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The Research-Practice Gap in I/O Psychology and Related Fields: Challenges and Potential Solutions

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Abstract

The gap between science and practice in I/O psychology and related fields is large and, some believe, getting larger. Although not everyone views this as a matter for concern, there is growing momentum to take actions to strengthen the interface between science and practice. This chapter examines three underlying sources of the gap: lack of awareness of what the other side knows and cares about; lack of belief or confidence in the knowledge generated or held by the other side; and lack of implementation of knowledge or ideas, even in the face of awareness and belief. Based on this analysis, proposed solutions are offered for each source of the gap, as are directions for future research and practice.

Key Words: Research-practice gap, I/O psychology, knowledge transfer, jury decision making, evidence-based management, persuasion

Introduction

The more I have tried to be an industrial and organizational (I/O) psychologist, the less I have been able to be of practical value to the organizations with and for whom I have worked. As a "practitioner," I have focused on day-to-day organizational problems and opportunities: starting up new plants, reorganizations, increasing teamwork, selecting and developing managers, improving morale, and so on. The more I have focused on solving these practical organizational problems, the more I have found myself drawn away from the I/O psychology community (Lapointe, 1990, p. 7).

Hundreds of thousands of talented researchers are spending their time producing little or nothing of lasting value. Because the usefulness of their research is so low, their social environment pays little attention to their research. Many researchers lose the idealism that brought them to their occupation originally, as they shift their priorities to social goals such as tenure and promotions. Seeing that their activities are benefiting no one, some researchers come to see themselves as having obligations to no one but themselves, and they engage in egocentric demands (Starbuck, 2006, pp. 3–4).

It has long been recognized that there is a considerable gap between psychology-based management research findings and management practices in organizations (e.g., Campbell, Daft, & Hulin, 1982; Dunnette & Brown, 1968; Johns, 1993). Although this gap is hardly unique to the field of I/O psychology (and related fields such as human resource management [HR], industrial relations [IR], organizational behavior [OB], or organizational development and change [ODC]), it has grown larger over the years, despite a fair amount of discussion about how to narrow it (e.g., Dunnette, 1990; Lawler, Mohrman, Mohrman, Ledford, & Cummings, 1985; Murphy & Saal, 1990). Indeed, the stubborn persistence of the gap has caused some to wonder whether a closer relationship between research and practice is possible at this point (e.g., Hakel, 1994; Kieser & Leiner, 2009; Oviatt & Miller, 1989), or even desirable (e.g., Earley, 1999; Hulin, 2001).

Despite the long-standing nature of the gap, broad economic and sociopolitical factors appear to be increasing the potential benefits of stronger academic-practitioner interactions (Rynes, Bartunek, & Daft, 2001). On the practitioner side, intensified global competition has escalated pressures for organizational innovation and efficiency, which in turn have increased managerial search for and receptivity to new ideas. As such, research showing consistently positive relationships between certain management practices and organization-level outcomes should be of greater value to practitioners than ever before. A considerable amount of such research now exists at both the within-organization (Arthur & Huntley, 2005; Katz, Kochan, & Gobeille, 1983; Wagner, Rubin, & Callahan, 1988) and between-organization levels (e.g., Arthur, 1994; Gerhart & Milkovich, 1990; Huselid, 1995; Welbourne & Andrews, 1996) Moreover, many of the practices associated with more positive outcomes represent "win-wins" for both managers and employees (e.g., Fulmer, Gerhart, & Scott, 2006; Katz et al. 1983; Orlitzky, Schmidt, & Rynes, 2003; Welbourne & Andrews, 1996). Although the direction of the causal relationships involved is not always clear (Schneider, Hanges, Smith, & Salvaggio, 2003; Wright & Haggerty, 2005), at least some longitudinal studies have shown that high-involvement or high-investment HR management practices are associated with subsequent organizational performance (e.g., Baum, Locke, & Kirkpatrick, 1998; Huselid, 1995; Welbourne & Andrews, 1996; Van Iddekinge et al., 2009).

There are other reasons for practitioners to develop stronger relationships with academics as well. For example, the Supreme Court's 1993 decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.* has increased the importance to employers of being able to empirically defend the validity of their employment practices (Faigman & Monahan, 2005). In addition, U.S. public policy has changed in ways that encourage cooperation between business and academia, such as the provision of tax breaks for corporate funding of university research and the development of government programs that require industry-university collaboration as a condition of funding (Cohen, Florida, & Goe, 1994). Finally, Ashford (in Walsh, Tushman, Kimberly, Starbuck, & Ashford, 2007) notes that, although organizations have a variety of other options for seeking knowledge or advice, there is "no other institution in society that is so purely aimed at the pursuit of truth and the production of knowledge than the university" (p. 149).

On the academic side, collaboration with practitioners on problems of mutual interest can provide researchers with access to better data sets (larger samples, higher response rates, better measures, fewer errors) that avoid some of the most persistent methodological problems in I/O and related areas of research (Amabile et al., 2001; Edwards, 2008; Starbuck, 2006). Helping practicing managers to deal with problems can be both intellectually stimulating and intrinsically motivating, and can surface questions of theoretical as well as practical importance (e.g., Campbell et al., 1982; Kelemen & Bansal, 2002; Latham, 2007a; Tushman & O'Reilly, 2007; Van de Ven, 2007).

Furthermore, collaboration between academics and practitioners often results in very high-quality research, particularly when both sides have a difficult puzzle that they want to solve (Daft, Griffin, & Yates, 1987; Hodgkinson & Rousseau, 2009). For example, since 2003, all but two of the *Academy of Management Journal*'s (*AMJ*'s) "best paper" awards have gone to intensive theory-building studies conducted in a small number of organizations (e.g., six TV networks and film studios, eight newspapers, eight medical providers, five accounting firms, and one church). As another example, Starbuck (in Walsh et al., 2007) tells how the Profit Impact of Market Strategy (PIMS) database was created by the Marketing Science Institute, a collaboration between 60 firms and a broad base of academic researchers. According to Starbuck, "Its influence in the field of marketing has been remarkable. During the 1990s, projects that MSI sponsored won every award for outstanding research in marketing, and they comprised 60% of the articles in *Journal of Marketing* and *Journal of Marketing Research*" (p. 146). Additionally, Rynes, McNatt, and Bretz (1999) found that researchers who spent more time on-site in projects with organizations were cited more often than those who spent less time in the organizations they studied. Many of these researchers appeared to employ what Craig Russell (personal communication) calls "guerilla research: solving firms' problems while simultaneously weaving in measures and interventions that advance a research agenda, with the firm's full knowledge and support."

Beyond the potential for individual researchers to create high-quality research through collaboration with practitioners, the universities in which academics are employed are also engaging more directly with private-sector organizations than ever before. To a considerable extent, this is because public funding for universities has been decreasing (both proportionately and in real terms) since the 1970s, leaving universities—especially

state universities—increasingly dependent on the private sector for monetary and other resources (e.g., Cohen, Florida, Randazzese, & Walsh, 1998; Slaughter & Leslie, 1997). While collaborations with private-sector organizations provide universities with valuable resources for research and teaching, the corporations and other entities that donate funds expect universities to produce not only basic research and theory-based education, but also applied research and skill-based education in return for their support.

Although research is still undoubtedly more highly rewarded than teaching or service, and basic research more prestigious than applied, the pendulum appears to be swinging at least somewhat in the opposite direction in some universities (Cascio, 2008; Latham, 2007a; Walsh et al., 2007). Changes in resource dependence are increasing the value of academics who develop bridges between research, teaching, and practice (Ashford in Walsh et al., 2007). Although this development has been more pronounced in business schools than psychology departments (due in large part to media rankings of business school programs that focus heavily on student evaluations of teaching and the quality of student placements; e.g., Corley & Gioia, 2002), broad trends in the funding of research and higher education suggest that psychology and other social sciences will increasingly be subject to similar pressures (Slaughter & Leslie, 1997). In this regard, it is somewhat worrisome that a number of well-known researchers have remarked on the declining influence of psychology and micro-organizational behavior in both management and public policy research and practice (e.g., Blood, 1994; Cascio & Aguinis, 2008; Ferraro, Pfeffer, & Sutton, 2005a; Hakel, 1994; O'Reilly, 1990; Miner, in Schwarz, Clegg, Cummings, Donaldson, & Miner, 2007). Other researchers have shown that psychology-based research is becoming increasingly isolated from research in management and economics, which are increasing rather than decreasing in both research and policy influence (Agarwal & Hoetker, 2007).

