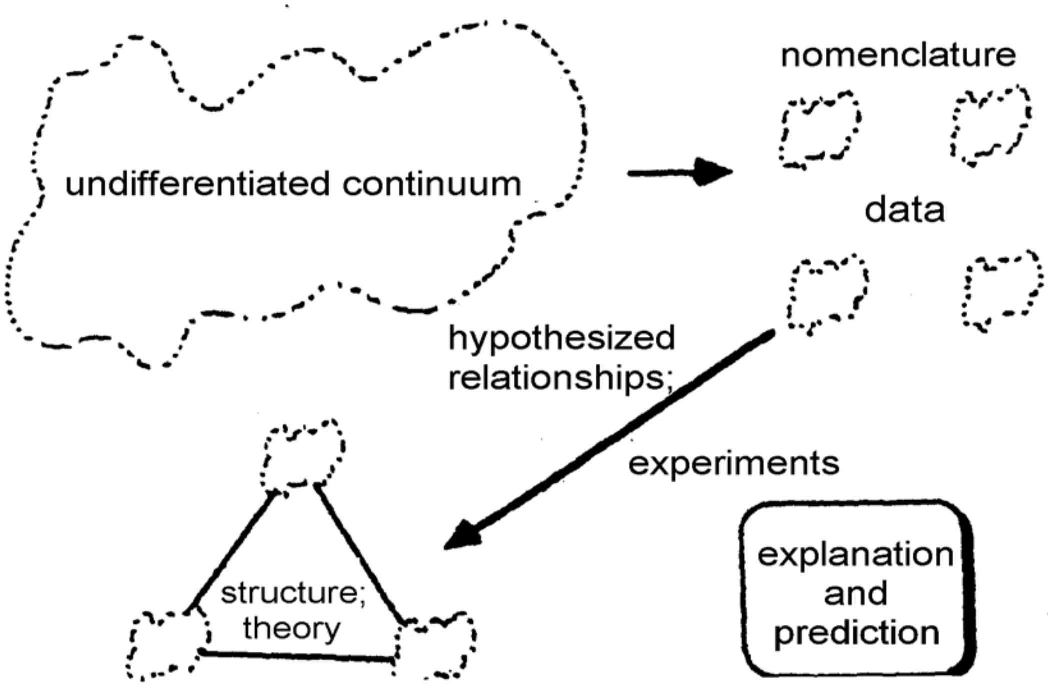

Fig. 1. Scientific activeity as a schema.

TECHNOLOGY, MANAGEMENT AND SCIENCE

Much of contemporary science focuses on issues of complexity [44], many of which are dealt with by approaches embodied in relatively new methodologies such as "computational physics" with its paralles in management sciences, and information processing uses of simulation models and algorithms. Cotemporary science in some ways resembles the earliest days of Western science, in that we again need to deal extensively with uncontrolled data as opposed to laboratory-generated data [3]; and many scientific prblems arise in the practical arena, reminding one of the discussions in Galileo's *Dialogue of Two New Sciences* [16].

Contemporary science was preceded by a post-Galilean era that produced achievements leading to the industrial revolutions and to consumer societies. It was characterized by primacy of the physical sciences, a search for univeral truths, and a reductionist drive to sumplicity [20]. The intellectual influence of this epoch is still pervasive. Indeed, it was the "only game in town" prior to the post World War II advances in science. It was not until the 1960s, when large scale computation and data storage become commonplace, that an information-age orientation diffused (the diffusion is not yet complete) throughout rhe general population of practising scientists.

Galileo's laboratory method emphasized controlled variables and reproducible experiments. Science in the ensuing era utilized Galileo's methodology in the pursuit of physical laws, the observer [42]. Any human-oriented consequences of this research were called "technolohy" or "engineering," and the prevailing scientific culture enforced a strict separation of science and engineering, resulting in the vocabulary of "theory" and "application." Jeremy Campbell [9] thus quotes John Pierce: "Physics deals with the works of God, engineering with the works of men;" and , according to Boorstin [5], the design of the solar calendar was a feat of science (astronomy), but the division of time into hours, minutes and seconds was a technological feat. Sundials, water clocks and sand clocks had to be made. These spun off scientific and craft endeavors in terms of clockmaking, studies of the viscosity of water and water pressure; materials science for fashioning water clocks that would not clog or erode; and metallurgy and acoustices for the making of bells. In particular [5, p. 64], "the clock … was destined to be the mother of machines. The clock broke down the walls between kinds of knowledge, ingenuity, and skill … Clockmakers were the first consciously to apply the theories of mechanics and physics to the making of machines." The led to die cutting machines, lathes, and thus to precision instrument making that served the further advance of science.*

*McKelvey [30] shares this view of the role of the clock in the hostorical interaction of science and technology. He adds, "The relationships between scientists and technologists were informal and sometiomes quite accidental in these historical examples. Each group influenced the other without any specific plan to do so. Such a relationship persisted for centuries, changing only around 1900, when a few companies such as the National Bureau of Standards in the United States and the National Physical Laboratory in Britain were also set up about that time. In these laboratories research proceeded along well-defined disciplinary paths that put engineers in the role of consumers of scientific understanding. This pattern suddenly changed during Workd War II in response to demands for highly sophisticated weanons and systems."

Where there is technology, whether it be the Egyptian pyramids or the space shuttle, there is management of technology. The marriage of management and technology can have an impact on science, as in the practical problems that gave rise to the surveying techniques that were developed in response to the flooding of the Nile which were precursors of Hellenic geometry. Conversely, the practical problems that have resulted from science can be embodied in technology with far-reaching consequences for management--as in, say, the telegraph (now obsolete), the telephone (becoming obsolete), and themodern revolution in telecommunications.

