

# Mitigating risks, visible hands, inevitable disasters, and soft variables: Management research that matters to managers

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## Executive Overview

*Management researchers lament the fact that their work has so little impact on management practice. Practicing managers, so it is claimed, search for knowledge that will help them improve organizational performance but rarely consult the work of university-based researchers—work that they often find incomprehensible and irrelevant to their day-to-day challenges. Researchers assert that rather than being interested in systematic and long-term solutions, managers are generally infatuated with the latest fads and fashions in their search for quick fixes.*

*We contend that management research can matter to managers, but for this to occur requires mutually beneficial partnerships involving managers and researchers, as well as the support of their organizations. To support our contention, we illustrate the importance of practice-relevant management research by drawing on four important contributions to management understanding that were prompted by the organizational experiences of a group of inquiring managers and curious researchers. Together these illustrations not only demonstrate how partnerships between practicing managers and management researchers can yield practice-relevant knowledge, but also provide insights into enhancing the likelihood that productive encounters will occur.*

Practicing managers appreciate the value of knowledge as a source of potential competitive advantage. Unfortunately, the search for practice-relevant knowledge rarely results in encounters between managers and university-based management researchers.<sup>1</sup> Instead, this search has resulted in the creation of a \$15 billion consulting industry in the United States alone. Another billion dollars is spent each year by managers on business books, very few of which are written by university-based management researchers.<sup>2</sup> In 2001, only 10 percent of *Business Week's* "Top Business Books" were authored by academics.<sup>3</sup> The same was true in 2002.

Management researchers lament the fact that their work has so little impact on management practice. They assert that many managers are

more interested in quick fixes to immediate problems than systematic and long-term solutions for enhancing organizational performance. Pointing to the sales figures of business books, management researchers contend that among practicing managers there is an almost unlimited infatuation with management fads and fashions.<sup>4</sup> They also warn that these fads and fashions may actually do more harm than good by overshadowing established management concepts and applications that have potential for improving organizational performance.<sup>5</sup> These same researchers further contend that to yield the maximum benefit, the results of their efforts should not be directed toward the mundane day-to-day events in a manager's life, but instead should be used to address long-term management challenges.<sup>6</sup>

## Management Research That Mattered to Management

Managers also have a point! Donald Hambrick, in his Presidential Address to the Academy of Management, asked the question, "What If the Academy Really Mattered?" He suggested that it did not matter much to managers because "Each August, we come to talk with each other; during the rest of the year we read each other's papers in our journals and write our own papers so that we may, in turn, have an audience the following August: an incestuous, closed loop."<sup>7</sup> There is, however, no *a priori* reason why the dissemination of management research should continue in such an incestuous state. Whereas one doubts that there will come a time when all management research will be or should be directed toward improving practice, as an applied discipline surely some management research should have real-world relevance. And, in fact, there are many examples of how managers have accessed and benefited from management research, as well as how researchers have either responded to challenges faced by actual practicing managers or have drawn on their own workplace experiences to investigate such challenges.

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To illustrate, J. T. Carney worked his way to the presidency of Sears, Roebuck by being a hands-on manager. He liked talking to employees in the mail-order plant, finding out what they thought about their jobs, and offering suggestions to aid their work. In his Chicago office, however, he felt out of touch with field operations. So, he instructed James Worthy, one of "his bright guys" in personnel, to "figure out a way to keep me informed on that important part of my job."<sup>8</sup> Working with scholars from various disciplines and institutions, workforce surveys conducted under Sears, Roebuck sponsorship formed the foundation of Worthy's classic articles on the influence of organization structure on employee morale that appeared in the *American Sociological Review* and the *Harvard Business Review*.

In addition, Worthy noted that the published results of the Sears surveys significantly impacted future management thought by influencing the research of academics such as Rensis Likert, Wright Bakke, Elliot Jacques, Douglas McGregor, William Newman, and Peter Drucker. Moreover, these results provided Worthy with opportunities to lecture

at Chicago, Columbia, Harvard, Michigan, MIT, Stanford, and other universities, further reinforcing an industry/university connection.<sup>9</sup> Thus, the concern of an "out-of-touch manager" led to an innovative solution that continues to influence management research.

What if more organizations undertook such practice-relevant research and used it to inform their management practices? What if more university-based researchers could find organizations as receptive as Sears to engaging in (and sponsoring) such research and using the results to improve their operations? How much money could organizations save on consulting fees and the unrelenting pursuit of quick fixes? What would it be worth to researchers to know that they were really having a positive effect on management practices?

Reflecting on these questions, this article illustrates the importance of practice-relevant management research by drawing on four important contributions to management understanding that were prompted by the organizational experiences of a group of inquiring managers and curious researchers. In doing so, the illustrations we present each highlight a challenge. All the collaborative encounters to be discussed were more accidental than intended. The challenge is to understand ways of increasing the likelihood of productive manager/researcher partnerships that are mutually beneficial and reinforcing. Basing our suggestions on lessons learned from each illustration and calling upon findings relating to the transfer of management research to practice, we offer an agenda for meeting this challenge.