Finally, for whatever combination of reasons, I/O and related researchers—as well as professional organizations such as the Society for Industrial and Organizational Psychology (SIOP) and the Society for Human Resource Management (SHRM)—have shown increased interest in attempting to narrow the research-practice gap. For example, we have seen:

• An increase in the number of special forums on this topic in journals such as *AMJ* (e.g., Shapiro, Kirkman, & Courtney, 2007), *Human Resource Management* (*HRM*; Burke,

Drasgow, & Edwards, 2004), *Journal of Occupational and Organizational Psychology* (Gelade, 2006), *Journal of Management Inquiry* (Walsh et al., 2007), *Journal of Organizational Behavior* (Greenberg, 2008), *Journal of Management Studies* (Kieser & Leiner, 2009), and *Academy of Management Learning and Education* (Adler & Harzing, 2009);

• An increase in the number of academic colleges, departments, centers, or even individual professors who provide web synopses or "translations" of research for practitioners (e.g., Cornell's "CAHRS' Top Ten," "Knowledge @ Wharton," or Craig Russell's explanations of the Cleary model of test bias and the Brogden-Cronbach-Gleser model of utility; http://www.ou.edu/russell/whitepapers/);

• Emergence of awards for research with important implications for practice (e.g., SIOP's M. Scott Myers Award for Applied Research in the Workplace and the Academy of Management's OB Division's Outstanding Practitioner-Oriented Publication award) and for researchers who have contributed to practice throughout their careers (e.g., the Academy of Management's [AOM's] Scholar-Practitioner Award and SHRM's $50,000 Michael R. Losey award);

• Ongoing efforts to build an Evidence-Based Management Collaborative database (www.cebma.org/);

• An emerging empirical base documenting the extent and content of science-practice gaps in I/O and related fields (e.g., Cascio & Aguinis, 2008; Deadrick & Gibson, 2007; Rynes, Colbert, & Brown, 2002; Rynes, Giluk, & Brown, 2007; Silzer & Cober, 2008);

• Increased government support for industry-university research collaborations both in North America and Europe (e.g., the UK's Advanced Institute of Management [AIM] research collaborative; see Hodgkinson & Rousseau, 2009).

Although it is unclear whether these developments represent long-term trends or just transitory fads, for the purposes of this chapter I assume that interest in greater collaboration will at least persist, and perhaps escalate. If so, understanding as much as we can about the nature of the current gap and its origins is essential to ensuring future progress in narrowing it.

Scope and Organization
One issue affecting scope concerns disciplinary boundaries. Although the central disciplinary focus

of this review is I/O psychology, it also includes evidence from the fields of HR, IR, OB, and ODC. This is because: (a) all these fields involve the management, motivation, and development of employees; and (b) research suggests that it is becoming increasingly difficult to clearly differentiate among them (Ruona & Gibson, 2004).

For example, in a large empirical study of the competencies associated with effective HR managers, Ulrich, Brockbank, Yeung, and Lake (1995) found that the ability to manage change (historically considered an ODC function) was more important to HR managers' evaluated effectiveness than the ability to deliver traditional HR practices and services. Similarly, in a historical review of the areas of HR, ODC, and human resource development (HRD), Ruona and Gibson (2004) found all three fields converging around four trends: (1) the increased centrality of people to organizational success; (2) increased focus on whole systems and integrated solutions; (3) emphasis on strategic alignment and impact; and (4) increased importance of the capacity for change and three major competencies: (a) mastery of technical basics, (b) knowledge of business and strategy, and (c) facilitation of organizational change and agility. Finally, a recent survey of SIOP members revealed that I/O practitioners would like SIOP to promote I/O practice and research to the larger business community and to place more I/O psychology articles in HR and general business publications (Silzer & Cober, 2008). Hence, a somewhat multidisciplinary approach seems most appropriate.

A second issue concerns the types of "practitioners" addressed. For purposes of this study, I define practitioners as "those whose decisions or recommendations in organizational contexts impact organizational stakeholders either directly, or indirectly through their influence on HR systems." This definition is a bit narrower than that used by Gelade (2006, p. 154) and Cascio and Aguinis (2008, p. 1062)—"those who make recommendations about the management or development of people in organizational settings, or who advise those who do"—in that it narrows the definition to having actual impact and accountability, as opposed to simply giving advice (as, for example, academics sometimes do). Despite this narrowed definition, however, the educational and experiential backgrounds of practitioners referred to in this chapter are still quite varied (e.g., sometimes they are doctorate-level I/O psychologists, while other times they are HR managers with a variety of educational degrees). Because

of this wide variation in practitioners, care will be taken to specify (wherever possible) the precise types of practitioners being discussed (e.g., whether survey respondents are I/O psychologists or HR managers).

The chapter is organized around two main themes: (a) the nature and sources of the research-practice gap, and (b) potential solutions for narrowing it. Each of these themes is then subdivided into three sections, each of which pertains to one source or cause of the gap: (a) lack of awareness of issues and knowledge on the other side (e.g., practitioners may not be aware of scientific findings, and researchers may not be aware of practitioner needs and challenges); (b) differences in beliefs (e.g., practitioners may hear of a research finding but reject it; researchers may be aware of an emerging practice but dismiss it out of hand); or (c) failures to implement a practice or to change behavior (e.g., practitioners may know about and believe that an alternative practice would be superior but fail to implement it anyway; researchers may believe that collaborative research ventures would produce more important work but fail to collaborate anyway). I first discuss evidence related to the nature and extent of each of these three components of the gap and then, based on those analyses, offer suggestions designed to ameliorate them.

Nature and Sources of the Gap
Gaps in Awareness
THE PRACTICE SIDE
Nature of the Gap

There is considerable evidence to suggest that the biggest discrepancies between research and practice in I/O psychology, HR, and related fields exist in the area of employee selection. One of the most widely documented and persistent of these gaps involves practitioner preferences for using intuitive methods of selection, particularly unstandardized employment interviews, over standardized predictors and/or mechanical (i.e., empirically derived and consistently weighted) combinations of selection techniques.[1] Highhouse (2008) reviewed multiple studies showing that mechanical or statistical predictions of employee behavior are superior to both intuitive methods (such as the unstructured interview) and combinations of mechanical *plus* intuitive methods (e.g., Dawes, 1971; Dawes, Faust, & Meehl, 1989; Grove, Zald, Lebow, Snitz, & Nelson, 2000). Nevertheless, the unstructured interview has been (and continues to be) the most

popular and widely used selection procedure over the past 100 years (Buckley, Norris, & Wiese, 2000). Additionally, written tests continue to be perceived by managers as inferior to interviews for evaluating both personality and intelligence (e.g., Lievens, Highhouse, & DeCorte, 2005).

A second well-documented gap concerns practitioners' tendencies not to believe that general mental ability (GMA) or general intelligence (*g*) is the single best predictor of employee performance (e.g., Schmidt & Hunter, 1998). For example, Rynes et al. (2002) found that, contrary to research evidence (see the original study for details), the vast majority of HR managers in their sample believed that both conscientiousness and values are better predictors of performance than intelligence. In addition, approximately half of the respondents believed that intelligence is a disadvantage for performance on low-skilled jobs, while in reality there is a positive validity coefficient (ρ = .23) for GMA in predicting performance on completely unskilled jobs (this coefficient rises to .58 for high-level managerial and professional jobs; Schmidt & Hunter, 1998). Finally, in a policy-capturing study of résumé-based prescreening decisions by campus recruiters, McKinney, Carlson, Mecham, D'Angelo, and Connerley (2003) found that 42% of recruiters ignored grade point average (a proxy for GMA) in deciding whom to interview, while 15% actually selected *against* applicants with high GPAs.

Another selection-related gap pertains to the usefulness of personality testing for selection purposes. As with GMA, Rynes et al. (2002) found that practitioners' beliefs about personality testing were generally more negative than is warranted by relevant research findings. For example, most HR managers in their study tended to believe that integrity tests are not valid predictors of performance and that such tests are likely to produce adverse impact against minorities. However, meta-analytic results have shown that the estimated mean operational validity of integrity tests for predicting supervisory ratings of job performance is ρ = .41 (Ones, Viswesvaran, & Schmidt, 1993) and that minority groups are not adversely affected by either overt integrity tests or disguised-purpose, personality-oriented measures of integrity (Sackett, Burris, & Callahan, 1989).

One other notable gap concerns the relative effectiveness of employee participation versus goal setting for improving performance. Specifically, most of Rynes et al.'s (2002) respondents believed that participation in decision making is more effective than goal setting for improving performance,

whereas research shows the opposite to be true (e.g., Latham, Erez, & Locke, 1988; Locke, Feren, McCaleb, Shaw, & Denny, 1980). For example, Locke et al.'s (1980) meta-analytic findings (based entirely on field research) showed that goal setting produced a 20% improvement in performance on average, while empowerment produced less than 1% on average, and with very high variability. Given the weak average results for participation and empowerment (see also Argyris, 1998; Highhouse, 2007; Wagner, 1994) and the apparent existence of large moderator effects (Wagner, 2009 versus the strong results for goal setting, this gap is also important.