We have just equated the task of management with the similar task of technology – that is , to improve the efficiency and effectiveness of human enterprises. The fact that the term "management science" has been current for some 40 years implies that the separation of science and technology should be questioned. "There's no [sharp] dividing line between the two," says Eric Bloch, quoted in Schrage [40]. "The knowledge floats back and forth. The separation between science and technology is a bad separation – there's no reality behind it." In this regard, W. J. Broad [7] quoted Barry Barnes: "We are now much less prone to think in terms which subordinate technology to science. Instead we recognize science and technology to be on a par with each other."

Certainly science informs management. Management, in turn, uncovers new problems for science, as in the need to discover the causes of malaria and yellow fever en route to constructing the Panama Canal. But the convergence of sciences directed specifically toward humans and human society; the clarification of the role of the observer in physical experiments; and the shortened commercialization cycles attending the development of the science and technology of information.

Referring to the late 19th and early 20th century, Barraclough [2, p.45] notes, "The primary differentiating factor, marking off the new age from the old, was the impact of scientific and technological advance on society, both national and international." There arose, at this time, social sciences explicitly directed to studying the complexities of humans and human institutions. Although the older field of economics remained highly abstract (see e.g. [25]), and Freudian psychology was decidedly mechanistic [9], the urbanology of Jane Addams and the psychology of mechanical analogies, making their core the study of the possibly uniquw condition of humanity. Important parts of science were now for and about people.

A second watershed in the relationship of sciemce and technology was Blotzmann's injection of probaility considerations into physics. A modern physicist, P. W. Bridgman, has said that "The second law [of thermodynamics] still smells of human orgins [9, p.50]." Campbell [9] comments,

Statistics belongs … to the domain of the organic, to fluctuating life, to Destiny and Incident and not to the worlds of laws and timeless causality … As everyone knows, statistics serves above all to characterize political and economic, that is, historical developments. In the 'classicla' mechanics of Galileo and Newton there would have been no room for them. And if now, suddenly, the contents of that field are supposed to be understood and understandable only statistically and under the aspect of probability … what does it mean? It means that the object of understanding is ourselves.

This recognition of the importance to science of human actors was reflected in Heisenberg's uncertainty principle and in the conundrum of Schrodinger's cat.

A third breach of the wall between science and technology was the umpact of information technology, which we will define to include genetic engineering. Informaiton technology is generic, flexible, and fundamentally different from the technology that had gone before. With slight exaggeration, we may say that any conceivable circuit can instantly implement any information flow model within even a globally dispersed organization. The commercialization cycle is now so short that many scientific advances cannot be discussed without joint consideration of technology and management. The seeds of this phenomenon can be seen in the early electric power industry [2, p. 46]:

Electrolysis, so important in the extraction of copper and aluminum and in the bulk production of caustic soda, only became a practical proposition when electric power became generally available; and the same was true of other electrochemical developments. The electrical and chemical industries of the late nineteenth century were therefore not only the first industries to originate specifically in scifically in scientific discovery, but in addition they had an unprecedented impact, both in the speed with which their effects were felt and in the range of other industries they affected … The food-canning industry [to which we will fefer again in the present paper], helped by new processes of tin-plating, now got into stride, and the sale of canned vegetables rose from four hundred thousand cases in 1870 to fifty-five million in 1914.

Nobel laureate James Buchanan [8] writes, "There is no set of relationships among persons that we can label to be 'natural' in the definitionsl sense of independence from human agency. The political economy is artifactual; it has been constructed by human choices, whether or not these have been purposeful in any structural sense." Ywt many discussions that attempt to bridge from physical science argument to a management lesson are still stymied when, as is inevitable, the issue of human purpose and intervention is raised. The misunderstanding ususlly centers on the idea of controllable variables and the role of managers.

Table1. Passive forecasting vs controllable forecasting in history and now

	Pure prediction	Controllable prediction
Pre-modern	Prophecy	Navigation
Modern	Time series analysis	Engineering

EXPERIMENT AND VERIFICATION IN MANAGEMENT: CONTROLLABLE FORECASTING

Normative models expressly expressly acknowledge human purpose, asserting not only "A implies B," but "If you want C to happen, you had better do D." More formally, normative models admit values as components, and are oriented toward specific goals [42].* Note the personal pronoun in the question. How can "you" get there? It should be abundantly clear that a personal involvement is necessary in the context of management decision and management science, even when such decisions are made by computers implementing one or more of the available approaches in artificaial intelligence or expert systems.

*"[The] positive-normative distinction … became familiar only in this century [8]." We emphasize it here because, as Kuttner [25] reminds us, it is not universally appreciated; "By reasoning deductively from axioms, economics confuses the normative with the descriptive. Theory stipulates, a *priori,* that perfect competition is both a description of the optimal world and a useful approximation of the actual world … Perfect competition, in a sleight of epistemological hand, is said to describe the best as well as the actual world.

We now turn to further differences in nomenclature that can help to elucidate some of what is involved in those approaches to management science we are considering. We turn first to "pure" and "controlled" prediction. Table 1 illustrate, by example, the difference between absolute or "pure" prediction, and the "controllable" prediction (Popper [38] called this "engineering prophecy") which we view as fruitful for management science. Entrail readers and prophets predicted immutable futures, implying that human action is ineffectual before the whim of the gods. The modern counterpart of this modern counterpart of this mode of prophecy is statistical time series analysis, which relies only on historical trend to predict the future, without regard (except in its "transfer function" variants) to the technological or social processes that drive the variable of interest. It also ignores human intervantion, e.g. that a downward trend in sales could be reversed by management of advertising and promotion. There is another mode of prediction, which we shall refer to as "controllable prediction," in which human goals direct attention to controllable quantities.* See the right side of Table 1 for an example, which may take a form such as, "If you follow a northwesterly course from here, you will reach Piraeus." Or, "If you use this design, this gauge of steel, and this many rivets, the bridge will carry 5000 cars per day for 30 years – with proper maintenance that can also be specified." On the other hand, without such maintenance it is not possible to predict with any accuracy how long the bridge will last. Note, therefore, that an uncontrolled (i.e. pure) predictionis not possible in the latter case.

