INTRODUCTION TO THE SPECIAL RESEARCH FORUM—
PUBLIC POLICY AND MANAGEMENT RESEARCH: FINDING
THE COMMON GROUND

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Practical men, who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back.

-Keynes, 1936

Little did we know when Sara Rynes first asked us to organize a special research forum on public policy and management research that it would appear at a time of such great need and opportunity. We write this introduction at what we hope will be the nadir of the worst economic downturn, indeed the biggest economic and institutional crisis facing the United States and world since the Great Depression of the 1930s. The crisis has triggered debates that ought to have struck right at the heart of the study of management. Did U.S. regulators grant Wall Street too much autonomy? In decreasing regulation and government oversight of new financial instruments and practices, did they fail to act in the broader social interest? Should we as scholars have paid more attention to the decline in the countervailing power of other institutions in society? If managers lost control of or turned a blind eye to excessive financial risk taking within their organizations, could different public policies have averted such actions?

If Keynes was right in noting that the ideas of most policy officials (madmen?) can be traced back to the scribblings of some academic, then now is the time to see how the scribblings of management researchers might inform efforts to rebuild our economies, institutions, and organizations. We have seen how fallible organizations can be and the public and private costs their decline can exact. Many firms, not just a few, that once seemed beyond reproach no longer exist. What does management research have to say about how public policies might mandate or encourage changes inside business organizations? How can management research inform the relationships between business and government? What about the role of educational institutions, including universities, and management schools in particular? Should governments impose solutions on business and civil society? And what about other nongovernmental organizations, such as labor unions, environmental coalitions, standards associations, and nonprofit service providers?

From the recent crisis will come the rebuilding of businesses and institutions, and the birth of new ones. Management researchers will only inform this process if we can reframe and broaden the dominant paradigm guiding management research in recent years. Some would argue that, in this community of scholars, the lack of a dominant paradigm is both our greatest asset and our greatest weakness (Pfeffer, 1993; van Maanen, 1995). Yet the de facto paradigm we have is one that focuses on pursuing narrowly scoped research topics leveraging available data to achieve publication in top journals. Gaining influence in policy debates will require more careful forethought as to the research subjects we choose and the design of our research programs as well as more direct interaction with the policy-making community—government officials and the full range of stakeholders that influence policy.

The articles in this issue are excellent illustrations of how management research can be framed and carried out in ways that speak directly to critical challenges in three key policy domains: work
and employment, globalization, and technology. With this introduction, we intend not just to summarize these articles but also to encourage management scholars to reframe their work in ways that can bring to bear their comparative advantage—sound theoretical and empirical research in the organizational domain—on short- and long-term public policy challenges and debates. We outline what we believe makes for useful policy research and use the work assembled here, in the *Academy of Management Journal’s* “Special Research Forum—Public Policy and Management Research: Finding the Common Ground,” to illustrate key features of this type of research.

**WHAT IS PUBLIC POLICY RESEARCH?**

It is easy to state succinctly what is not public policy research. It is not just a paragraph on implications for public policy tagged onto the end of a paper. Instead, the entire paper, from the framing of the problem statement to the theoretical argument and hypotheses developed and tested, should incorporate a public policy focus. Then, if the results warrant any, policy implications should be offered.

**Framing the Problem: The Role of Normative Assumptions**

Policy papers should start from a clear set of normative values that help define problems of interest. Indeed, all social sciences rest on a set of normative assumptions that serve as the starting points for theory building (Etzioni, 1990; Merton, 1973). To take one example, classical and neoclassical economic analyses start from the normative premise that perfect competition produces socially optimal results (Marshall, 1890; Smith, 1776/1979). Marx (1867/1978) and early 20th-century institutional economists (Commons, 1934) challenged the use of this assumption in labor market analysis by noting that “labor is not a commodity” but rather individuals with free will, needs, power, interests, and motivation. Industrial relations theorists subsequently built on the work of these critics of classical economics to build a field of study around the normative premise that employment relationships are inherently driven by mixed motives and entail both some enduring conflicts of interests and some shared interests. The intellectual and policy task of these scholars is to find the appropriate balance among conflicting interests and to seek ways to better promote shared interests (Barbash, 1984; Budd, 2004; Walton & McKersie, 1965).

Normative premises also guide the subfields that management research comprises today, but these premises are rarely made explicit. Different starting points provide a variety of normative foundations on which positive theories rest and from which problem statements are derived. Much of the research in business strategy, entrepreneurship, and organizational behavior, for example, focuses on explaining the performance of firms or groups and individuals within firms. The implicit normative premise is that higher firm, group, or individual performance is good for society. What can be lost in such research is whether costs accompany the achievement of such goals.

