

RESPONSES TO OUCHI PANEL ACADEMY OF MANAGEMENT JOURNAL, DECEMBER 2005

COMMENTS BY STEVEN KELMAN

Harvard

PUBLIC MANAGEMENT NEEDS HELP!

Much of the pioneering work in organization theory was written about public organizations, or with public organizations in mind. When Weber wrote about bureaucracy, he was thinking of the Prussian civil service. Philip Selznick began his scholarly career writing about the New Deal Tennessee Valley Authority in TVA and the Grass Roots (1953). Herbert Simon's first published article (1937) was on municipal government performance measurement, and Simon also co-authored early in his career a book called Public Administration (1950), and a number of papers (e.g. Simon 1953) published in Public Administration Review. Michel Crozier's classic, The Bureaucratic Phenomenon (1954), was about two government organizations in France.

Yet, as the field of organization studies has grown enormously over the last decades, the attention the field pays to in public organizations and public-policy problems has withered. This despite the fact that the public sector, as a percentage of GNP, is much larger now than it was when these classics were written.

This change reflects larger social trends. Since the 1970's, the salary gap, for professional and managerial work, between government and industry has dramatically increased. (Donahue 2005) For much of this period, business was culturally "hot" as a place of both glamor and excitement. Reflecting these larger trends, business schools have grown enormously, so that today the overwhelming majority of scholars studying organizations work in that environment.

During this same period, research about public organizations became ghettoized, the province of a traditional field called "public administration" and a new one calling itself "public management" arising in connection with establishment of public policy degree programs at a number of universities in the 1970's and 1980's. Although there are real differences in research focus, methods, and teaching orientation between these two areas, they share common shortcomings. They are relatively small in size compared with the much larger domain of business-school based organization studies. And, generally (though this is changing) they are relatively primitive in their research methods – with excessive reliance on case studies, selection on the dependent variable, and broad theoretical frameworks with weak empirical grounding.

To me the case is fairly straightforward that we have a problem. Our country, and other countries, face serious challenges of managing public organizations effectively, and of solving intractable public problems that have a strong management component. Not enough scholarly firepower is being directed at helping with these challenges.

Two things need to change. The small band of scholars working on public administration/public management need to connect to the broader world of mainstream organization theory, which can help enrich our understanding of the public-sector problems we study. And more scholars in the mainstream organization theory/behavior communities need to work on public organizations and public problems.

Happily, there are small signs that this is happening. On the public-management side, one sees an increasing number of cites to mainstream organization theory/behavior work in leading field journals, such as The Journal of Public Administration Research and Theory. Another journal, The International Public Management Journal, seeks consciously to bridge the gap between researchers in public management and mainstream organization theory/behavior, and has added people such as Paul DiMaggio, James March, and Karl Weick to its editorial board. On the organization theory/behavior side, recent work such as Ouchi (2003) and Bazerman and Watkins (2004) reflect a new interest in public management issues on the part of well-established organization researchers. This symposium, and the theme of the 2006 Academy of Management meeting ("Knowledge, Action, and the Public Concern") are themselves extraordinarily promising.

Where might organization research make contributions to better public-sector performance?

- Management of routine government operations: States run organizations that licence drivers and register motor vehicles. The federal government answers citizen questions about taxes and social security, makes weather forecasts, and develops predictions about future demand for different occupations that are used by high school and college counselors.
- Responses to high-visibility public problems that significantly involve how government organizations are managed: Successfully dealing with problems such as educating children – the

subject of Ouchi's interest -- requires (in a world where many or most schools are public) improved organizational performance by government organizations. So do reducing crime, fighting terrorism, managing emergencies, and protecting against public-health threats, as well as improving the environment or maintaining a securities marketplace the public trusts (the latter through regulation of private actors). In many countries, this list would include the delivery of health care.

- Policymaking in small groups: Senior government officials, generally in groups, are constantly making important decisions about high-visibility foreign and domestic policies – ranging from whether to invade Iraq to whether an old city neighborhood should be torn down for an urban renewal project.

In each of these areas, current research in organization theory/behavior can make contributions. Until proven otherwise, it may be assumed that research findings involving such standbys as team performance, networks, organizational citizenship behavior, and organizational learning apply to public as well as private organizations. Even here, it would be extremely helpful to locate much more field-based research on topics such as these in government organizations, to see if publicness acts as a moderator of relationships between independent and dependent variables we study.

But obviously there are differences between private and government organizations as well. (Rainey 2003: Ch. 3) Among the most obvious are

- operation of these organizations in a political (in both the good and bad senses of the word) external environment;
- lack of profits as a performance measure;
- less ability to use monetary incentives to influence the behavior of individual employees and managers;
- the stronger orientation of many organization members to the substantive purposes of the organization;
- the greater need for organizations looking at different aspects of a problem (such as “connecting the dots” on terrorism or dealing with the educational, nutritional, and cultural problems of disadvantaged youth) to work together across organizational boundaries;
- the government role in delivering not only services but also obligations, such as duties to pay taxes and obey the laws (Moore 1995: 36-38);
- the greater use of contracting with private organizations – i.e. market rather than hierarchy -- for some core organizational functions (such as weapons production, and studies of the costs and benefits of some environmental regulation);
- the greater public visibility of the organization's internal activities (and the greater symbolic importance of the organization's activities to people's feelings about the society in which they live);
- the greater sensitivity of those (in the political system) providing the organization with resources to avoiding scandals as opposed to creating results.

These differences mean that there are many issues involving organizational behavior that are relatively more important in public than private contexts, and there are others that arise almost exclusively in a public organization context. Examples of the former include the impact of non-financial performance measures on organizational performance, eliciting good performance through other than financial incentives (including the role of what public management researchers, e.g. Crewson 1997; Jurkiewicz *et al* 1998; Houston 2000, call “public-service motivation”), the organization of interorganizational collaboration for reasons other than profit maximization, and the management of complex contractual relationships. Issues arising more or less only in public organizations include the management of obligation delivery, and relationships between elected officials (metaphorically government's counterpart to a board of directors) and career agency officials.

There is one important thing I believe public management researchers have to teach mainstream organization theory/behavior ones, and that is the legitimacy of prescriptive research – i.e. research seeking explicitly to theorize and gather empirical evidence about effective practice. Public management scholars typically see prescription as an important role. I would speculate that this may partly be because we identify more with the organizations we study than do many business-school based organizational researchers, and partly because the organizations we study so clearly need help. My own strong view is that, as long as research is rigorous, prescription is something to embrace, not shun.

Researchers who have never done work in public organizations will want to know the answers to two questions: Can I get access? How do I learn about what's already been done? As to the first, I would guess that access to government organizations is easier on the whole than to private ones, because many government organizations believe their public status more or less obligates them to cooperate with researchers. In over 25 years of doing

empirical research in government organizations, I have never once been refused access, even as a graduate student. Groups such as the Partnership for Public Service in Washington are able to offer limited help in gaining research access. As to the second, people might wish to look through recent volumes of journals specializing in public management, and at Rainey (2003) or the recently published Oxford Handbook of Public Management (Ferlie et al: 2005) to get started..

COMMENTS BY JANE E. DUTTON U. of Michigan

Bridging the gap between management-organizational research and public policy

When I read about Bill Ouchi's decision to move to the educational public policy realm it inspired reflection. It is a move that I wish more of us would make—either as a full leap or as an incremental step. I believe that organizational-management researchers have much to say about public policy questions, but I also believe that we are not at the table and are not having influence.

The reminder from Hurricane Katrina

The problem of management-organizational researchers not being at the table and not having influence is particularly salient to me as I write this response in the wake of the aftermath of Hurricane Katrina. As the wrenching images and stories of organizational failure circulate, and the momentous task of rebuilding organizations face government leaders and citizens, I wonder, what difference would it make if organizational-management researchers were at the table or anywhere on the scene to wrestle with and contribute to these action-based discussions? What difference would it make if our research informed, influenced or inspired useful thoughts and actions about organizational capability-building and rebuilding? How would being able to make contributions to knowledgeable actions in the wake of Hurricane Katrina, but also making contributions to public policy questions more generally, alter the professional identity of organizational scholars?

I believe that the answer to both questions is that it would make a substantial positive difference if management-organizational research were impacting issues and questions arising from public policy and public management more generally. I believe that organizational-management researchers have many useful research-based ideas that could meaningfully address questions about how to rebuild and reinvigorate organizational capability in times of trauma and also in more “normal” times. The challenge remains, how do we make management-organizational research accessible and usable for the people how need it?

While I do not have definitive answers, I suggest four action domains for making progress in bridging the gap between management-organizational scholarship and public policy. Each suggestion domain represents my answer to the question of what we as a field could do differently if we wanted management research to link more usefully to questions for public policy and public sector management. Each suggestion could be implemented at the professional level (e.g., through professional associations) and/or at the university or departmental level.

Four action domains

Enrich research knowledge distribution channels. Traditional avenues for publishing organizational and management research are valuable means for distributing research to a specialized group of interested academics and practitioners. However, two challenges to linking management-organizational research to public policy domains include the specialized knowledge distribution channels and the relatively lengthy time periods from research completion to knowledge availability.

What new possibilities would arise if organizational-researchers had multiple web-based outlets for distributing refereed research that allowed for rapid and useful dissemination of knowledge that was applicable to pressing short-term or enduring long term public policy questions? If such outlets were available, I think there could be more enriched debate and useful ideas that could influence public policy debate and practice, while also providing feedback to the community of researchers about the utility of the ideas.

We have much to learn about how to distribute knowledge in ways that makes it useful, relevant and timely for public policy makers. If we return to the Bill Ouchi's book and the issue of educational reform, we might ask, what experiments in knowledge distribution could be deployed (by professional associations, by universities, by state and local governments) to learn how to take this knowledge accessible and usable in a timely way?

Create incentives to contribute to public policy knowledge and debate. We need incentives to participate in public policy knowledge building. These incentives include opportunities to publish management and organizational research relevant to public policy questions in journals that count for tenure and career

advancement. The fact that this debate and commentary is happening in the Academy of Management Journal is a promising sign that the Academy of Management journals will be open to these types of topics. I hope that this opening is more than a temporary blip and that it represents a real shift to seeing management and organizational theory as relevant to public sector organizations and to policy questions.

We need incentives and opportunities to participate in knowledge-building conversations with public policy implications. Business schools and other employers of organization-management researchers, as well as professional associations like the Academy of Management, need to encourage and host meaningful debates and dialogues that link management research to public policy. Faculty and students who create and participate in these kinds of interdisciplinary conversations need to be recognized and rewarded. Thus, I hope that the Public Affairs forum created for Bill Ouchi's talk at the Academy of Management is more than a one year acknowledgement that management research can speak to public policy practice. I hope it becomes an enduring feature of our professional conversational space and that the people who contribute to it are rewarded.

Build social networks linking researchers and public policy makers. If we build incentives for more public-policy –relevant conversations, then we may make progress in building social networks that link management scholars with public policy makers and users. In Bill Ouchi's story about involvement in the educational policy debate, there is a clear story of friendship and social connections that drew and sustained his dramatic career shift. Without social networks that enable and sustain connectivity between management researchers and public policy makers and users, attempts to build management knowledge relevant to public questions are likely to be less fruitful.

How can management scholars create meaningful social networks that forge high quality connections to public policy makers? I think the creation of joint endeavors such as courses and classes in universities, joint institutes, conferences, etc. illustrate the possible use of legitimate organizing forms to interconnect knowledge and practice in public policy and management. There are also possible new role structures that could be developed to interconnect public policy with management e.g., through internships or post-doctoral opportunities. The key to success in any of these endeavors would be to build familiarity and trust across boundaries to create a viable social fabric for sustaining knowledge work across these two critical domains.

Alter doctoral education. I think the real place of impact for developing a more meaningful link between management research and public policy is in the world of doctoral education. Future management scholars need to be encouraged and prepared to link their work to public policy issues. I sense a deep hunger from the students I work with, to do work that is more relevant to public policy domains. However, there is limited food for feeding this hunger in terms of courses, skill training and professional preparation for being the kind of scholar who could do this work well. The new domains of training might include how to build alliances and partnerships, how to effectively translate and communicate research, or how to build contexts for high impact dialogues across multiple intellectual and practical boundaries. If we are really going to do management research that matters through its connection to public policy issues, we must be imaginative and committed to re-visioning doctoral training.

In sum, speaking to public policy in a meaningful way requires restructuring our ways of organizing as a profession. It means expanding and innovating around knowledge distribution. It implies new forms and new bases for rewarding. It calls for new forms of social connecting and it invites new visions of educating future management scholars. Are we up for the challenge? Good question.

COMMENTS BY DON HAMBRICK

Penn State

I'm pretty sure I was invited to participate in this panel because of my presidential address, now more than a decade ago, in which I asked, "What if the Academy actually mattered?" Oddly, my speech is often misinterpreted as an indictment of the irrelevance of management research, when, in actuality, I was elbowing our main professional society – the Academy of Management – for its timidity in bringing the excellent scholarly work of our field to the public's attention. In the intervening years, the Academy has improved substantially in its willingness and ability to let the world know what we're doing and why it's important. For example, our research is featured in the business and general press far more than it used to be. Indeed, I'm not sure I could levy the same vigorous attack on the Academy today as I did in 1993.

Now, after learning about Bill Ouchi's awe-inspiring contributions to the improved management of public schools, I am moved to make the point that many mistakenly thought I was making all along: *Each of us, as individual management scholars, could and should matter much more than we do.* Bill Ouchi's impact on the world around us is noteworthy not only because it is grand, but also because it is so damn rare. Very, very few management theorists step forward in the way Bill has. And he's done it on his own. Contrary to the theme of

my presidential address, he didn't rely on the Academy of Management or any other formal organization (as far as I can tell) to pave the way or trumpet his ideas. He spotted a big-time practical problem, saw the relevance of proven management theory for helping to solve the problem, undertook an eloquent translation of that theory, observed the theory in practice, wrote and talked, talked and wrote – all while investing mightily of himself.

Granted, Bill has a gift for practical insight, a distinguished track record, and enviable connections, all of which helped make his public school project so successful. But each of us, even with our more limited endowments, can find ways to take what we know about management and place it in the world of practical affairs. I'll highlight three potential ways for us to make a difference. Bill Ouchi's approach, as reflected in his public school project, is in the third category.

First, and most natural for those of us in business schools, we can work to help business managers do a better job. Let's face it: A lot of managers need a lot of help. Where are they going to get it? Airline magazines? Glib consultants? Other panacea providers? How about if they get their insights from those of us who have thought intricately and comprehensively about management and business? After all, we are the ones who have studied voluminous data on what works, why, and when. Each of us should increase our commitment to deriving the practical implications of what we know, translating those implications into understandable and persuasive language, and then conveying our best insights to managers – by writing more for practitioner audiences, speaking more to business groups, and participating more in executive development programs.

Second, we should aspire to have a bigger voice in the shaping of public policy toward business. When state and national legislatures, regulatory bodies, and stock exchanges deliberate on policies that affect businesses and their stakeholders, they routinely invite economists and legal scholars to weigh in. But it is very rare for management scholars – who of course know quite a lot about the functioning of firms – to have a say. Indeed, the insights of our best management scholars could help to improve the quality of public policy in such diverse areas as workplace safety, employment practices, corporate disclosure, and governance. Now, I am not so naive as to think that just any of us can simply show up at the halls of Congress or the SEC and be heard. We have to be invited, which in turn means we need to be visible, assertive, and persuasive. The starting point is believing that it's possible and desirable, which I do.

The third way that management professors can increase their impact, and the way taken by Bill Ouchi, is to transport our best insights from the world of business into other organizational spheres. Bill took a wealth of accumulated knowledge about the benefits of decentralization and injected it into the mess we call public education. Just think of all the public sector and not-for-profit organizations that are under- or mis-managed, including health care providers, municipal governments, the U.S. security establishment, and the United Nations. Over the years, I have had the good fortune to conduct executive development programs for college presidents, police chiefs, military leaders, and mayors. I've always learned a great deal from these groups, but I've also been gratified and impressed by how they embrace and put to use the concepts and tools that are at the heart of effective management. To be sure, the techniques of business require certain adjustments and sensitivities when applied in the non-corporate world; but, as we see from Bill Ouchi's successes, they can work wonders. More of us need to extend our research to non-business settings and write for academic and practitioner audiences in non-business fields (including public administration, education policy, and health care administration). I am very appreciative of the members of the Academy of Management who are already dedicated to the improved functioning of these other organizational sectors, but I believe more of us should join them.