## Origins of Influential Management Ideas: A Starting Point

Where does one go to find the genesis of the ideas that influence management theory and practice? One logical starting point is an examination of the lives of the individuals who developed and conducted research on the theories and, in turn, originated the ideas that have influenced contemporary management practices. We identified two principal sources for this type of information: biographical and, more valuable for our purposes, autobiographical.

### ***Biographical Insights***

Gaining insights into the lives and ideas of the members of distinguished groups by studying their common biographical characteristics has a long tradition in the sociology of science.<sup>10</sup> Biographical studies have yielded insights into the lives of No-

bel laureates, presidents of the United States, Olympic athletes, chess grandmasters, psychologists, and movie stars. Perhaps the best-known biographies in the management literature are Lyndall Urwick's original *Golden Book of Management* and an earlier publication by Urwick and Brech that likewise emphasized the importance of biographies in understanding the evolution of management thought.<sup>11</sup>

Much of Urwick's interest in biographical research was predicated on the desire to understand management, like other fields, as a cumulative flow of ideas. The same argument was made more recently by Witzel in the preface to his biographical dictionary of management, when he stated: "We came to the conclusion that our main interest was not in *managers* as individuals, but in *management* as a set of ideas and practices. We wanted to see if we could use the study of people to tell us more about their ideas."<sup>12</sup>

### **Autobiographical Insights**

Because of their reliance on secondary sources, biographical investigations provide limited insights into the origins of ideas. Autobiographies, on the other hand, as self-exemplifying exercises, offer a unique means for gaining behind-the-scenes insights that are especially valuable in understanding the personal experiences of their authors as well as the connection between their life experiences and the development of their interests and ideas. The autobiographies of various business leaders, for example, have allowed them to describe in their own words the events and people that shaped their lives and brought them into leadership positions in industry.<sup>13</sup>

The value of autobiographies is highlighted by the fact that in a very real sense, autobiographers are the "ultimate participants in a dual participant-observer role," having privileged access to their own inner thoughts.<sup>14</sup> By providing self-relevant information that is direct and not second-hand, the autobiographer is better qualified than anyone to document thoughts and experiences that are unobtainable from other sources. Research indicates that autobiographical accounts of personally salient and specific episodes or events are typically more accurate than recollections of more general and emotionally neutral experiences.<sup>15</sup>

This is not to suggest that there are no potential problems with the use of autobiographical data. Autobiographies, for example, may be self-serving and inaccurate. Details may be forgotten or misremembered, and completely new details may enter into one's memory.<sup>16</sup>

### **Management Research Autobiographies**

Bedeian's six volume series titled *Management Laureates: A Collection of Autobiographical Essays* provides a unique database for studying the autobiographies of a sample of individuals who have made significant contributions to the field of management.<sup>17</sup> Included in the series are individuals who have been instrumental in the development of theories that have led to innovative management practices. Contributors were selected for inclusion in the series because they were considered to have had a significant impact on the field of management and to be longstanding observers of management research and practice.

We examined the autobiographies of 52 laureates contained in the Bedeian series to identify those experiences, events, and people that had significantly impacted the formulation of their ideas regarding management.<sup>18</sup> Toward our goal of illustrating the importance of practice-relevant management research, we wanted to gauge the extent to which the laureates' own organizational experiences, as well as their interactions with practicing managers, had aided in shaping their ideas about management and their research.<sup>19</sup> The analysis confirmed that the laureates, when discussing influences on their careers, recalled specific places where influential events occurred as well as individuals who influenced their thinking.

### **Management Practice and Management Research**

Table 1 provides a list of four important areas of practice-relevant research that resulted either from the organizational experiences of a laureate or evolved from a laureate's interactions with practicing managers. Each area is discussed in some detail to illustrate the benefits that can accrue to managers and researchers from this type of reality-based research. Lessons learned from each experience are briefly noted.

#### ***High-Reliability Organizations, Mitigating Risks, and the Nuclear Navy***

Modern technology has given rise to new forms of organizations. Management, as a formal discipline, has been repeatedly called upon to update its prescriptions in light of changing environmental conditions. Fortunately, organizations have evolved to deal with the increasing demands of ever more complicated environments. Today, a special type of organization, the high-reliability organization (HRO), has created special challenges for those who manage. These organizations