There are noteworthy exceptions: those working in the recent tradition of “positive organizational scholarship” (Cameron, Dutton, & Quinn, 2003) not only seek explicitly to broaden the range of outcomes management researchers investigate, but also feature substantial discussion of the view of human potential that provides this tradition’s normative underpinnings. A second exception, explicit in its questioning of capitalism, is the field of critical management studies (Alvesson & Willmott, 1992). Unfortunately, these kinds of research programs are rare, and much of the work that has taken a broader look at outcomes for multiple stakeholders inside and outside firms has not made its normative goals more explicit than has the research focusing on performance outcomes.

Differences in normative premises are not likely to be adjudicated solely through the results of empirical research. Such differences shape researchers’ definition and framing of problems and, over time, define distinct research paradigms (Kuhn, 1970) governing a field of study. Breaking from these paradigms can be challenging but may still provoke public dialogue. For example, Juliet Schor (1993) explicitly rejected neoclassical views of efficient labor markets, focusing instead on the premise that the American labor market featured “too much work, too much debt, too little meaning,” in her book *The Overworked American*. By Schor’s own account, her work was not well received by her peers, but the public attention it received in media and policy forums helped her gain grudging acceptance of normative concerns that her field had previously neglected (Burawoy et al., 2006).

Here, we reveal an assumption of our own: the goal of management research need not be to seek reconciliation of alternative paradigms or normative assumptions (e.g., van Maanen, 1995). Instead, authors should be clear, in their heads, in their research, and in their involvement in policy debates, about the normative perspectives underlying their work. Some argue that management scholars cannot compete with economists in policy forums.
because we lack the homogeneous paradigm that allows economists to present a united and persuasive front regardless of the validity of their arguments (Burawoy et al., 2006; Ferraro, Pfeffer, & Sutton, 2005; Pfeffer, 1993, 1995). Yet we have much to gain from our pluralism as a field.

First, as the recent crisis showed, the idea that markets self-regulate has proven fallible; even fervent believers in this paradigm were forced to acknowledge that self-interest was inadequate protection against high-risk behavior (Greenspan, 2008). Second, a globalized world, “flat” or not (Friedman, 2006), may be too complex for a single paradigm: multiple and competing perspectives are needed to appreciate how even small changes affect not just production systems but whole social systems. Thus, scholars within a broad interdisciplinary and yet applied field of research such as management need not be dogmatic nor insist everyone adhere to a set of orthodox values. Rather, we should insist on clarity of values and of normative assumptions. By making our values and assumptions more explicit, we will be better positioned to foster a constructive and fruitful dialogue that others can enter—and from there to move toward effecting change and acquiring influence.

In fact, diversity of values and of normative assumptions among management researchers is critical to policy relevance. Competing values give rise to the multiple interests that lie at the heart of most policy debates and contribute to policy innovations. Rarely are the answers to vexing policy questions purely technical. Designing policy-relevant research therefore requires identification of key and legitimate stakeholders, understanding these stakeholders’ interests and values, and engaging them in ongoing dialogue. Here we push further in the normative direction: to have an impact outside the academy, we must consider how this full range of stakeholders is affected when designing, analyzing, interpreting, and communicating our research programs. Management research that informs policy most effectively will frame problems to capture the full range of interests at stake and will be open to cross-examination by those who weigh these interests differently.

To take one example, the special forum article by Bidwell and Briscoe (2009) examines the determinants of individuals’ decisions to work as contractors in the information technology (IT) sector. Early in this work, Bidwell and Briscoe reveal an important normative premise underlying their work, that the growth of independent contracting challenges many public policies that were originally developed to meet the needs of regular employees. Moving from this premise to their research, they argue that employment policies therefore require updating to match the needs of contractors. Their investigation into the determinants of IT workers’ contracting decisions in turn informs the design of such policies.

Similarly, the article by Leana, Appelbaum, and Shevchuk (2009) looks at the causes and effects of what they refer to as “job crafting,” a term drawn directly from the management literature. The authors carefully examine the ways in which workers in early childhood education strive, both individually and collectively, to shape their own jobs. The normative premise is that job definition should not be understood as wholly the province of managers—that workers also have an important stake in their jobs. Job crafting has important effects on childcare outcomes, and thus on the families who are also important stakeholders in childcare organizations. Leana and colleagues show that researchers, and policy makers interested in effective childcare and education, should therefore be open to the possibility that such workers will redraw the boundaries of their job assignments.