Modern management science focuses on those variables that can be controlled in the situation at hand. ** This use of the word "control" is different from "controlled experiment." In a laboratory, I may test the response of human subjects to deliberately varied stimuli while controlling (i.e. holding constant) other factors like humidity that do not directly bear on my hypothesis. However, if I sell picnic supplies in the real world, I can control (willfully vary) the advertising stimuli, but I cannot control the weather. I must, nonetheless, think systematically and scientifically about how best to manage the enterprise.§ Any model of a picnic supply company that does not address variationin price, advertising, production rate, resources devoted to competitive position in the marketplace.

In addition to context, one must also cinsider the choice of variables. A model may be of no interest to me as a manager if it calls for experiments on, for example, price. Adams and Russell [1] note the decline in controlled advertising and marketing experiments in the years 1972-1982. (An exception is the growth in experimentation in direct mail formats, where uncontrollable effects are easily randomized.) They distinguish laboratory experiment (almost totally extinct in marketing) from "large scale real-world" experiments and "controlled enviroment" experiments, e.g. tests of alternate advertisiing copy in isolated cable TV markets. Large scale real-world experiments are expensive, able to discern only very broad effects, and often inconclusive [1]. This assessment was borne out by a recent 6-year, million-dollar study of the media exposure and purchasing behavior of 30,000 households. The study, which ended in 1991, "distinctly underwhelmed" its sponsors [27], who deemed the conclusions "broad, obvious and uninstructive." Controlled environment experiments were on the increase in the impact of competitors' concurrent experiments. Adams and Russell see increased use of retrospective experiments, in which time spans without significant variation in uncontrollable factors may be focused on, as source of principles for action.

Because fewer variables can be controlled outside the laboratory than in it, reproducibility of results is less useful as a criterion of scientific success, at least from the point of view of a manager who is considering whether to adopt and apply the findings. Experimentation may be essential for advancing theory in the physical sciences, but in the management sciences experimentation is often too expensive and may be impossible or meaningless. To a large extent, managerial *use* provides a better or more meaningful test of a decision model, replacing test-by-experiment [11]. If a new model produces higher profits than available alternative practices – or if management *believes* this is or will be the case – then the model is used repeatedly and adopted by other practitioners and organizations. Other evidence of a model's worth comes from the judgment of scientific peers on the nature and extent of its assumptions, and the rigor of its scientific content. Also imporant is whether alternative analytic methods validate the model's conclusions [6, 12, 15].

*Irving Kristol notes that the only forecasts "actually demaned by law" by the Federal Government are economic forecasts of the pure prophecy type [23].

**Again, there are exceptions; notably, descriptive studies of organizational structure and descriptive or causal studies of the psychological basis of decision making.

§By stressing controllable forecasting, we do not wish to ignore the usual distinction between decision and control, the latter being an important part of management science. Control in this sense is the sequence of monitoring actions that ensures a decision is executed as planned.

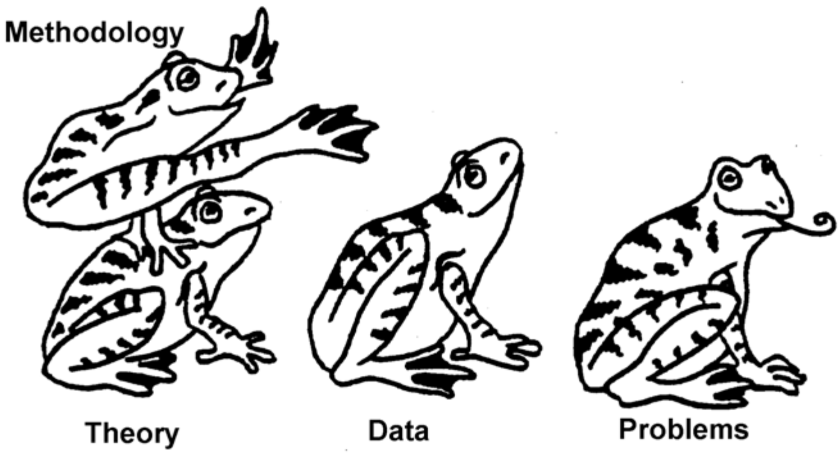

Fig. 2. A leapfrog model of scientific progress.

A MODEL OF PROGRESS IN THE MANAGEMENT AND DECISION SCIENCES

We can now usefully draw a distinction between "substantive theory" and "methodological theory," along the following lines. Suppose an astronomer points a telescope at a star. Any statement he makes about the star may be considered substantive, since the star is presumably his primary object of study. The telescope is merely his tool; any statement he makes about the telescope is methodological. To an optical physicist, the situation may be quite the reverse, of course, but we make this distinction in order to point out that, e.g. in marketing, an abundance of published articles on methodological advances (conjoint measurement, database management, etc.) disguises the fact that we are learning very little about actual markets.

The process illustrated in Figure 2 drives the unbalanced progress we now wish to consider. Thus, science progress when any one of its four components – substantive theory, data, methodology and problems – advances by building on the current state of the other three [11].* In a continuing game of leapfrog, any one of the four sectors may experience a breakthrough (at a moment when the others are less active), leaping to the forefront and often spurring support activity and progress in the other three. Anew methodology may lead to new substantive theory (or explanations) as did Newton's calculus reformulation of Galileo's "odd integer law" of falling bodies, which, in turn, enabled Newton to progress from pure prediction of astronomical bodies, as Kepler had previously done, to "controlled preictions," as in his calculations of the trajectories and escape velocities needed to achieve Earth orbit. New methodology can also lead to both new data and new theory, as witness, for instance, the germ theory of disease, which could not emerge until the microscope (a methodological advance) revealed a new body of previously unobservable data.