**Table 1**  
**Selected Examples of “Practice-Relevant Research”**

<b>Organization Where Research Originated</b>	<b>Person Suggesting or Championing Research</b>	<b>Research Generated</b>	<b>Individual Who Conducted the Research</b>	<b>Significance of Research</b>
United States Navy—USS Carl Vinson	Admiral Thomas Mercer, Second Captain of the Carl Vinson	Since 1987 research used to inform practice has defined and refined management of high-reliability organizations (HROs) including how these organizational forms are structured to mitigate risks, etc.	Karlene H. Roberts	Aids in understanding the increasingly important class of organizations that operate under trying conditions but manage to have fewer than the expected number of accidents. Examples of these organizations include power grid dispatching centers, air traffic control centers, nuclear power generating plants, submarines, aircraft carriers, and hospital emergency rooms.
Three Mile Island Nuclear Plant	Cora Marrett—Commissioner appointed to direct a study of the Three Mile Island disaster	Studies led to recognition that individuals have created catastrophically risky technologies that cannot be managed. Suggested that “normal accidents” happen in all systems, but when relatively small, independent systems fail, the damage can be contained. As systems become larger and more interdependent, irretrievable system failures occur.	Charles Perrow	Irretrievable systems failures such as the Challenger disaster and Union Carbide’s Bhopal, India chemical release accelerated the concern for and urgency of “thinking about the unthinkable,” “managing the unexpected,” and ultimately led to the better appreciation of the need for crisis management.
Rolls-Royce	Barney Mathias, Personnel Director	Observations of the process by which managers decided on changes “provided direct insights” into what later became the strategic choice perspective. This perspective emphasizes the importance of managers, the legitimacy of participation in developing organizational arrangements, and denies that structural matters are purely technical problems best left to experts.	John Child	Controversy continues among the advocates of strategic and organizational concepts ranging on one extreme from “managers make the difference” in the success or failure of an organization to the population ecology model at the other extreme that suggests organizational survival and success are more the result of luck, chance, and randomness than managerial choices.
RAND Corporation and Lockheed Aircraft Corporation	Robert Gross, CEO of Lockheed	Examinations of organizational myopia based on experiences at RAND and problems involved in managing organizations in the face of environmental discontinuities.	H. Igor Ansoff	Strategic decision making, organizational effectiveness (doing the right things), and strategy formulation remain the most basic of all questions that managers must ask and ultimately answer to the satisfaction of stakeholders. Dealing with these issues culminated in what is arguably the first, and one of the most influential, books published on corporate strategy.

"operate under very trying conditions but have less than their fair share of accidents."<sup>20</sup> Nuclear power generating plants are HROs, and so are aircraft traffic control systems and nuclear submarines. The nuclear aircraft carrier has been the setting for the most important HRO research. Admiral Thomas Mercer, the second captain of the USS Carl Vinson, was familiar with the work that Karlene Roberts and her colleagues had done on the Navy's introduction of F-14 squadrons into the U.S. Pacific fleet. Admiral Mercer's knowledge of this work was instrumental in securing his sponsorship for research using nuclear aircraft carriers as prototypical HROs.<sup>21</sup>

The first captain of the Carl Vinson was also the first commanding officer of the F-14 squadron and had completed all but his dissertation for a Ph.D. in physics. In searching the scholarly literature for research that might be useful in ensuring the operational readiness of the squadron, he came across communication research by Karlene Roberts and Charles O'Reilly. Later, when Admiral Mercer voiced concerns about managing dangerous technologies, he suggested that Mercer should contact Roberts.<sup>22</sup> When Mercer and Roberts met, they talked about the possibility of conducting research on his ship into the ways organizations learn to mitigate risks. This meeting, and the research that followed, dramatically changed Roberts' life.<sup>23</sup>

Roberts literally "wrote the book" on managing one of the most complex organizations imaginable: the nuclear aircraft carrier. Moreover, not only was this organization incredibly complex, it demanded near perfect reliability. HROs are "enormously rigid when nothing very interesting is going on," but as a battle or simulated battle develops, "flexibility takes over, people are trusted to do their jobs in the way they have been trained, and the job of the commanding officer is to have the big picture. Micro-managing is not only unsatisfactory in such situations; it can be disastrous."<sup>24</sup> The results of Roberts' research have been applied to the decision-making of commercial airline pilots, firefighters, pediatric intensive caregivers, and police negotiators.<sup>25</sup>

Roberts' work teaches us two important lessons about making management research matter to managers. First, practice-relevant research is most likely to occur when there is a "predisposition" to collaborate. Admiral Mercer was informed about the potential benefits of risk-mitigating research because a fellow naval officer familiar with searching for scholarly guidance that might help solve real problems recommended Roberts. In Roberts' words, he "was looking off or on the shelf for

research that might help him." Moreover, Roberts' personal work experience at Pacific Gas and Electric Company's Diablo Canyon nuclear power plant made her aware of the unique management challenges of HROs.<sup>26</sup>

Second, practice-relevant research may require that researchers leave comfortable and familiar settings and managers who are willing to push the envelope by allowing access to seemingly inaccessible research settings. Roberts, for example, was discouraged when she heard complaints about "letting a research team board a ship during part of a deployment." Even more challenging was an Act of Congress that prohibited women from assignment to sea duty. As Roberts noted, however, a "Navy officer suggested I persist. I did and spent some part of the next four years at sea with the Navy."<sup>27</sup>

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### ***Rolls-Royce, Strategic Choice, and Why Managers Matter***

John Child did something few management researchers do—work full time in a global corporation. Child's two years at Rolls-Royce affected his thinking about management in many ways. This influence was perhaps most clearly demonstrated by his formulation of the strategic-choice perspective, describing the "process through which top managers decide on pursuing change in an organization's strategic direction."<sup>28</sup> Child attributes much of his thinking to the tolerance of Rolls-Royce Oil Engine Division personnel manager Barney Mathias, who allowed him the freedom to learn, experiment, and research issues relating to management and organization design.