In another technology-focused article in this forum, Huang and Murray (2009) reveal their normative stance in the direction of their research question. Although economists are primarily concerned with how public stocks of knowledge impel economic growth, they rarely examine how the property rights essential for economic growth affect future stocks of knowledge. However, this is a short-lived cycle if public stocks of knowledge are ultimately not replenished. With a broader view of stakeholders, Huang and Murray reverse the question by examining what happens when gene patents are awarded. How do patent rights affect the genetic research that follows? Their analysis of the patent history of 2,637 gene sequences shows that patents decrease the long-run supply of public knowledge on genes, and the effect is large: 17 percent. In consequence, Huang and Murray estimate that one in ten genetic research projects may be foregone. These effects are most pronounced on genes associated with cancer and disease. The implications go beyond policy and extend to universities, doctors, patients and their families—even the millions of people “walking for breast cancer” while key patents on the BRCA1 gene suppress much needed research. If we are to make any kind of difference in the war on cancer, greater reflection on whether this is a cost policy makers can afford is needed.

McDermott, Corredoira, and Kruse (2009) examine why some regions of developing nations are able to upgrade their products, thus increasing their share of exports, while others do not. In doing
so, they reveal the normative stance that industry upgrading is valuable as a means to foster economic development. By comparing a positive and negative case in two regions in Argentina with similar natural resources, they found that new private-public institutions instilled with pluralistic governance rules helped cultivate diverse ties that fostered the recombination of knowledge needed to create internationally acclaimed wines. The stakeholders involved are many (wineries; their suppliers, distributors, and marketers; and universities), and this research shows that a broader inclusion of stakeholders into the design and governance of public-private institutions to spur industry innovation was critical.

**Theory**

Good policy papers are not atheoretical. Kurt Lewin’s famous dictum that “there is nothing so practical as a good theory” (1951: 169) captures the essence of a researcher’s advantage in policy debates over pure advocates. Indeed, most sound policy is grounded on sound theory. In the field of labor relations, a good example comes from Stevens (1967), a highly theoretical article asking “Is compulsory arbitration compatible with collective bargaining?” This purely theoretical work was motivated by and framed around a well-known policy problem, namely, that the presence of arbitration could reduce the incentive of parties to negotiate a voluntary settlement. This problem posed a challenge to policy makers seeking to design bargaining procedures for public sector employees without giving them the right to strike. Stevens’s publica-
tion provided the intellectual basis for the development of what became known as “final offer arbitration,” a process that is now part of a number of states’ public sector collective bargaining statutes (and is used in baseball player salary arbitrations). By framing the question in a way that reflected a critical public policy challenge or objective (i.e., maximizing the incentive of negotiators to reach a voluntary agreement), Stevens drew upon a rich mix of game theory, behavioral models of bargaining, and more specific evidence from prior research on collective bargaining to develop a new model of arbitration that addressed this question.

The articles in this special research forum amply illustrate how theory can be brought to bear on policy questions. For example, Madsen (2009) examines the effects of environmental regulation on the ability of countries to attract foreign investors. The key idea is that posing the question at the country level ignores that firms are heterogeneous in their capabilities for coping with regulation. Though the impact of environmental regulation on investment and job creation is often assumed to be negative, the author draws on the resource-based view of the firm, one of the most influential theories in the field of management (Barney, 1991), to offer a contradictory theory that actually broadens and enriches this debate. A similar insight comes from Cuervo-Cazurra and Dau (2009), who demonstrate that market deregulation and liberalization in developing countries do not necessarily provide foreign multinational firms with an advantage over local firms. Using data on the profitability of Latin America’s largest corporations between 1989 and 2005, they document that state-owned enterprises and locally owned firms benefited the most from economic reforms.

Another example of how theory helps to inform public policy debates draws on insights from management research into entrepreneurship. Policy makers interested in job creation might wonder about the circumstances under which entrepreneurial ventures actually generate jobs. In this forum, Dencker, Gruber, and Shah (2009) develop Shane’s (2003) theoretical conception of the “individual-opportunity nexus” to show how job creation depends upon the interaction between the characteristics of entrepreneurs and the characteristics of the opportunities those entrepreneurs pursue.

McDermott and colleagues (2009) were motivated by theories of economic development and political theory as well as the prevailing network theory of embeddedness, but their findings also turn out to be important guides to practical action. They show that it is not just the centrality of institutions per se, but the diversity of ties that those institutions create that encourage industries like the wine industry to adopt new practices. This is an important modification not only of our understanding of what “centrality” really means within industry networks and the benefits that are critical for innovation, but also of what we draw from those theories and put into practice.