Sources of the Gap

If all practitioners had advanced degrees in I/O psychology or HR, there might not be much of a problem with lack of awareness of research findings. However, this is far from the current situation. Unlike law or medicine, neither general management nor HR management are true professions (Leicht & Fennell, 2001; Trank & Rynes, 2003), although I/O psychology is a different matter. For example, there are no requirements that managers be exposed to scientific knowledge about management, pass examinations in order to become licensed to practice, or pursue continuing education in order to maintain employment. Even in HR, where a certification exam exists and is advocated by the largest professional association (SHRM), certification is not always used by employers in hiring HR practitioners (Aguinis, Michaelis, & Jones, 2005). As a result, elevating the qualifications of individuals placed in HR positions has been a long-standing challenge (Hammonds, 2005; Rynes, Owen, & Trank, 2008).

Second, even if managers or HR managers pursue formal education in the fields of business or management, they are likely to confront a curriculum that is heavily weighted toward mathematically based courses (e.g., finance, economics, accounting, operations) rather than behavioral ones. For example, Navarro (2008) found that while more than 90% of the top 50 MBA programs require courses in marketing, finance, financial accounting, operations, strategy, managerial economics, and quantitative analysis/statistics, only 56% require courses in OB, 36% in general management, and 28% in HR. Moreover, those schools that are most likely to focus on behavioral topics (often referred to as "soft skills" in program descriptions) often do so via case, discussion, and role-playing pedagogies that do not emphasize research results (e.g., Harvard's case

study method; see Ellet, 2007). Moreover, recent research by Stambaugh and Trank (2009) suggests that new research findings—particularly those that deviate from currently dominant paradigms—are not readily integrated into textbooks, while largely discredited theories (e.g., Herzberg's two-factor theory or Maslow's hierarchy of needs) continue to be included. Putting these factors together, it seems likely that fewer than half of graduating MBAs have had much if any exposure to the most important I/O findings—at least presented explicitly as "research" findings meriting special consideration among all the other information and opinions provided in textbooks, cases, and classroom discussions.

Third, once students graduate from MBA (or I/O, IR, or HR programs), they are even less likely to be exposed to research findings. For example, Rynes and colleagues (2002) found that less than 1% of HR managers at the manager, director, or VP levels usually read *Journal of Applied Psychology* (*JAP*), *Personnel Psychology* (*PPsych*), or *AMJ*, and most (75%) never read any of the three. Rather, these managers most commonly look to other HR practitioners in their own organizations for help in solving HR problems, and least commonly (of seven possible sources of help) to academics. Another study by Offermann and Spiros (2001) surveyed members of the Academy of Management (66% with Ph.D.s and 29% with master's degrees) who spent either all or part of their time in ODC or team development. Even among this highly educated group, only a minority reported that they read *AMJ* (32.5%) or *JAP* (12.6%) to keep up with recent developments in OD or teams.

Finally, the publications that practitioners do read once they leave college or graduate school sometimes do little to report on recent research findings. For example, Rynes et al. (2007) examined *HRM*, *HR Magazine*, and *Harvard Business Review* (*HBR*) with respect to three topics revealed by the Rynes et al. (2002) study to exhibit large gaps between research findings and practitioner beliefs: the importance of intelligence to job performance, the relationship between personality characteristics and job performance, and the relative effectiveness of goal setting versus participation in improving performance. The authors examined both: (a) how much coverage each of these topics received in each journal, as well as (b) the accuracy of coverage in *HBR, HRM* and *HR Magazine* when evaluated against research findings (i.e., how consistent the information in each periodical was with established research findings).

With respect to the first question, coverage of these three topics was almost nonexistent in the three periodicals. Specifically, as a percentage of total articles published in each outlet, intelligence received 1.2% coverage in *HRM*, 0.4% in *HBR*, and no coverage in *HR Magazine* between 2000 and 2005. Personality received 1.2% coverage in *HRM*, 0.6% in *HBR*, and 0.4% in *HR Magazine* over that same time period. Goal setting received 0.6% coverage in *HRM*, 0.6% in *HBR*, and 0.6% in *HR Magazine*. With respect to the second question (research consistency of coverage), results suggested that *HRM*'s coverage was research consistent, while *HR Magazine*'s and *HBR*'s was mixed. For example, *HBR* published two articles on intelligence during the relevant period. One of these was research-consistent (Menkes, 2005), while the other (called "Deep Smarts") confounded the construct of intelligence with specific job experience (Leonard & Swap, 2004).

In summary, research suggests that practitioners are often unaware of basic research findings, particularly in the areas of selection and performance improvement (e.g., goal setting versus employee participation). Practitioners may be unaware of research findings because: (a) they may not have received formal education in psychology, management, HR management, or related fields; (b) even if they received formal education, they may have received little exposure to research findings; (c) once they graduate and become practitioners, they are not likely to read the research literature; and (d) the periodicals that they are most likely to read do little to disseminate research findings. For any or all of these reasons, there may be many practitioners who rarely think of research findings as a potential aid in solving their problems.

THE ACADEMIC SIDE
Nature of the Gap

Although there have been multiple studies about what practitioners don't know about research findings and what they do or don't read, there do not appear to be analogous studies of academic awareness of practitioner issues or concerns. For example, I was unable to find any studies that have directly asked academic researchers such things as whether *they* read practitioner journals, or whether they have accurate knowledge of the major issues and environmental conditions confronting I/O or HR practitioners. This void may exist for several reasons. For example, researchers may simply not think these issues are important to investigate or, even if they

do, they may perceive a lack of interest among other academics–and hence, difficulty in getting such research published).

Nevertheless, researchers have attempted to address the issue of gaps on the academic side at least indirectly, by studying how the content of academic journals differs from the content of publications aimed at practitioners. For example, Deadrick and Gibson (2007) examined the primary content areas of more than 4,300 non-methods-focused articles published in four HR-I/O focused journals (*HRM, HR Magazine*, *JAP*, and *PPsych*) over a 20-year period (1986–2005). The first two journals were characterized as being primarily aimed at practitioner audiences (although *HRM* is probably more accurately described as a "bridge" publication), and the latter two primarily at academics.

Deadrick and Gibson (2007) found that there was a consistently large gap in coverage between academic and practitioner journals with respect to compensation and benefits issues. Specifically, articles on compensation and benefits comprised 14.3% of the content of practitioner journals, but only 2.0% of academic ones. It should be noted, however, that the compensation articles in practitioner outlets focused mostly on compliance-based factual information (e.g., Fair Labor Standards Act, Family and Medical Leave Act, exempt versus non-exempt status, overtime pay)—topics that are more likely to be illuminated by legal advisers than academic researchers. Other areas of apparently greater interest to practitioners than academics were (in order of gap size): HR department issues (11.5% versus 1.7%), strategic HR (9.5% versus 2.8%), technology (8.0% versus 2.6%), and international or global HR (6.0% versus 2.2%).

In a second study, Cascio and Aguinis (2008) compared the content of articles appearing in *JAP* from 1963 to 2007 (broken into five-year periods) with a compilation of the most important human resource trends (as identified by literature reviews) over those same periods. Based on their analysis, Cascio and Aguinis (2008) concluded that academic I/O research is not meeting practitioners' interest levels in a variety of areas, such as recruitment, HR effectiveness at the organization level, and the implications of changing worker demographics for managers, employees, and their organizations. Extrapolating from past research into the next ten years, they predict that "I/O psychology will not be out front in influencing the debate on issues that are (or will be) of broad organizational and societal appeal. It will not produce a substantial body of research that will inform HR practitioners, senior managers, or outside stakeholders, such as funding agencies, public-policy makers, or university administrators who control budgets" (p. 1024).

Using a somewhat different methodology, a study by Heath and Sitkin (2001) compared the "actual" coverage of organizational topics (as measured by keyword prevalence in *Journal of Organizational Behavior* [*JOB*], *AMJ*, *JAP*, *Organizational Behavior and Human Decision Processes* [*OBHDP*], and *Organization Science* [*OS*]) with "ideal" levels of coverage (as measured by beliefs of the editorial board members of *JOB*).[2] They found that there was *less* research than believed desirable in many areas (norms, communication, organizational change, performance, family, risk, cross-cultural, trust, interdependence, cooperation, and learning), and *more* coverage than desirable in four others (job satisfaction, decision making, organizational citizenship, and goal setting).