Within a research context dominated by "contingency theory," many contemporary researchers had formulated concepts of organization design that appeared to be primarily determined by the environment faced by an organization. Child noted, however, that the different departments in the Oil Engine Division at Rolls-Royce offered their own preferred options for succeeding in their respective task environments. Whereas all departments faced essentially the same general environment, there was no agreement on a preferred

organization design within the uniform environmental context. Based on his observations, Child formulated his strategic-choice perspective, which further maintains that an organization's environment is not an objective factor but is constantly being re-interpreted and disputed by the organization's top-management team. Managers are not dominated by deterministic environments but make many strategic choices relating to design factors, to technical systems, whether to grow or contract, to remain in or gravitate to a task environment that is less dynamic, and so on.<sup>29</sup>

The strategic-choice perspective addresses one of the most fundamental considerations confronting management research and practice: If an organization's environment determines its structure and critical decisions, managers matter very little, and if managers do not matter, management research matters even less. On the other hand, if the state of an organization's environment is debatable and subject to interpretation, managers matter a great deal and so do our attempts to understand strategic decision-making processes.

Both contingency theory and environmental-determinism advocates had greatly challenged the importance of managers in modern economies. Alfred Chandler illustrated the importance of non-owner salaried lower-, middle-, and top-level managers in the development of modern business enterprises when he convincingly argued that the "visible hand" of professional management had replaced the "invisible hand" of classical economics in "coordinating the flows and allocation of resources."<sup>30</sup> In the process, according to Chandler, business enterprises had become a source of "permanence, power, and continued growth."<sup>31</sup> Environmental determinism challenged the need for a visible hand, but Child reinforced the role of managers' skill and judgment in strategy formulation.

Child's work teaches us two important lessons about making management research matter to managers. First, although managers realistically need practical justifications for allowing researchers access to organizational settings, unconstrained access can result in what James March, from an academic perspective, has called the "development of fundamental ideas that shape management thinking." Former Citigroup chairman and co-CEO John Reed expressed a similar view, noting that it was essential for him to get away from the day-to-day pressures of doing business and obtain access to research and thinking that improved "the opportunity space" for Citigroup's future.<sup>32</sup>

Second, researchers must enter organizations with a willingness to go where the issues take

them rather than interpreting phenomena in the context of conventional academic wisdom. Child was able to go beyond a research context dominated by contingency theory and, in doing so, develop an explanation for observed behavior that was inconsistent with prevailing thinking.

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### ***Three Mile Island: Thinking About the Unthinkable***

Crisis management, as a serious field of research and practice, has recently "come into its own." Centers of excellence have been established at leading universities, and consultants make fortunes advising corporations on how to survive crises and control the associated damage. Since September 11, 2001, managers have spent even more time thinking about and planning for the real and present dangers posed by crises associated with terrorism, as well as the increasingly complex technologies that are characteristic of our modern industrial society. The basis on which many decisions are made regarding crisis management in board rooms today can be traced to research resulting from the 1979 nuclear power generating plant meltdown at Three Mile Island, Charles Perrow's thinking about this event, and the publication of his book *Normal Accidents: Living with High Risk Technologies*.

Perrow tells a chilling story about how he was asked by Cora Marrett (a former student and government commissioner) to study the accident, to provide a social-science perspective on a predominantly engineering and scientific investigation and the effect this had on him personally and professionally. As he reviewed the material regarding the investigation, he stated:

*I went through the transcripts that afternoon and couldn't sleep. When I got to sleep, I had the worst nightmares I'd had since my army days. The testimony of the operators made a profound impression on me. Here was an enormously, catastrophically risky technology, and they had no idea what was going on for some hours. . . . I suddenly realized that I was in the thick of it, in the very middle of it, because this was an organizational problem*

*more than anything else, and that I was an organizational theorist.*<sup>33</sup>

Although attention today is focused on unthinkable terrorist acts, the subtitle of Perrow's book, *Living with High Risk Technologies*, underscores the fact that highly complex organizational and technical systems present equally serious dangers. Prior to his immersion in the events at Three Mile Island, Perrow had been formulating a thesis for a planned book tentatively titled *A Society of Organizations*. He had noted that normal or systems accidents occurred in all the organizations he had observed including corporations. His thesis, even before Three Mile Island, was that when systems are small and relatively independent, the resulting damage can be contained. As systems become larger and more interrelated, they become increasingly unmanageable and irretrievable failures are more likely to occur. Perrow had a "primitive theory" about complexity and coupling. Three Mile Island provided him a case whereby he could test the theory. There is little doubt that organizations today and, to a greater extent, those of the future are becoming ever more interrelated. Normal accidents will happen, and managers will have to become experts at how to respond appropriately.

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The opportunity offered to Perrow by Commissioner Marrett to study the social-science aspects of the Three Mile Island crisis has since led to other original research on how to "think about the unthinkable" and, ultimately, "manage the unexpected." This research has investigated topics ranging from airplane crashes to railroad shipment delays. The lessons learned at Three Mile Island in 1979 did not have to wait long to be tested and refined by managers facing crises: Procter and Gamble's 1980 crisis with the Rely tampon, Johnson & Johnson's crisis in 1982 with the cyanidelaced Tylenol Extra-Strength capsules, and the 1984 Union Carbide Bhopal, India chemical release that, by conservative estimates, killed 2,000 people and blinded a quarter-million more.<sup>34</sup> Roberts and Bea, for example, argue that Perrow's work has been useful to practitioners contemplating why airplanes crash, dams collapse, ships collide, and plants explode.<sup>35</sup>

Perrow's work teaches us two important lessons about making management research matter to managers. First, opportunities are created when managers recognize the need to think "outside the box" about conventional problems. Three Mile Island was an engineering problem to most people, but Commissioner Marrett recognized the potential benefit of obtaining a social-science perspective to augment and enlighten an exclusively engineering view.