As McDermott et al. show, policy research can often benefit from an interdisciplinary approach to theory. Similarly, Pil and Leana (2009) consider a range of policy research on teacher and school effectiveness and school reform, noting that much prior research (and corresponding policy debates) have emphasized “human capital,” particularly that of teachers themselves. Pil and Leana examine human capital in conjunction with “social capital,” the strength of communication and ties among teachers and between teachers and administrators. As management theories of social capital would
predict, more communication and ties contribute to improvements in student performance.

Generally, choosing or developing a theory capable of guiding policy actions or analyzing policy effects requires identifying explanatory variables that policy makers can influence, either directly or indirectly. Direct influence typically requires consideration of laws, regulations, executive orders, court decisions, and administrative/enforcement processes. And subtleties in understanding the results of possible policy changes must also be considered, among them, that passage of a statute or a shift in regulatory or administrative strategy may reflect political or other factors that also affect the policy outcome of interest. To take one example: In labor law reform, a debate exists over whether or not to require arbitration of first contract negotiations. Kochan, Lipsky, Newhart, and Benson (2009) brought management research to bear on this policy question by analyzing the effects of differing public sector arbitration statutes on wage changes of police and firefighters. But to do so they had to first account for what had long ago been shown to be systematic differences among states that enact such bargaining laws (Kochan, 1973). This is a common issue in public policy evaluations and predictions of the effects of alternative policy options, and it leads us naturally to further consideration of research design and methods.

**Methods**

Policy research benefits enormously from triangulation of research methods (Jick, 1979) as well as from cross-case comparison (Barley & Kunda, 2001). Framing a problem for research requires in-depth understanding of the institutional and historical contexts in which the research is carried out. It is no accident that most of the articles in this special forum provide more historical background on their research problems and deeper discussion of the contexts in which the research was situated than is the norm in most articles published in *AMJ* and other management journals. Such understanding encourages a research design that accounts for subtleties in the effects of policy adoption as well as a clear sense of the extent to which the findings can—and should—be general enough to inform policy.

Marquis and Huang (2009), for example, take a historical view of the evolution of U.S. commercial banking, drawing on data from 1896 to 1978, the year when federal law permitted interstate bank branching. Taking a similar approach, Lee (2009) examines the proliferation of state organic food laws from 1973 to 2000, the year when the federal government initiated a nationwide organic certification program. Before the federal statute emerged, local certification organizations and chapters of a federated organization vied for control over what was “organic” and who could participate in this emerging market. Lee’s careful empirics show where particular policy innovations emerged and the remarkable influence local organizations achieved. In another article here, Weber, Davis, and Lounsbury (2009) track the creation of stock exchanges in developing and emerging economies since the 1960s as a function of the network characteristics of countries. In yet another, Madsen (2009) studies the establishment of automobile assembly plants worldwide from 1996 to 2004 to gauge the effect of environmental regulation in the host country. And Cuervo-Cazurra and Dau (2009) observe changes in the profitability of the largest Latin American firms between 1989 and 2005, assessing the impact of market-oriented reforms.

Discussions of organizational context and industry dynamics also support good policy research. For example, Leana, Appelbaum and Shevchuk’s (2009) forum article is greatly strengthened by the authors’ deep understanding of and careful attention to childcare centers. Similarly, McDermott and colleagues’ (2009) knowledge of the different wine regions of Argentina and their successes and failures clearly guided their research design. The centrality of accurate descriptive accounts of management phenomena to policy relevance suggests another way in which management research must be contextualized to inform policy debates. Effective policy research must address the extent to which its findings are drawn from data representative of the population the researchers seek to engage. Some policies and findings may apply to all firms. Others may be more narrowly tailored to firms in a given geographic or industrial sector, or of a certain age or size. In any of these cases, research is more persuasive with respect to policy recommendations when its findings generalize clearly to the appropriate population. Often limited data availability make clear matches impractical or even impossible. In such cases, researchers must explicitly consider the limits of their claims, as well as shed whatever light they can on the boundaries beyond which their findings may not extend. Deep knowledge of a study context, as well as an understanding of the risks of generalizing from one context to others, are critical in drawing appropriate policy implications from management research.