As management scholars, it is our job – collectively – to know more than anyone else about how organizations work and how they can be improved. And I happen to think we do. For the sake of society, we mustn't just pass our knowledge around among ourselves. We must step out of our monastery. We need to tell the world what we know, in language that makes sense for them. We need to listen and watch carefully to see if our ideas are really on the mark or not, then refine our ideas, and so on. Bill Ouchi has provided an extraordinary model of what individual management scholars can do to influence the world around us. Let's not simply admire Bill's endeavors, but try to follow them each in our own way.

COMMENTS OF KARL E. WEICK

U. of Michigan

Daniel Robinson (2003) provides an apt frame to examine the question, what does it take to really matter on policy issues? “The relationship between a contribution and its impact, then, is not unlike that between rhetoric and the attending audience. The effect of the content will depend, of course, on the content itself, but also on the perceived standing of the speaker, the powers and purposes of those who hear it, the inevitable ‘noise’ in the system, the selective and evanescent nature of memory, and competing signals coming from other

and comparably authoritative sources” (p. 206). Faced with this context can we, can Ouchi, enact persuasive rhetoric for policy? If so, how? I want to take a closer look at some specifics in Ouchi’s experience with schooling and use those as a platform to suggest that whether our work matters is only partly under our control.

Mattering is a difficult aspiration to guide with any precision because policy and plans are loosely coupled to outcomes (Clarke, 1999). Given this slippage, we may be in a stronger policy-relevant position if we direct attention to ‘public sector management’ of policies and policy making (Lyman Porter’s phrase) rather than to the substance of policy per se. In other words, our strengths may lie in articulating the dynamics that loosen and tighten couplings between policy and execution, in working backward from outcomes toward intermediate practices that implement policy, and in articulating how movements at the grass roots shape policy at other levels. For example, we know that people often act their way into strategy and policy (e.g. Mintzberg & McHugh, 1985). Grass roots actions on the frontline enact the pathway that policy eventually codifies. Our contributions may lie in the exploration and encouragement of this frontline mechanism (see Ouchi, 2003, pp. 251-252).

Ouchi’s impact can partly be traced to just such a mechanism. Having found a school system (Edmonton) in which autonomy produced the effects he expected it would, Ouchi codified potential mechanisms by which this effect was produced, exported his understanding of the mechanisms to other systems, watched the effects in other systems, identified comparable settings where different mechanisms were operating, and consolidated what he found into a compelling storyline. The policy-relevant assertions in Ouchi’s example started with successful outcomes energized by Superintendent Mike Strembitsky on the frontline. Ouchi took these efforts and, through a series of inductions, abductions, and deductions, grew them into a potent force for policy change. A perfect example of how the frontline mechanism works is found in the maps and globes story. Given the task of buying globes and maps for a school system Strembitsky asks, “How do I know which maps and globes to buy?” The answer, scripted by central office routines, is to hire a consultant, run focus groups, issue an RFP, buy products, warehouse them, send them to schools when schools request them. In a moment of autonomy “Big Mike” asks, ‘Why don’t I just cut a check for the principals and let them buy their own maps and globes?’ Here is an instance of a small win for schools, an instance of acting your way into policy, and an example of grass roots policy making. Management researchers add policy-relevance to this scenario when they codify it, appreciate its significance, trace its diffusion, generalize it to other policy bureaucracies, frame it as a prototype, and treat it as a small win that is likely to uncover other opportunities for change.

Ouchi describes himself as a person who has “spent 35 years studying the design and structure of very large business organizations” (p.2). I think that description needs to be refined. What Ouchi has done for 35 years is pay particular attention to issues of control, its forms, its costs, and its effects on felt autonomy and outcomes. Ouchi has had an especially keen eye for what Perrow (1986) calls third order controls: beliefs and premises held by a community of people that hold the community together. Community, interaction, relations, decentralization and their dynamics were studied long before Ouchi made his mark in ordering and documenting these dynamics, and Ouchi’s rich depiction of these effects preceded the simplifications and statics of language focused on M form structures. Ouchi’s ability to grasp and describe the dynamics of decentralizing and centralizing up close seem more crucial for his policy success than does draping the language of an M form structure over these observed nuances. The point is, policy relevance increases when investigators have deep understanding, clinical skills, and thrive on immersion in the phenomenon. Notice, incidentally, that when Ouchi was searching for his next project several years ago, the issue of how large public school systems were organized was his third choice of project (Barney, 2004, pp. 114-115). His first choice was investigation of internet strategy, of which he found none. His second choice was corporate governance, which he dropped because of anticipated resistance. Even though the study of schools was something of an also-ran, the important point is that control is an underlying theme in all three of these options. And in all 3 cases the issue was a fundamental issue in management about which he had considerable expertise and about which he felt strongly.

The role of strong feelings should not be discounted when it comes to what it takes to matter. When Ouchi looked at schools, his initial diagnosis was that schools lacked autonomy. A similar concern with issues of autonomy was implicit in earlier discussions of loose coupling in educational institutions (e.g. Weick, 1976). Earlier work suggested that schools whose core technology was loosely coupled to administrative structures tended to be more responsive to local contingencies. Ouchi refocused this conclusion and paid closer attention to issues of control that were fuzzy in those earlier discussions. Ouchi said in effect, it’s important for schools to have autonomy and I’ll show you why. Ouchi’s success as discussed in the symposium may, therefore, be due in part to the fact that he had a conviction in search of evidence rather than a hypothesis in search of data. But this was not just any old conviction. It was a conviction informed by repeated prior observations that autonomy close to the action improves meaning and relevance, that local culture shapes behavior through guidance and direction rather than through regulations and rules, and that autonomous but accountable principals could enact responsive schooling through culture change backed up by their control over significant resources.

In what I've said up to this point, and in much of what was said at the AOM symposium, there is a recurring focus on a mere handful of individuals. Richard Riordan, for example, mentions that Ouchi "almost single-handedly changed the whole national discussion." Riordan underlines this singular impact by quoting Peter Drucker's statement that "All great movements in the world start in the heart and mind of a single person". It's conceivable that all we see here is that the fundamental attribution error (FAE) is alive and well and making it harder than ever to matter for policy. But maybe the FAE is neither quite so fundamental nor quite so much of an error. Individuals do make things happen. That assertion may understate the extent to which context makes it possible for individuals to have 'single-handed' impact. After all, decentralization of resources seems to be especially effective in settings where parents can choose which schools they use and budgets are set by the number and kind of students that a school attracts. Nevertheless, the assertion that single-handed action is a key determinant in policy success could also underscore the importance of agency for policy impact. To focus on agency is to reaffirm that strong, persistent, creative people enact the environment of receptiveness which then draws in their policy proposals as the solution.

The opening quotation in this commentary suggested that policy impact is only partly under our control since it varies as a function of content, speaker, power and purpose of audience, noise, memory, and competing signals. If we use Ouchi as a prototype of policy impact as the editors have done, and review his efforts in terms of Robinson's categories, we find that the *content* he espouses has intuitive appeal, directness, and avoids the common social science pitfall of "wordy elaborations of the obvious" (Taylor, 1985, p. 1). We find with regard to the standing of the *speaker* that, in Riordan's view, the speaker has legitimacy because he is seen as "applying ideas to public education that have proved themselves over and over again in the corporate sphere". Legitimacy in this example also comes from Ouchi's expertise and track record. We find with regard to *powers and purposes* of those who hear the message that funding agencies resonate to improving school achievement, that principals are more enthusiastic about Ouchi's message than are superintendents, and both are more enthusiastic than boards and central office personnel. Nevertheless, student achievement is something that all of these constituencies have some stake in. We find that there is considerable *noise* in the system in the form of inadequate information about school performance, mixed instances of centralized and decentralized control (e.g. Houston school system), threatened prerogatives, and confounded changes (e.g. decentralization coincides with changes in size of enrollment). We find that the dangers of selective and fleeting *memory* are counteracted by vivid stories that are easy to recall, a complex intervention boiled down to 7 key principles (Ouchi, 2003, Part 2), and a compact takeaway, namely, decentralization with accountability and choice improves performance. Finally, we find *competing signals*, best exemplified by Governor Lingle's statewide board of education (e.g. "We in Hawaii have the quintessential bureaucracy that is centralized and can block reform") and Federal requirements that dampen local initiatives.

Robinson's dimensions and Ouchi's placement on them suggest some of what strengthens and weakens policy impact. Ouchi does not control all of those dimensions. Furthermore, placement on these dimensions will vary as a function of issue. For example, issues of healthcare delivery and environmental policy seem to have more noise, more competing signals, fewer vivid memory traces, and less legitimacy on the part of social scientists than is true for the context in which Ouchi works. Thus, to matter more to funders and practitioners in the healthcare and environmental arenas we would need to pay closer attention to public-sector management of policy making. We would need to highlight mechanisms by which people are heard under noisy conditions (Tsoukas, 2005), prevail in agenda setting, consolidate memorable images, and become more credible by means of consistent action.

In our eagerness to matter we need to be careful not to confuse impact with enthusiasm. Enthusiasms tend to have a short half-life, are driven by more than social science evidence, and tend to be fallible guides to policy significance. As Jim March often notes, 'breathlessness is not a substitute for rigor.' This is not to downplay the importance of enthusiasm because enthusiasm gets things done. But enthusiasm is easily satiated, which means it typically matters for only a short time. Ouchi is unmistakably enthusiastic. The very last word on the last page of his book is "Revolution!" (p. 262). But, he can afford to be enthusiastic because on the other 261 pages he has documented a useful managerial design that simultaneously embodies a policy (decentralization) and is the means to implement that policy (enactment of M form structures). That twofold relevance is crucial because it plays to our expertise although it does not guarantee impact. Policy consists both of directions that manage and the deployment and enactment of these policy directions in managed occasions. That gives us two pathways to policy relevance. Occasionally, as in Ouchi's work, that is enough to matter.

Organizational Scholarship and the Eradication of Global Poverty

There is a great gap in wealth among people of different nations. Millions in rich countries have comfort and affluence far beyond what anyone could have imagined two centuries ago, while those in poor nations live in unconscionable poverty and insecurity. What is more, our widespread mutual knowledge of these differences challenges us both morally and practically. Global poverty is one of the most important challenges of our time, one that I believe organizational scholarship can help address by bringing to bear its understanding of organizations and the managers who run them.

Because large, meritocratic organizations are a feature of “modernism,” organizational and management scholars have rarely thought there was much use for their specialized knowledge in places where organizations are few, and rarely large or complex when they do exist. However, in the late 1980’s I found myself in a country undergoing the collapse of communism, and I discovered that organizational and managerial scholarship can be a useful guide to the challenges of steering a society through such a transition (c.f., Pearce, 2001). Recently, I have extended that work to the application of organizational and managerial scholarship to the problem of national poverty.

The question of why some nations are so much wealthier than others has long been studied by economists, political scientists, and sociologists. While some scholars have proposed that poorer nations have little chance of gaining in relative wealth (Wallerstein, 1979), others have provided evidence for the wealth-creating effects of cultural values (Weber, 1988), economic growth (Chernery et al., 1974), capital markets (Leff, 1976), trade policies (Behrman & Srinivasan, 1995), geography (Diamond, 1997), and property rights (De Soto, 2000), among others. Because of this important work we know a great deal about the role of geography, physical resources, market institutions, and government policies on wealth creation. These streams of research have produced important policy changes that have had materially positive effects. Nevertheless, they all have neglected an important explanatory factor in wealth creation, the role of large, meritocratic organizations that are free of direct governmental control (see Pearce, Xin, Xu & Rao, 2005).

Why organizations? Recently, poverty scholars have indirectly addressed complex organizations in work that converges on what has been called the quality of governance, or the practices of national governmental organizations. Governance quality is state effectiveness (the relative capacity of states to control their territory and officials), and state capture (law making and implementation is captured by elites who use this power to benefit from government weakness). Poor quality governments are unpredictable, corrupt and weak; extensive empirical work consistently finds a strong positive relationship between governance quality and the average individuals’ income in those countries. Further, Evans and Rauch (1999) and Kaufmann and Kraay (2002) provide evidence for a strong causal effect of state governance quality on growth in per capita income.

However, with the notable exception of the World Bank (1997), these economists, political scientists, and sociologists have not focused on how governance quality does so. Rather, they provide simple post hoc speculations about how individuals might make different investment decisions under quality governments. So, while these scholars are accumulating data that incontrovertibly establish that better governance quality produces greater national wealth, they have not conducted thoughtful analyses of *why* governance quality is effective.

I contend that organizations are central to understanding why and how government quality matters. Governance quality has powerful effects on wealth creation through its better facilitation of large, meritocratic organizations independent of governments. These kinds of organizations are not owned by governments, are largely independent of government in their ability to set their own objectives, and are independent in their sources of investment capital and revenue (Rao, Pearce & Xin, 2005). Certainly the largest independent organizations are found in the handful of wealthiest countries (Pearce et al., 2005). The richer the country, the more likely it is to have the largest, meritocratic independent organizations, even when the country, like The Netherlands or Switzerland, is not large. The strong congruence of all three -- governance quality, large, independent, meritocratic organizations, and national wealth -- suggests that governance quality may work as much through such organizations as it does through atomized individuals’ incentives. After all, there are wealthy individual investors in even the poorest countries. High levels of *aggregate* national wealth require wealth to be dispersed beyond a handful of national elites, and as an organizational scholar I suspected that organizations might be central to that process.

As organizational scholars, we know that the larger the organizations, the more they disperse knowledge and responsibility via decentralization (Donaldson, 2001). Size-induced decentralization makes middle managers and professionals more important to organizational success, and provides them with the market and technical knowledge necessary to develop their own spin-off organizations (Child, 1972). As organizational scholars, we also know that resource dependence drives strategy, and organizational dependence on government favors making cultivating government officials their most important strategic contingency (Hickson, Hickson, Hinings, Lee, Schneck & Pennings, 1971; Pearce, 2001). Organizations independent of government ownership

and revenue, whether for-profit or non-profit, face performance pressures that foster innovation and meritocracy. And of course, large meritocratic organizations can produce more complex products and services more efficiently than a handful of individuals (Weber, 1947).

Large, meritocratic independent organizations produce and disperse wealth, and so one of the most important ways governance quality creates aggregate national wealth is through supporting such wealth-creating and dispersing organizations. Individuals can become wealthy under even the most adverse governmental circumstances, but national wealth appears to require large, meritocratic, independent organizations. Such organizations, and the managerial behaviors that sustain them, are not the sole engines of national wealth creation, but their critical role is poorly understood by policy makers concerned with alleviating poverty.

As organizational scholars we can provide the nuanced understanding of organizations that can help to craft governmental policies and practices that facilitate them. Because we work directly with managers in our research and teaching we have a richer understanding of the decisions they make and why. Because we are scholars we have a broad theoretical understanding of the organizational dynamics that influence their actions. Because we are organizational scholars we notice organizations.

How can we use our insights to make a difference? Here Bill Ouchi's work is a model. First, like Bill, we have to test our insights. I have begun to do this by laying out the argument and providing some preliminary tests based on managerial actions (Pearce et al., 2005). Second, like Bill, we need to recognize that really important societal problems require more than a single limited study. To further test and refine these ideas, I have begun to take advantage of many nations' changes in governance quality over the past fifteen years to better test these arguments about the role of large, meritocratic, independent organizations in wealth creation. And I hope the provocative tone of the claims made here might spur others to elaborate, modify and empirically debunk these claims in their own work. Third, like Bill, I don't think policy recommendations can wait for the necessarily long accumulation of evidence. Therefore, simultaneously with these further tests, we have begun trying to identify which particular governmental policies seem to be the most facilitative of large, meritocratic, independent organizations. Once we have a better understanding of these particular policies we hope to follow the model of Bill Ouchi, as well as other scholars such as Hernando De Soto, who have worked with great creativity and persistence to translate their scholarly insights into action.

Organizational scholars have long noted the connection between large, meritocratic organizations and modernism (that is, national wealth; Meyer & Rowan, 1977; Weber, 1947). We also have learned a lot about organizations and why managers make the strategic choices they do. Why shouldn't we be as willing to apply that knowledge to our most important societal problems as we are it to the technical problems of businesses?