Second, researchers can create new and productive directions in knowledge development by thinking beyond their conventional research models. Although initially skeptical, Perrow came to appreciate Three Mile Island as an organizational problem and set to work applying his training as an organization theorist. He had had a comfortable and distinguished career in traditional organizational sociology. His research on Three Mile Island, however, not only resulted in practice-relevant knowledge but influenced the direction of his future work for years to come.

#### ***RAND, Lockheed, and Modern Corporate Strategy***

Researchers have sometimes called upon their personal organizational experiences in a particular place to define and refine their ideas. Curiosity about why organizations operate the way they do is a valuable source of research questions and hypotheses as illustrated by Child's experience at Rolls-Royce. Igor Ansoff's experiences at the RAND Corporation while managing a major project to evaluate the vulnerability of NATO air forces to Soviet attack also provided a unique platform for his subsequent thinking about organizations.

According to Ansoff, a model he developed in his work for NATO included a number of nontechnical variables that were uncharacteristic of RAND studies. At RAND, the custom was to present studies to internal staff for criticism and evaluation before they were passed to clients. Ansoff observed that the RAND staff viewed the introduction of "soft" variables, which made his model's predictions more realistic and relevant, to be a deficiency. Writing in his autobiography, he noted that this was his first encounter with organization myopia.<sup>36</sup> In a related experience involving a study for the Strategic Air Command (SAC), Ansoff observed:

*Although I did not realize it at the time, the rejection by SAC and near rejection by the RAND staff contained valuable insights into the manner in which organizations react to studies which contradict their historical behavior and experience. These insights sur-*

*faced from my subconscious some 20 years later and helped me understand and deal with the problems of managing resistance to discontinuous strategic change.*<sup>37</sup>

Even more germane for our purposes was Ansoff's decision to join the Corporate Planning Department at Lockheed Aircraft Corporation. Robert Gross, Lockheed's CEO, placed Ansoff in charge of a Diversification Task Force. As a consequence, Ansoff increasingly focused his attention on the challenge of managing organizations confronting environmental discontinuities, which became the central theme of his work for three decades.

All of these incidents indicate the influence that RAND, Lockheed, and Mr. Gross had on the thinking of the individual who was to become known as the "Father of Strategic Management." Ansoff was a major contributor to contemporary managers' understanding of strategic decision-making, and much of his classic book *Corporate Strategy: An Analytic Approach to Business Policy for Growth and Expansion* was based on his experience as a practicing manager.

Ansoff's work teaches us two important lessons about making management research matter to managers. First, organizational practices and the managers who develop them frequently drive rather than simply apply research. In actual fact, much management research is based on observations of organization practices. Ansoff's experience demonstrates that an open, perceptive mind will likely encounter a wealth of research possibilities relating to day-to-day organizational realities.

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Second, research based on observation of or participation in organizational settings may require developing innovative research methods to capture the richness of organizational reality without sacrificing the internal validity needed to generalize research findings. As a consequence, there may be challenges in gaining acceptance of unorthodox methods, as well as in using the research thus generated to inform management practice.

**Knowledge Spiral: Requirements for Conscious Encounters**

These four illustrations offer particular lessons regarding management research that matters to managers. Clearly, management research can be

and has been useful to practicing managers. A few books, such as *Built to Last*, written by academic researchers are widely read by managers.<sup>38</sup> However, they, like the illustrations we have discussed, are more the exceptions than the rule. Why are we impressed by successful manager/researcher encounters or when an academic researcher's book makes a best-seller list? Should we not imagine that many, perhaps most, conventions of management practice have emerged from mutually beneficial manager/researcher partnerships? What would be needed to encourage the generation of new scientific knowledge about organizations and managing and at the same time produce research that can be used by those who are responsible for today's organizations?

The lessons learned from the previously discussed illustrations tell us much about what needs to be done in order to encourage more practice-relevant management research. Such research requires managers, researchers, and organizations with certain qualities. The encounters described in each illustration occurred between informed managers who trusted and supported researchers and were not averse to taking professional and, sometimes, personal risks. The managers were willing to champion new ways of understanding their organizations and had enough confidence in themselves to invite and encourage new perspectives. The researchers were willing to enter unfamiliar and sometimes uncomfortable environments and move beyond the limits of existing theories by perceiving organizational realities outside the boundaries established by conventional descriptions. All of these are important lessons, but the insights they provide are collectively insufficient to offer us a means of enhancing the likelihood of mutually beneficial manager/researcher partnerships. Fortunately, we have related research that does provide a basis for such understanding.