Research, particularly that of the quantitative, multivariate type, typically relies on estimates of mean effects—reporting what one might expect for the average worker or firm. Yet, much public policy
is universalistic, applying to all firms in a given sector or segment. Public policies therefore affect not only those in the middle range, but those at either end. In fact, effects on the ends may be greater in degree than those in the middle, and this logic can inspire research at the extremes (e.g., Weick, 1979). Management research provides a much stronger basis for policy action when it examines not just the mainstream but also the outliers, for these may be the very areas that policy makers miss and where the most severe policy problems exist.

Policy makers (and the media that report on policy debates) thirst for hard numbers that can be used to quantify the magnitude of a problem or provide an estimate of the likely effects of a specific policy action or alternative. The current debate over health care is a good example; who can guarantee that the administration’s plan will cost less than the status quo? Yet the need for change remains: about 46 million Americans are without health insurance. That simple number has captured the attention of policy makers and is used to motivate discussion and action as projections invite debate.

In general, simple descriptive data are highly valued by policy makers but less relevant to management researchers; researchers seeking to build or test theory need to go beyond contextualizing problems with descriptive data. Numbers that are of interest to researchers as well as meaningful with respect to policy are estimates representing attempts to isolate the effects of a particular policy action, net of other confounding factors or variables. Arriving at such estimates requires careful research design and quantitative analysis. The researcher’s challenge here is twofold. First, as in any scholarly work, the analysis must meet the standards of the profession and pass at least the level of scrutiny received in the normal academic peer review process. So care should be taken to avoid rushing results for public release before benefiting from peer review, either through the normal scholarly publication process or through an equivalently tough review by trusted colleagues. Second, the results have to be communicated in a clear fashion that is understandable (but not condescending) to nontechnical but equally critical readers.

This thirst for hard numbers has benefits and risks. Once a number is in the public domain, it is hard to retract and at the same time is sure to be attacked by the parties who oppose the recommendations or actions the number is used to justify. The implication is that researchers need to be extremely confident that any number that is likely to become a lightning rod is in fact accurate and able to withstand the scrutiny it is bound to attract. In their efforts, researchers might take particular heed of the long-running pleas in the methods literature to shed more light on effect sizes (Cohen, 1994) while placing less emphasis on conventional measures of statistical significance.

FROM RESEARCH TO POLICY CHANGE: COMMUNICATION AND IMPLEMENTATION

Crossing the boundary between academia and policy can be challenging, and numbers alone are unlikely to be sufficient to influence policy. Analogy, communication, social networks, and repeated exchange are key enabling conditions. For example, 17 years after her seminal work on the Challenger launch disaster (1997), Diane Vaughn was revisited by the media when the Columbia shuttle failed. Almost immediately, she received more than 800 e-mails asking her opinion on a disaster that she had not studied (Vaughn, 2006). Regrettably, notable analogies between the Columbia and the Challenger disasters were immediately apparent to many familiar with Vaughn’s 1997 work, and e-mail provided a conduit. “NASA and contractor engineers sent their experiences with NASA, despairing over this latest accident and the organizational parallels with Challenger” (Buraway et al., 2004: 32). Vaughn used her exchanges with NASA, the press, regulators, and the disaster review board as teaching moments to collect new data, explore the comparisons between the two disasters, and eventually inform policy on the organizational changes needed to avoid future disasters. As she explained, “Repeated contacts allowed me to reinforce sociological concepts and interpretations . . . soon print journalists began teaching me—sending information, recent development, story ideas or data and asking for interpretation” (Buraway et al., 2004: 33).

With sustained policy engagement, Vaughn made the social as well as the technical causes of both accidents more understandable to nonsociologists and thus more actionable to a broader variety of constituents. Vaughn (2006: 380) noted that “complexity and nuance, theoretical assumptions and supporting evidence” do not travel well, but her repeated engagement with policy makers enabled the institutionalization of sociological principles to the degree that “key concepts from the book—missed signals, institutional failure, organization culture, the normalization of deviance—began appearing in the press and early in the investigation and continued, whether I was quoted or not” (Vaughn, 2006: 360).

Nor is the work finished when policies are en-
acted, for policies and administrative or enforcement actions are a function of their environment and the strategic behavior of those affected by them. As Marquis and Huang’s (2009) study of the rise of branch banking in this forum shows, the same laws permitting branch banking yielded very different outcomes depending upon a state’s transportation infrastructure. A contingent appreciation of policy’s effects asks policy makers to consider the enabling or inhibiting factors that may affect execution of policy. Otherwise, potential confounds may lead to the type of unintended consequences identified by Huang and Murray in this forum.