COMMENTS OF PAUL ADLER

Developing a Field With More Soul: Standpoint Theory and Public Policy Research for Management Scholars

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We believe that the question put to us (whether we should be more involved in scholarship and research that is relevant to public policy) is of paramount importance for the development of the Academy of Management (AOM) and for each of us as scholars of management. We live in a world of immense and unnecessary suffering and destruction. Immense: this needs no explanation; this season's news reports suffice. Unnecessary: much human suffering and environmental destruction are not the inevitable result of our earthly existence but are rather of humanity's own making. They are the result of wanton exploitation and, therefore, are remediable. Facing this human-made misery, a posture of quiet acceptance would mean tacit endorsement. In our view, this position cannot be defended ethically. The ethical imperative is even greater for people like us whose comfort is due, in part, to these processes of exploitation. But, if we must chose action over inaction, then what type of involvement would put us on the right side of the issues and would help us contribute to solving such problems?

The stakes associated with these questions, as highlighted in the editors' introduction, do involve esteem and funding for the profession and its members. More importantly, the questions also raise provocative ethical issues about the nature of the profession and about the research each of us undertakes. Attempting to answer these questions brings us face-to-face with some of the thorniest issues in contemporary philosophy of science -- and with some severe limitations of management research as it is currently practiced.

The AOM Code of Ethical Conduct and Research in Management and Organization

It is against this backdrop of exploitation, suffering, and destruction that we find inspiration in the AOM Code of Ethical Conduct, which includes the statement, "members of the Academy can play a vital role in encouraging a broader horizon for decision making, by viewing issues from a multiplicity of perspectives, including the perspectives of those who are the least advantaged." How successful are we as a community of scholars and educators in articulating the perspectives of the least advantaged, understood as exploited people and natural environments? This is an enormous question and one that we raise here to provoke thinking more than to provide a complete answer.

Let us start with some data. In a recent study, Walsh, Weber and Margolis (2003) coded every empirical paper published in the *Academy of Management Journal (AMJ)* and its predecessor journal, the *Journal of the Academy of Management*, for the years 1958-2000. Using an expansive definition, they classified each dependent variable as *welfare* if it addressed any facet of health, satisfaction, justice, social responsibility or environmental stewardship. They classified the dependent variable as *performance* if it addressed technical, accounting or financial performance at any level of aggregation. Over this period, some 13% of articles addressed welfare but not performance, 22% performance but not welfare, 7% addressed both, and 58% neither. The trend is even more telling: since 1978 the proportion of articles that have addressed welfare of any kind at any level of aggregation (with or without addressing performance) has declined almost continuously from a high of around 32% to an average of around 15% in the last 5 years of their period. The proportion that addresses performance has grown continuously. These data may not be definitive but do lend credence to Walsh et al.'s (2003) conclusion that our research has focused little (and progressively less) on the social welfare objectives of enterprise. To the extent that this pattern is a valid estimate of tendencies in one of the field's flagship journals, it raises questions about the values and politics that generate this set of research priorities and questions about whom and what interests are best served by this emphasis.

In a similar type of study but one focused on the natural environment (for the period 1990-2004 using the ABI/Inform ProQuest Database), Jermier, Forbes, Benn and Orsato (2006, in press) found rapid increases in the absolute numbers of scholarly articles published on environment-related topics. However, even for the most recent period 2000-2004, articles on environmental topics constituted only a tiny proportion of the total published -- about one percent. This same tiny ratio also characterized the 15 top-rated management and organizational studies journals for the years 1996-2004. (Ratings of the journals were based on the citation impact quotient in the *Social Science Citation Index* for the year 2004.) This means that for every study on an environment-related topic in the management and organizations literature, there are 99 others that do not significantly address environmental issues. In light of the rapid and continual deterioration of the health of our ecosystems, this pattern again raises questions about the field's research priorities and about whom and what interests are served by this emphasis.

If the pattern that is reflected in these two studies accurately gauges tendencies in the management and organizational research literature, it is difficult to see how we are advancing the perspectives of the least advantaged. Without a strong articulation of these perspectives, we are limited in our ability to contribute positively to public policy, which arguably depends on knowledge about social and environmental problems. How can we do better?

Alternative Standpoints and Conscious Engagement with Public Policy Issues

The main point of our commentary is that entanglement with public policy issues is inevitable in our field but this entanglement is often unrecognized. Bill Ouchi's recent work devoted to remedying problems in public education in the United States is one good example of how management research can consciously and explicitly speak to pressing issues of public concern. However, all management research inevitably engages with politics and, therefore, has implications for public policy.

We realize that this point of view will seem misguided to some readers. Many management scholars believe that all forms of partisanship should be purged from scientific research and theory development. They contend that politics should not enter into processes of knowledge creation, and many hold that it is inappropriate for scholars to engage actively in the application of knowledge. They believe that value neutral objectivity is the hallmark of proper scientific work and that advocacy would undermine that objectivity.

We urge our colleagues to consider another view, one that is more skeptical of the goal of value-neutrality and one that advocates *reflexive inquiry* about the premises underwriting our work. In a world with so much unnecessary suffering and destruction, Agger's (1991: 111) statement on value-neutrality rings loudly: "the seeming avoidance of values is the strongest value commitment of all." Various writers in the philosophy of

science have addressed the question of values; one of the best-developed approaches is Standpoint Theory (ST) (see Anderson 2004 for an overview and comparison with other epistemologies). ST challenges aspirations of value-neutrality, arguing that these aspirations require scientists to do the “God trick” or adopt a “view from nowhere” (see selections in Harding, 2004a). That is, they require scholars to speak authoritatively and without bias about and for the world, and do so as if they were speaking from no particular human position or social location. Standpoint Theorists contend that this is impossible. They argue that objectivity and understanding are better served if we aware of, and make explicit, our epistemological and political baggage rather than deny we carry any (cf. Kinchloe & McLaren, 1994). Because there are no facts without theories, and because all theories are based on a standpoint that is shaped (at least in part) by political considerations, scholars should reflect on their underlying epistemological assumptions and develop an awareness of their standpoints. Moreover, they should consciously choose their standpoints and take responsibility for the impact (or lack of impact) of their scholarship on the world.

Obviously, ST is controversial in that it raises fundamental questions for all scientists about the appropriateness of conventional research philosophy. Yet, if our research is going to help alleviate -- rather than ignore or exacerbate -- the unnecessary suffering and destruction around us, this concern should guide the entire research process. ST argues that all phases of a research study -- from identifying research issues, to theorizing research questions, to how we gather and analyze data and draw conclusions, to how the knowledge produced is used -- are conditioned to some extent by the researcher’s standpoint (Jermier, 1998). According to this approach, deeper and more objective knowledge results not from attempting to eliminate politics from science but from embracing politics and reflexively (consciously) adopting a standpoint.

But what standpoint should we adopt? To adopt dominant elite standpoints inevitably encourages perspectives that legitimize and naturalize the status quo, creating unacceptable limits to what can be learned and what change is possible. Although all standpoints are limiting and all knowledge is partial, ST argues that the view “from below” has greater potential to generate more complete and more objective knowledge claims. As Harding (1991, p. 150) put it: research should begin with the concrete circumstances and lived experiences of the “systematically oppressed, exploited and dominated, those who have fewer interests in ignorance about how the social order actually works.” That is, if our desire is to heal the world, we will learn more about how the root mechanisms of the world work and about how things can be changed by adopting the standpoints of those people and other parts of nature that most deeply suffer its wounds¹.

Standpoint Theory provides recommendations for where to begin inquiry and what to study, leading to greater pluralism in the mix of research and greater potential for progressive public policy implications. It also provides recommendations for how to study. The primary advice that is given is to “study up,” with the intent of mapping the ways “dominant institutions and their conceptual frameworks create and maintain oppressive social relations” (Harding, 2004b, p. 31; also see Wedel, Shore Feldman & Lathrop, 2005). Empowerment of the least advantaged is part of the main purpose of the standpoint approaches, which contend that it is important to create knowledge that raises consciousness about exploitation and helps movement toward emancipation.

Some readers might think our mandate as scholars of management requires us to choose the standpoint of managers. Some might argue that researchers who place such emphasis on social and environmental issues are simply in the wrong field. Some might assert that our primary audience (outside of academics) is managers themselves, and that managers are obligated by their fiduciary responsibilities to consider social and environmental issues if and only if they promote short-run profit maximization. We are all familiar with arguments that society is best served when firms maximize profits and leave welfare concerns to philanthropists, government and civil society. This line of argument is difficult to sustain, though, particularly in the face of evidence that business performance is enhanced when managers are accountable to a broader range of stakeholders (e.g., Margolis & Walsh, 2003; Orlitzky, Schmidt & Rynes, 2003). Managers lives may not be made easier by a greater awareness of issues as they are experienced and interpreted by the least advantaged; but as managers, in most cases as employees themselves, and as human beings, they owe the exploited their solidarity -- even if they are simultaneously tugged in other directions.

As management scholars, therefore, we see no reason why much more of our research might not take as its primary referent exploited groups and the broader abused natural environment. Clearly, fields that rely only or primarily on elite standpoints have blind spots that fields with a more pluralistic epistemology are able to avoid. There is, therefore, good reason to encourage more management scholarship that takes alternative standpoints, such as those of lower-level employees, women in poverty, racial, ethnic and sexual minorities, disadvantaged communities, and the natural environment. We believe that there is much to be learned if we begin the research process by formulating questions from these alternative standpoints and then examine the reality of management and organizations from these perspectives. This seems entirely consistent with the letter and spirit of the AOM Code of Ethical Conduct referenced above.

If this epistemological pluralism were taken seriouslyⁱⁱ, we would expect to witness the development of a field with more soul. We would expect to find more management research motivated by, for example, the desire to understand the challenges facing union organizers, or low-wage women workers, or undocumented immigrant laborers, or local community activists fighting a polluting factory -- topics central to discussions of contemporary social and environmental policy. Thus, even if our specific expertise may not often equip us to work *directly* on public policy problems, our research always has public policy *implications* and it can provide actionable knowledge for the exploited and their advocates. This should promote more just and democratic public policy debates.

A more complicated issue is how private and public policy makers might use the knowledge produced from alternative standpoints that has an emancipatory intent behind it. Some advocates of the exploited might fear appropriation or cooptation; we see a greater probability that this type of knowledge would persuade some elites (even owners and corporate managers who are far from monolithic in their values and interests) to take enlightened steps in the direction of humane and ecocentric policymaking.

By way of illustration, consider a program of research by social scientist Al Gedicks. This research begins from an explicit standpoint, is driven by an emancipatory intent, and has both scientific and activist objectives. In a recent article, Gedicks (2004) describes his involvement as a researcher, consultant, and advocate for the native Sakogon Ojibwe people of Wisconsin in their successful struggle against Exxon and BHP Billiton. This struggle involved an unusual alliance of Native American, environmental, and sportfishing interests. Gedicks' account highlights a different way in which scientists can contribute to public policy: not as servants of elite power, but as a resource helping to empower other actors by amplifying their voices in policy debates, and building supportive action networks.

As we see it, our research community faces some challenges when it comes to steering the discipline in the direction of greater positive impact on a broader range of public policy issues. First, the prevailing philosophy of science tends to emphasize disengagement with the real world of politics in favor of attempts at value-neutrality and non-partisan objectivity. Although this philosophy of science commands respect and seems attainable, it is inevitably misleading because all research has a political dimension. All research moves in the direction of either reinforcing or undermining existing relations of power, even if the researchers are not aware of these possible impacts. Second, where there is intentional engagement with the real world of politics, it appears more often to be on behalf of managers as agents of owners and other elites. The standpoints that are adopted (explicitly or implicitly) make it more likely that the knowledge produced will be useful to the relatively privileged rather than helpful in generating policy to protect the less privileged and the natural environment. We think the AOM's Code of Ethical Conduct, which encourages a multiplicity of perspectives, can serve as a basis for more reflexive inquiry and more progressive contributions to public policy.

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COMMENTS OF JAY BARNEY

Should Strategic Management Research Engage Public Policy Debates?

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Comments from Sharon Alvarez, Jay Anand, Jay Dial, Konstantina Kiouisis, David Greenberger, Michael Leiblein, Alison Mackey, Ty Mackey, Joe Mahoney, Steve Mangum, and Jim Walsh have been helpful in the development of this essay.

Should Strategic Management Research Engage Public Policy Debates?

Bill Ouchi's challenge to management scholars to begin to engage in debates about the broader social policy issues of our day represents a significant opportunity for strategic management scholars. Using the recent accounting scandals as an example, this paper suggests that not only can such involvement raise the quality of these debates, it can also benefit the field of strategic management.

Once again, Bill Ouchi's research has challenged the academic status quo. If we accept his logic, then no longer is it "good enough" for management scholars to "talk among themselves" about their research findings. Nor is it even "good enough" for management scholars to share their research findings with the business community for application. To realize the full potential of management scholarship, he argues, we must begin to bring our powerful theories and methods to bear on the broader social questions of the day, in short, to become part of today's critical social policy debates.

As a strategic management scholar, I willingly accept Professor Ouchi's challenge. Indeed, beyond any moral obligation that we might have to benefit the broader society within which we live, I believe that bringing strategic management theory to bear on social policy debates will benefit both the quality of many of these debates and the quality of the field of strategic management. The quality of many of these debates will improve because strategic management theory often generates social policy insights that are different from those derived from more traditional policy-oriented disciplines—including economics. Engaging in these debates, in turn, will benefit the field of strategic management by enabling its theories to become more widely known and examined, by gaining access to new and rich data, and by securing resources that have historically been closed to it.

This paper provides just one example of the multiple benefits that could be generated if insights from strategic management theory were used in social policy analysis: recent debates concerning corporate

accounting scandals in the U.S. and abroad.¹ As suggested earlier, not only does strategic management theory have something important to say about social policy responses to these scandals, research in this area could also benefit the development of the field of strategic management.

Strategic Management Theory and Recent Corporate Accounting Scandals

In the last few years, the U.S.—and world—economy has been rocked by several corporate accounting scandals. Senior managers in firms like Adelphia, Enron, Global Crossing, Parmalat, Tyco, and WorldCom have all been accused, or convicted, of engaging in various forms of accounting fraud. An important, and on-going, debate has emerged about what caused this fraudulent behavior and what should be done to make sure it does not happen in the future. Strategic management theory has implications for understanding both the cause of this behavior and how it can be avoided in the future.

The Primary Cause of Accounting Fraud

The general consensus seems to be that the emergence of these accounting scandals can be largely attributed to a lack of ethical behavior on the part of senior managers. The logic seems to be: “If senior managers would simply have behaved in an ethical manner, then they would not have engaged in the ‘shady’ accounting and other practices that ultimately led to widespread fraud and deceit.” Not surprisingly, this conclusion has led to widespread calls for additional ethical training in business schools and a refocus on ethical standards in business. Indeed, government regulators have weighed in on this question by requiring, in Section 404 of the Sarbanes-Oxley Act of 2002, publicly traded firms in the U.S. to establish and report a code of ethics for their senior financial officers.

While not denying the importance of managerial ethics, in general, strategic management scholars would probably suggest that ethical lapses are more likely to play an intermediate role in the chain of events that ultimately led to fraudulent behavior rather than being the ultimate cause of this behavior. There is little doubt that the ethical predispositions of managers often vary, both within firms and across firms. This was true before the accounting scandals, during the scandals and is probably true today after the scandals (Muth and Donaldson, 1998; Williamson, 1985). However, the distribution of ethical predispositions of managers, as a group, has probably remained relatively constant over time. Since this distribution has remained relatively constant, it cannot be used to explain, by itself, why there was a sudden surge in unethical behavior of managers in the late 1990s.

However, several things did change in the economy during this time period. For example, this time of fraud and unethical behavior took place at the end of a period of unprecedented economic growth and prosperity. As the economy grew, expectations about the future performance of some firms also grew—perhaps to the point that it was going to be difficult for these firms to meet market expectations even if the economy continued to grow at a rapid rate.

When it became apparent to managers that their firms were no longer able to meet these growth expectations, some managers began exploring ways to exaggerate their reported performance so that it more nearly matched market expectations. This was especially true for managers who owned significant stock options and thus managers who had very strong incentives to keep their firm’s stock price high. This, almost inevitably, led to accounting irregularities.