Where do we stand today? Figure 2 suggests the current state of marketing: many unsolved problems, lots of data, methodology making advances on the use of data, and substantive theory lagging far behind. A similar situation exists in astronomy and planetary science: astonishing methodological successes in space probes and space telescopes have resulted in gigabytes of data that have not yet been satisfactorily analyzed.

Of course, these advances need not occur in one jump. A succession of jumps may produce similar effects. The clock, for instance, evolved into ever more accurate versions in response to a succession of problems. First, according to Boorstin [5], the clock was needed to gauge the hour of commercial meetings. Parties to a transaction traveled great distances to meet a place, data and hour that were arranged the previous season, with no communication possible in the interim. Later, James Burke tells us in an episode of his PBS series *Connections,* monks working in the fields needed to know the proper hour to gather at the abbey for prayers. The invention of the alarm colck, a difficult project beset by comical false starts, was occasioned by the monks' need eo wake for midnight prayers. Later, astronomers used and elaborated these advances to accurately time stellar and planetary movements. Better known are the efforts during the age of navigation to time a ship's journey so as to measure longitude.

Different writers ascribe primary importance to different frogs. According to Price [29],

> By far the most common way [to effect an advance] is that you get a new machine, a new instrumental technique. Somebody gets a radio telescope and measures the velocity of light, or markes a new substance like Silly Putty … Take the discovery by Francis Crick and James Watson of the duble helix structure of DNA. Here I focus upon the superb technical contribution of Rosalyn Franklin, who could make very good X-ray photographs of mucky little wet organic crystals. Franklin provided the evidence that enabled Crick and Waston to see their conclusion. Once you get such a piece fitted in, you get a chain reaction of things that follow from the new knowledge. The technical abilities to make the X-ray photographs of tiny, fragile, wet, large (sic) molecule crystals that Franklin obtained enabled us to get the new theory. That enabled us to get new techniques of genetic engineering, on the one hand, and all of our understanding of the way that living matter really works on the other.

Pacey [35], whose book focuses on the historical transfer of innovation from place to place, give equal weight to "survival technologies" (i.e. problem-driven innovation); communication of innovation via trade, and its adaptation for local use; and the "dreams, fantasies and poetry of a society or its rulers." The latter is exemplified *inter alia by* 9^{th} century Islamic culture's poetic fixation on gardens – which led to ever finer technologies for fountains. James Burke taskes a more extreme view, giving problems star billing in the pageant of technological progress. Burke sees any imbalance of supply and demand as a prblem that ingenuity solves by creating new markets or new technologies.

PROBlEM-DRIVEN RESEARCH

We have indicated that progress in science can come at any time from either of the sources associated with the four frogs depicted in Fig. 2, and that this may occur in a one-leap breakthrough or in a succession of smaller jumps. New we focus on management and marketing science, and argue that progress here is best served by application-driven research on real-world management and social problems.

Defenders of pure research cite the progress made as a result of work on problems that scientists have constructed from criteria or responses to others which are wholy internal to their research disciplines. They may say, "Why should we turn our attention to management problems, when our fertile imaginations provide us with problems sufficient for a hundred years of investigation? Just be patient, and you will, in time, see the results of our labor." Indee, the public suspects scientists of a certain narrowness in selecting problems for investigation. To paraphrase an often-heard criticism of social scientists regarding their selection of problems: "Science and the scientific establishment attend only to problems for which a theory or a methodology exist, to problems which stem consequentially from ongoing areas of research. There are things going on in the world which should be of interest to scientists and are worth investigating; yet scientists label these areas 'unscientific', and resolutely ignore their existence." This is reinforced by Kuttner [25]:

> An equally serious consequence of the professional obsession with model making is that the most pressing economic questions lie outside the frame of reference. The issues that standard economics can't explain and doesn't address are of far greater moment than the ones 'solved' by the formal proofs. A non-economist reading the economic journal is struck mainly by what is left out. The literature of standard economics recalls Tom Stoppard's *Resencrantz and Guildenstern Are Dead.* Minor subjects have usurped center stane, while the truly important ones remain tantalizingly out of view.

This ciriticism has some merit. Kuhn [24], as we have noted, highlights the rarity with which a scientific community re-examines its basic conceptual categories. But one may reply that, unlike other sciences, management science is routinely forced to deal with areas for which there is no prior theory, methodogy, or data. See, e.g. Charnes *et al.* [11], who describe a management science response to a government agency's expressed need to evaluate the efficiency of its programs. Concepts from economics and engineering were assembled as a basis for a new operational meghodolgy, later called Data Envelopment Analysis, designed to evaluated the activities of not-for-profit entities. Data could be collected only after the new operational constructs were defines. New databases and new theoretical speculations proliferate on the efficiency of not-for-profit enterprises. Another criticism, "If science can put men on the Moon, why can't it feed the hungry?" has much less merit. Space flight medical telemetry and satellite imaging are examples of the "problem-oriented research" we describe below. The transfer or generalization of these innovations to (respectively) ordinary hospital use and to agricultural monitoring, and the lives saved thereby, do not seem to be widely understood.* Although improving the public image of science is at most a secondary objective of this paper, we do want to emphasize the potential contribution of problem-oriented research to this end. Wider appreciation that management problems spur dcientific action might help conteract the public's perception of science as an ivory-tower game. In this regard, wouldn't it be better if managers were the most vocal and publicized suggesters of new scientific problems?**

Furthermore [19], people with problems tend to take action on those problems – with or without scientific support. Without scientific presence to observe, record and advise, actions will generally result in missed opportunities. Finally, the puzzles facing scientists – the origins of the iniverse, global warming, economic davelopment – attract bright students to scientific careers just as so and their articulation as elements of scientific progress.