Nonaka and Takeuchi have introduced the notion of "knowledge spiral" to describe how increasingly useful interchanges can occur between managers and researchers.<sup>39</sup> A knowledge spiral begins when managers and researchers socialize or associate with one another through consulting engagements, professional meetings, and in executive-in-residence programs. Managers and researchers make one another aware of their assumptions, perceptions, and values and in doing so achieve a deeper understanding of one another's perspectives. It then becomes possible for managers and researchers to share explicit knowledge required for research projects and cooperate as members of research teams. Finally, the ulti-

mate goal is achieved when research is used to inform and influence management practices.

### ***Interaction: The Necessary First Step***

Although manager/researcher interaction will not ensure collaboration, partnerships cannot develop without face-to-face interchanges. When managers and researchers come together for the purpose of truly learning from one another, a "virtuous circle" begins to form.<sup>40</sup> The circle will not, however, form easily. Important differences and incompatibilities in problem-solving styles must be resolved.<sup>41</sup>

In a study conducted by Dossabhoj and Berger of business school researchers, business school deans, graduates of executive MBA programs, and senior executives, an attempt was made to determine what each group thought were the properties of exemplary management research. The properties identified by researchers were similar to those identified by the deans, whereas the properties identified by executive MBAs and senior executives were similar. The researchers and deans, however, differed significantly from the executive MBAs and senior executives in the properties they identified.<sup>42</sup>

The "executive model" (executive MBA and senior executives) viewed exemplary research as beginning with an explanation of managerial reality, written in short and crisp terms, and having direct implications for managerial action. The implications should lead to explicit recommendations that assist in solving real management problems. Management research in this model should improve corporate performance and be helpful in running a business.<sup>43</sup>

The culture and competitive environment of business enterprises require that managers make decisions fast and often with insufficient information. The executive model is considered essential in the fast-paced business world but can lead to "quick and sometimes dirty" decision-making and is alien to the cautious and careful researcher.<sup>44</sup> Researchers are trained to search for information, even at the risk and expense of taking more time. In an academic environment, information is seen as an asset rather than a cost. Quality research is directed toward questions of management and organization that are "bigger" than solutions to immediate problems and is focused on "the basic ideas that shape the discourse about management."<sup>45</sup> Interaction between managers and researchers can lead to an understanding, if not an appreciation, of the perspectives of different professional communities.

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### ***Expanding Perspectives***

Research has confirmed that the greater the degree of perspective-taking between managers and researchers, the greater will be the use of research to inform management practice. Perspective-taking, however, is not the automatic result of interaction. Perceived usefulness of research requires more than jointly defining topics to be researched. It involves a relationship between managers and researchers.<sup>46</sup> It has been said that "being both too academic and insufficiently academic inevitably gives rise to a rather persecuted imagination."<sup>47</sup> Persecuted imaginations are dysfunctional. True perspective taking, however, can reduce many of the resulting negative effects on researchers and managers.

An excellent illustration of the importance of interacting with an aim toward perspective-taking is provided by Roberts. When she and her colleagues were beginning their research on high-reliability organizations, one of their first acts was to bring together "ship officers, and managers from the FAA air traffic control system in the Bay Area, and PG&E's nuclear power plant in Diablo Canyon. The ship sponsored a one-day workshop in which managers from these units could talk about their common challenges."<sup>48</sup>

Perspective-taking should begin with the recognition that not all researchers should attempt practice-relevant research and not all organizations should sponsor it. In this respect, there are two types of management knowledge. Each type is likely to come from a relatively well-defined but separate researcher subculture. Mode 1 knowledge (M1K) is scientific in the conventional sense that it is the concern of universities, is disciplinary, and focuses more on theory than practice.<sup>49</sup> M1K is derived from adherence to the traditional academic model. This model, subscribed to by academic researchers, argues that research should begin with interesting questions and conclude with additions to theoretical knowledge. Research should be based on sound premises from which hypotheses can be developed and tested. Academic research requires an appropriate sample and rigorous data analysis from which logical inferences can be made. Findings must be internally consistent and

propose defensible generalizations that can be replicated by others.<sup>50</sup>

Mode 2 knowledge (M2K) is rigorous but trans-disciplinary, being more concerned with knowledge as it works in applied practice. This type of knowledge aims at gaining insights into a particular context with a view to providing a practical solution to identified management problems.<sup>51</sup> The users of M2K need enough, and only enough, methodological rigor and sophistication to ensure that the evidence upon which decisions are based is sound.<sup>52</sup>

In general, university researchers have an interest in generating M1 knowledge and should be protected and sheltered in their pursuit and dissemination of knowledge for its own sake.<sup>53</sup> In contrast, business enterprises typically have little interest in sponsoring research that may result in a competitive advantage becoming a public good. Understandably, just as academic researchers committed to the development of M1K should not be expected to generate M2K, managers interested in the use of knowledge as a competitive tool should not be expected to make proprietary M2K research available free.

When managers, however, debate internal management issues and the debate can be linked to more universal research agendas, perspective-taking on the part of all parties has the potential of facilitating the development of effective collaborative knowledge-development networks.<sup>54</sup> Ideally, the dissemination of relevant findings in these networks should take place as an integral part of a research process.

