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SCHOLARSHIP THAT WORKS

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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' working 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,* who said 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 & Associates, 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 the "experts" whom, according to Hennigner, 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 as if 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 1940s, Kurt Lewin, whom 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 to be more eager to interact with researchers than vice versa. This becomes clear if you attend one of our conference sessions (at, for instance, a meeting of the Academy of Management [AOM] or of the Society for Industrial and Organizational Psychology [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 Leap

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

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

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 imbalance 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 pattern cannot be denied; look at the Web sites of these universities. In fact, the more resources a university holds, the more it seems to want basic researchers; its job advertisements will 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 *BusinessWeek* because they impact resources. Ironically, these rankings are boosted by basic research publications. This fact has lead some universities to stipulate 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, where they produce 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 conference presentations made by applied researchers where audience members have questioned the motives of the researchers; attacked, rather than questioned, their 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, it was, I believe, an experience that is useful for assessing *how* practical research can be done well. Our research on cross-functional teams at this company illustrates 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 from Bill Ouchi's more macro research on school systems.

Action research differs from typical social science field research involving data collection and feedback to an 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 an 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 online survey. It took six months for our team of eight 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 we 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, a method that has never disappointed me in field research: the comparative multiple case study method (see Druskat and Wheeler [2003] for an example [cf. McClelland, 1976]). It proved to be the most informative research tool we used, and it is similar to the comparative method used by Bill Ouchi, so I will elaborate a bit. We identified criterion samples of teams with truly exceptional performance and teams with average performance. We then studied these teams to determine what differentiated the exceptional from the average. Blind to performance categories, we interviewed members and leaders of these teams, using critical incident interview methods. As an aside, I note that I learned long ago not to ask direct questions in interviews with organization members. The answers I get from direct questions say more about interviewees' values and 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 they 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 focused, so that mute buttons didn't turn on and keyboards start clicking, and the real challenge of keeping side conversations among the members in Belgium (who were all sitting together in one meeting room) from becoming distracting. I learned that Europeans laugh at different jokes than do Californians—a distinction that might very well have been a consequence of the ungodly hours that these team members needed 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 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 was competing for resources with teams that met face-to-face in Missouri, had 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 impel 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. What you'll gain, however, 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 we worked with. 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 organization members ranging from the CEO of the division to the members of some of the roughest teams. I've learned that good data that fit 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 step 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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