The way in which policy is implemented can also have an impact on outcomes, as this forum’s article by Weber et al. demonstrates. In their study of the creation of stock exchanges in developing and emerging economies, they argue that coercive policy making ultimately yields inferior results. Specifically, when countries created exchanges in order to meet lending terms mandated by the International Monetary Fund or the World Bank, market capitalization was lower than in the absence of such pressures.

The Research Program

It would be difficult, if not impossible, for a single research paper to meet all of the criteria we have outlined above: clear identification of normative premises; solid and deep theoretical development; a historical description of the problem; methods that provide convincing quantitative estimates as well as discussion of generalizability; results clearly communicated to both academic readers and policy makers; and careful consideration of the likely subsequent effects of implementation. We consider the works in this volume exemplars of the ways in which management research can inform policy. But none meets every criterion listed above; like all scholarly publications, each has its own strengths and contributions.

In fact, effective policy research papers seldom stand alone in a research program. Most of the studies described in this issue’s articles are parts of much larger and longer-term research programs in which some research output contributes to theory while other output combines theory and empirical evidence to address an important public policy question driven by the phenomena examined. For example, Huang and Murray’s study of the effects of gene patents on the subsequent diffusion of public knowledge is one output of a multiyear, multiresearcher study of the role of gene patents and the accumulation of genetic knowledge. Pil and Leana’s research follows clearly and logically from their prior theoretical and empirical work on the effects of social capital. Bidwell and Briscoe’s article can be understood in conjunction with other work by these two scholars: Briscoe’s examination of a variety of alternative employment arrangements, and Bidwell’s studies of how IT workers structure their careers.

Time Frame: Practical and “Pie in the Sky” Perspectives

Policy research can vary enormously by the time frame for having an impact. A general rule might be, the closer to the action, the shorter the time horizon. Researchers and other staff working for or advising high-level government officials have time frames for producing policy memos that begin with receipt of a request and often end at 5 PM the same day, when the memos are expected to be on the officials’ desks. Such an analysis is expected to be short but well grounded in research and evidence, and above all practical—what position should the official favor on a bill or in a policy debate likely to occur that evening or the next day? In these settings, there is not enough time to propose the perfect strategy. Even if a request does not require an immediate response, the time horizon for the policy to have an impact is short, since the expected tenure of most high-level officials is two years or less, and the political window of opportunity to move on any particular policy issue is often much shorter than that. Optimal, long-run policy proposals that can’t be achieved in the short run are not well received by those close to the action.

Still, a longer-term policy strategy should be in a researcher’s mind before he or she responds to a request for advice. There is no time to develop such strategy while the researcher is in the fray. Long-term programs remain essential, even as short-run, feasible steps are sought. Henry Kissinger was famous for saying his worldview, developed through his academic research career, was essential to helping him decide how to interpret and respond to events as they arose during his tenure as U.S. secretary of state. George P. Shultz, who served as secretary of the Labor, Treasury, and State Departments following a career as a labor relations scholar, made a similar point in a recent retrospective address to the Labor and Employment Relations Association: “The more I look back the more I realize how much my actions were informed by what I learned from my work in labor relations” (2009, personal notes). This is Keynes’s (1936) dictum in action and illustrates the power of theory and prior research in shaping the interpretations of
events and decisions of those on the front lines. These are both stories of accumulation, where it is the accumulation of research programs over time that eventually shapes a policy maker’s point of view.

Those on the front lines of policy making will weigh short-run feasibility heavily, yet sometimes ideas that are not acceptable in the short run but bring new thinking into longer-term debates need to be raised. Former Secretary of Labor John Dunlop once accused several academics working with him on a national labor policy commission of committing what in his view was the ultimate sin: Proposing “pie in the sky” ideas that no constituent group—neither business nor labor nor anyone but a few academics—supported. (The idea involved “works councils,” institutions commonly found in Europe but not in the United States.) Without “pie in the sky” ideas, we will not develop a full menu of innovative solutions to intractable problems like health care. These ideas may lead to unexpected research trajectories with a critical bearing on policy. For example, Powell, Packalen, and Whittington’s (2010) research on the conditions that led to biotechnology clusters forming in only a handful of cities, despite widespread efforts to create clusters everywhere, spurred Atul Gawande’s (2009) comparison of low- and high-cost treatment centers in different parts of the country. This public policy discussion moved rapidly from the New Yorker, to National Public Radio, and then to the Obama administration: “He [Obama] came into the meeting with that [New Yorker] article having affected his thinking dramatically,” said Senator Ron Wyden, Democrat of Oregon. ‘He, in effect, took that article and put it in front of a big group of senators and said, “This is what we’ve got to fix”’ (Pear, 2009).