Manager/researcher partnerships or networks offer the promise of making management research more useful to managers by increasing its quality and relevancy. It is generally recognized that managers make operational decisions based on their understanding of an enterprise's history, its background, and the past experiences of all those involved. In designing studies, researchers do likewise. As Neustadt and May note, context requires that both managers and researchers think "in time streams."<sup>55</sup> Whereas doing so aids researchers in identifying high value-added research issues, it also aids managers in probing how operational recommendations might be affected by changes in assumptions and industry conditions.<sup>56</sup>

### ***A Glimmer of Hope***

Although the research of Dossabhoy and Berger confirmed that there is indeed a gap between the properties of exemplary management research as perceived by academics and executives, it re-

vealed an area of agreement that could provide a starting point in transcending the gap and, in the end, could lead to more examples of mutually beneficial manager/researcher partnerships. Dossabhoy and Berger found that there were properties of exemplary management research that academics and executives agreed were important. Both groups agreed that exemplary management research should surface critical problems, have a clear purpose, be coherently written, use credible data sources, be objective and unbiased, provide a conceptual framework, be original and innovative, be generalizable beyond the current context, challenge current assumptions, have recommendations supported by data, provide lots of good ideas, and be based on valid premises.<sup>57</sup>

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### ***There is indeed a gap between the properties of exemplary management research as perceived by academics and executives.***

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Dossabhoy and Berger further suggest a model for bridging the gap, a transcending rather than a mere integration of the academic and executive models. Exemplary management research conducted under this model would transcend the pedestrian aims of either academics or managers, address critical management issues, be clearly presented, result in recommendations for action that are based on valid data and analysis, and be subject to verification. This research should meet the requirements of the most demanding scholarly communities and assure a basis for quality control. It would also add real value to management decision-making. Recommendations would be based on data rather than speculation and would provide guidelines for managerial action.

Management research that is critical, verifiable, valid, useful, and clear can not only make academic research more useful; it can improve it. Managers, on the other hand, who are overwhelmed with problems and speculative solutions will welcome research generated according to a transcending model, will be more confident about its usefulness, and will become more discriminating consumers of management research.<sup>58</sup>

### **An Action Agenda**

Our examination of the genesis of selected practice-relevant research and its transfer to management practice suggests an agenda for enhancing the likelihood that research will be used to inform

management practice. There are, indeed, some specific steps that university-based researchers and practicing managers can and should take to encourage more and better practice-relevant management research. Occasionally, as our four illustrations indicate, M2K develops as the result of accidents or unusual coincidences. The potential value of such information encourages us to offer the following agenda for enhancing the likelihood that management research will be used to improve management practice.

1. Both university-based researchers and practicing managers should actively seek opportunities to develop joint-interpretive forums involving managers and management researchers.<sup>59</sup> Such forums encourage socialization and may take the form of co-sponsored symposia at professional meetings, executive-in-residence programs, and so on. Joint-interpretive forums are useful in engaging managers and researchers in defining issues, reflecting on and interpreting information, and suggesting organizational self-design efforts using research results. For example, the sponsorship of the United States Navy in convening a workshop on mitigating risks was instrumental in involving managers in influencing the initial direction of Roberts' research.
2. Joint-interpretive forums should be encouraged and sponsored by professional societies, business firms, and universities because the evidence clearly suggests that more frequent interaction between managers and researchers leads to deeper relationships and encourages perspective-taking between managers and researchers. Merely doing research on topics relevant to managers will not lead to perspective-taking. Successful perspective-taking occurs when researchers understand what a topic means to managers and managers understand how researchers perceive different issues.
3. While sheltering M1K research, universities should actively encourage and reward M2K research that results in organizations engaging in self-designed improvement activities informed by practice-relevant research.
4. The importance of criticality, verifiability, validity, utility, and clarity should be emphasized in the training of management researchers along with the conventional skills associated with scholarly research. Efforts should be made to demonstrate these qualities in all management research in the hope that practicing managers will begin to see that research has the potential to be understood, improve organizational performance, and add genuine value.
5. Researchers should commit to long-range follow-up and evaluations of applications of management research to determine the extent to which research really is useful in management practice. Rarely is any scientific finding perfect in its first application. Management practice shares an important characteristic with scientific discovery. Both are made better by application, testing, and refinement. Universities, in the same manner, should become more tolerant of research that evolves over time and overcome the tendency to judge scholarly productivity by numbers of publications rather than the significance of their impact on the research of relevant scholarly communities.
6. Universities should recognize the importance of research that is critical, verifiable, valid, useful, and clear even if it is not published in the most prestigious journal of a field. The most prestigious journals should also become more receptive to research that follows a transcendent model as well as the traditional academic model. This tolerance should also apply to books written primarily for practicing managers and published by more trade-oriented presses. Perhaps university presses could become important players in publishing works that transcend the academic to applied audiences. Such a niche might find willing partners in corporations and other organizations that could see such investments as value-added alternatives to exorbitant consulting fees.
7. Business firms and other organizations should seek opportunities for partnerships with university-based researchers to investigate issues that have been opened to debate, but only when there is a clear understanding as to whether or not research results will be freely disseminated to the broader public.
8. Productive manager/researcher partnerships can be valuable to all concerned, but enhancing the likelihood of conscious encounters must be a deliberate process. Encounters, whether accidental or conscious, should be actively encouraged. However, there is a paradox. Productive partnerships will not develop until managers, researchers, universities, and organizations think in the very unconventional terms that partnerships are designed to encourage. In a classic *Catch-22*, however, we cannot think unconventionally about our assumptions, roles, and values until we first develop effective partnerships. Nothing short of a "reengineering" of the manager/researcher relationship is required.