New scientific problems arising outside the laboratory are vital for the progress of the process depicted in Fig. 2; yet many scientists disdainfully refuse to work on applications. Table 2 develops the idea of problem-driven (or problem-oriented) research, contrasting this mode of research with applications. A problem-driven research project is generally characterized by dual goals: to solve the management problem at hand, and to subsequently ues what is learned to enrich and advance science. The early and continuous involvement of managers in the research project, and a sensitive approach to implement of managers in the research, at leasst in marketing and management science. Althougy this approach has been central to the activities of many of the institutions with which the present authors have been affiliated, it is by no means true that all management science is performed according to this model. However, we believe that its benefits include (i) enhancing the status of science and scientists among practicing managers; (ii) accelerating the progress of management science; (iii) increasing the proportion of scientific contributions actually used by enterprises; and (iv) providing a "scientific method" for management science. The first and third benefits are related. They stem from the active and interactive partnership between scientist and manager that is central to the problem-driven philosophy of management science.

Problem-driven research accelerates the progress of management science because, according to the leapfrog model, breakthroughs in theory, methodogy or data may drive scientific progress, but these are rare. *There is never a shortage of real-world problems.* These problems can drive progress in science if addressd by problem-driven researchers. Not incidentally, their solution often leads to potentially generalizable knowledge, sometimes to commercial benefit, and uaually to a better public image for science.

*The Department of Defnese objective of "dual-use technologies" is oriented to such transfer, although this is a narrower concept than problem-driven research.

**This applies within the proper public relations framework, of course. It woukd hardly do for a company to bill itself as "XYZ Corp.: Solving Today's Problems and Creating Tomorrow's!"

Following is an outline sketch of the scientific method we have drawn from the problem-driven research approach. Of course, some of its guidelines are also useful for performing straightforward applications.

1. Determine that a problem is new or has significant subproblems that are new.
2. In collaboration with an involved manager, distinguish the pertinent elements of the problem.

Identify the goal (or set of goals) that will be served by solving the problem. Examine available data and hypothesize about variate relaitonships.

3. Devise a prototype model for the procticing manager's approval. If he or she see that the prototype captures the major problem features and may significantly serve the specified goal, then collaborate with the manager to plan further data collection. Begin to abstract the significant mathematical innovations in the model.
4. Collect data. Use the data collection activity to give mor eof the enterprise's workers a chance to "sign on" and have a stake in the scientific solution of the problem. Recognize that the means of organizing to collect the data will have parallels with the ways to organize for implementation of the solution.
5. Refine the second-stage model and its solution algorithm as needed. Implement the production version; hand off to the collaborating manager, who is now its "champion."
6. Refine the mathematical contribution for journal publication – with the manager as co-author. Look for paralledl problems in other management areas that will lead to further refinement and abstraction of the new method, and constructive solution of additional management problems. Confer with scientists who are competent to effect these refinements.

The following passage from Phillips [36] further clarifies the meaning of problem-driven research.

> In the fourteen years since its founding, the [University of Texas] IC^2 Institute has focused research attention on many relatively unexplored or unstructured aspects of the commercialization and implemention of innovations, and related areas. Several individual Fellows of the Institute, and their collaborators, have had active research interests along these lines for many years prior to the Institute's founding, and significant portions of this research have occurred in the context of solving real problems for real organizations. Thus it has been clear for decades that measurement problems arise in these areas that cannot be solved on the Procrustean bed of multiple least-squares regression and other standard econometric methods. These researchers have responded to the challenge of application-driven or problem-driven research by inventing and/or refining techniques that may be described as new, creative, and sometimes unorthodox alternatives.
>
> Happily, but by no means coincidentally, these new methods are applicable to wider classes of problems than those for which they were originally devised. This means their devising was a *scientific* activity as well as a *technological* one, and that the act of solving the original problem was *research* as well as *application.*
>
> Methodology is "the science of methods," according to the *Oxford English Dictionary.* In current academic usage, the word has also come to mean "a system of methods, especially one having wide applicability." For problem-driven research to lead to a new methodology, the researcher must have not only problem solving skills, but the skill to recognize the mathematical structure at the core of applied problem's solution and to elucidate its properties and implications for applied mathematics and for other application areas.

Among the examples of problem-driven research featured in Phillips [36], perhaps the most instructive is "constrained regression," a contribution due to Abraham Charnes and W. W. Cooper. The immediate problem was that of fairly compensating excutives at the General Electric Corp. in the 1950s. Because ordinary regression on the current salary structure resulted in a negative coefficient for seniority (and management judged this undesirable), it was evident that the compensation problem could not be solved by a formulaic application of traditional regression methods [10]. The then-new methodology of linear programming provided a means of computing a regression line with the seniority coefficient constrained to be positive. The resulting constrained regression technique proved eminently generalizable and was the seed for a much broader methodology, goal programming. Goal programming was an apt testing mechanism for Simon's "satisficing" theory, and led to many applications in a myriad of organizations and subject areas.

A Director of Bell Laboratories, when asked what makes a great research lab, said, "We keep the researchers close to the problems" [45]. In a private communication, W. W. Cooper responded to Kuttner's [25] critique of economics (parts of which we have quoted in this paper): "Economics needs some channel for having real problems thrust into the discipline with responsibility for the attendant consequences fixed upon those who do (or don't) address them." And finally, we quote senior plicy analyst Christopher Hill of the Congressional Research Service: "I think technology's driving of science is an important policy issue because it makes nonsense of the idea that science is driven only by its own ideas. The practical areas were always important. I think we would be surprised by the amount of Nobel Prize research that was undertaken with the aim of a practical outcome [26]."

