I am often told that it is somewhat unusual that I’ve spent my entire twenty-five-year academic career at the same institution—namely, the University of Washington (and that I speak Norwegian, Danish, and an obscure dialect of Chinese as well). During that time, I’ve had the opportunity to work with many fine colleagues and students. In the last ten years I’ve had the wonderful experiences of being an associate editor and then editor of the Academy of Management Journal, of being vice president and then president of the Academy of Management, and—a somewhat different kind of experience though still a good one—of being an associate dean of faculty at the Foster School of Business. It is from the vantage point of these experiences that I want to talk about the role of the management professor, although I suspect that much of what I say applies to virtually all professors. More specifically, I want to offer my thoughts on our research and teaching missions.

VALUING RESEARCH

I think it’s fair to say that most Academy members believe that our research is valuable. Recently, for example, Academy members spent considerable time discussing important issues in the research process, such as relevance, rigor, the value of theory, and the value of studying interesting ideas in a non-theory-based fashion. In the October and December 2007 issues of AMJ, for example, there are two very enlightening editorial forums on these topics that I encourage everyone to read.

After reading these essays and other published voices on our scientific inquiry, I think it is also fair to say that we collectively accept the goals and values of subjecting our research to some sort of “marketplace of ideas” and to some sort of a “test of time.” In particular, we often attempt to judge the value or contribution of our academic and popular press publications. Over the years, for instance, we’ve seen some ideas resonate and some ideas quietly slip away. Other scholars with minds far better than mine have talked about the publication process, and I have little to add to that conversation. Instead, I will share my views on how we might think about the value of our research.

To do so, I first ask myself, “What are academic professors good at?” I think they are good at deliberative, thorough, and slow thought processes. We take years, for example, to develop ideas, to design and conduct studies to test these ideas, and to improve upon our original ideas based on our empirical findings, while trying to publish our research in prestigious journals. I next ask myself, “What are other users of research tools good at?” For the sake of simplicity, I’ll call these other users “consultants.” I think that consultants are good at focused action on immediate organizational problems that are often ill defined and that require knowledge outside our areas of expertise and education. These differences are salient to me because I recently completed my first “pure consulting project” in about twenty years. (As an aside, I receive inquiries about consulting approximately once a month. My standard [and honest] reply is that other members of my faculty would perform these tasks far better and far cheaper than I. Usually, these inquiries go away. To my surprise, one came back.) I should also add that I don’t believe that what academic professors do is more or less important for organizations than what consultants do. It is simply different. Professors and consultants work at a different speed, depth, and thoroughness, and they focus on different kinds of end products. What we have in common is that we all try to do the best we can on behalf of organizations, given our constraints. (In my recent consulting gig, I may well have driven my employer crazy...
because I worked at a professor’s pace while being paid per hour.)

When I think about our academic research, it’s been my experience that one publication seldom has a large influence on our theory and research. Instead, it is a stream of programmatic research involving many different kinds of books, articles, and chapters that deeply affects our thinking and actions. Most of us, for example, have been deeply affected by Simon’s (1976) *Administrative Behavior*, March and Simon’s (1958) *Organizations*, Thompson’s (1967) *Organizations in Action*, Weick’s (1969) *The Social Psychology of Organizing*, or Pfeffer and Salancik’s (1978) *The External Control of Organizations*. But as amazing as these books are, the work that they inspire is more impressive and important.

In my view, the value of scholarly contributions should be evaluated by the subsequent knowledge they inspire. I do not mean judging value only or primarily by numbers of top-tier publications, citation counts, or similar meaningful but imperfect measures. Instead, I believe that what we should value most are those bodies of programmatic research that lead us to say, “I am confident that we know a lot about this topic, and I’d feel good advising executives and managers to spend a lot of money implementing our ideas based on our research.”

With perhaps only a millisecond of thought, we can think of the thirty plus years of research by Ed Locke and Gary Latham on goal-setting theory for a deep understanding of a very powerful effect on work behavior that anyone would feel comfortable advising a client organization to adopt. The same could be said for the twenty-five plus years of leadership research by Bernie Bass and Bruce Avolio—the latter, I’m delighted to say, now my colleague at the Foster School of Business—or the thirty plus years of research on validity generalization by Frank Schmidt and John Hunter, or the twenty plus years of research on organizational justice by Jerry Greenberg or Russell Cropanzano. Further, there are the hundreds of researchers who have worked directly with these scholars and thousands of others who have worked on issues inspired by this scholarship. In my judgment, these kinds of programmatic research are the magnificent contributions to our science.

As one example, I am proud to say that my colleagues (including Terry Mitchell, Brooks Holton, and Miriam Erez, to name only a few) have produced theory and research on employee turnover that have been well received. In 1990 I was promoted to associate professor with tenure. (This was good.) On the day that I received word of my promotion, I drove to Terry Mitchell’s house and said that I had some new ideas about employee turnover. (I can only imagine Terry thinking, “Oh gosh, here comes another set of hair-brained ideas.”) During the next ten years, however, Terry and I developed the unfolding model of voluntary turnover, which we think answers the question “Why do people leave their jobs?” in a way that substantially differs from prevailing theory and research. We published several top-tier articles and academic book chapters on the theory. Among turnover researchers, this work received modest notice, whereas among the broader audience of management scholars, this work was at best an asterisk.