### **A Final Suggestion**

No one would suggest that any but the most arrogant and myopic researchers and managers are not interested in ways to facilitate the translation of management thinking into management actions. Arguably, most people, at least intellectually, do see the need for mutually beneficial manager/researcher partnerships. In fact, management researchers often do follow certain conventions to suggest how practicing managers might apply their work. For example, a standard part of most management research articles is a "So What" section. Customarily, this section is at the end of an article and is designed to provide indications of how the reported research might be used in improving management practice. This is a positive step aimed at perspective-taking. Unfortunately, it is a unilateral approach. The assumptions requiring a "So What" section for management journals are numerous, and many are faulty.

For example, for a "So What" section to be valuable, managers must first read the article in which it appears. In many cases, an article's aim is to report M1K research, and managers typically do not read journals in which this type of research is published. An equally questionable assumption is that M1K researchers are able to recognize what their work means to practicing managers. In the absence of the perspective-taking that comes about through joint interaction, there is a high probability that both managers and researchers will simply "miss the point." In other words, the "So What" section in articles reporting M1K research is unlikely to add anything to researchers' understanding of the implications of work or managers' understanding of how they might use the reported research in improving practice. In articles reporting M2K research, the implications should be apparent and a "So What" section unnecessary.

Perhaps a more important section should be placed at the beginning of articles and be titled "Why This Research Should Matter to Managers." This section would contain brief background information on how the ideas behind the research that is being reported were developed and might contain introductory applications such as those of Carney and Sears, Child and Rolls-Royce, or Roberts and the USS Carl Vinson, and so on. This might "grab" the interest of managers and even suggest larger issues of concern rather than focus exclusively on day-to-day challenges. The goal would be to provide verification at the onset that the issue being examined is potentially important to managers. Admittedly, this too is a unilateral

view that favors managers in directing management research.

### **Future Challenge**

We have called for manager/researcher partnerships based on a perspective-taking which ensures that managers and researchers truly understand and respect one another's assumptions, values, and methods. It is suggested that the likelihood of managers and researchers being involved in all stages of management research will be enhanced by such activities as joint-interpretive forums sponsored by professional societies, business firms, and universities.

We have pointed out the benefits that can be acquired by managers and researchers in forming meaningful partnerships, and we also argue that partnerships work best where the different parties enact the roles they are uniquely suited to perform. We challenge managers and researchers to think about these roles and work to perfect them so as to maximize the synergy that can be obtained from joint experiences that facilitate effective perspective-taking.

We further challenge managers to catalog the recurring challenges they face in running their organizations and document, to the extent possible, why the challenges keep recurring. When they are cataloged, managers are encouraged to discuss the challenges with researchers, offer access to their organizations, run interference for researchers, question their findings, and involve researchers throughout the implementation of new practices. In this way, real issues can be addressed and data-based decision-making can be applied to important future challenges.

We urge researchers to be receptive to managers who want to make self-design decisions informed by research. Researchers are urged to look at these invitations as research rather than consulting opportunities. Perspective-taking is achieved when real challenges are identified and sound research is applied to their solutions. Approaching such assignments within the context of consulting creates conflicts of interest, casts suspicion on results, and limits opportunities for access. Moreover, consulting is the core competency of consulting firms, not university-based researchers. We urge universities to protect and shelter M1K research, but to value and where appropriate encourage M2K research. Rather than being looked on as second rate because it is applied, it is hoped that management faculties will more and more value research that can inform and improve management practice. Finally, we call for a serious recognition of a tran-

scendent model for exemplary management research based on the criteria of criticality, verifiability, validity, utility, and clarity. These criteria provide the framework for new and exciting approaches to management research that avoid the irrelevancy of the academic model and the obsessive practicality of the executive model.

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### **Consulting is the core competency of consulting firms, not university-based researchers.**

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We cannot help but wonder how much important management research would never have taken place and how many improvements in management practice would have been lost if not for the encouragement of managers like Barney Mathias at Rolls-Royce, the questions and directions of C. T. Carney at Sears, Roebuck and Robert Gross at Lockheed, the support of Admiral Thomas Mercer, and the foresight of Cora Marrett in wanting to understand the human reasons behind the Three Mile Island disaster. Perhaps even more important is the question of how much more and better management research and practice might be expected in the future by encouraging mutually beneficial partnerships built on effective perspective-taking between managers and researchers.

If we choose to be pessimistic and conclude that business best-seller lists accurately reflect the relative influence that academic researchers, journalists, and consultants have on business decision-making, we must then ask, "Do we contribute so little because we are irrelevant?" Or, "Are we irrelevant because we contribute so little?" Regardless of the response, the situation is not hopeless. We must first become less incestuous and commit to producing, encouraging, and rewarding research that is transcendent in nature and then develop and nurture mutually beneficial researcher/managers partnerships. Management researchers must take the first step. Business firms have proven that they can survive and be successful without depending on scholarly management research. Researchers must demonstrate how they can add value by offering ideas that improve organizational performance.

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