The ensuing discussions helped “localize” the national healthcare debate. Instead of debating whether specific policy options were “socialist” or “free market,” Gawande’s (2009) reading of Powell’s institutional insights suggested how we could learn from the natural laboratories our pluralistic system offers. Instead of projecting how “pie in the sky” healthcare policy ideas would affect the entire system nationwide, the idea of comparing different models that already exist within our current patchwork system (for example, salaried and fee-for-service care [Harris, 2009]) emerged. Although Powell and colleagues did not set out to inform the healthcare debate, their discovery of the mechanisms that underlie the formation of regional clusters helped move a contentious and sensitive debate in a constructive direction.

There are many examples of scholars who carried out research and proposed policy ideas that were not acceptable in the short run but gained support when the political environment changed. John R. Commons is widely acclaimed as the intellectual father of the New Deal labor legislation, but his ideas for unemployment insurance, industrial safety, and labor management relations came from research conducted over the three decades that preceded the New Deal (Cohen, 2008; Kaufman, 1993; Schlesinger, 1939). It took a change in the political environment and the economic crisis of the Great Depression for the work of Commons to be transformed from “academic scribblings” to national policy. Given the time lags involved, it often takes a network of researchers carrying on a research program to have an impact and/or to translate research results into policy proposals. It was mostly Commons’s students who carried the ideas of his three decades of research to Washington and helped shape New Deal policies. Similarly, it took Powell et al. (2010) and a large network of graduate students and scholars 19 years to collect and analyze all of the data needed to compare the research and funding networks critical to regional biotechnology clusters in the United States.

Both short-term, immediate and longer-term policy analysis are needed. If researchers just propose what is “acceptable” to all the stakeholders in the short run, they risk losing the comparative advantage they bring to policy debates—theory, evidence, data, and an independent view not captive to the positions of powerful stakeholders. But only focusing on “pie in the sky” ideas without considering their viability in the short or long run also misses opportunities to exert influence when the opportunities arise. So the trick is to have a research portfolio that focuses on long-term needs and options and to know when to draw from midrange findings to propose compromises or small incremental steps that move policy in a needed direction.

The articles in this issue, not surprisingly, reflect that middle range by posing research questions grounded in longer-term debates that are not limited to specific legislative or administrative action but can usefully inform future strategies. Such an outlook is appropriate for an article in a scholarly journal such as AMJ. To translate publications like these into policy actions, however, normally requires more direct engagement of both the authors and the policy stakeholders.

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1 However, exceptions, in which research applies to specific bills, do exist. For example, Huang and Murray’s results could inform bills on the scope or legality of gene patents, such as the Genomic Research and Accessibility Act (in committee; http://www.govtrack.us/congress/bill.xpd?bill=h110-977).
Translation requires writing for multiple audiences in multiple outlets, extensive use of modern print and electronic media, and direct exposure to government, business, and nongovernmental stakeholders. This mix is more easily accomplished if the research is part of a larger ongoing project or network that links academics, professionals in the private sector, and government officials. More senior researchers can invest the time needed to build and maintain these relationships while protecting the time of junior scholars to focus on designing, analyzing, and publishing their work, as they need to do to build successful careers. Thus, the management and design of research organizations and extended networks play key roles in whether or not research is linked to policy.

**Thick Skin, Modest Expectations, and Timing**

Policy researchers need to be prepared for intense criticism, sometimes for personal attacks (and even sometimes, personal investigations), and for their results and ideas to be used and misused for partisan purposes. It is not uncommon for opposing sides in a policy debate to use the same findings to support their views! Exhibits 1 and 2 provide for some practical suggestions for would-be policy researchers. Research seldom changes the views of partisan groups. What it can do is require partisans to respond to findings and thereby help to frame some, but seldom all, aspects of policy debates. Policy research seldom changes the political balance of power, except perhaps at the margins. Instead, policy research can be put to use when the politics are ready for it. Given the present economic crisis, now might be just such a time.

**THE CURRENT CRISIS AND OPPORTUNITY**

So far we have focused on the technical or operational aspects of conducting policy research. But the current economic crisis calls on all social scientists, and perhaps especially management researchers, to reexamine their research paradigms and programs to ask: Why did we not see this crisis coming? Or, if we did see some of the problems that led to the crisis building, why weren’t our voices heeded? What changes in our research paradigms are needed to learn from this crisis and reduce the chances of it or something similar reoccurring? Perhaps the first two questions are best left to be answered by future business historians. It is the third question—What can we learn from the current crisis that should inform our future work?—that we choose to take up here.