In trying to make sense of these results, Heath and Sitkin (2001) concluded that the areas where more research was desired were ones that are "most central to the task of organizing" (p. 54). As a result, they suggested that in order to have greater impact, researchers might think more in terms of studying "organizing" behavior rather than "organizational" behavior:

> Under this definition, researchers would devote relatively more attention to topics that help us understand how groups of people organize and carry out their goals. As we view the results of the survey, a number of topics toward the top of the list fall under (this) definition because they help us understand the task of organizing. For example, if we understood more about *social norms,* we would understand more about how groups of people implicitly coordinate their action when they face a complex environment. We would also understand more about organizing if we understood how organizations can facilitate effective *communication* across divisions and hierarchical levels.…
> (Heath & Sitkin, 2001, p. 54)

A fourth relevant study was performed by Silzer and Cober (2008). They conducted a recent survey of 1,005 SIOP members with varying interests in practice (61% full-time practitioners, 10% part-time practitioners, 19% occasional practitioners, and 10% non-practitioners). One question they asked respondents was whether science, or practice, was "leading" knowledge in a number of areas. Overall, 17 of 26 topics were identified as mainly

practitioner-led, including consulting and advising, employment branding, HR technology, executive/management coaching, strategic planning, succession/workforce planning, talent management, labor relations, HR general practices, compensation, employee relations, employee recruitment, organizational development, litigation, and leadership and management development. On the other hand, researchers were seen as leading in the areas of measurement and statistics, job and work analysis, selection and staffing, cross-cultural issues, and individual assessment and assessment centers. Interestingly, there were few notable differences between full-time practitioners and full-time academics in their perceptions, with two exceptions being in the areas of recruitment and leadership/management development.

While the preceding articles have addressed differences in academic and practitioner coverage in I/O psychology, HR, and OB, Austin and Bartunek (2003) looked at differences between research and practice journals in organization development and change (ODC). They found that research in ODC academic journals focused almost exclusively on *change* processes (*how* organizations change; for an excellent review, see Van de Ven & Poole, 1995), while practitioner journals focused primarily on *implementation* processes (*how to* change organizations). Moreover, they found that the more academically oriented publications showed virtually no awareness of prominent ODC implementation technologies, such as appreciative inquiry (Cooperrider & Srivastva, 1987), large-group interventions (Lindaman & Lippitt, 1980), and learning organizations (e.g., Argyris & Schön, 1978; Senge, 1990). The authors highlighted appreciative inquiry (AI) as an area where the gap was particularly notable: "The academic silence and practitioner enthusiasm about AI illustrates the significance of the practitioner/academic theoretical divide. . . . AI challenges several assumptions of previous research on resistance to change. . . . Academic theorizing about change would benefit from more attention to the questions raised by AI practitioners" (Austin & Bartunek, 2003, p. 323; see also Yaeger, Sorensen, & Bengtsson, 2005).

Finally, Shapiro and colleagues (2007) directly assessed whether a sample of 548 members of the AOM (438 academics, 39 business people, 40 consultants, and 31 unidentified) believe there is a gap: (a) in translation of research findings to practitioners (which they call "lost *in* translation"); and/or (b) even before research is translated (which they

call "lost *before* translation"), as when academics ask questions of little interest to practitioners. Results showed that AOM members perceive both types of gaps although, consistent with the predominance of academic respondents, a larger gap was perceived "in" translation ($\times$ = 3.98 on a five-point scale) than "before" it ($\times$ = 3.65).

Academic Sources of the Gap

Although the scientist-practitioner model[3] has been an important ideal in the history of both I/O psychology (e.g., Dunnette, 1990; Fleishman, 1990) and business schools (e.g., McGrath, 2007), at present most I/O psychologists and management specialists are either one (e.g., scientist) or the other (practitioner). In the face of limited contact between the two groups, the communities have developed very different assumptions, values, goals, interests, and norms (e.g., Beyer & Trice, 1982; Boehm, 1980; Daft & Lewin, 2008; Kieser & Leiner, 2009; Shrivastava & Mitroff, 1984). These differences have tended to reduce the perceived (and almost certainly the real) relevance of academic research to practitioners.

Boehm (1980) argued that one major source of the gap is the difference between the traditional scientific method (emphasizing theoretical problems, formal hypotheses, statistical controls, isolation of phenomena of interest, and search for facts or the "truth") and the messy reality of organizational settings (with multiple processes, stages, and interactions among them, and emphasis on solving real problems). She goes on to say that "the reaction of behavioral scientists, when faced by the realities of the organizational research environment, has been either to attempt modification of the environment to fit the traditional mode of inquiry or else to opt out of the scientific establishment" (Boehm, 1980, p. 498). The results of this divergence are at least fivefold: (1) the failure of "real world" research to disseminate to academics; (2) the failure of academic research to spread to practice (or unanticipated outcomes when it does); (3) limitation of the types of questions asked by academics (to those that come closest to meeting the academic model); (4) a tendency to overlook the positive features of conducting research in organizational settings (e.g., a more accurate model of the world, valuable cross-fertilization between lab and field studies, emphasis on "what works?" more than "why?"); and (5) a variety of in-group/out-group attitudes, stereotypes, and behaviors (see also Empson, 2007; Gulati, 2007, and Vermeulen, 2007).

In addition to differences in norms, goals, values, and research models, academics and practitioners also have different incentive systems. For example, while practitioners often have *dis*incentives to publish their research (e.g., proprietary data, competitive advantage, and legal vulnerabilities; Boehm, 1980), academics generally have more incentives to publish than they do to teach well or perform various types of service (Mowday, 1997). Gomez-Mejia and Balkin (1992) showed that the largest predictor of academic salaries in management (controlling for a variety of personal characteristics) was the number of top-tier publications, followed by the number of moves from one university to another. Such moves are also facilitated primarily by publication records, which are more visible (and more valued) than service or teaching accomplishments.

Not only are the strongest academic incentives to publish, but also to publish in a relatively narrow set of journals with high "impact factors" or citation rates (see Adler & Harzing, 2009; Judge, Cable, Colbert, & Rynes, 2007, and Starbuck, 2006). Critics argue that the growing importance of metrics such as the number of top-tier publications, number of citations, journal impact factors, and the various academic rankings based upon these measures is distorting research away from its original goals and making it less helpful in solving organizational and social problems. For example, Lawrence (2008), cited in Adler & Harzing (2009) says:

> Measurement of scientific productivity is difficult. The measures used…are crude. But these measures are now so universally adopted that they determine most things that matter (to scholars): tenure or unemployment, a postdoctoral grant or none, success or failure. As a result, scientists have been forced to downgrade their primary aim from making discoveries to publishing as many papers as possible—and trying to work them into high impact-factor journals. Consequently, scientific behavior has become distorted and the utility, quality, and objectivity of articles have deteriorated. Changes…are urgently needed. (p. 1)

Several characteristics of top-tier journals appear to make their research less accessible and relevant to practitioners than research in "lower tier" ones. One such characteristic is the tendency of top-tier journals to uphold the traditional scientific model more religiously than do other journals (e.g., Boehm, 1980; Daft & Lewin, 1990; Locke, 2007). As a case in point, one of the main reasons for the founding of *OS* in 1990 was to "break out of the normal science straitjacket" (Daft & Lewin, 1990, p. 1). However, 18 years later, Daft and Lewin (2008) concluded that this part of the original mission—to be an immediate source of knowledge for practical managerial applications—"was unrealistic and has not been realized" because "as a journal evolves over time, its focus systematically narrows to reflect the orthodoxies of the community of scholars that emerges around it" (p. 178). In the case of *OS*, the "orthodoxies of the community of scholars" served to widen rather than narrow the research-practice gap. (Of course, *OS* is hardly unique in this regard; see Cascio & Aguinis, 2008).

Another characteristic of top-tier journals that decreases their accessibility and relevance is the growing emphasis on theoretical contribution as a publication requirement (e.g., Colquitt & Zapata-Phelan, 2007; Daft & Lewin, 2008; Sutton & Staw, 1995). Although the benefits of good theory are taken for granted by many academics (although they might disagree about what constitutes "good" theory; e.g., Russell, 2009, a number of prominent researchers believe that the current emphasis on theory has gone too far. For example, Hambrick (2007) argues:

> A blanket insistence on theory, or the requirement of an articulation of theory in everything we write, actually retards our ability to achieve our end, (which is) understanding. Our field's theory fetish, for instance, prevents the reporting of rich detail about interesting phenomena for which no theory yet exists. And it bans the reporting of facts—no matter how important or competently generated—that lack explanation but that, once reported, might stimulate the search for an explanation. (p. 1346)

The growing incentive for academics to publish in top-tier journals is perceived to be a very important, if not the *most* important, reason that research has become inaccessible to practitioners and largely ignored by them (Adler & Harzing, 2009; Shapiro et al., 2007; Starbuck, 2006). However, there is the further problem that even when practitioners (and sometimes academics) become aware of research findings, they do not always believe them. I turn now to this issue.