This analysis suggests that the inability of these firms’ strategies to generate returns equal to the market’s expectations, linked with managerial incentives to keep the stock price as high as possible, led managers to begin to explore unethical business practices. It was not that managers, on average, suddenly got “less ethical” in the late 1990s; it was that their strategies could no longer deliver on what turned out to be unrealistic market expectations. Strategic failure led to ethical failure; ethical failure then led to accounting fraud. While additional statements of ethics and ethical training might have reduced the level of accounting fraud somewhat, strategic management theory suggests that better strategies—including strategies about how to bring market expectations in line with what a firm could actually deliver—and different incentives would probably have done more to avoid the accounting scandals.

Avoiding Accounting Fraud in the Future

¹ Other obvious social policy debates where strategic management research may be important include anti-trust policy (Ellig, 2001) and policies concerning the relationship between employees and firms. The implications of strategic management theory for these two public policy debates are discussed in more detail in Barney (2005).

If the main cause of these accounting scandals has generally been attributed to a lack of ethical behavior on the part of managers, then the primary proposal about how these problems can be avoided in the future focuses on more rigorous accounting standards and reporting. This “solution” to the accounting scandals was operationalized in the Sarbanes-Oxley Act of 2002—an act that, among other things, requires firms to invest in and report on internal financial controls and requires a firm’s senior managers to personally attest to the veracity of their financial accounting statements.

While acknowledging that some additional reporting requirements might benefit a firm’s outside investors, many strategic management scholars would argue that the assumptions underlying these proposed accounting solutions to the scandals of the last few years are, at best, incomplete. Sarbanes-Oxley is based on the typical finance and accounting assumption that fully informed outside investors will always be better off than outside investors that are not fully informed (Bettis, 1983). In contrast, strategic management theory suggests that rarely will it be possible for outside investors to be fully informed about a firm’s resources and strategies, and that if they could be so informed, it would often not be in their self-interest.

To be fully informed, a firm’s outside investors would have to understand the relationship between the resources and capabilities controlled by a firm, the strategies that are chosen and implemented with these resources and capabilities, and the impact of these strategies on a firm’s performance. While some of a firm’s resources and capabilities are easy to describe and measure—including, for example, a firm’s tangible and financial assets—many of its resources and capabilities are intangible and difficult to measure (Dierickx and Cool, 1989). Managers themselves may not fully understand the relationship between the resources and capabilities they control and the strategies they pursue—a condition described as causal ambiguity in the strategic management literature (Lipman and Rumelt, 1982).

Moreover, it is just these kinds of resources and capabilities—the intangible, difficult to measure, and causally ambiguous resources in a firm—that are most likely to be sources of sustained competitive advantage (Barney, 1991). In the face of these kinds of resources and capabilities, it seems unlikely that outside investors would ever be in a position to fully understand the portfolio of resources and capabilities controlled by a firm, how those resources influenced a firm’s strategies, and ultimately, its performance—no matter how rigorous the required accounting standards.

Moreover, suppose a firm’s resources and capabilities were fully transparent to its outside investors. Would this be consistent with equity holders’ interests? Strategic management theory suggests that in informing its outside investors about its resources and strategies, a firm is also informing its competitors about its strategies and strategic intent. This can sometimes reduce the level of performance associated with a firm implementing its strategies (Barney, 1991). This reduced level of performance may have the effect of reducing the residual profits available to be distributed among a firm’s outside equity investors. Thus, paradoxically, fully informed outside equity investors may not realize the economic gains that less than fully informed outside investors might realize.

Strategic management theory thus suggests that a tradeoff may sometimes emerge between the advantages gained by equity holders from being fully informed about a firm and its strategies and the profits equity holders forego when a firm’s competitors become fully aware of its strategies. It is not clear if Sarbanes-Oxley reporting requirements generate sufficient information about a firm’s resources, capabilities, and strategies to begin to put a firm’s competitive advantage at risk. However, strategic management theory does suggest that both the benefits, and the costs, to a firm’s outside investors of more rigorous accounting standards need to be considered in developing and implementing these standards.

Recent Corporate Accounting Scandals and the Field of Strategic Management

Thus, strategic management theory has much to say about the causes of and solutions for the recent accounting scandals. However, each of the arguments that has been presented about the relationship between strategic management theory and recent corporate accounting scandals can also be thought of as hypotheses derived from strategic management theory that could be tested in the context of the accounting scandals. In this sense, these accounting scandals, and government responses to these scandals, provide a natural experiment within which strategic management theories can be tested and improved. Thus, not only can strategic management theory improve these kinds of policy debates, these policy debates can create a context within which strategic management theory can be extended and improved.

For example, strategic management theory suggests that statements of ethical standards and increased ethical training are less likely to have an impact on fraudulent behavior in firms than the adequacy of a firm’s strategies and the incentives of its senior managers to keep a firm’s stock price high, no matter what. The implementation of Sarbanes-Oxley provides an excellent opportunity to empirically test this assertion.

Strategic management theory also suggests that it is unlikely that Sarbanes-Oxley reporting requirements will end up fully informing outside investors about a firm’s resources and strategies. This suggests that, since competitors are not likely to become fully informed about their competitor’s strategies because of

Sarbanes-Oxley, the implementation of this law will not change the competitive positions of firms in a particular industry. This is also a testable hypothesis.

This theory also suggests that while, on average, Sarbanes-Oxley is not likely to put a firm's competitive advantages at risk, it may put the competitive advantages of at least some firms at risk—for example, firms with relatively easy to measure and imitate resources and capabilities. To retain their advantages, these firms may take actions to reduce the impact of Sarbanes-Oxley by, for example, increasing efforts to raise the cost of imitating their resources (e.g., through the more extensive use of patents and copyrights) or, in the extreme, taking the firm private. These, again, are testable hypotheses.

These are just three of numerous propositions that can be derived from strategic management theory and examined in the context of government responses to the recent accounting scandals. And these scandals are just one of several social policy debates where strategic management thinking might be relevant (Barney, 2005). Each one of these debates represents an opportunity to examine specific predictions of strategic management theory and thus an opportunity to improve the field of strategic management as a whole.

Conclusion

The general point is simple: By bringing strategic management theory to bear on some of the critical social issues of our time, not only will the level of discussion in these debates often be elevated, but this can also provide opportunities to test and extend received strategic management theory. It is thus in the self-interest of both social policy experts and strategic management scholars to discover ways to engage in this conversation (Ellig, 2001).

Of course, significant institutional barriers exist to these conversations—strategic management scholars are generally not well-trained in social policy issues; social policy experts are often unaware of strategic management theory and its progress over the last two decades. However, that barriers to conversation exist does not mean that conversation cannot exist. To the extent that strategic management scholars recognize both the practical and theoretical advantages from bringing their theories to bear on social policy debates, such conversations—like this special section of AMJ—are likely to emerge.

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COMMENTS OF THOMAS D'AUNNO

Management Scholars and Public Policy: A Bridge Too Far? Thomas D'Aunno, Ph.D.

Should management scholars and the Academy of Management become more involved in public policy and public management issues? For someone who has spent much of his career doing research and teaching in the public sector, my answer to this question should be a resounding "yes." And, to a

great extent it is. I begin by providing an example from my work that supports this view. Nonetheless, I have several major concerns and I also discuss these below, using as a “mini-case study” a current project of mine.

The good news. Organizational and management research, teaching and consultation can make a remarkable difference in public policy in several ways. First, our research can examine the effects that policies have “at ground level.” Such studies can show, for example, the unintended consequences of policy choices. Similarly, research can show how well organizations and managers are implementing public policies. Importantly, by examining the provision of public services (such as education or health care) at the organizational level (individual schools or hospitals), management scholars can be well-positioned to analyze the causes for policy successes or failures (e.g., to what extent is a policy itself poorly designed or has it been poorly implemented?).

Further, we can work as consultants, drawing conclusions from prior conceptual and empirical work to inform the formulation of new policies. Finally, we can, and should, take our learning into classrooms (or, more recently, on-line) to train policy-makers and managers in the public sector.

In January of 1992, I published a paper in the *Journal of the American Medical Association (JAMA)* (D’Aunno & Vaughn, 1992) that examined the extent to which the US drug abuse treatment system was providing adequate care for tens of thousands of heroin users. I wanted to understand how the organization and management of treatment clinics affected their performance. Our data showed that the great majority of patients in dozens of clinics were receiving doses of medication (methadone) that were too low to be effective, probably prompting their relapse to heroin-use (at that time, individuals commonly injected heroin into themselves with shared-needles that carried HIV-infected blood).

JAMA published an editorial to accompany our paper and the Associated Press released a story on our work that appeared in many newspapers across the US. We had caught policy-makers’ attention, and the influential Institute of Medicine (IOM) convened a panel to examine the situation. In 1994, IOM published a study titled “Federal Regulation of Methadone” that called for policy changes to improve treatment practices. Another federal agency, the Center for Substance Abuse Treatment, issued a new set of treatment guidelines and distributed them to hundreds of treatment clinics across the US.

More recently, federal laws have changed again, attempting to make more major improvements in treatment services for heroin users. In subsequent papers, colleagues and I continued to monitor treatment practices, and our data show marked improvements since 1992 (D’Aunno, Foltz-Murphy & Lin, 1995; D’Aunno & Pollack, 2002; Pollack & D’Aunno, 2005). In short, it appears that our 1992 paper sparked important policy changes and improvements in treatment for many thousands of heroin users.

Unfortunately, of course, our efforts to inform public policy and management rarely proceed as smoothly as the above example would suggest. Indeed, the battle to improve policy, management and treatment in the substance abuse field is far from over. Another of my current projects illustrates several important-- and typical -- problems that management scholars face in trying to influence public policy and management.

A mini-case in public policy and management. In May, I accepted an invitation that, for me, was rather exotic: I became a member of the governing board of Qatar’s new National Health Authority (NHA). Qatar is a fascinating Persian Gulf desert kingdom (about the size of Connecticut) that has vast reserves of oil and natural gas, and a population of about 850,000, most of whom are guest workers for the booming energy and construction industries. The rulers of Qatar have charged us with the daunting task of “creating a world-class health care system.”

After attending my first board meeting, I had dinner with three members of a US-based, international consultancy that has a speciality practice in health care. These consultants had been working with the new NHA board and key leaders in Qatar for the past year. During the course of a wide-ranging dinner conversation, I learned more about the consultants, their background and prior experience, their views about the challenges that Qatar and the NHA face, and, interestingly, about their other work in the region, including a project that aims to build a health care system for Palestine.

Who’s at the table? It should come as no surprise that neither the Board members nor any of the consultants has any significant training or education in management. Rather, the consultants are economists and the other board members are physicians or business leaders (without MBAs). Fortunately, in this case, my perspective as a specialist in management has been quite welcomed, but my prior 20 years of experience tell me that this is certainly far from the norm. In the health care field, for example, economists and physicians are two powerful, well-established groups who have little incentive to make room for management perspectives.

Thus, if we are to make additional contributions to public management and policy, we need to wrestle with the fact that we face severe competition in markets for influence. Indeed, the market problems exist right at home in our universities. The youngest consultant at dinner had recently graduated from a top masters-degree program in health management and policy; she specialized in international health policy and financing for health care systems. In many ways, this background made her an ideal fit for the work in Qatar. But, she had

graduated without taking a single course in management. How well will she and her colleagues be able to understand the management challenges involved in implementing policy and transforming a health care system?

Perhaps hers is an unusual case, but I argue that unless we better integrate public policy and management training in professional education, we will continue to experience wide gaps between these worlds. Teaching management in public policy schools and programs is not for the faint-of-heart; among other challenges, the pay for teaching management in non-business schools is dreadfully low. It's also true that it is quite difficult to teach public policy and public sector management to MBA students. How willing are we as individuals or as an Academy to deal with the institutional, political and market forces that often keep management scholars out of decision-making?

Conflicting stakeholder interests. When I do have access to policy-makers, I invariably face questions about whose interests I am serving. Organizations and social systems involve multiple stakeholders whose interests are, at best, inconsistent, and, often, conflicting. This is certainly the case in Qatar, even though it is a small country, ruled by a central authority, the Emir. For example, based on the evidence available about how best to pay for health care, the NHA Board has decided to create a health system that is primarily publicly-funded and that offers universal health insurance. The Board faces a challenge, however, from powerful actors in Qatar who own private insurance companies: they are lobbying for a large role for private insurance in the new system.

Servants of power? In situations with multiple, conflicting demands, I am tempted to serve the interests of the most powerful stakeholders; this is, of course, not always the "wrong" choice, as the case in Qatar illustrates. But, fighting this temptation is often necessary because powerful decision-makers often want to pursue courses of action that contradict the best available evidence and theory (similarly, powerful actors often would like us to design our studies in ways that will not produce information that can harm their interests). At the same time, I find that "speaking the truth to power" demands the skilful use of influence strategies and tactics. In short, even after struggling for a seat at the table, all-too-often our voices serve mainly to support the views of those in power.

Ideology, politics, and less-than-rational decision-making. Much empirical evidence and theory suggest that organizations and their leaders, especially in the public sector, often make less-than-rational decisions. Further, leaders often have limited ability to make radical changes in systems and organizations. To what extent do our efforts to apply or develop management science in this context fly in the face of what seems evident: only a (small?) fraction of the time can social science guide decision-making?

At our dinner meeting, the most senior of my consultant colleagues in Qatar described his efforts to build a health care system in Palestine. In utter frustration, he declared that the project has stalled for lack of funds and political interest. In his support, I ask: how many projects are more important than building a healthy Palestinian state? We are trained to be dispassionate scholars who can analyze why such decisions are made. Changing them is another matter.

At the same time, I would be naïve to think I am an ideal-type, dispassionate scholar who is less ideological and political than the decision-makers he hopes to influence. Can we ever be value-neutral or value-free? Perhaps the best way to think about this problem is that, though we cannot help but bring our values to research, teaching and consultation, we should be clear about what these values are; how they might influence our work; and how they might alienate us from particular actors whose views differ radically from ours. I certainly need to develop a deeper understanding of how my values affect my work in the field. At a minimum, we should understand our biases. Over the long-run, both as individuals and as an Academy, what role will we play in society if we fail to remain rooted in systematic, rigorous empirical and theoretical work?

Concluding comments. I have agreed to a 4-year term on the NHA board and I'm confident (perhaps naively) that we can make important improvements in Qatar's health care system; I will certainly learn a lot in the process. Further, with support from the Qatari government, I will be establishing a research center there to study health care management issues and inform policy choices. But, it's clear that this is a long-term project that does not lend itself to the quick publications that tenure clocks demand. In fact, if I were a junior faculty member I would not have accepted this invitation. More generally, traditional reward systems in our schools and departments do not recognize the kind of work I am doing in Qatar. Perhaps we need to re-examine such systems if we are serious about affecting public policy and management.

Beyond problems with reward systems and career issues, if we intend to make a difference in public policy and management, I think we also need to re-examine how we conduct research. Pressing issues in public policy will need to drive our research agendas more than they currently do. Similarly, it is likely that we will need to use a wider range of research methods and we'll need to pursue research in a more collaborative fashion so that we build relationships with the decision-makers whom we intend to inform and influence. Attacking large-scale problems also means that we will often need to work more in large, multi-disciplinary teams. At a minimum, we will need to communicate much more clearly and easily about the policy and management implications of our work.

At the same time, I think it's very important to recognize that many of our colleagues in the Academy have been effectively engaged for many years in public policy and management research, teaching and consultation. Many of these individuals are members of Divisions such as Public and Non-Profit Management; Health Care Management; Management Consulting; Social Issues in Management; and Organizations and the Natural Environment. Moreover, these individuals have found ways to overcome the barriers discussed above, and we should learn from them.

In the end, it's not clear to me that more organized efforts on the part of the Academy to stimulate additional work in the public sector will pay off. Much of the motivation to do such work probably needs to come from individuals who find it, as I do, intrinsically interesting for both professional (intellectual) and personal reasons.

Finally, given the above considerations, I am sometimes pessimistic about the extent to which management scholars can affect public policy. Nonetheless, I also think we are wrong to cede the field entirely to others; too much is at stake. The relatively successful work I have done with research on the organization and management of methadone treatment services supports this view. At the end of my career, I would like to think that I spent time and effort trying to influence important public policy and management decisions rather than speculating about either the worth of such efforts or the barriers to their success.