Table 2. "Problem-oriented research" and "applications" compared

Problem-oriented Research	Application
Real-world problem spurs development of new methodology and/or theory.	No new theory or methodology is generated.
Real-world problem is solved.	Real-world problem is solved.
Generalizable. Parallels can be drawn to problems in other fields.	Few or no parallels to other field.
Takes longer.	Usually fast and straightforward.
Manager with problem becomes part of scientific process. Manager becomes "champion" or "change agent," mediating the implementation of the project and the feedback of results.	Solution is "dropped on" the company (although implementation may be handled sensitively).
Leads to more and deeper research.	When it's over, it's over.
Tighter integration of science and practice interaction results in faster commercial progress.	Chance encounter of scientist and manager results in inefficient interaction.
Iterative process involving problem definition, model, prototyping, etc.comprises "scientific method" for the management sciences.	One-way process, useful only when problem is well-tested – both prior to the application.

Table 3. Cooper's classification scheme

	Pure	Applied
Basic	Gödel's theorem	Pasteur's immunology
Nonbasic	Medieval schoolman: locating angels on a needle	Modern dean: locating faculty in offices

THE COOPER MATRIX: DYNAMICS AND IMPLICATIONS FOR TECHNOLOGY DEPLOYMENT

The leapfrog model suggests that theory, data, methodology and problems do not advance synchronously or in lock step. One of the four typically advances at a moment when the others are not advancing. But what makes a frog jump? In a child's game of leapfrog, the second frog jumps because the first one has jumped. To wring a little more from the metaphor, the second frog jumps because he has seen the first frog jump, and he perceives the first frog's leap as relevant to his own interests.

In terms of scientific progress, an advance in, say, data does not benefit science as a whole unless it is effectively communicated and it is seem as important other scientists' pursuits. The issue of relevance or importance to other scientific fields we shall call generalizability. This section of the paper discusses the interwined issues of generalizability and communication. Table 3 is taken from Cooper's [14] account of academic planning within Carnegie-Mellon University's School of Urban and Public Affairs. Here we interpret the table more broadly to pertain to research policy and scientific history, and find that it complements the leapfrog model by emphasizing question of generalizability.

Cooper defines pure research as the pursuit of knowledge for its own sake. Applied research, he goes on, pursues knowledge which is wanted for its potential usefulness. These definitions are well-known and seem self-evident; yet they are often confused with the ideas of "basic" and "nonbasic" research. The former, writes Cooper [14], deals with foundations that open new paths upon which to build further research. That is, basic research is generalizable. Nonbasic research "does not lead anywhere beyond the instant problem."

Table3, by crossfooting these definitions, implies that there are four kinds of research. Pure research may lead to avenues of generalization and further research (as Gödel's theorem did), or may not. Pasteur's immunology (basic-applied) led to further scientific progress. The allocation of faculty to offices (applied-nonbasic) can use complex but well-known techniques. It would not lead to further advance in science, and so would not be a high-leverage use of research funds. Contrast this classification with that used by U.S. government funding agencies [20]: "… three standard categories of R&D – basic research, applied research, and development."

As a given line of inquiry progress it may change its character, migrating from noe cell of Table 3 to another. This is how pure research eventually becomes usable – often via new technologies that have been introduced, or through the other process of Fig. 2 and its following discussion. For example, the discovery of microorganisms via the microscope reopened the pure question of the spontaneous generation of life froms.

Controlled experimentation revealed that material which had been heated and sealed later showed no growth of microbes. These experiments led to the preservation of food by canning [13] which had social implications far beyond its obvious impact on marketing. For instance, the Northern victory in the Civil War was partially due to the greater mobility of the Union Army, which carried canned food while the Confederate Amy did not [39].

But movement occurs in the other direction, too; nonbasic research will surprise us by being more general than we anticipated. The Egyptian who first used measurement to delineate his landholdings after the Nile flood receded (an activity in the nonbasic-applied cell), had he or she lived long enough, would have seen this act grow into the sciences of surveying and cartography (basic-applied) and thence in Hellenic times into geometry (basic-pure). Hill [20] goes on, "On the other hand, R&D programs intended to develop ideas into products, or to solve important social problems, can enhance our fundamental understanding of the universe. For example, research on improving combustion in automobile engines can reveal new insights into basic principles of molecular bonding. But because such studies are motivated by practical concerns, they are labeled 'applied research' and are often not considered appropriate for federal support." However, nonbasic-pure lines of inquiry (like conuting angels on pinheads!) are likely to remain so, becoming neither more general nor more usable.

Pure research yields ideas that may not find application in the near term. It also yields ideas of the type we have elsewhere characterized as "useful" (and placed in opposition to ideas that are "use") [11]. These useful ideas, rarely defined operationally or suited for measurement, can shape our culture – this, indeed, is another indication of the tremendous value of pure research. A favorite example is the "rational man hypothesis." While no one regards this hypothesis as operational or even strictly accurate, it has led to much of the free enterprise prientation of our society and, at the same time, to much of the theory-based economic regulation with which we live.* Likewise, price elasticity is a concept familiar to and taken for granted by every marketer. Yet it is a "pure" concept in that a marketer, aware that price effects are confounded with promotional and other market forces, will rarely attempt to compute an exact elasticity for a particular product preparatory to makin ga marketing decision.

As for the communication issue, there are three channels along which the exchange of management science results is desirable: between scientists and practicing managers; and between scientific disciplines. The volume of scientific publication is now such that an investigator may not know about the existence of much relevant prior work. Thus the impotrance of indexing and abstracting services, electronic database and rapid E-mail messaging, conferences of new interdisciplinary interest groups, university industry associate programs, and so no.

Science without communication is merely craft. Indeed, new mechanisms of scientific communication almost quality as a fifth frog in Fig. 2. In the academic-practitioner channel, the Requests for Proposals (RFPs) of the Advanced Technology Programs of the National Science Foundation (NSF) and the Texas Higher Education Coordination Board require the investigator to specify how project results will be transferred to industry [32, 46]. In other words, innovativeness in communicating results is no longer of minor importance compared to innvativeness in suvstantive research.** As Cooper's definition of "pure" and "appiled" seems to correspond closely to research that generates "useful" and "used" ideas, respectively, and as his idea of "basic" implies a value beyond the immediate application, practicing managers can readily understand research priorities based on this four-way classification. The applied/basic cell of Table 3, equivalent to what we have called problem-driven research, will appeal to them especially. The pure/nonbasic cell is, however, probably of little relevance to practicing managers.