Gaps in Beliefs

Studies such as Rynes et al. (2002) cannot definitively determine whether research-inconsistent beliefs are a result of (1) lack of awareness or (2) disbelief of

research findings. Nevertheless, Rynes et al. inferred that the gaps were probably due to lack of awareness, based on circumstantial evidence regarding the reading habits of HR practitioners and the publishing patterns of the major practitioner and bridge journals in the field (i.e., sparse coverage of research findings related to intelligence, personality, and goal setting by practitioner and bridge journals). Of course, the preceding section further suggests that practitioner lack of awareness is also due to the publishing practices of top-tier academic journals, which often make their articles unwelcoming and uninteresting to practitioners.

However, even if a gap is due to lack of awareness, it cannot automatically be assumed that practitioners would actually change their beliefs after being exposed to relevant research. In this section, I first review the (somewhat limited) research on this topic in three areas—utility analysis, predictors of employee performance (i.e., selection), and jury reactions to expert testimony. I then present some of the most difficult challenges to overcoming practitioners' disbelief, including the vexing problem that researchers often do not agree among themselves on many issues.

PRACTITIONER BELIEFS ABOUT RESEARCH FINDINGS
Utility Analysis

In the 1990s, two studies were conducted by Gary Latham and Glen Whyte to assess how practitioners react to utility analysis, a research-based tool that attempts to convert the effect sizes of various HR interventions into financial terms (e.g., Boudreau, 1983; Schmidt & Hunter, 1983). In their first study, Latham and Whyte (1994) had 143 enrollees in an executive MBA program respond to one of four experimental stimuli. Participants were presented with a scenario about a company that was having trouble hiring high-quality clerical workers. The company had hired a psychologist to investigate the issue, and had recommended implementing systematic selection practices. Participants received one of four "systematic practices" to evaluate:

1. Standard validation: the psychologist creates a test tailored to the organization's clerical jobs, validates it against the performance of the organization's current clerical workers, and then uses the results to modify the company's existing practices;

2. Standard validation plus expectancy chart: the psychologist applies the procedures described

in "1" above, but also uses an expectancy chart to show the performance improvements attained by another client who used a similar procedure (the amount of improvement suggested by the expectancy chart was not specified in the article);

3. Standard validation plus utility analysis: the psychologist uses the procedures in "1" above, but also explains utility analysis and gives an estimate of the financial gain achievable ($60,208,786) if her advice is followed;

4. All three: The psychologist uses a combination of standard validation, expectancy chart, and utility analysis/financial projection.

The dependent variable was an eight-item scale addressing such participant reactions as commitment to implementing the psychologist's results, confidence in the psychologist's solution, and ability to justify their decision to others about whether or not to accept the psychologist's advice.

Results showed the most favorable reactions to condition "1" and the least favorable to "3." In other words, adding utility analysis to typical validation procedures produced the *least* convincing scenario. In seeking to explain these results, Latham and Whyte (1994) speculated that their results might have occurred either because "managers are suspicious of behavioral consultants who claim to be able to accurately estimate the dollar value of their recommendations" or because of "the large size of the gain typically estimated by utility analysis, which may strain the psychologist's credibility" (p. 42). They also raise the possibility that managers "(may not actually) want sophisticated and systematically collected information regarding their human resources in order to improve their business decisions" (p. 32), citing Mintzberg's (1975) observation that managers rely much less heavily on rational analysis than utility analysis assumes.

In a second study of 41 executive MBA students, Whyte and Latham (1997) used the same basic scenario "1," but added two different conditions. In condition "2," participants received scenario "1" plus written support of standard validation practices from a hypothetical trusted advisor. In condition "3," participants received both "1" and "2" above, but also a written explanation of utility analysis, an actual utility analysis showing large financial benefits from selection validation, a videotaped presentation from an expert on utility analysis (Steven Cronshaw) explaining the underlying logic and benefits of utility analysis, and a live appearance by Cronshaw to answer managers' questions. Similar to

the first study, conditions "1" and "2" produced the most favorable (and nearly identical) reactions, and condition "3" the least favorable (by far). Of the 10 items on the "reactions" scale, the three showing the largest differences were commitment to implementation, ability to justify the decision to others, and importance of financial consequences in their evaluations.

Based on these results, Whyte and Latham (1997) concluded that managers do not perceive utility analysis to be a useful tool for HR decision making, and advised I/O psychologists to "reconsider their assumptions regarding the information managers value when making HR policy decisions" (p. 608). They also noted that their results were consistent with Johns's (1993) contention that the adoption of I/O practices is not strongly influenced by technical merit.

In a somewhat unusual (and amusing) addendum to Whyte and Latham (1997), Cronshaw (1997) gave his own view as to why condition "3" was the least effective. He cited Eagly and Chaiken's (1993) theory of psychological reactance, which suggests that positive attitudes toward an attitudinal object can "boomerang" if people "perceive that their freedom to adopt or keep an attitudinal position is threatened by the coercive pressure of high-pressure persuasion" (Cronshaw, 1997, p. 613). Although Cronshaw may be correct in the assumption that his presence was perceived as coercive, this explanation cannot account for the fact that utility analysis did similarly poorly in Study 1, where there was no videotape and he was not present.

PREDICTORS OF EMPLOYEE PERFORMANCE (I.E., SELECTION)

> "Perhaps the greatest technological achievement in I/O psychology over the last 100 years is the development of decision aids (e.g., paper-and-pencil tests, structured interviews, mechanical combination of predictors) that substantially reduce error in the prediction of employee performance. Arguably the greatest failure of I/O psychology has been the inability to convince employers to use them" (Highhouse, 2008, p. 333).

Although practitioners' lack of enthusiasm for actuarial prediction methods might be due to lack of awareness, Highhouse (2008) argues that the more likely source is failure to be convinced: "Although one might argue that these data merely reflect a lack of knowledge about effective practice, there is considerable evidence that employers simply

do not believe that the research is relevant to their own situation" (p. 333). In attempting to explain this phenomenon, Highhouse focused on two widespread beliefs that reduce enthusiasm for scientific advances in selection.

The first is the common belief that it is possible to achieve near-perfect precision in hiring decisions. Given this belief, people tend not to view selection as a probabilistic process with a low validity ceiling. Thus, validity coefficients that sound good to researchers (such as .5) fail to impress practitioners, particularly when they hear that this translates to "only" 25% of variance explained. Moreover, those who wish to dispute the importance of GMA, particularly proponents of emotional intelligence (EI), have made heavy use of the "variance explained" construct to discredit GMA testing. For example, in *Working with Emotional Intelligence*, Goleman (2000) contends that "IQ alone explains surprisingly little of achievement at work or in life. When IQ test scores are correlated with how well people perform in their careers, the highest estimate of how much difference IQ accounts for is about 25%. A careful analysis, though, suggests a more accurate figure may be no higher than 10%, and perhaps as low as 4%. This means that IQ alone at best leaves 75% of job success unexplained, and at worst 96%—in other words, it does not predict who succeeds and who fails" (p. 19). What, according to Goleman, accounts for the rest? Emotional intelligence!

The second underlying belief is the notion that one can become an expert at predicting human behavior merely through experience. This belief, which has received support (and widespread readership) in the popular press book *Blink: The Power of Thinking Without Thinking* (Gladwell, 2005), leads to an overreliance on intuition and overconfidence in one's judgments (see also Hakel, 1982; Ayres, 2008). Unfortunately, the finding that actuarial methods outperform clinical ones in a wide variety of settings is well documented (e.g., Grove & Meehl, 1996), as is the fact that adding clinical judgments to actuarial ones does not improve things, or makes them worse (Ayres, 2008; Highhouse, 2008).

In addition to the "actuarial versus clinical" gap discussed by Highhouse (2008) (and evident in other fields such as law and medicine as well; Ayres, 2008), Rynes et al. (2002) surfaced a gap between HR practitioners' beliefs and research findings about the importance of GMA to performance. However, their methodology did not permit them to tell whether practitioners were unaware of, or

simply disbelieved, research on the (relative) predictive efficacy of GMA.

To remedy this deficiency, Caprar, Rynes, Bartunek, and Do (2011) studied the question more directly. They designed a study in which participants were exposed to three published essays regarding the merits of GMA, emotional intelligence, and employee-organization fit, respectively, for predicting job applicants' subsequent performance. Texts were abstracted from Schmidt and Hunter (2003) for GMA, Goleman (2000) for emotional intelligence (EI), and Pfeffer (1998) for cultural fit (although author names were not included with the texts). Each participant read and responded to all three texts. Texts were equal in length, and order of presentation was balanced across participants.