During the mid to late 1990s, Terry, Miriam, Brooks, and I developed some new ideas built, in part, on the unfolding model around the question “Why do people stay in their jobs?” Since 2001 we’ve published several top-tier academic articles, academic book chapters, and managerially focused journal articles on the idea that people become embedded in their jobs. Among turnover researchers, our collective work is getting substantial notice, whereas among the broader audience of management scholars, this work is now getting a bit of very nice mention. For example, the recent AACSB International (2008) report cites this work as one of several research streams that has had a meaningful impact on managerial practice. I am proud to say that our research has received several awards and grants and some interest by several Fortune 500 companies and medium-size organizations as well. Most gratifying to me, however, is that many scholars, independent of the Foster School of Business, have published research based on our original ideas.

What do I draw from my personal experience and that of others? First, we should be less concerned about the contribution of individual studies and more concerned about the contribution of programmatic bodies of research. Further, I believe that such programmatic research should be our gold standard for meaningful academic scholarship. Second, most contemporary research issues and the everyday constraints imposed by a business school require a diverse skill set and
multiple points of view for programmatic scholarship, particularly in today’s global marketplace for research and publication. In other words, I believe that team-based research is the most effective strategy to conduct programmatic scholarship in today’s environment.

VALUING TEACHING

I would also like to say a bit about teaching. Again, I speak as a faculty member who has spent his entire academic career at one major state-supported research university and who has spent the last four years as an associate dean of faculty. Nonetheless, I believe that my remarks apply more broadly to virtually all business schools.

At many points in my career, in the many doctoral consortia at the Academy that I’ve participated in or heard about, and in a recent conversation with a faculty member from a teaching university, I have heard senior and junior faculty members say that teaching really doesn’t matter and that only research counts. I think many of us in this room, particularly those of us who are at least middle-aged, have heard similar comments. Candidly, I find these kinds of comments quite troubling. Truth be told, I believe there was much veracity in such comments ten or more years ago. But today I don’t think there is as much truth in them, and I believe that there will be virtually none very soon. Instead, professors of management will soon be required to be excellent—or at least good—at both research and teaching for promotion, tenure, and annual pay increases.

Why do I say this potential heresy? It’s no secret that state support for higher education is declining at a fairly rapid rate. Because business schools and maybe most professional schools have opportunities to generate extra revenue from what are euphemistically called “self-sustaining,” “revenue-generating,” or “non-state-supported” programs, we can offset the declining public support and pay our bills. Typically, these revenue-generating programs are EMBA, part-time MBA, and nondegree executive education. Equally important, there are the ubiquitous rankings by BusinessWeek, U.S. News & World Report, and the Financial Times (to name only a few).

I see three unintended consequences of this ongoing emphasis on self-sustaining programs and rankings. First, the rankings may well direct many prospective students to our programs, both state supported and self-sustaining, which will help us both to fill classes and to reliably pay our bills. Second, many small, medium, large, and gigantic financial gifts are often directed toward the higher-ranked programs, which will also help us pay our bills. In my June 2008 “President’s Column” in the Academy of Management News, I explained why I think that these rankings have improved the quality of our various academic programs. Thus, a third unintended consequence of the rankings is that they create great pressure on institutions to improve the quality of our degree and nondegree programs such that students want to attend and donors want to contribute to winning and visible programs. Because we like the ability to pay our bills, we want to maintain that income over time, which means that we need to continually improve the quality of our teaching and of our programs.

With both good and not so good consequences, the increasing demands for higher-quality research and teaching encourage faculty salaries to increase because, in part, the supply of faculty who can conduct high-quality research and teaching is perceived to be in somewhat short supply. Further, I am told that many business school deans lament this increase because they have to find a way to pay these higher salaries. As a faculty member, however, I see a virtuous cycle in which self-sustaining programs, rankings, escalating salaries, and pressures for improvements lead to enriched student experiences, demand for excellence in teaching, and demand for excellence in research, all of which, in turn, start the process again. At my school, for example, we now tell all professors that promotion and tenure require excellence in teaching, and demand for excellence in research, all of which, in turn, start the process again. At my school, for example, we now tell all professors that promotion and tenure require excellence in both teaching and research. We recently hired a full-time teaching coach who is assigned to help all faculty members improve the delivery of their content. Finally, we now actively encourage case writing, and we want all professors to be able to teach well across all of our programs.

CLOSING

In closing, there are many, many wonderful people whom I should thank for the honor of being your president, but I will mention only a
few. I want to thank my dean, Jim Jiambalvo, who provided the financial and social support that allowed me to serve the Academy of Management. I want to thank Rick Mowday, who started me on my professional journey while I was a student at the University of Oregon; Gary Latham, who helped me start as a junior faculty member; Terry Mitchell, who keeps the research and scholarly process fun; Anne Tsui and Greg Northcraft, who trained me to be an AMJ editor; Sara Rynes, Dov Eden, Marshall Schminke, and Don Bergh, who made AMJ a most wonderful experience; and Peter Hom and Rodger Griffeth, who are my role models for intellectual discipline. (By the way, Peter Hom and I speak the same obscure dialect of Chinese.) Most of all, I must thank my wife, Janet Thompson, and son, Joe Lee, who keep me grounded and remind me about the richness of my life. Thank you all for allowing me to serve as your president.

REFERENCES


