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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 if they are in some existential crisis and asking, "Why are we here?" and "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 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 (antithesis).

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 asked, "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), and Rynes, Bartunek, and Daft (2001) discussed "the great divide" between management practice and academia. Now, the theme of the upcoming 2006 AOM meeting, "Knowledge, Action and the Public Concern," prompts us to identify areas of inquiry that matter most—areas such as public policy (cf. Ouchi, 2003, 2005)—hoping that then we'll have more impact.

These concerns have been accompanied by various pleas asking scholars to engage more in different research methods, such as qualitative research and action research; or to use different research designs, such as designs engaging practitioners; or to study different areas (for an overview, see Rynes et al. [2001]), such as the topic of this forum, the public sector. Let me refrain from adding yet another plea to this list of suggested 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 20th century, business schools were akin to what Bennis and O'Toole (2005) described as trade schools, institutions in which semiretired executives told students war stories, and little systematic

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research was conducted. As an antithesis, with the rise of the Graduate School of Industrial Administration 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 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" (1956: 102). Although the war stories by executives in the early trade schools addressed questions highly relevant to managers, this quest for more systematic and objective inquiry represented a much-needed shift, as many would argue (Augier, March, & Sullivan, 2005), toward academic rigor.

The pendulum swung a long way, and rigor gradually crowded out much—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 practitioners as an audience out of 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, a game that 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, toward relevance? I think not, because that would imply sacrificing rigor. I for one feel that research that is not rigorous (in the sense that it would not pass the standards for acceptance of, for example, the *Academy of Management Journal*) cannot be considered relevant. For example, Bennis and O'Toole (2005) described 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, alas, "That finding could not be proved in a strictly scientific sense" (Bennis & O'Toole, 2005: 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.[1] 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 counterintuitive claims that sell well but that imply unsupported prescriptions whose consequences are unknown for executives that take them at face value.[2]

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 antithesis 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 to be of little interest to practitioners. However, I would contend that the answers that researchers provide are not to blame for this deficit. In any study, it is the research question that was asked in the first place that determines the usefulness of the study's findings. Thus, academic answers often lack practical meaning because the questions that were asked to start with lacked relevance. 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 in the method applied 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

[1] 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 support. Moreover, I do agree that there should be a place in academic journals and/or articles to speculate about the bigger picture that comprises specific findings.

[2] After examining a full year of articles from both *Administrative Science Quarterly* and the *Harvard Business Review,* Dunbar (1983) concluded that the majority of articles in *ASQ* emphasized objective analysis but showed little effort to relate findings to practice. *HBR,* in contrast, published articles that made practical recommendations, the basis for which was often not apparent. I doubt, if we were to repeat this analysis today, that the outcome would be much different.

in some way more relevant than another area, but that he sets out in his research to solve a question 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 from 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 but does not sacrifice rigor in searching for 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 have any of the previous pleas for relevance. I believe this lack of change occurs 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 do so (Bennis & O'Toole, 2005).[3] To correct this, I believe, we have to break open Hambrick's "incestuous, closed loop" (1994: 13), 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 of relevance being addressed in the questions that people pursue (whether it examines the private or the public sector), unless the organizations we study also enter the loop as a valued, separate group of *recipients* of our research.

Hence, I do not want to make more suggestions about how people might go about trying to make sure that their research provides relevance (e.g., "Do more qualitative research," "Involve managers in the research," "Study the public sector"). 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" would become recognized in our system, so that research output directed toward practitioners would be certain to be identified, valued, and rewarded. Such recognition would likely have to involve a range of measures and output criteria (practitioner journals, executive education, contributions to the business press, outsider opinions, and so forth). Yet I do not feel this shift should change the role of our academic journals, such as the *Academy of Management Journal*.[4] In this system, it would be the role of academic journals to assure rigor (as they do), and it would be the role of a different, separate track to assure relevance.

Changing a system usually requires more than just changing incentives; it also requires adaptations of culture, people, and more (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 suggest that. Furthermore, I notice from reading the many applications to our Ph.D. program at London Business School 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); rather, these applicants are much more inspired by the thought of gaining and developing truly relevant knowledge that

[3] For example, currently, in many if not most business schools, publications in practitioner journals, executive education materials, and business books are frowned upon and "don't count" (at best) for tenure decisions and academic prestige.

[4] 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 had appeared—presented 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 such linked publications include work by Denrell (2003, 2005), Brown and Eisenhardt (1997; Eisenhardt & Brown, 1998, 1999), Birkinshaw and Gibson (2004; Gibson & Birkinshaw, 2004), and (in a more loosely coupled set of publications), Wiersema (2002; Bigley & Wiersema, 2002).

might change the world of organizations.[5] Therefore, I believe that many people in our profession (although certainly not all) would welcome changes to our incentive system (e.g., tenure criteria), and such changes could make people rediscover their original motives for becoming management academics. Therefore, I am hopeful change in our incentive system could fairly swiftly generate subsequent change in the culture of our field toward appreciation of managerial relevance (Coleman, 1993). Moreover, dissatisfaction with the existing system is one necessary precondition for the progress of a synthesis (Engels, 1955). Thus, some relatively simple changes to encourage 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 had 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 like 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), 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 well-being. Note that I am not saying that increasing the profitability of businesses will automatically solve all society's problems—surely other conditions are also 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 (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 not only to society but also to our own personal ambitions to fulfill the great potential of management research, and do justice to our desire to make a difference.

[5] Admittedly, by the time these people exit their Ph.D. programs this intrinsic motivation has often disappeared (Deci, 1971; Deci, Koestner, & Ryan, 1999), and they are firmly assimilated into speaking exclusively to other academics.

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