Based on copious psychological research that people either avoid or devalue information that is threatening to their self-image (e.g., Steele, 1988; Swann & Read, 1981) or self-interest (Miller, 1999) and that people generally dislike the idea that intelligence is important to success (e.g., Hofstadter, 1964; Pinker, 2002), Caprar et al. (2011) predicted that participants would be least persuaded by the essay on the importance of intelligence. Conversely, because nearly everyone can "fit" somewhere, the essay on fit was predicted to be least threatening and hence, the most persuasive (on average). In addition, the authors predicted that there would be individual differences in reactions to the essays, with individuals scoring highest on proxies for GMA (i.e., college entrance exams and grade point averages) being more likely than other participants to find the GMA essay persuasive, and those scoring higher on a measure of EI being more likely than other respondents to be persuaded by the EI essay. All hypotheses were confirmed, suggesting a general dislike of the idea that intelligence is important to performance, a general belief in the importance of fit, and individual differences in beliefs about GMA and EI consistent with theories of both self-affirmation and self-interest.

JURY DECISION MAKING

Although jury decision making is outside the immediate realm of I/O psychology, it is an interesting area to examine because juries are often expected to incorporate expert testimony into their decision processes. A review of this literature shows several important parallels between jury decision making and selection research. For example, as with selection, some expert witnesses use actuarial methods, while others rely on clinical judgments.

Another similarity is that in law, too, actuarial evidence has been shown to consistently outperform clinical judgments in predicting outcomes such as whether a defendant has actually committed a crime or is likely to become a repeat offender in the future (e.g., Ayres, 2008; Krauss & Sales, 2001; Lieberman, Krauss, Kyger, & Lehoux, 2007; Monahan & Steadman, 1994). A third similarity is that, just as with selection, juries are more likely to be persuaded by clinical opinions than by actuarial or statistical evidence (Bornstein, 2004).

For example, evidence obtained from mock juries shows that jurors: place less weight on general statistics than individuating information (Loftus, 1980); are reluctant to base verdicts on statistical information alone (Niedermeier, Kerr, & Messe, 1999); underutilize expert probabilistic testimony compared to Bayesian norms (Kaye & Koehler, 1991); and perceive experts who present anecdotal evidence as more credible than those who present non-anecdotal evidence (Bornstein, 2004). In addition, the stronger influence of clinical prediction remains, even after the presentation of adversarial procedures (e.g., cross-examination) or contradictory opinions by other experts (Krauss & Sales, 2001). This is particularly troublesome because the Supreme Court and many state courts have assumed that juries will appropriately weight scientific evidence according to its quality and that the adversarial system will expose the weaknesses of inferior scientific testimony.

Unfortunately, such does not appear to be the case. For example, in an analysis of how the tobacco companies prevailed in lawsuits brought by injured smokers for more than 40 years, Givelber and Strickler (2006) deconstructed how lawyers for the tobacco industry discredited actuarial epidemiological evidence. This was accomplished via a three-stage process: (1) downgrading the status of epidemiology by getting plaintiffs' expert witnesses to agree that there were no certification processes for determining who was an epidemiologist; (2) insinuating (despite expert witnesses' resistance) that epidemiology was "really only a matter of statistics"; and (3) getting expert witnesses to admit that there is a difference between a "risk factor" and a "cause." Step #2 was particularly important, according to R. J. Reynolds's lawyers.

Reminiscent of Highhouse's (2008) contention that selection practitioners are disappointed with validity evidence because they believe that prediction can approach perfection, analysts of the *Galbraith v. R. J. Reynolds Tobacco* trial said: "The fact that the

jurors came into the trial believing that the epidemiological experts would testify to [the fact that cigarettes cause cancer] worked in Reynolds's favor: the jurors *seemed to expect something more* from the witnesses and be disappointed when they did not hear it" (Givelber & Strickler, 2006, pp. 34–35; emphasis added). The authors concluded that "despite judicial efforts to eliminate 'junk science' from lawsuits, a well-financed defendant may succeed in persuading jurors of the epidemiological equivalent of the proposition that the earth is flat" (p. 33).

In summary, research on juries shows the same (generally erroneous) preference for clinical over actuarial evidence, as does research on selection. Moreover, actuarial evidence appears to be more vulnerable than clinical evidence to adversarial persuasive techniques (Givelber & Strickler, 2006; Krauss & Sales, 2001). Combining the evidence from both selection and jury decision making (as well as medical and judicial decisions; see Ayres, 2008, chapters 4, 5) suggests that there are some fundamental challenges involved in getting practitioners or laypeople to accept probabilistic, large-sample research findings. Indeed, most people seem to prefer to take their evidence from samples of one, or anecdotes. Changing this preference will require a better understanding of what causes it.

UNDERLYING SOURCES OF DISBELIEF
Sources Unique to Each Belief

There are many possible reasons that people may not believe research on each of the preceding topics. For example, in the case of intelligence, research findings are likely to conflict with many people's already established beliefs, such as the cherished notion (at least in the United States) that with hard work, anyone can achieve anything, or that emotional intelligence (Goleman, 1995), intuition (Gladwell, 2005), or luck (Gladwell, 2008) are more important than GMA for attaining success. In addition, many people hold negative stereotypes about intelligent people that are not upheld by large-sample evidence, such as beliefs that they are more likely than others to be deceitful, selfish, lacking in empathy, and devoid of "common" sense (Hofstadter, 1964). Along the same lines, people may believe that integrity tests have adverse impact against minorities because they assume that minorities are less honest than whites.

Alternatively, in the case of utility analysis, it is easy to imagine that many people might find the estimates of financial benefits to HR practices unbelievably large, perhaps reasoning that if such practices were *that* effective, employers would have figured it out by now. (A version of this same logic was used by economists to explain how there "couldn't be" discrimination in labor markets. The reasoning went that if women, Asians, or African Americans were indeed as capable as white males, then some clever employer would have figured this out, started hiring women and minorities [probably at lower wages, given their limited market opportunities], and put everyone else out of business; see, e.g., Blau, Ferber, & Winkler, 2005.)

Thus, although one might come up with unique explanations of each area where there is a research-practice gap, there are also some common sources that undergird multiple gaps.

Common Sources

One factor that is common to findings regarding utility analysis, the importance of GMA, and the superiority of actuarial over clinical prediction is that the research evidence is likely to be threatening to many (if not most) individuals, at least upon first hearing. For example, one can well imagine that the vast majority of practitioners (and perhaps academics as well) who hear about utility analysis for the first time might become apprehensive about the equations involved and their ability to understand them, let alone their ability to explain them to others. Others may be upset by the idea that human efforts can (or, implicitly, "should") be quantified in terms of economic metrics. Similarly, with respect to peoples' preference for clinical or intuitive decision making, the use of actuarial methods not only takes away control from managers (e.g., Ayres, 2008; Dipboye, 1992), but also threatens their self-image as people of good judgment (Ayres, 2008; Highhouse, 2008).

But perhaps the most threatening of the three research-inconsistent beliefs is the idea that intelligence might have a measurable impact on one's vocational and financial success. Pinker (2002) discusses in considerable detail the numerous reasons that people on both sides of the political spectrum tend to find this research threatening.[4] At heart, Pinker argues that many people reflexively disavow scientific findings about intelligence (and the role of genes in human behavior) for two major reasons: they see them both as *deterministic* (omitting or dramatically reducing the possibility of free choice and personal improvement) and *reductionist* (assessing people as collections of specific traits, rather than as integrated "whole" entities). Others reject the Darwinian notion that humans evolved from "lower" animals, rather than having been given dominion over them by God. According to recent

opinion polls by Roper, Gallup, and others, only 15% of Americans say they believe that Darwin's theory of evolution and natural selection is the best explanation for the origins of human life, while 76% admit to believing in creationism, angels, the Devil, and ghosts (Pinker, 2002, p. 2). As such, scientific findings about intelligence (and its partial genetic heritability) appear to threaten some very deeply held personal beliefs that are exceedingly difficult to address in either classrooms or boardrooms.

Conflicting Opinions Within Academia

To this point, I have emphasized differences between large-sample research findings and practitioners' beliefs. However, there are also important differences *within* the academic community on some issues, including the importance of intelligence. For example, Pinker (2002) documents how some of the most vitriolic attacks on scientists who assert the importance of intelligence or genetic inheritance of various traits have come from other academics. Specifically, he traces in considerable detail how the writings of academics such as sociobiologists E. O. Wilson and Richard Dawkins and psychologists Paul Ekman and Richard Herrnstein have been misquoted, distorted, and extended far beyond the original treatises to accuse those who assert the importance of intelligence of providing "excuses" for all sorts of social maladies such as racism, promiscuity, male dominance, amorality, and societal inequality (Pinker, chapter 6).