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COMMENTS OF VANESSA DRUSKAT

Scholarship That Works

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This essay has benefited from conversations with my colleagues Ross Gittel, Richard Hackman, Peter Lane, and Tuck Pescosolido, and from the editorial comments of Carole Barnett.

Scholarship That Works

In this essay, I intend to convey more optimism than pessimism. Optimism should be evident in my message that management scholars can and should have greater impact on decision makers (in government and in business). Pessimism will surely come across in my argument that our current system (reward, socialization, structure) inhibits that optimistic outcome. I'll end with an optimistic proposal about how we can benefit from engaging in research that matters more.

We Can and Should Increase the Visibility and Impact of our Scholarship

I see a number of challenges related to whether organizational scientists should work to have more influence over decision-makers in government and other public sector institutions.

We *should* be motivated to seek greater influence over public policy and organizational decision makers. The environmental pressures pushing us toward change have already been outlined in this issue. Several of our most respected colleagues have also recently pleaded with us to recognize the limits of our “knowledge for the sake of knowledge” approach to research and writing (see Bazerman, 2005; Ghoshal, 2005; Mintzberg, 2004; Pfeffer, 2005). The most unsettling comment I’ve heard came from Daniel Hennigner, a deputy editor at *The Wall Street Journal*, whom I heard say the following on a recent Sunday-morning talk-show:

“Back when the great society was building, they were present at the creation. Academics, especially in the social sciences, laid the groundwork; they testified before Congress, they contributed to the process. Now, you know, they’ve largely become irrelevant. The locus of power has shifted to specialists in think tanks, many of whom are refugees from [academia], or [to] the media and even radio talk shows...” (Gigot, 2005).

Contrast that with Sabrina Zirkel and Nancy Cantor’s (2004) review of the central role played by Isodor Chein, Kenneth Clark, and Stuart Cook in creating the social science briefs for *Brown v. Board of Education, 1954* -- legislation that ended segregation in public schools in this country.

I’ve met some of Hennigner’s “experts” whom decision-makers approach for ideas about human behavior (economists, lawyers, and consultants). Their opinions can’t compare to our knowledge about human behavior, leadership, and decision-making gained through sound theory and systematic research.

It seems like we have always had some of our most prominent social scientists arguing that our research should serve more practical purposes and that research and practice in our field should go hand-in-hand. In the 1940’s, Kurt Lewin, who some consider the grandfather of organizational psychology, argued that a better synthesis of the two would benefit both science and practice. This insight was the impetus of his famous quote – “There is nothing so practical as a good theory” (Cartwright, 1978). Despite these persistent calls for synthesis, I have always found practitioners more eager to interact with researchers than vice versa. This becomes clear if you attend one of our conference sessions (e.g., AOM or SIOP) on integrating practice and research. While researchers sit quite prominently on the panels, practitioners, who crave learning about practical research findings, fill the seats in the audience. Why the imbalance and what should we do about it?

Both individual and structural issues can get in the way of undertaking a change within our system. Our research interests may not fit what government and organizational decision-makers want to know. Some of us also cringe at that thought of becoming prescriptive in our discussions and of taking on the sales-like demeanor necessary for interacting effectively with government and organizational decision-makers (see Bazerman, 2005). Yet, as always, the largest impediments to change are structural.

Structural Hurdles to Climb: Going Back Before We Move Forward

There are a number of hurdles to overcome if we want our discipline to become more involved in helping to solve practical problems. To begin with, our academic institutions don’t reward applied research (defined here as research for the purpose of advancing practice) to the extent that they reward basic research (defined here as research for the purpose of advancing theory). This becomes clear when we look at who gets hired by the universities with the most resources. Top-tier research universities provide reduced teaching loads, good doctoral students, money for research, and access to research centers that can enable entry to organizations for research purposes. These universities favor hiring individuals who’ve published in prestigious basic research journals over those who’ve published in prestigious applied research journals. This is cannot be denied – look at their websites. In fact, the more resources held by a university, the more they seem to want basic researchers; their job advertisements frequently seek candidates with degrees in the basic social sciences. To be fair, many of these universities are concerned with business school rankings like those in *Business Week* because they impact resources. Ironically, these rankings are boosted by basic research publications. This has led some universities to stipulate that that junior faculty must publish in basic research journals. It also means that many of our best “applied researchers” get caught in a cycle of being hired by institutions with fewer resources, resulting in less research and fewer publications.

Another related hurdle concerns status within our profession. Although there are exceptions, basic researchers earn our highest respect. They receive our most “scientific” awards, pack the rooms at professional meetings (e.g., AOM, SIOP), and receive deference from the harshest critics among us. It is clear that their motives are to advance science and there seems no more respected quality among social scientists. On the other hand, I’ve been to the conference presentations of applied researchers where audience members have questioned the motives of the researcher, attacked, rather than questioned, research methods, and hurled insults. I’ve overheard colleagues talking about someone who “used to be a serious scholar” before he began consulting to organizations, and I’ve heard about others who fear losing the respect of colleagues for the same reason (see Lawler, 1985). Take, for example, Kurt Lewin who in his later years developed the idea of “action research,” a form of applied research that involves partnerships between researchers and practitioners. It isn’t too surprising to hear that his research standards were sometimes considered loose and that he has been called more of an *idealist* than a *scientist* (see Seashore, 1985; White, 1992).

Key Reasons for Change: What We Can Learn Through Action Research

There are reasons to change the way we do research that have nothing to do with environmental pressure. About three years ago my colleague, Steve Wolff, and I got involved in an amazing learning opportunity. We convinced six internal team and leadership development consultants in a Fortune 50 company to let us help them conduct research with the aim of gaining knowledge about how to improve the performance of their cross-functional teams. Although this was not public sector research, I believe our experiences are useful for assessing *how* practical research can be done well. They illustrate the benefits of involvement in action research, which is formally defined as research that brings together “action and reflection, theory and practice in participation with others, in pursuit of practical solutions to issues of pressing concern to people” (Reason & Bradbury, 2001: 1). Also, our positive experience can provide a model of micro-level action research that is quite different than Bill Ouchi’s more macro research on school systems.

Action research is different from the typical field research opportunity involving data collection and feedback to the organization. Our study was an intense two and a half year process conducted as a partnership between researchers and practitioners. This partnership allowed us to develop research questions, analyze data, and generally walk the halls of the organization with a group of insiders who fully understood the human issues in this organization. Their jobs *required* that they improve team and leader performance, and there was little that they hadn’t tried to make that happen.

We began our partnership by developing an on-line survey. It took six months for our team of eight talk to talk through our literature review and determine the concepts we’d test (our theory) through the survey. Listening to the six of them banter and debate about the value of the concepts that we team researchers hold dear was spellbinding. Equally exciting was the process of creating survey items. They were meticulous about words. Steve and I had been developing and using surveys in research for ten years prior to this experience, but had never learned so much about the politics of words and the attention spans of managers.

Another method we used was a tried and true one I learned as one of David McClelland’s graduate students; it has never disappointed me in field research: The comparative multiple case study method (see Druskat & Wheeler, 2003 for an example; McClelland, 1976). It proved to be the most informative research tool we used and is similar to the comparative method used by Bill Ouchi, so I will elaborate a bit. We identified criterion samples of truly exceptional performing teams and of average performing teams. We then studied these teams to determine what differentiated the exceptional from the average. Blind to performance categories, we interviewed members and leaders on these teams using critical incident interview methods. As an aside, I learned long ago not to ask direct questions in interviews with organization members. The answers I get from direct questions say more about interviewee values or their theories of effectiveness than about what happens on the job. Critical incident interviews involve asking interviewees to describe specific experiences on the job in great levels of detail and produce detailed information about how a team operates.

To provide you with an example of the kind of information one can learn from such an interview, I’ll describe one of the teams we studied. This virtual team had members in California, Belgium, Italy, and two locations in New Jersey. The interview taught me about the real challenge of time zones, the real challenge of keeping team member attention so that mute buttons didn’t turn on and keyboards start clicking, and the real challenge of keeping side conversations among the Belgium members (who were all sitting together in one meeting room) from becoming distracting. I learned that Europeans laugh at different jokes than do Californians – which very well might be due to the ungodly hours that these team members need to get on these phone calls. There was also the challenge of members who talked too long and members who didn’t speak up enough because their English language wasn’t perfect. Finally, there was the challenge of the insensitive team leader who spoke too often and ignored suggestions. On top of it all, this team, which is competing for resources with teams that meet face-to-face in Missouri, has little chance of receiving the funds to meet face-to-face.

It is this kind of contextual information that can inform new ways of thinking about issues and can be the impetus to the development of exciting new theory. The statistics we obtained from our surveys were interesting – but they were all the more informative when combined with the rich data obtained through interviews. That learning became richer during the hours we spent discussing the data with our team of researchers and practitioners.

There is no doubt that such a partnership is time-consuming, even more so if the research site is not in your backyard. Though what you’ll gain is the ability to test your ideas in the real world, knowledge about the situational boundaries within which our theories exist, important new information about the tumultuous work context that cannot be learned another way, and enough data to sit back and write to your heart’s content.

I should also tell you about the positive results for the organization. By using the results of our research, the practitioners in our partnership have been able to improve the processes of their teams. The results have been praised by organizational members ranging from the CEO of the division, to the members of some of the roughest teams. I’ve learned that good data that fits the experience of team members can capture attention and motivate change.

Refocusing our Scholarship so it Works

I hope I've been optimistic enough to convince you that one of the greatest reasons to jump the socially constructed hurdles that can discourage applied research is the immense amount of knowledge and wisdom to be gained. Relevant research does not need to be far removed from our own research interests and learning goals. During my years of involvement in the partnership I've discussed, I've learned enough about team and organizational effectiveness to be a different person. I worry less about being prescriptive when I talk because I feel like I know what works (in some contexts) and my theoretical ideas are more refined and mature. I'm certain Bill Ouchi would say the same about what he has learned from his applied research. My next stop will be to study teams in school systems. I'd like to add micro-level information to Ouchi's findings. There is both basic and applied knowledge to be gained from such a study.

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COMMENTS OF MARTHA FELDMAN

Management and Public Management

by

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Hurricane Katrina once again reminds us of the importance of well-run government organizations. Comparing FEMA during hurricane Floyd (1999) and during hurricane Katrina or comparing FEMA and the Coast Guard during hurricane Katrina is enough to show that management of public organizations matters. There are three big problems with making sure that public organizations have good management.

- *First, we need more political pressure for good public managers.* If management skills are seen as optional or, worse, good management is seen as impossible, then we will systematically have people running public organizations that do not have fundamental management skills.
- *Second, we need more training for public managers.* There are extraordinary managers in the public sector, but, for the most part, they either are naturally talented or have developed their skills on their own. Until training for management in the public sector becomes as important as training for management in the private sector, private organizations will tend to be better run than public organizations.
- *Third, we need more research about how to manage public organizations.* Though managing in the public sector has much in common with managing in the private sector, public management has special challenges (and opportunities) that we need to understand in order to develop ways of dealing with them. Greater involvement of management scholars in issues of public management can help with all of these.

My entire career I've studied and worked in public organizations. Mostly I've worked in universities, but I spent a couple of years as a policy analyst in the U.S. Department of Energy. I've studied a range of public organizations that includes courts, federal agencies, city administrations and public university housing departments. I'll talk later about what I've learned from observing and talking with people who work in public organizations. First, however, I want to talk about what I learned by seeing public organizations through the eyes of a private sector CEO who became the Administrator of the General Services Administration under President Clinton.²

Roger Johnson was the CEO of Western Digital when its profits went from millions to billions. He had a lot of experience managing large organizations. He went to Washington armed with the skills to manage a large private sector organization and some common myths about what was wrong with Washington bureaucracies. Much to his credit, he quickly realized that the people who worked for him in Washington had at least as much expertise and dedication as the people who had worked for him in the past. He also found that most of the people he worked with, both managers that worked for him and managers, including top managers, in organizations he worked with, did not have basic management skills. They were often experts in a particular policy issue, or they were lawyers. These are important areas of expertise, but the lack of management skills made them less effective at using their expertise for the good of the country. Finally, he found that even with the considerable management skills he possessed, he needed more. It turns out that managing in the public sector, while having substantial overlap with management in the private sector, presents challenges that are integral to the political nature of the public sector. His observations are related in his book, *It Can be Fixed*, which documents his experience in Washington and proposes that management expertise become a new political priority.

Roger's message maps onto the three points I made at the beginning of this essay. Part of his message is that basic management skills are essential even or especially in a highly charged political environment. For that reason, making management skills a political priority and making it clear to elected officials that we all expect public managers to have management skills is important. To make sure this happens, we need to provide more opportunities for public managers to learn management skills. Some of this training can come from public policy schools, but there are not enough public policy schools or management scholars within public policy schools to provide training for the vast number of people who hold management positions in the public sector. Another part of Roger's message is that the political nature of public management makes the exercise of management skills different and more challenging. To respond to this part of the message, we need to do research that focuses on the specific challenges of public management and on translating what works in the private sector into the public sector.

Challenges of the public sector

Any challenge that I identify in public sector management can be found in some form in private sector management, but there just seems to be more of it in the public sector. There is more political influence from more directions. There are more stakeholders with a greater variety of interests and the stakeholders are more present. The boundaries between the organization and the external environment are more permeable. There are more rules and more constraints. Public management is at least as much about managing the external environment as about managing the internal organization. In the public sector, the best efforts and analysis can, and often do, come to naught because of influences that public managers have little or no direct control over. Managing people, providing services and accomplishing goals under these circumstances can be frustrating.

² Sadly, he passed away in February 2005.

Some people give up, feeling as if they cannot win. Others see politics and efforts to bring multiple perspectives together as part of the process.

What can management scholars do to help?

Proposals for new ways to organize for the provision of public services, like Ouchi's proposal for decentralizing decision-making in education, are important. Also important is the work that Ouchi has been doing to help people enact this proposal. This work may not be as clear from Ouchi's paper that emphasizes the importance of the idea of decentralization more than it emphasizes the considerable work that goes into effective implementation of the idea. Both the relative lack of management skills in the public sector and the challenging nature of practicing public management make it important to focus on effective implementation. Without this emphasis, many public sector managers will not know how to enact proposals, the proposals will be enacted poorly in many places and the reputation of the proposals will suffer as a result. Many more of us need to be both making proposals and doing research on how to enact them in order for such proposals to be effective.

In recent years, my research has focused on inclusion as a new way of organizing.³ Inclusive management involves empowering employees and empowering the public. It also involves working collaboratively across organizational and sectoral boundaries. Finally, it involves finding ways for politicians, technical experts and the public to work together to solve problems and implement solutions. Many public managers have come to the conclusion that they have to practice inclusive management. They see inclusive management as a way to create new resources in order to continue to provide services with shrinking budgets and also as a way to deal with the lack of public trust in government and government employees. Inclusive management often involves the decentralization that Ouchi proposes and, like it, is a way to use resources more efficiently.

I have tried to understand how inclusion can be effectively implemented by studying managers who appear to be good at it. This research shows that practicing effective inclusive management involves new ways of thinking and acting. The managers we have observed practicing inclusion have several characteristics in common. For one, they do not look for things to be fixed once and for all. They believe in continuous change. They believe in small wins. Two, they do not accept that the world consists of zero sum dichotomies such as you can have either participation or control; you can have either flexibility or accountability; you can have a technical solution or a political solution. They live in a world in which accepting such dichotomies spells failure and believing in these dichotomies is the route to paralysis. Finally, they know that the most under-rated structures in the public sector are relational, that there are many ways to build relationships with politicians, with external stakeholders and with people who work in your organization, and that these relationships will be important to preserving and enhancing their ability to act. These relationships are not about cronyism; they are not even particularly personal. They are about listening to and legitimating a variety of perspectives and trying to find ways that people of differing perspectives can work together.

Ironically, the topics most relevant to these understandings, topics such as continuous learning, small wins, competing values and social capital have been much more developed in the research on private sector than public sector management. Because we have focused on managers who are practicing inclusion effectively, we find that many of them have read the research on private sector managing and have translated it for themselves. Expanding the pool of people who can practice inclusive management effectively, however, requires the involvement of management scholars. Translating the insights from private sector research to the public sector context is an important first step. Such translation is not only useful for helping public managers but is also useful for management research. Expanding our exploration of concepts to include how they work out in public sector organizations will increase our understandings of the concepts and their strengths and limitations overall. Understanding how social capital, for instance, is the same and different in the public sector would expand our understanding of social capital and how it works. In other words, studying how to manage (and how to manage well) in the public sector, enriches our ability to understand how to manage (and manage well) in general.