*The equilibrium model of competition is "obviously … unrealistic, and unhelpful in understanding markets …" [25].

**Derek DeSolla Price asked, "How big is the job of tracking total scientific knowledge?" Price's second passion (after antique scientific instruments) was the Institute for Scientific Information in Philadelphia, "the biggest computerized data bank in the world." [29]. In the connection note also the NSF's data archiving policy" [33].

Few scientists are able to address all the mathematical, theoretical, algorithmic, informatic and implementation issues implied by a new management science problem or technique. Nor are they generally conversant with all the developments in other fields that may be brought to bear. Hecne, the importance of communication within and between disciplines. Cross-disciplinary communication takes on greater importance as investigators in one disciplines unknowingly duplicate much of another field's knowledge – wasting their own time as well as valuable research grants.* Eric Bloch's NSF's recognized the value of institutionalizing interdisciplinary discourse via the centers for engineering research (see, e.g. [21]). NSF's Decision, Risk, and Management Science (DRMS) Program leaflet also suggests such a view, endousing "research directed at increasing the understanding and effectiveness of problem solving, information processing, and decision making by individual groups, organizations and society. The overall objective of DRMS is to build an interdisciplinary science base of decision making and management. This includes descriptive and prescriptive research." Texas' Higher Education Coordination Board, also, looks for a proposal's "leveraging of funds from other sources; innovative, collaborative efforts across academic disciplines, among two or more eligible institution or institutions and private industry; and potential for commercialization and technology transfer [46]." Zaltman [47] rasies another virtue of interdisciplinary study: "The [consumer] behaviors we want to study do not conform very well with customary disciplinary divisions. Accepting disciplinary concepts creates the polite fib that we are using the appropriate frame of reference when strdying consumers. Evidence exists that we are not. As a result of this polite fib, we systematically overlook certain envents."**

Cooper's seemingly simple matrix thus implies a framework for many instances of scientific, managerial and technological evolution, and resolves the confusion of terms that ocnfounds Hill [20] and other writers in the area of research polict. Its emphasis on generalizability complements of scientific progress, broadents the picture that Cooper's matrix provides.

SUMMATION

We have directed much discussion toward clarifying issues of pure vs applied and what drives scientific progress. This is because we agree with Broad [7] that "the seemingly arcane issue bears heavily on federal policy. Whether the government should increase the financing of pure science at the expense of technology or vice versa often turns on the issue of which is seen as the driving force … The whole issue … lise at the heart of an ongoing debate over how to spend billions of federal dollars in order to best spur innovation in research."

Little management science is performed in a laboratory environment. Theory is moving away from the universal and teward a treament of the varied goals of indiviuals and interest groups – and so is becoming, with the help of computers and databases, less idealized and more applied. Theory and application are no longer to be placed in opposition; through the mechanism of problem-driven research thet can operate in tandem, wherein an advance in theory, methodology, data or problems feeds back to the benefit of the other areas. Indeed, management practice is an important source of new scientific problems, and so must be regarded as an integral part of scientific progress.

The basic/nonbasic distinction is more relevant for research ploicy than the pure/applied dichotomy. Support for research should be based on its generalizability, regardless (or nearly so)of its pure or applied nature.

*However, according to Hill [20], "We must be careful about accepting th epremise that it is wasteful for firms to duplicate R&D efforts and more efficient for them to cooperate."

**More testimonials to the value of interdisciplinary research come form McKelevy and from McLafferty, the former implying that interdisciplinary approaches are especially applicable to problem-driven research. "Under these [wartime pressures], engineers and scientists had to work and even live to gether, and they often had to do each other's jobs.

Disciplinary lines usually meant little or nothing [30].” “Modern instrumental methods … have originated from a wide variety of disciplines, often in combinations, including spectroscopy, unclear and ion physics, electronics, computer science and biology. Development of such analytical instrumentaiton thus usually requires competence in other relevant disciplines without diminishing the requirements in chemistry [31].”

Within the set of generalizable activities, due but sceondary regard should be given to the unbalanced growth of theory, methodology, data and problems to ensure no one of the four lags the others excessively*. For example, new competitive forces have led to new problems for marketing managers who are already overwhelmed by the quantity fo available data. New economic theories are beginning to address the new realities of global competition. What is needed is new methodologies to integrate and comprehend massive data sets. Such new methodologies will serve to test the new theories and to solve the operational problems fo managers.

It would be wasteful to attempt to over-manage a nonlinear process like the leapfrog advance of science. Boorstin [5, p. 52] reminds us that “One of the most effective uses of public funds to advance science and technology was the prize that … was announced by the British parliament in 1714 for a practical way of finding longitude at sea.” Galileo had undertaken research based on his perception of Dutch sailors’ need for such a technology, and a Spanish king had offered a similar prize. As it would today, the announcement of the prizes attracted cranks, and set off acrimonious competition among legitimate scientists. But Boorstin stresses that the ultimate success of the British prize was due to the fact that parliament specified *what* was needed, but did not dictate *how* it was to be done.

In an unsettling example of Lessons Not Learned From History, a recent United States Coast Guard Small Business Innovation Research (SBIR) solicitation etitled “Kalman Filters for Loran-C Geodetic Positioning” announced funding for research “to convert Loran-C Time Differences into a lattitude/longitude (sic) position … The Kalman Filter is viewed as a possible resolution …”** Gilder [17, p.93] quotes integrated circuit pioneer Carver Mead: “We depend on the innovations of the citizens of a free economy to keep ahead of the bureaucrats and the people who make a living on controlling and planning. In the long term, it’s the element of surprise that gives us the edge over more controlled economies.” Former Presidential Science Advisor George Keyworth [21, p.50] adds, “We also concerntrated funding in areas most likely to benefit society or where we could sense pure scientific excitement waiting to be turned loose.”