Somewhat closer to home, Murphy, Cronin, and Tam (2003) found that I/O psychologists, too, have diverse beliefs about the usefulness of intelligence tests in employment contexts, despite the fact that all of them have Ph.D.s. For example, within their sample of 703 I/O psychologists, Murphy et al. (2003) found considerable divergence of opinion on items such as "general cognitive ability is the most important individual difference variable" and "the dollar value of diversity can be measured," even though there was widespread consensus that intelligence tests are both valid and fair.

Academic disagreements exist in other areas of I/O psychology as well. Examples include debates about whether or not the validities of personality tests are high enough to be useful as selection devices (e.g., Morgeson et al., 2007, vs. Ones, Dilchert, Viswesvaran, & Judge, 2007) or whether pay-for-performance is an effective motivator (see Rynes, Gerhart, & Parks, 2005, vs. Pfeffer, 1998); or the "dueling meta-analyses" of Eisenberger and Cameron (1996) and Eisenberger, Pierce, and Cameron (1999) vs. Deci, Koestner, & Ryan, (1999a; 1999b).

Thus, the beliefs discussed in this section appear to reflect values and emotions as well as cognition, and to affect academics as well as practitioners. In light of such evidence, it may be difficult to dispel these misperceptions merely through the provision of "information" via essays, lectures, data charts, and other cognitive approaches.

DISTRUST OF STATISTICS AND THE SCIENTIFIC METHOD

Lack of belief in utility analysis, actuarial selection processes, and empirical evidence in lawsuits may all result in part from a general distrust of statistics, nicely captured by Benjamin Disraeli's quip that there are "lies, damned lies, and statistics" (Best, 2001). A concrete example of the distrust of statistics was mentioned in Givelber and Strickler's (2006) account of the *Galbraith v. R. J. Reynolds* trial. According to R. J. Reynolds's lawyers, "The jurors distrusted statistics; in fact, one juror said that she did not believe conclusions which were based on statistics. All the jurors said that the plaintiffs' charts showing the worst statistics from the Surgeon General's reports were ignored, and one juror…dismissed the charts by describing the information contained in them as mere answers to questionnaires" (Givelber & Strickler, 2006, p. 35).

Although Best (2001) believes that most distrust of statistics stems from lack of numerical literacy, skepticism about large-sample research findings can also be found among practitioners with plenty of statistical education (such as I/O psychologists). Boehm (1980) argues that one reason this happens is that the interconnected, messy world in which practitioners operate causes them to be suspicious of findings obtained in the pristine, highly controlled, and decontextualized environments in which much academic research takes place. In other words, it is not lack of knowledge that causes high-level practitioner skepticism of certain results, but rather the perceived lack of generalizability of the findings. This insight sheds some light on Highhouse's (2008) observation that "there is considerable evidence that employers simply do not believe that the research is relevant to their own situation."

Some interesting examples of skepticism about the scientific method (as well as academics' communication skills and lack of agreement on research implications) came up in "friendly reviews" of earlier versions of this manuscript. In order to take the reader "backstage" to these conversations, I reproduce some of the reviewer comments (and the texts that provoked them) in Table 13.1.

Table 13 1. "Friendly Reviewer" Comments on Earlier Versions of this Manuscript*

Text	Reviewer Reaction
Even if a gap is due to lack of awareness, it cannot automatically be assumed that practitioners would actually change their beliefs after being exposed to relevant research.	Perhaps the reason for this is that practitioners' beliefs are consistent with their experiences. Perhaps the experience of practitioners is different from what a controlled experiment concludes. Researchers assume they are correct yet they don't know the reason that someone believes what they believe. Research may not be real to practitioners or may not represent a real phenomenon to them. For example, many practitioners find structured interviewing to be too confining and would never think of hiring someone without an interview at all. (P)
With respect to practitioners' lack of enthusiasm for actuarial prediction methods, Highhouse (2008) argues that the source is failure to be convinced, rather than lack of awareness. He says, "Although one might argue that these data merely reflect a lack of knowledge about effective practice, there is considerable evidence that employers simply do not believe that the research is relevant to their own situation."	Isn't it possible that this is correct—for practitioners, there is no relevance? (P)
Despite practitioners' preference for clinical prediction, the finding that actuarial methods outperform clinical ones in a wide variety of settings is well documented (e.g., Grove & Meehl, 1994), as is the fact that adding clinical judgments to actuarial ones does not improve things or even makes them worse (Highhouse, 2008).	Yes, well documented by research. You are using (understandably) a research frame to argue a research point and to in some ways discount the practitioner's view. Practitioners don't see it this way. (P)
In summary, research on juries shows the same (generally erroneous) preference for clinical over actuarial evidence as does research on selection.	If this is erroneous, are you saying that jury decisions are generally incorrect? Are you also saying that organizations, operated by practitioners, are generally ineffective? How do we explain the successes that practitioners have who don't regularly apply research findings? (P)
For example, Rynes et al. (2002) found that, contrary to research evidence, the vast majority of HR managers in their sample believed that both conscientiousness and values are better predictors of performance than intelligence.	I don't believe that intelligence is the best predictor, either. (A)
Third, even after decades of cumulated research and hundreds of meta-analyses, researchers in various areas still do not agree on the implications of extant findings (e.g., whether or not the validities of personality tests are high enough to be useful as selection devices [see Morgeson et al., 2007 vs. Ones et al., 2007], or whether pay-for-performance is an effective motivator [see Rynes et al., 2005 vs. Pfeffer, 1998; or the "dueling meta-analyses" of Eisenberger and Cameron, 1996 and Eisenberger et al., 1999 versus Deci et al., 1999a, 1999b]).	Maybe part of the reason that practitioners don't leverage research more effectively is because academics can't agree on anything? I hadn't thought about this before your diatribe regarding selection (a rant worthy of Bill Maher, by the way ☺). If this is an example of an area where we know a lot, and the leading researchers can't agree on what it is that we know, it's no wonder that practitioners don't leverage research effectively. We're consumed with the debate whether the true r between GMA and performance is .39 or .44…and the practitioner world yawns. (P)
I was unable to find any studies that have directly asked academic researchers such things as whether they read practitioner journals, or whether they have accurate knowledge of the major issues and environmental conditions confronting I/O or HR practitioners.	We as academics can be moderate to severe intellectual elitists who abuse practitioners with quantitative clubs, rather than finding ways to explain basic concepts like correlation and regression with minimal pain. (A)

(*Continued*)

Table 13 1. (continued)

Text	Reviewer Reaction
The quickest and most direct way to make research more accessible to practitioners is to get findings into outlets and venues that are already widely used by those who practice. This requires knowing where different types of practitioners go to get their information…the most effective ways of reaching I/O practitioners would appear to be the SIOP website and for HR practitioners, HR Magazine and the SHRM web site.	I suspect that even if all these things happened, managers would stop looking in these locations and go somewhere else. It is the CONTENT of research articles that is turning them off, not the location in which research articles are published. (A)
During the past three decades, pleas have been escalating to give greater attention to the context in which our research is conducted (e.g., Bamberger, 2008; Johns, 2001; Roberts et al., 1978; Rousseau & Fried, 2001; Porter, 2008).	I agree, though unfortunately, pressure from journal publishers to maximize number of articles published while minimizing individual article pages keeps this stuff out—a detailed explanation of strategic utility using a chicken restaurant franchisor was cut out of one of my articles in PPsych. (A)
Although it is difficult to know for sure, it is possible that many students (i.e., future managers or other practitioners) leave college without a solid grasp of research principles due to ineffective teaching of research methods, statistics, and analysis.	What? I suspect we all know this—ask undergraduates how they liked their required stats classes. How many additional statistics electives did they take? How do they like running into applications of the stats they learned in their major? I suspect most HR majors chose it because they "like people" and hope the area will permit them to hide from any additional exposure. (A)
For example, the most frequent recommendation (in "implications for practice" sections of top-tier journal articles) was for practitioners to "become aware" of a certain phenomenon, which doesn't seem very likely to be translated into action	Besides, it smacks of something a condescending scholar would say. (A)
In order to increase implementation of research ideas, perhaps few things would help more than some sort of "help line" or chat room for those who are considering a change but have specific questions or problems with respect to implementation.	Most academics would fail miserably here—it is simply not in their skill set. (A)
Although there are certain reasons and situations that call for separation of science and practice, there are others that would benefit from closer interactions between them.	I can't think of any justifying separation. (A)

∗ Comments from academics are marked (A), and comments from practitioners are marked (P).