New ways of managing in the public sector (e.g., inclusive management, Ouchi's decentralization) are easier said than done. There are always a few people who are really good at it naturally. I am convinced that there are many others who would like to achieve the same results, but don't quite know how to make it happen. These are the people we, as management scholars, need to help. The strategy I have been following is to try to observe people who are engaging in inclusive management with at least some success and to try to conceptualize what works (and what doesn't) in ways that can be imported and customized in other, often quite different, contexts. Other management scholars would have other strategies and, in this case, it is certainly true that the more strategies the better.

³ I have undertaken this research with the collaboration of Anne M. Khademian.

Conclusion

In concluding, I want to make one thing clear. My message here is not a political one. Political decisions will be made about how much government to have and what government should do. My message is about having effective public organizations and about the effective use of public resources. Management is critical to those ends. In order to have good management of the public organizations we decide to have, we need to make management a priority that our politicians feel responsible for executing, we need to provide training for public managers and we need to do research that develops and makes accessible ideas that will be helpful to public managers.

COMMENTS OF MIKE HITT

Management Theory and Research: Potential Contribution to Public Policy and Public Organizations

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Ouchi's path-breaking work provides a vision of translating management theory and research for public organizations and public policy. One might surmise from the lack of management theory application to public organizations and public policy that it is inappropriate to do so. However, such a conclusion is inaccurate. In fact, there are many areas of management theory and research that could be used to enrich public policies and increase their ability to serve constituencies more effectively (Hitt, 1998).

I have chosen a sample of areas of management theory and research that could contribute to the efficacy of public policies to provide examples. These areas are ones in which I have more knowledge and expertise, assuming that commentaries from my colleagues illuminate many other valuable contributions. This commentary focuses on unemployment, corporate governance, international trade and management of public organizations.

Policy on Unemployment

Much of the public information available on public policy regarding "unemployment" focuses on 'unemployment compensation'. While obviously important, there are many other important issues related to unemployment in the economy. Recently, I read a draft report on the economic competitiveness of U.S. business (Gordon, 2005). While the report had some valid observations, it focused largely on macroeconomic issues. Management research contributes knowledge that can provide a richer understanding of competitiveness. Furthermore, I was surprised by a statement that the unemployment rate was *controlled* by the government's monetary policy. Given that the report's intent was to analyze competitiveness on a global basis, ignoring the effect of globalization on the integration of economies throughout the world seems odd.

Perhaps a more important question is how to avoid high unemployment in regions, countries, states or cities. One obvious answer is the promotion of economic growth and development (certainly affected by monetary policy). Based on the data available, new venture firms are the primary net creator of much economic activity; in fact, it is not uncommon to observe that some of the largest and resource-rich firms are replaced over time with new venture firms offering better technologies, products and/or services to the market (Amit, Lucier, Hitt & Nixon, 2002). It follows that many of the new jobs created in the economy are derived from new ventures. These net new jobs increase employment in a region (Kirchhoff & Acs, 2000). Therefore, rather than policies and resource allocations designed for old mature businesses, policies that promote and support the development and success of new ventures may have greater payoffs.

The support of entrepreneurial activity is in keeping with the importance of innovation to competitiveness in international markets. The Council on Competitiveness sponsored the recent National Innovation Initiative (NII). In the NII report, it was argued that innovation supports new ideas, technologies and processes that lead to more jobs, higher wages and a stronger standard of living. The report also stresses that creating innovation requires especially strong human capital (National Innovation Initiative, 2004). The research in management on the importance of human capital provides strong support for the NII report's conclusions (e.g., Berman, Down & Hill, 2002; Hitt, Bierman, Shimizu & Kochhar, 2001, Huselid, 1995). The NII report also recommended that government policies that encourage enlightened risk taking and optimize the financing climate for entrepreneurial firms (National Innovation Initiative, 2004).

Corporate Governance

Much of the legislation and regulation related to corporate governance is based on agency theory and is derived from the research in economics and finance (e.g., Jensen, 1986). And, the research in management

provides support for some of the basic principles of governance derived from that research (e.g., need for a balance between independent outside directors and inside directors). Yet, research in management also suggests that corporate governance activities and therefore the policies and regulation related to them have not been highly effective over time (Dalton, Daily, Ellstrand & Johnson, 1998; Daily, Dalton & Cannella, 2003). Perhaps the ineffectiveness of the regulation and policies is because corporate governance is much more complex than the assumptions on which the policies and regulations are based. For example, the work of Westphal argues that board members have complex network ties that can affect the strategies that they develop and implement in their board service (e.g., Westphal, 1999; Westphal & Khanna, 2003). Management research also suggests that all owners do not necessarily have the same goals thereby creating potential conflicting expectations for top executives. This is especially true for institutional investors who own a significant amount of the equity in U.S. firms (Hoskisson, Hitt, Johnson & Grossman, 2002). This research should inform government policies and regulations related to board and top executive behaviors.

Likewise, the research in economics, finance and management originally supported the importance of equity ownership by managers and directors. However, a recent meta-analysis concluded that no systematic relationship exists between ownership structure and firm performance (Dalton, Daily, Certo & Roengpitya, 2003). Stock options became popular mechanisms for tying executive compensation to firm performance and to promote executives' equity ownership in the firm. However, anecdotal and academic research suggests that they have not been highly effective in meeting these goals. In fact, the research shows only a small relationship between executive compensation of any form and firm performance (Tosi, Werner, Katz & Gomez-Mejia, 2000). This research suggests the legal requirement for linking top executives' pay to firm performance has been ineffective.

Internationalization and Trade

The international trade policies and regulation are largely based on work in economics and political science. The research in these fields provides significant guidance for these public policies. However, research in international management also could add value to international trade policies. For example, research has shown that firms from developed countries learn valuable knowledge from their operations, partners and/or acquired firms in emerging market countries (Vermeulen & Barkema, 2001; Hitt, Dacin, Levitas, Arregle & Borza, 2000). In other words, they gain value beyond access to the local markets. If they can diffuse this knowledge to their other international operations, this new knowledge enhances the value of the firm's operations and products in other countries (Hitt, Hoskisson & Kim, 1997). Such knowledge has motivated firms to locate R&D centers in foreign countries. These R&D operations provide returns by developing products valued in the local foreign markets where they are based. However, they also produce knowledge and products that can be used in other international markets as well. Therefore, policies that encourage such learning and support its diffusion could enrich firms' competitiveness in global markets and perhaps enhance economic activity in the home country.

Another stream of management research that could be valuable for government policy focuses on the development and competitive behavior of emerging market firms. Such behaviors have been exemplified in recent times by Chinese firms' forays into the U.S. and other developed country markets. In fact, some of them have made or tried to make highly publicized acquisitions. These firms' strategic actions are based on the knowledge that they have developed over time, by working with western firms in their home markets. Many emerging market firms are becoming more resourceful (Hitt, Li & Worthington, 2005). It is unlikely that the U.S. government can and will bar Chinese firms from U.S. markets (unless there is a major political problem between the two countries). Policy makers need to understand the motives and resources of these firms based in countries with growing economic power. One analyst recently referred to China as Wal-Mart with an army.

Managing Public Organizations

Many management theories are applicable in public organizations, most notably those in organizational behavior. This is exemplified by the "five pillars" promoted in the KIPP Academies for managing special contract schools with state or school district charters. This private non-profit organization delegates substantial authority to the principals to manage their schools but within the boundaries of the five pillars: (1) more time on the task—students and teachers devote more time to learning, (2) well-trained and motivated principals are given the power to lead, (3) students and teachers have clearly defined and high expectations against which their performance is measured, (4) all students, parents and faculty members choose to participate in the school and make commitments to achieve (meet the expectations) and (5) emphasis placed on results, performance at the highest level. The intent is to prepare students to succeed in the nation's top colleges in the world. Students are almost totally from poor, non-white households and their performance is exceptional with almost 100 percent performing in high percentiles nationally in all academic areas by the time they graduate (Leschly, 2003).

In KIPP, the new principals are carefully selected, as are the teachers. Thus, KIPP places strong importance on strong human capital. Additionally, the leaders are carefully trained in a Leadership Institute staffed by faculty members from Berkeley's Haas Business School and Stanford's Business School. The Institute

emphasizes skills in organizational leadership, academic leadership, operations management and community development. The KIPP Academy schools use organizational behavior theory and research on leadership, goal setting, organizational culture and individual self esteem and commitment. Additionally, the emphasis on effective management in these schools shows the importance of managing resources, a recent stream of management research that extends our knowledge of the resource-based view in organizations (Helfat & Peteraf, 2003; Sirmon, Hitt & Ireland, 2006).

The KIPP Academies are the product of a private nonprofit organization. Yet, much of what they do parallels a number of the recommendations made by Ouchi for public school systems. For example, the decentralization of authority and decision making to the school (academy) level allows the principals/leaders to relate to the students and community in each particular school. In addition to empowered principals, the emphasis on the careful selection of leaders, the training to help them build the skills needed for effective leadership and the development of expectations communicated to the students (believing in them and expecting them to succeed) should enhance the success of any school system, public or private.

Demonstrating similar principles, William Bratton transformed the New York City police department after he was appointed police commissioner. Similar to Ouchi's recommendations and the approach used in KIPP, he decentralized the department, delegating greater authority and providing more autonomy to precinct commanders. The new autonomy allowed them to focus on combating crime and building relationships in their communities instead of dealing with the bureaucracy. Bratton also instituted strategic planning, allowing for more efficient use of resources focused on the areas where they were needed most. He improved communication and developed trust within the department and in the communities served. And, similar to KIPP and Ouchi's recommendations, he strongly focused on performance and results. The end results included a significant reduction in crime and police officers who were more satisfied with their jobs and with the organization in which they worked. These outcomes helped the department attract more and better people to work for the NYC police department (Hitt, Miller & Colella, 2006).

The best organizations in the public and private sectors recruit, select, train and retain the best human capital. This has been shown in scholarly research (Hitt et al, 2001) and argued in the popular press (Byrnes & Barrett, 2005). The most successful businesses have high quality employees who are empowered and managed effectively (e.g., Southwest Airlines, Dell, Schlumberger, Johnson & Johnson). The same is true for other public and private organizations such as schools (e.g., KIPP; public school systems) and police departments (NYC police department). High quality and empowered principals and teachers translate into learning and high performance for the students. They find creative solutions to problems and motivate and support the people whom they serve.

Implications of Management Research for Public Policy and Organizations

From the brief reference to management theories and research presented in this commentary, it should be evident that they have significant implications for public policies and operations of public programs and organizations. Ouchi's work exemplifies the opportunities to apply them and the benefits that can be derived; yet, much of this management theory and research is ignored in developing and implementing public policies and programs. It is incumbent on management scholars to emphasize and communicate the policy implications of their work. Perhaps, they should do so not only in their scholarly articles but also through more articles designed to translate their work for managers and public policy makers. We should include public policy makers and leaders of public organizations as an important constituency of management research.

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COMMENTS OF ANDREW PETTIGREW

THE CHARACTER AND SIGNIFICANCE OF MANAGEMENT RESEARCH ON THE PUBLIC SERVICES

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THE CHARACTER AND SIGNIFICANCE OF MANAGEMENT RESEARCH ON THE PUBLIC SERVICES

There are many ringing phrases from history which have carried forward into the lives of future generations. One of these is the observation by Sir Thomas More shortly before his execution in 1535 that the “duty” of intellectuals in society is to make a difference. More lost his head for engaging in critical policy debates about the form and influence of religion in sixteenth century England. His views collided with the views of Henry the eighth, the monarch of the day, and More’s principled courage was no match for the kingly prerogative.

Here in the twenty first century we as a management research community are hardly so endangered for engaging in policy debates and yet few of us are deeply attracted by policy engagement in either the private or public sectors. It seems our fate is not to be embroiled in the risky and critical, but to be accommodating to the management of indifference. Aside from the thin echo of our own voice, who really cares about research and scholarship in the various fields of management?

Of course, for most of the twentieth century, social scientists have debated ‘knowledge for what purpose’ (Lynd, 1939). As these debates have moved in and out of fashion so they have been mirrored by changing social and economic contexts, altering expectations of science and a greater variety of conduct of knowledge generation in different natural and social science disciplines (Nowotny, Scott & Gibbons, 2001). One of the lessons from the natural history of development of the social and management sciences is that there can be no one best way of framing, producing, disseminating and using knowledge (Pettigrew, 2001). As Morgan (1983) contended many years ago, our future lies in conscious pluralism and yet modernist science has hardly produced a broad church of experimentation. Scholarly routines are a great comfort, but they hardly stretch us to produce different types of knowledge, or to encourage us towards different forms of user engagement and impact. Indeed, impact has become the dog that doesn’t bark in the social and management sciences. The loudest barking dog is just output, and often one form of output, that which is made available in ‘A’ rated scholarly journals. Making a difference implies more than just output generation, and the impacts I have in mind derive from the challenges of the double hurdles of scholarly quality and policy relevance (Pettigrew, 1997).

The authors of the essays in this volume of the *Academy of Management Journal* have been asked to respond to two questions:

1. Do you think we should or shouldn't get more involved in issues involving public policy or public sector management? Why/why not? And,
2. If you believe we should be more involved, please suggest how this might be accomplished?

It should be clear from my opening remarks that management scholars should be much more involved in policy matters of substance in both the private and public sectors. However, behind and underneath the above two questions lie some fundamental choices for us which challenge our identities and conduct as intellectuals, scholars and researchers. Indeed, how many of us see ourselves as intellectuals, scholars and/or researchers, and what is implied by way of differences of conduct and value in these three alternative identities? The Oxford (paperback) dictionary provides a little help in discriminating between the three nouns, but perhaps not enough help. Thus intellectuals are persons having a well developed intellect and a taste for advanced knowledge, whilst a scholar is a person with great learning in a particular subject. And a researcher is a person who engages in careful study and investigation in order to discover new facts or information. Even from these rather limited definitions the narrowness of the researcher identity and role is very evident. I fear the incentives and disincentives of contemporary academic life are encouraging many to be subsumed and cornered by the researcher role. A scholar to me implies not just great breadth of learning and appreciation, but the duty to make this available in dedicated learning, teaching and professing. An intellectual would be capable of the appreciative system of a scholar, but would be harnessing that competence to engage way beyond the boundaries of academia and into the wider reaches of society.

I wonder how many of us have made explicit choices of engagement with one or other of the three identities and roles of intellectual, scholar and researcher? And what of Sir Thomas More's other resonant noun of duty? Not perhaps a fashionable term in our lexicon. Our duties are repetitively confined to our institutions, our students and peers, and to our profession. But an intellectual would be seeking a much broader canvas to express their duty than that.

But what are some of the other fundamental choices and questions which lie beneath the two questions posed to us by the editors of the *Academy of Management Journal*? Surely we should start with purpose? What is the purpose of our endeavours as intellectuals, scholars, or researchers? What kind of knowledge are we seeking to provide, for whom? What kinds of questions need posing and answering to create such knowledge and what varieties of method and evidence do we need to sustain our credibility and self respect as knowledge producers? What kinds of engagement with whom are necessary to produce apposite and useable knowledge? And what is, or might be, or could be, the impact of our endeavours, as intellectuals, scholars or researchers?

These are big questions which demand our attention. The *Academy of Management's* 2006 theme for next August's annual meeting in Atlanta is "Knowledge, Action, and the Public Concern." Atlanta should provide some space for these kinds of questions to be addressed. Here I can only offer a brief personal and therefore partial view of some of the above questions about purpose, knowledge, engagement and impact. In the second part of this essay I will identify 5 factors (each beginning with "I" - hence what I call the "5 I's") that can assist us in making a difference within and beyond academia. But before I take on the 5 I's, what is the importance of the public sector, the public services and public policy to management research?