NSF’s Decision, Risk, & Management Science Program leaflet indicates DRMS will evaluate proposals based on “(1) research performance competence; (2) intrinsic merit of the research; (3) utility or relevance of the research; and the effect of the research on the infrastructure fo science and engineering … Research should (a) have relevance to an operational context, (b) be grounded in theory, (c) be based on emporical observation or be subject to emporical validation, and (d) be generalizable.” [32]. These statements are reasonable for the most part. The combination of “relevance to an operational context” and “generalizable” can be interpreted in a manner consonant with problem-driven research, though one might wish this to reflect the problem-driven scientific method more explicitly. “Effect on the infrastructure of science” might be read to encompass communicating results and inciting other frogs to leap. “Grounded in theory,” though, is a potentially obstructive requirement, because of the importance to management science fo new problem areas for which theory does not yet exist. Then too, unfortunately, the emphasis on “operational” and “empirical” seems to exclude research of the “pure” variety.

We hopefully look forward to fuure DRMS guidelines that will explicitly encourage (i) research into new problem areas; (ii) basic research, in the sense defined by Cooper, which can include some pure research; and (iii) models that move beyond mechanistic approaches, to address the manager's role and the volition, mixed motives and sundry complexities that characterize that role.§

*As has happened in economics, according to Kuttner [25]: "… deduction drives out empircism. Those who have real empirical curiosity and the insights about the workings of banks, corporations, production technologies, trade unions, economic curiosity and the indights about the workings of banks, corporations, production technologies, trade unions, economic history, or individual behavior are dismissed as casual empirics are graduating a generation of idots savants, brilliant at esoteric mathematics yet innocent of actual economic life."

**The complete solictation read, "Research is needed no utility to convert Loran-C Time Differences (TDs) into a lattitude/longitude (sic) position within a given geodetic coordinate system in real time. Methods such as 'Sodano's Iterative Algorithm' have previously been pursued, but have proven to be excessively noisy or unresponsive in situations problems. The filter should be capable of operation with TDs from two or four trasmitters within the same Loran chain (same GRI). Cross chain (multiple GRIs) capability will enventually be necessary."

§Buchanan [8] predicts, "Game theory's search for solutions to complex interactions under complex sets of rules will surely replace extensions of general equilibrium analysis at the frontiers of formalism."

Encouraging in this regard is the Joint NSF/Private Sector Research Opportunities Initiative, under which DRMS has already made some awards. This initiative "… is designed to encourage theory building through applied studies in private sector settings … University researchers and educators will benefit from exposure to the problems private sector organizations currently face." Proposals to the initiative must contain a section entitled Generalizability of Research Project, explaining "how the proposed research can be generalized to other contexts to transcend the problem of interest to the Cooperating Organization." [33].

In this paper we have tried to clarify issues such as the above while proposing and explicating a constructive, practicable model of management science. The model is intened to serve as a framework for the funding and conduct of research. Timely and carefully funded policy and scientific attention these issues will spur scientific advance while ensuring that the United States will continue to generate the economic surplus that makes pure research possible.

Additional, related thoughts are offered in the Appendix.

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APPENDIX

An Afterword on Marketing Science

We close with a view on pretensions to "marketing science", and thoughts on how a marketing science would fit into the scheme presented above.

Marketing is a verb; the name of the endeavor is not marketology, marketry, or marketics. The parallel with accounting and engineering is clear. In both of the latter fields, applied research has a successful and respected history (industrial processes, organizational reporting). Yet they have not yielded nor attempted to yield universal laws of behavior (although some of their principles are remarkably general).

Slaughter [43] discussed engineering science in his address to the American Association for the Advancement of Science meetings in Toronto in 1981, defining it as "… the theoretical body of knowledge and technique underlying the practice of engineering. Much of this work is every bit as fundamental in character as corresponding work in the disciplinary sciences … Although engineering was born as a practical and intuitive art, today it embraces a range of knowledge from the highly theoretical to the very practical."

Contrast this definition of engineering science with Hall's [18] recollection of the goals of management science: "… the original goal of management science as a synthesis of all the sciences applicable to management, not just mathematical representation of management decisions. One step in this direction may be the recent creation of an operational science unit in the National Science Foundation …" Hall describes a process of bringing-in-from-outside. This notion differs from the building-up-from-within process that Slaughter describes.

Can we profit from making a parallel between the above and recent attempts to delineate a "marketing science?" Little [28] writes of marketing decision support systems as "collectively … an advance in management science, or more specifically marketing science," implying that the latter is a subset of the former.

There is a pure and an applied trend evident in academic marketing studies. Mos t of the latter is methodological rather than substantive, as our model suggests it must be. All of it of course strives to be basic; yet there is a very large situational, or nonbasic, element in the lives of marketing practitioners. This split characterizes the gult between marketing practitioners and academics. The gult is widened by insufficient blending of "new problems from practice" with "new problems from pure theory," as well as indufficient communication concering what is controlled in the marketing environment and what is not. Academics mistake methodology for the bridge between town and gown. Methodology is not synonymous with implementation or technology transfer. The latter requires equal parts methodology, theory, data, and a responsiveness to organizational imperatives.

Should we define marketing science using a combination of Little's and Hall's definitions, i.e. "the collection of all scientific ideas having a bearing on marketing?" This may be too easy a route to scientific legitimacy for marketers. Or should we strive for a harder-to-achieve definition of marketing science that would parallel Slaughter's definition of engineering science? If so, it is premature to speak of an extant marketing science.