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**EXHIBIT 1**

Practical Suggestions for Designing Policy-Oriented Research

1. **Look for natural experiments and state changes.** Can you compare before and after? Don’t be afraid to reach back in history to do so.
2. **Look for comparative cases.** What baseline conditions are common? How does variance in process contribute to variance in outcomes? You can avoid making functional arguments by paying close attention to processes versus features or attributes.
3. **Look for local experiments.** Local and state governments and nonprofit organizations can often try innovative policies that would not be prudent to try at the national level. How do local experiments emerge and travel? How do organizations create or respond to policy innovations? How might local experiments lend themselves to comparison and generalization to a larger level?
4. **Look for all parties.** When Barley and Kunda (2004) conducted their study of high-technology contractors, they examined the practices of the companies that hired them, the brokers that placed them, and the contractors themselves to understand how the costs and benefits of contracting affected all parties.
5. **Look for leverage.** If it is hard to tackle big social problems, then look to the most critical proximate mechanisms that drive those problems. For example, defending our country from terrorists is a big problem. Designing programs to help young people in war-torn countries attend and stay in school is a more proximate problem with enormous leverage as it removes them from the hands of terrorist recruiters.
6. **Look at the margins.** Case studies of outliers—firms or individuals that are particularly successful, or unsuccessful—in dealing with a problem that has policy implications can be especially interesting, as they may be more affected by policy changes than those at the mean. Such case studies can also inspire large-sample research that may provide more insight into the generalizability of various policy approaches.
Reframing Management Research

At this point, we return to and make explicit our own normative assumption: informing public policy debates is an appropriate goal for "management research" and for the scholars who contribute to journals like *AMJ*. In short, we assume that management research should be research about organizations and their place in society, and that it is not limited to research that is for managers or solely addresses their interests. Thus the term “management research” itself may need to be reframed.

Thirteen years ago, Stern and Barley (1996) argued that the study of organizations’ effects on the social systems in which they were embedded was a “neglected mandate.” This is a mandate because part of our discipline’s agenda is to attend to the role of organizations in larger social systems (Parsons, 1956; Scott, 2000). It is neglected in that, if organizations have become the only prominent actors in society to have significant cultural and political influence (e.g., Coleman, 1990; Perrow, 1991), few scholars have developed theories in which organizations are the primary actors (Stern & Barley, 1996: 148-149). What is odd is that in a world of rapidly growing and failing organizations, the “hot shops” of scholarship that have gained policy makers’ attention are behavioral finance and decision theory, both rooted at the juncture of psychology and economics and focused on individual decision making (Bazerman, 2005; Levitt & Dubner, 2006; Thayler & Sunstein, 2009). However, as the current economic crisis and others, such as the Challenger and Columbia shuttle disasters, reveal, crucial decisions happen in complex organizations where causation is not as simply defined as experimental conditions will reveal.

Is this mandate still neglected? Was our research community too reluctant to challenge prevailing managerial and organizational practices? If so, this may be because our normative assumptions implicitly or explicitly took organizational goals as the starting point for framing problem statements. If policy research inevitably requires consideration of multiple stakeholder interests, we have to view “management” as simply one of these competing interest groups. This is the ultimate implication of a social systems perspective.

This is not to say that some management research, including research published in previous issues of this journal, didn’t identify behavioral patterns and organizational processes that warned of impending problems, such as the incentives for stock analysts to issue positive reports on firms that reciprocated with favors (Westphal & Clement, 2008), the growing power of finance inside American firms and the increased influence of Wall Street on CEOs (Fligstein, 1990; Jacoby, 2005; Useem, 1993), or the mutually reinforcing network of executives, board members, and compensation consultants (Bebchuk & Fried,
Looking Forward

We hope the Special Research Forum—Public Policy and Management Research: Finding the Common Ground encourages more management scholars to broaden their perspective by asking, What values and social problems are relevant to my work? How can I frame a research question that speaks directly to some dimension of the problem? What natural experiments or changes in the environment can I leverage to explore deeper policy implications? What theories can I mobilize to speak to this question and how do I expand the range of causal variables to incorporate the full range of policy options/actions that address the problem? And for more senior members of the profession: How can I build and engage a network of colleagues and students in an ongoing study of an important societal issue that can be informed by management and organizational research? Asking these questions as a regular part of our scholarly work is what it will take for the management research community to become a more significant player in shaping future public policy, and perhaps help to avoid the next crisis or meltdown in management practice.


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