The continuing importance of the Public Sector and the Public Services

In the 1980's Prime Minister Thatcher and President Reagan regularly proclaimed they were going "to roll back the State." Others have also prophesised "the end of big government." But in spite of many cycles of reform and attempted transformation in many parts of the world, there has been little withering away of the public sector and public services. Table 1 shows the bald facts of general government total outlays as a percentage of nominal GDP in selected OECD countries from 1987 until 2005. The withering away proposition is poorly supported. Of the countries reported in Table 1 only Australia, Sweden and the USA show downward trends, but Australia and the USA are still spending 35% of GDP in government outlays in 2005. The sheer size of the government and public services in these OECD countries is one signal of their importance. As Dean of a substantial Management School in the UK context, how can I possibly ignore 44.5% of the market? Clearly I cannot and will not. Similarly, I believe that the sheer size of government and public services ought not to be ignored by management scholars whose research-based insights could possibly help to guide efficient and innovative forms of management and delivery of needed resources.

Insert Table 1 about here

But it is not just employment and financial scale which is the attractor for the management research community to the public sector. Evidence and argument across large stretches of the globe by authors such as Ferlie, Ashburner, Fitzgerald and Pettigrew (1996), Pollitt and Bouckaert (2004), Ferlie, Lynn and Pollitt (in press) all point to public sector reform as one of the great preoccupations of governments over the last twenty years. As politicians increasingly lose legitimacy, so there is widespread concern about the ungovernability of

the state. This sense of ungovernability and the rising expectation of citizens have in turn encouraged politicians and civil servants to seek answers in reforming the leadership, management, organisation and systems of the public services.

As Ferlie et al. (in press) argue, a crucial phenomenon in the public services is the increasing permeability of boundaries and interdependency between the private and public sectors and civil society; between national and international bodies; and between different parts of the public services within the same economy. Thus in the UK context policy, management and service delivery arrangements between health and social care are increasingly being joined up. The 9/11 outrage in the USA and the London bombings of July 2005 have also made crystal clear that security matters have to be thought about and acted upon in a total system which includes the military, the intelligence services, the police, fire and ambulance services and the health care system. These are big issues which demand attention and action from management scholars.

How we get involved in public sector scholarship is partly a question of leadership, commitment and political will from institutions and research teams. In Europe there are few real barriers as evidenced by the commitment and energies of teams of scholars at, for example, the Erasmus School of Management, Warwick Business School and the School of Management at the University of Bath. In the USA I realise the building of separate Schools of Public Policy and Business may have inhibited some Business Schools from crossing the boundary into matters of public sector and policy concern. No such barriers exist in much of Europe. Business Schools do not need to be private sector enclaves; indeed, if they are they are turning their face against a very large market and away from some of the most intellectually exciting leadership and management issues we face today.

However, leadership, commitment, political will and resources are of no avail if we do not capture the right kind of knowledge and engage with the public sector in a way which will build a more receptive context for scholarly and policy impact. In this final section I will signal some of the central matters of research practice and capacity which may shape our ability to make an impactful contribution to the understanding and development of public sector reforms.

The Five I's of Impact, Innovation, Interdisciplinarity, Internationalism, and Involvement

The action steps to resolve the old dichotomy of theory and practice are often portrayed with the minimalist request for management researchers to engage with practitioners through more accessible dissemination. *But dissemination is too late if the wrong questions have been asked.* Asking apposite questions takes us right back to fundamental questions of the purpose of our scholarly work. Our purpose should be to meet the double hurdle of scholarly quality and relevance; in so doing, we will have impact - the first and most important of the five I's. My starting contention is that if impact is the goal and if we are to increase the probability of impactful social and management research, we need to encourage the generation of research which offers both 'what to' and 'how to' knowledge. The 'what to' knowledge informs us where to intervene and why and the 'how to' knowledge offers us the additional and complementary knowledge of how to intervene and why. To generate research that offers 'what to' and 'how to' knowledge - that is, to achieve our impact ambition - I propose that we need the other "4 I's" - namely; (2) innovation in theory and method; (3) interdisciplinary openness; (4) internationalism of investigation and collaboration; and (5) involvement and independence in engagement with the producers and uses of management research. I briefly elaborate on each of these next, each in turn.

The first action I have named for increasing scholarly impact regards the need for us to increase innovation in management theory and method. Specifically, I believe research designs need to be more sensitive than they typically are to context and time. Generalisations are hard to sustain over time, they are even tougher to uphold across and within different parts of the public sector, and as I shall shortly argue, across international, institutional and cultural borders. Thus 'what to' knowledge needs to be sensitive to variations in different levels of context and 'how to' knowledge can only really be generated in longitudinal research designs. Public sector and public policy research interested in rates and extent of reform must be designed within an embeddedness tradition (Dacin, et al., 1999) which locates innovation at the institutional or sub-institutional level to broad social, economic and political changes. **The process of public transformation cannot be explained just by appeals to managerial action and associated drives for efficiency and effectiveness. Context does matter, space and time do matter in accounting for the emergence and fate of public sector reforms. A basis for a more impactful management research in the public and policy spheres lies in building a more contextualist and dynamic view of knowing.**

The second action I have named for increasing scholarly impact regards the need for us to increase interdisciplinary openness. This is because few of the big public sector and policy issues which I signalled earlier in this essay can be satisfactorily studied from within the myopia of a single discipline, or management field. One of the biggest policy issues of our day is anticipating, leading and managing the response to state and renegade induced terrorism. Actionable knowledge in this sphere is probably out of the reach of most management school Professors. But even if access was forthcoming, the research tasks would have to involve the

analysis of different levels of context over time from international to national to institutional bodies, and the careful analysis of the networks of interdependency, co-operation and competition between often fragmented agencies in the military, the police, the fire and ambulance services and health care. Studies of performance driven reforms of such a multi-level and structurally diverse system could only be enriched by a team with interdisciplinary strength and led by a research leader with interdisciplinary openness. In my experience such teams and leaders are thin on the ground and we do very little as a profession to build scholars capable of working in and leading such teams. One initiative that is easing this team-building is funding provided by the UK's ESRC/EPSRC's Advanced Institute of Management for geographically dispersed teams of management and social science researchers engaged in international comparative research, the importance of which I elaborate on next (cf. Pettigrew & Knight 2005).

The third action I have named for increasing scholarly impact regards the need for us to increase internationalism of investigation and collaboration. This is not a woolly appeal for greater international networking and collaboration between scholars, though that in itself would be of value as a counterforce to ever present cultural and scholarly myopia. There is plenty of evidence that the study of management is not culture free, but culture bound. This tendency to assume a pattern observed in one country will automatically be found in another is made easier by the rarity of large scale international comparative investigations by social and management scholars. International collaborative research is more intellectually, socially and politically demanding and requires bigger commitment and higher risk than within country studies, but if the transaction costs can be contained there is much of real value for scholar and policy maker from international comparisons. International comparative work has to be sensitive to context and time. It does offer the opportunity to challenge easy assumptions about convergence or divergence in patterns of change (Pettigrew et al 2003; Pollitt & Bouckaert, 2004). And from the policy makers' perspective, there is the chance for later adopters of an innovation to take learning from earlier adopters, albeit in possibly quite different temporal and spatial contexts. For an account of some of the practical challenges of engagement in international collaborative research see Pettigrew (2003).

The fourth action I have named for increasing scholarly impact regards the need for us to increase involvement and balance involvement and independence in engagement with the procedures and users of management research. Woolgar's (2000:169) view of user needs remains sharp and shrewd: "we should accept that user needs rarely pre-exist the efforts and activities of producers to engage with them." This 'learning by doing' approach is reaffirmed by Robson and Shove's (1999) contention that user interest in research is fickle and highly time and context dependent; often rests upon quality informal interaction established between researchers and policy makers over long periods of time; and is frequently dependent on the institutional position of user and researcher. And this presupposes the researcher has built social capital in the public sector and policy worlds, not an easy assumption for scholars who have confined their empirical research to the private sector. It is a noticeable feature of the Ouchi case in this edition that his exemplary involvement with teachers, students, parents, school principals, and state government leaders helped him to achieve the bridging and challenging he did between his longitudinal work on bureaucracy-related effects and interventions for strengthening public schools. One way to achieve the bridging amongst stakeholders is to do "feed" empirical evidence from public policy-related studies in forms *supplemental to* journal-outlets, as illustrated, for example, via a series of workshops for UK management researchers interested in building their capacities to plan and carry out this kind of work (Pettigrew & Knight 2005).

In conclusion, to me a central purpose of intellectual and scholarly endeavour is not just to inform and intervene, but to challenge. We are not just scholars, we are also citizens. We are capable of many voices and we are part of university institutions which are not just there to confirm and conform, but to challenge. Engagement with users does not need to entail cooptation by users. We are capable of transcending current beliefs and assumptions and not just reproducing them in the way we formulate research problems. There is a role for iconoclasm and criticism in management research. To work on double hurdle research is not simply to address problems of current interest to power figures framed in their terms. This is a particular, even essential, requirement in the domains of public sector management and public policy where the fate of politicians and political officials is often tied to their last big initiative. Management researchers should engage with power systems, but resist the easy option of being servants of those power systems.

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TABLE 1:
General Government total outlays
Per cent of nominal GDP
1987-2005

	1987	1990	1993	1996	1999	2002	2005
Australia	38.9	36.2	39.8	38.0	35.8	36.4	35.5
France	51.9	50.7	55.3	55.4	53.5	53.4	54.5
Germany	45.8	44.5	49.3	50.3	48.9	48.7	47.1
Japan	32.6	31.8	34.3	36.4	37.8	38.3	37.3
Spain	41.0	43.4	49.4	43.7	40.2	39.9	40.1
Sweden	60.5	61.7	72.9	65.2	60.3	58.3	57.4
United Kingdom	43.6	42.2	46.1	43.0	39.7	41.8	44.5
United States	37.0	37.1	38.0	36.5	34.3	36.3	35.9
Euro Area	48.8	48.6	52.9	51.5	48.9	48.5	48.5
Total OECD	40.6	40.4	43.1	42.0	40.1	41.0	40.7

Note: Data refer to the general government sector, which is a consolidation of accounts for the central, state and local governments plus social security. Total outlays are defined as current outlays plus capital outlays.
Source: OECD Economic Outlook 77 database.

FREEK VERMEULEN COMMENTS

ON RIGOR AND RELEVANCE: FOSTERING DIALECTIC PROGRESS IN MANAGEMENT RESEARCH

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A guide in the employ of the famous Lewis and Clark expedition to the Pacific Northwest one evening over the campfire announced to the explorers that he had both good and bad news for them. “The good news”, he said, “is that we are making excellent progress. We have covered more miles than scheduled. The bad news is: we are lost.” Researchers in the field of management to me often seem to suffer from a similar feeling, as in some existential crisis; “why are we here?” “What are we trying to achieve?” The question guiding this set of essays – “should we get more involved in issues of public policy?” (cf., Ouchi, 2003, 2005) – seems inspired by a similar desire; to matter more. Although I share the desire, I do not think that guiding researchers to study issues of public sector management – or any other issue for that matter – provides the way forward. I believe that for our field to make real progress, and matter more, we will have to change the system in which we work, rather than explore a different set of topics to study. Don’t get me wrong, I think public sector management could be a great area of inquiry; I am just sceptical that, without such changes to our academic system, it will lead to research that actually has an impact. In this essay, I will argue that only a systemic change can synthesize both relevance (thesis) and rigor (anti-thesis).

The Nagging Concern

The feeling that management research does not sufficiently influence management practice has been around for some time. For example, in 1982, in the introduction to a special issue of *Administrative Science Quarterly*, Janice Beyer wrote, “recently, increasing numbers of organizational scholars have begun to express concern that organizational/administrative science has had little effect on life in organizations. Coupled with their concern is a growing interest in finding ways to achieve greater utilization of organizational research” (Beyer, 1982: 588). In 1984, in a paper in the *Academy of Management Review*, John Miner wrote, “Analyzed are 32 established organizational science theories in terms of their rated importance, validity, and usefulness. Little evidence of any relationships among these three variables is found” (Miner, 1984: 296). In 1990, in the inaugural issue of *Organization Science*, Richard Daft and Ari Lewin said, “is the field of organization studies irrelevant? Organizations have become the dominant institution on the social landscape. Yet the body of knowledge published in academic journals has practically no audience in business or government” (Daft & Lewin, 1990: 1), prompting them to ask for “research that is motivated by the problems faced by practitioners” (Daft & Lewin, 1990: 3). Subsequently, Donald Hambrick’s 1993 Presidential Address to the Academy of Management was entitled “what if the Academy actually mattered?” (Hambrick, 1994), Rynes, Bartunek, and Daft (2001) discussed “the great divide” between management practice and academia, and now the theme of the upcoming AoM meeting in 2006, “Knowledge, Action and the Public Concern”, prompts us to identify areas of inquiry that matter most – such as public policy (cf., Ouchi, 2003, 2005) – hoping that then we’ll have more impact.

These concerns have been accompanied by various pleas, for instance to engage more in different research methods, such as qualitative research and action research, different research designs, for instance engaging practitioners, or to study different areas (for an overview, see Rynes et al., 2001), such as the topic of this forum i.e. the public sector. Let me refrain from adding yet another plea to this list of suggestion solutions, because I have little doubt that it would be to no avail. Instead, let me simply try to understand how we got into this situation in the first place, and why we haven’t escaped it yet.

Dialectic Progress in Management Research

Rigor versus relevance. In the first half of the twentieth century business schools were akin to what Bennis and O’Toole (2005) described as trade schools, where semi-retired executives told students war-stories and little systematic research was conducted. As an anti-thesis, with the rise of the Graduate School of Industrial Administration (GSIA) at Carnegie Mellon (then the Carnegie Institute of Technology), rigorous academic research into the functioning of organizations was promoted (Mintzberg, 2004) and in 1956, in the inaugural issue of *Administrative Science Quarterly*, Thompson (1956: 102) wrote “Research must go beyond description and must be reflected against theory. It must study the obvious as well as the unknown. The pressure for immediately applicable results must be reduced.” Where the “war stories” by executives in the early trade schools addressed questions highly relevant to managers, many would argue that this change represented a much-needed shift towards more systematic and objective inquiry.

The pendulum swung a long way, and rigor gradually crowded out much – or according to some even most – of the research’s relevance (Bennis & O’Toole, 2005; Ghoshal, 2005; Mintzberg, 2004). By 1993, Hambrick argued “we read each others’ papers in our journals and write our own papers so that we may, in turn, have an audience []: an incestuous, closed loop” (Hambrick, 1994: 13). By cutting out practitioners as an audience from the loop we cut out reality from the academic cycle. As a result, I believe, our research has become much like the Glass Bead Game as described in Nobel Prize laureate Hermann Hesse’s novel of the

same name, which is “sublime and aristocratic [...] though not active and directed toward goals, not consciously serving something greater or profounder than itself. Rather, it tends somewhat toward smugness and self-praise, toward the cultivation and elaboration of intellectual specialism” (Hesse, 1943: 329).

Relevance without rigor? Should the pendulum swing back, then, towards relevance? I think not, because that would imply sacrificing rigor. I for one feel that research that was not rigorous (in the sense that it would not pass the standards of for example the Academy of Management Journal) cannot be considered relevant. For example, Bennis and O’Toole (2005) describe a paper submitted to an academic journal that in their opinion should have been published because it made the interesting claim that certain indicators of leadership misbehavior could be monitored to identify ethical problems before a crisis occurs. Potentially interesting indeed but, as the authors continue, alas “that finding could not be proved in a strictly scientific sense” (p.99). Then, I do not want to hear about it! Just because something sounds intriguing and makes an interesting claim does not mean it should be said and published. Claims unsupported by thorough academic research, no matter how intriguing they may sound, to me are not relevant.ⁱⁱⁱ Actually, I fear they could be dangerous. Academic journals may be guilty of publishing dull and irrelevant findings. Practitioner journals are often guilty of publishing provocative and counter-intuitive claims that sell well but that imply unsupported prescriptions whose consequences are unknown for executives that take them at face-value.^{iv}

Synthesis. But how to deal with these seemingly opposing ends of rigor and relevance? Real progress, following dialectic theory (Engels, 1940; Hegel, 1812, 1830), would not be achieved by finding some balance between the two (Staw, 1995), but by reconciling the thesis with its anti-thesis at a higher level of abstraction and understanding. It seems to me that rigor is often at odds with relevance because the answers that can be supported by rigorous research seem of little interest to practitioners. However, I would contend that the answers are not to blame for this deficit but that it is the research question that was asked in the first place that determines the usefulness of a study’s findings. Asking questions that are of importance to reality, while not making concessions in terms of rigor in developing theory and empirical evidence, would provide most value. Relevance is then found in the question, rigor should be present in the method to provide the answer.

What, for example, makes Ouchi’s work (2003, 2005) relevant is not, in my opinion, the fact that he studies an area (public sector management) that is in some way more relevant than another area, but that he sets out in his research to solve a question which is of importance to practitioners working in that field. His primary mind-set designing and executing his research project has obviously not been to just please and interest other academics, but to solve a very real problem—that is, his research question was *relevant*; his research design and execution (as far as I can tell based on the available sources) was academic and *rigorous*. This, to me, is the lesson from Ouchi’s work: not that we should engage more in public sector research, but that we should do more research that synthesizes rigor and relevance, because it asks a research question that matters while not sacrificing rigor in search of the answer.

Changing the System

Do I think my appeal to synthesize rigor and relevance will change the behavior of academic researchers – at least of those who agree with? No I do not; nor did any previous pleas for relevance. I believe this is because, ultimately, our academic system does not value relevance. The only way to change the attitude and behavior of people is to change the system that they operate in (Coleman, 1993; Ghoshal & Bartlett, 1997). Hence, people will only start addressing and caring about managerial relevance if that is what the system will support and appreciate. And currently it does not (Bennis & O’Toole, 2005).^v To correct this, I believe, we have to break open Hambrick’s “incestuous, closed loop” (Hambrick, 1994: 13), i.e., our vicious circle of only writing about our research for other academics whose work we read. Our system will likely remain an incestuous cycle of academic producers and consumers only, without much relevance being addressed in the questions that people pursue (whether it examines the private or the public sector), unless “practice” enters the loop as a valued, separate group of recipients of our research.

Hence, I do not want to make any suggestions of how people might go about trying to make sure that their research provides relevance (such as others have provided in the past, e.g. “do more qualitative research”, “involve managers in the research”, “study the public sector”, etc.). My argument is that all such suggestions are likely to be futile until a systemic change is brought about that assures that, at the end of the cycle, practitioners also become valued as recipients of the knowledge that we produce. I am (merely) positing that *if* we make sure that our system values relevance, it is up to individual researchers to figure out how they want to achieve that, and we might find that different things work for different people. It implies that “communicating to managers” becomes recognized in our system, so that it is assured that directing research output towards practitioners is identified, valued, and rewarded. This would likely have to involve a range of measures and output criteria (e.g., possibly including practitioner journals, executive education, contributions to the business press, outsider opinions, etc.). Yet, I do not feel it should change the role of our academic journals, such as the Academy of

Management Journal.^{vi} In this system, it would be the role of academic journals to assure rigor (as they do) and a different, separate track to assure relevance.

Changing a system usually requires more than just changing incentives; it also requires adaptations in terms of culture, people, etc. (e.g., Ghoshal & Bartlett, 1997). However, in this case, I feel that the people may not be the bottleneck. Many of the academics in our system seem eager to change – the many pleas for relevance in forums like these are suggestive of that. Furthermore, I notice from reading the many applications to our PhD program at London Business School, for example, that very few people aspire to become business academics with the intention to publish journal articles that will only be read by other academics (at best), but that they are much more inspired by the thought of gaining and developing truly relevant knowledge to change the world of organizations.^{vii} Changes to our incentive system (e.g., tenure criteria) would be welcomed by many (although currently certainly not all!), and could make people rediscover their original motives to become management academics in the first place. Therefore, I am hopeful it could fairly swiftly generate subsequent change in the culture of our field, in terms of appreciation of managerial relevance (Coleman, 1993). Moreover, a feeling of dissatisfaction with the existing system is one necessary condition for a synthesis to progress (Engels, 1955). Thus, some relatively simple changes to incentivize people to bring their work to the attention of practitioners could set in motion a chain of systemic reactions that just might alter our world.

Epilogue: Making a Difference

Some time ago, I was having dinner with a fellow management professor, who told me of a friend who, through his research, had discovered and developed a form of pain relief for a disease that several dozen people in the world were suffering from. My dinner companion was reflecting on how valuable and useful his friend's research was, making such a direct and significant contribution to the quality of life of these people (and, I believe, rightly so!). Moreover, he commented on how our research on how to improve business, in comparison, was devoid of such meaning, and how great it must be to be able to make a real, direct contribution to society; all we did, at best, was help businesses make more money.

I think he would have liked the work of Ouchi (2003, 2005). Ouchi's work is not about increasing profit, it is about helping children learn, and such a topic seems to appeal to many people. However, notwithstanding the value of such work, let me also say something in defense of research that attempts to help companies make more money. Income has been linked to such things as malnutrition (Strauss & Thomas, 1998), crime (Bailey, 1984; Land, McCall, & Cohen, 1990; Williams, 1984), infant mortality rates (Hales, Howden-Chapman, Salmond, Woodward, & Mackenbach, 1999), and (on a macro scale) simply happiness (Frijters, Haisken-DeNew, & deShields, 2004). Fueling the economy by aiding companies to increase their profits is a potent way to contribute to society and human wellbeing. Note that I am not saying that increasing the profitability of businesses will automatically solve all society's problems – surely there are also other conditions that are relevant – I am merely positing that there is nothing inherently wrong with helping businesses make more money. I believe helping organizations become more efficient, effective, and profitable is a great way to help build society, and a worthy cause in itself.

To conclude, I am not joining a plea to follow Ouchi's example and do more research that addresses public policy. For one, I think it is up to individual researchers to figure out what they want to examine, and advancing "profitability" is one worthy cause. Moreover, what I found inspiring and most important about Ouchi's work is not the area it addresses (i.e. public sector management) but *how* it addresses it: with a clear zest to tackle a real practical question, while searching for the answer in a rigorous way. I think that is where the real lesson of his work lies. And don't be mistaken, our potential to make a difference is huge. Organizations are omnipresent in human society (Simon, 1991), and if William Ouchi shows one thing, it is that management really can make things better, in the lives of the people embedded in organizations (e.g. teachers) and those they serve (e.g. students). We are obliged to society but also our own personal ambitions to fulfill the great potential of management research, and do justice to our desire to make a difference.

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COMMENTS OF MARY ANNE VON GLINOW

LET US SPEAK FOR THOSE WHO CANNOT

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Our lives begin to end the day we become silent about things that matter
--Dr. Martin Luther King Jr.

Modern organizations may well be the greatest social innovation of the human race; they are the primary source of wealth, well-being, and most of our day-to-day experiences (Werther & Von Glinow, in process). Sometimes however, organizations or organizational functioning sours, as Ouchi (2003) captures in his book entitled Making Schools Work. When our collective voice wanes on things that matter, such as the way we educate our children, we fail in our duties to society, and to ourselves if our lives are to have meaning. If we are to help the lives of others to potentially have more meaning, which is often facilitated by the building and sustaining of organizations, then we must not be silent on things that matter. We must not lose our way, nor fail to lend our voices when and where they are most needed. Failure to act on what is *right*, or on issues that matter,

prevents us from fully expressing what we feel, who we are, and building organizations that are socially conscious and collectively responsible.

As I reflected on the question of whether or not management scholars should have more visibility and impact on issues of public policy or public sector management, I concluded that management scholars had indeed been far too silent on 'things that matter,' and that we have not explicitly addressed 'the public good.' Of one thing, however, I was certain: The extent to which we *can* have impact on societal issues such as the public good, requires organization. Joining with others to achieve goals that cannot be achieved alone is, of course, why organizations are created in the first place.

Sadly, as organizations grow, many of the early 'lofty' goals seem to disappear in favor of more expedient ones. Ouchi (2003) chronicled this when he discussed the liability of size in educational systems. He noted that with size comes thick administrative bulk-ups for which the main cure is decentralization of decision making to the operating units. However, as Ouchi's research documents, the benefits of decentralization are often *not* realizable when operating units don't speak to one another and/or lack sufficient resources to do what needs doing. Although he was speaking about our school systems, I believe the same logic applies to our flawed decentralized systems for dealing with animals, pets, and companion animals, particularly in times of emergency, crisis or disaster. I suspect that there are numerous other examples of poorly executed decentralization; however, this one stands out for me as especially amenable to management thought and expertise.

In the remainder of this essay I focus on two things. First, I focus on what I consider to be one of the most recent vivid illustrations of how ill-managed decentralization can wreak havoc on the lives of those inside and outside the organization—on anyone or *any sentient being* who is victimized by poor decision making. Specifically, I refer to the (mis)management of evacuating residents in general, and those with companion pets in particular, during Hurricanes Katrina and Rita, just weeks apart from each other, that ravaged the U.S. gulf coast in September 2005. Second, I will suggest ways that we as individual scholars can exert voice such that we can 'make a difference.' I conclude by raising questions about whether it may be time to rethink the role of the Academy in enabling voices to join together in actions that include public policy influence for the public good of society.

The (Mis)Management of Evacuating Residents With Animals

It has been said that you can tell a great deal about a society based on how it treats its children and its animals. People in the U.S. love their pets and companion animals, and the pet-business is a multi-billion dollar industry. However, across the U.S., pets are not generally permitted in emergency shelters. Thus, during the hurricanes, in numerous states, animal lovers were put into a deadly zero sum game: save yourself at the expense of your animals, or stay with your animals and put yourself in harm's way (unless of course you had the means to evacuate). Most of the people who ended up in shelters had no such means and most animal shelters were jam packed too with abandoned animals. So, public bureaucrats across the flood-ravaged Gulf States made decisions with one management model in mind—that of the zero sum game. Furthermore, there is no centralized arm of the government (like FEMA) to help *any* animal pre-, during-, or post-disaster, so the federal government is equally negligent on managerial alternatives.

This is not to denigrate the kindness of rescue groups, but to *challenge* the decentralization of decisions regarding pet rescue to local operating units. This is also to suggest that we as management scholars DO know something about managing organizations, that we can and should have a voice on some matters of public policy, and that we should speak for those who cannot—animals, children, and the infirm. I believe we have an ethical obligation to use our expertise to influence local-level leaders responsible for animal and human welfare, as well as state and federal authorities. The fact that there is no public policy (other than to exclude them from local shelters) as regards animals in times of disaster preparedness or relief seems to me a painful neglect of our responsibilities to our animals, our families, ourselves.

What Can We As Individual Scholars Do?

Hence our challenge. Like Ouchi, who had to get educational administrators to listen to him and to try to generalize from business to education, we need to begin the public policy influence process. This takes coordinated, organized effort to offer a better way of management.

When decisions are relegated to local operating units that appear ill-equipped to deal with the task at hand, as management scholars we know how to manage the process. We know how to 'translate' our research into practical implications for business owners, so why not our community leaders and leaders of not-for-profit organizations? Sometimes the local unit does not have the 'authority' to deal with the crisis-at-hand; our goal could be to assist them in learning how to incorporate these lessons into standard operating procedure, or to begin to think outside of their existing management tool box procedurally.

But, it is our knowledge of management science that allows us to offer alternative models for dealing with our animals and other helpless beings in times of disaster at all government levels. We know a lot about what works, why it works, and why it fails, from an organizational perspective, as Ouchi has so artfully

discussed. I suggest we start to use this knowledge in this and numerous public policy areas that could benefit from our insights. I am focusing on only one. Join a local board, give presentations locally or even in state-wide venues—they need our help on how to better manage businesses, including not-for-profits like pet shelters. Even the largest of the animal welfare organizations, i.e. Humane Society of the U.S., or PETA, or ASPCA need these skill sets to assist them to incorporate better management skills into their rescue operations for effectiveness and efficiency.

Is it Time to Rethink the Role of the Academy in Enabling Collective Voice?

Lest any reader think that I have chosen to express my beliefs on a narrow issue of particular concern to me, let me argue next that “my issue” is far larger than the one that I’ve discussed to this point. The underlying issue that I am raising involves our need, both as an Academy *and as individual management scholars who have chosen to be members of the Academy*, to ask and resolve:

1. What actions must we take-- as individual management scholars and/or collectively as an Academy of Management-- to get SOUGHT by future groups (such as, but not limited to, evacuation-operation-related groups, animal rescue groups, etc.) for counsel regarding how to organize efficiently and effectively, how and when to decentralize decisions effectively, and how to do anything that potentially has the likelihood of affecting massive numbers of people and their families?

2. Are there actions we have not yet taken that may explain why public policy groups and/or managers in the public sector tend not to seek our counsel? Why hasn’t the U.S. Federal Emergency Management Association (FEMA) sought ‘best practices’ from us or the Academy of Management regarding evacuation operations?

3. Might the tendency for management scholars to be absent in public debates regarding the relative effectiveness of various policy-options be related to whom we do (and don’t): 1) communicate our management-knowledge?; 2) include in our research networks?; 3) include in our development of research questions?; 4) include as relevant audiences for the “implications” we describe in our publications?

4. Might the people falling into the “don’t” categories listed above generally be those actively involved in determining public policy and/or managing the public sector? The absence of public policy- and/or public sector management-representatives does indeed seem to be the case when one peruses the affiliations of most authors of top-tier management journal publications, as well as these publications’ descriptions of the genesis of research questions and/or measurement-strategies.

5. Is the risk of acting as “an Academy” (hence as one voice) in response to any particular issue of concern to us—i.e., voicing a view that may not be shared by all Academy members— greater than *never* acting in a collective voice-based way? Might there be options for voicing that are not merely “all” or “none”?

I do not know the answers to all of these questions, but I do think the time has come for us to begin to debate them, and to determine what the set of (network-building, research-based, and communication-based) actions might be to increase our visibility and impact as a management profession on issues of public policy and public sector management. Why is now the time? Because in fact the time has always been, as Martin Luther King Jr. so eloquently noted. Being silent on things that matter is destructive...to us professionally, and personally.

References

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ⁱ We advocate Standpoint Epistemology because it provokes thinking about the sometimes hidden and otherwise unexamined assumptions that can guide scholarly inquiry and because it makes a reasoned case for working with alternative assumptions and disadvantaged referents. In the space available for this commentary we cannot do justice to the complex philosophical issues involved in affiliating with the exploited (see Harding, 2004a).

ⁱⁱ And here we want to be clear that by epistemology we mean to emphasize that which extends the domain of research methodology back to the start of the context of discovery, and not merely whether we choose quantitative or qualitative data analysis—see Harding (2004b).

ⁱⁱⁱ Let me apologize to Bennis and O’Toole for (over)simplifying their point in order to make mine. I have not read the original manuscript they refer to (it apparently did not get published in its original form) and hence do not know to what extent the authors’ conjectures received some support. Moreover, I do agree that there should be a place in academic journals and/or articles to speculate about the bigger picture that encompasses particular well-supported findings.

^{iv} In 1983, examining a full year of articles in both Administrative Science Quarterly and Harvard Business Review Dunbar (1983) concluded that the majority of articles in ASQ emphasized objective analysis but with little effort to relate findings to practice. HBR, in contrast, published

articles that made specific general recommendations but the basis for these recommendations was often not apparent. I doubt if we were to repeat this analysis today that the outcome would be much different.

^v For example, currently, in many if not most business schools things such as publications in practitioner journals, executive education, or business books are frowned upon and “don’t count” (at best) in for example tenure decisions and academic prestige.

^{vi} One thing academic journals could do, is accept that findings of a research project may have already appeared in a managerial journal, and vice versa. One of my colleagues, for example, recently had a paper rejected in between the first and second round of revision for one of the Academy journals because, in the meantime, the findings appeared – be it in a different format and without much attention to the study’s methods – in a journal aimed at a practitioner audience. In the system I envision, research projects that lead to publications in both academic and practitioner journals would become the ideal, rather than the exception. Current, scarce examples of this would include work by and Denrell (2003, 2005) Brown and Eisenhardt (1997; Eisenhardt & Brown, 1998, 1999), Birkinshaw and Gibson (2004; Gibson & Birkinshaw, 2004) and, more loosely coupled, Wiersema (2002; Bigley & Wiersema, 2002).

^{vii} Admittedly, by the time these people exit their PhD programs this intrinsic motivation has often disappeared (Deci, 1971; Deci, Koestner, & Ryan, 1999) and they are firmly assimilated to speak exclusively to other academics.