Gaps in Implementation

It has long been noted that research-supported I/O and HR practices often are not put into place in organizations (e.g., Bretz, Milkovich, & Read, 1992; Lawler, Mohrman, & Ledford, 1992; Kersley et al., 2006), even when practitioners know about them and believe that they would improve organizational and employee outcomes (Pfeffer & Sutton, 2000). At the same time, however, HR departments have long been accused of being "faddish," moving rapidly from one new program to another (and not always the best programs; e.g., Abrahamson & Eisenman, 2001). These observations have led

a number of researchers to try to understand why some research findings get adopted, while others do not (Sturdy, 2004).

FACTORS INFLUENCING ADOPTION OF NEW PRACTICES

An implicit assumption behind evidence-based management (EBM) and discussions of the academic-practice gap is that practitioners "should" adopt practices supported by large-sample research because these offer, on average, technically superior solutions. However, it has long been known that technical or technological superiority is far from the

only variable considered when organizations decide whether or not to adopt a new practice. For example, Rogers (2003), who analyzed the diffusion of many kinds of innovations for over 50 years, argued that, in addition to technical performance, adoption of a new idea also depends on its: (1) perceived advantage relative to the idea or practice it is attempting to supersede; (2) compatibility with existing values, past experiences, and needs of potential adopters; (3) simplicity; (4) trialability (the degree to which the idea can be experimented with on a limited basis); and (5) observability (the degree to which the results of its application are visible).

By treating I/O research findings as "innovations," Johns (1993) extended Rogers's framework to explain the limited adoption of technically superior I/O practices. Johns argued that three broad factors explain why technically superior I/O innovations often fail to be adopted. First, managers tend to see I/O practices as administrative, rather than technical, innovations. As such, proposed new practices are evaluated at least as much in terms of their likely effects on social systems (and personal careers) as on production outcomes. (Note: This might be another reason that some people are concerned about hiring intelligent applicants or using actuarial selection models.) Second, Johns (1993) suggested that administrative innovations are perceived by managers as more uncertain than technological ones (see also Highhouse, 2008; Ledford, Lawler, & Mohrman, 1988), resulting in their being adopted less often (Symon & Clegg, 1991). Moreover, even when they are adopted, uncertain innovations are more likely to be adopted for reasons other than technical soundness (e.g., politics or imitation of other organizations; Ayres, 2008; Abrahamson, 1991; DiMaggio & Powell, 1983; Nutt, 1989). Third, decisions to adopt or abandon administrative innovations are affected by extra-organizational factors such as economic crises, interorganizational relations, government regulations (e.g., equal employment legislation), and employee power.

Another model of why some innovations are adopted over others is Abrahamson's theory of "management fashion" (1991, 1996). Abrahamson argues that management fads and fashions[5] are, to some extent, comparable to aesthetic fashions (e.g., hairstyles, clothing, and home décor), which are driven almost exclusively by sociopsychological forces. However, unlike purely aesthetic matters, management fashions are also driven by technical and environmental changes that create performance gaps and a true need for new solutions. Thus, the management fashions that emerge are a joint result of: (a) sociopsychological persuasion by "fashion purveyors" such as consultants, business professors, or management gurus (i.e., the supply side); and (b) real managerial needs, as determined by economic competition, technological change, and other environmental factors (demand side).[6] Furthermore, Abrahamson proposes that the small number of fashions that are ultimately adopted are ones that are seen as both rational (capable of fixing the problem or reducing the gap) and progressive (improving on earlier techniques and solutions).

If Abrahamson's model is correct, then one would predict that management fads will not "go away," but rather are likely to become even more frequent. This is because the supply of knowledge purveyors and the pace of change (and hence, the emergence of new managerial needs) are both increasing.

Indeed, an empirical study of management fashions from the mid-1950s to the late 1990s suggests exactly that. Specifically, Carson, Lanier, Carson, and Guidry (2000) found that management fashions arose more frequently at the end of the twentieth century than during its middle. In addition, they found a shift from people-centered fashions (e.g., management by objectives, employee assistance programs, sensitivity training, and quality of work life programs) to technically and strategically oriented ones (e.g., total quality management, ISO 9000, benchmarking, and reengineering). Carson et al. (2000) also found that more recent fashions tended to have a shorter "shelf life," a finding which they attributed to the greater difficulty of implementing these later fashions. However, their measures of adoption and shelf life were quite indirect—based on the emergence, peak, and eventual decline of academic and popular publications on each fashion—so it is difficult to know the precise reasons behind the apparently shorter life cycles. For example, shorter cycles could also be due to consultants pushing product obsolescence, reporters focusing on "what's new" rather than what works, or top managers having less persistence or shorter time frames than in the past.

Still other studies have examined the rise and fall of "rational" versus "normative" rhetorics urging managers to adopt new practices (Abrahamson, 1997; Barley & Kunda, 1992). According to Abrahamson, rational rhetorics are reminiscent of scientific management and Theory X in that they assume that "work processes can be formalized and rationalized to optimize labor productivity, as can the reward systems that guarantee recalcitrant

employees' adherence to these formal processes" and conceive of employees as "largely averse to both responsibility and work" (p. 496). In contrast, normative rhetorics assume that "employees (can be made) more productive by shaping their thoughts and capitalizing on their emotions" (e.g., human relations and corporate culture approaches; Abrahamson, 1997, p. 496).

At the micro level, there is evidence that the adoption of rational versus normative policies and practices is subject to individual differences in such characteristics as cognitive style, openness to experience, political ideology, and "implicit person" theories (Heslin, Vandewalle, & Latham, 2006; Rogers, 2003; Tetlock, 2000). At a more macro level, these techniques and rhetorics also appear to alternate in "long waves" that correspond with upswings and downswings in the economy (e.g., Barley & Kunda, 1992). For example, Abrahamson (1997) found an upsurge in normative rhetorics just before the end of long macroeconomic upswings (e.g., increase in quality- and culture-related techniques in the early 1970s) and under conditions of high employee turnover, unionization rates, or strikes.

Thus, in general, rhetorics (and presumably, practices) appear to become more normative and employee-centric when workers are more powerful and more rational/technical when they are not (see also Kochan, 2007). At the time of this writing, there have been growing disparities in worker power across occupational categories, with a small number of highly placed executives and technicians having very high market power, but most other workers having less and less. In such an environment, it is perhaps not surprising to find calls for highly differentiated HR practices for employees with different skill and ability levels (e.g., Becker, Huselid, & Beatty, 2009; Lepak & Snell, 1999; Stewart, 1998).

HOW INNOVATIONS DIFFUSE

While the preceding section focused on factors that influence the adoption of new practices, it is also useful to examine the processes by which practices diffuse. Two of the most central findings about innovation diffusion processes are that they are (a) social and (b) non-linear (Rogers, 2003). Specifically, according to Rogers, rates of diffusion follow an S-shaped curve, with only a small number of innovators in the beginning, followed by more rapid acceleration as thought leaders and "early majority" adopters join in, and then approaching an asymptote as fewer and fewer organizations (or individuals) are left to adopt. In contrast, unsuccessful

innovations either do not capture many thought leaders or early adopters beyond the original inventors, or else they lose momentum as problems with their adoption become widely known (as with quality circles).

At the individual level of analysis, Rogers (2003) indicates that the following characteristics are (positively) associated with early adopters: education level; social status; empathy; openness to experience; positive attitudes toward science; internal locus of control; high aspiration level; strong (and more cosmopolitan) social networks; exposure to mass media communications; high information seeking; frequent contact with change agents; and demonstrated opinion leadership. These, then, are the types of practitioners that academics are most likely to influence with their research.

At the organization level, the first adopters of a new idea are often those for which the particular innovation is seen to be appropriate for solving some real or perceived problem (e.g., Greenwood & Hinings, 2006; Tolbert & Zucker, 1983). However, as the innovation diffuses, it tends to do so earlier among organizations that are larger, less centralized and formalized, more highly skilled, and that have higher connectedness with other organizations, more organizational slack, and change-oriented leaders (Tolbert & Zucker, 1983). Furthermore, as adoptions spread, they tend to do so "locally" (i.e., through managers seeking evaluations of the innovation from earlier adopters in their same industry or labor market Guest, 2007) and through direct rather than indirect ties (Burt, 2007). One exception to this organizational pattern appears to involve *radical* innovations (i.e., ones that destroy current capabilities), which are more likely to be adopted by smaller, newer organizations that are outside the dominant network and that in recent years have destroyed some major organizations that did not adapt quickly enough (Christensen, 1997; Greenwood & Hinings, 2006; Tushman & Murmann, 1998).

While social networks are important in the later stages of diffusion, at the early stages, mass media may play a larger role. The communication medium that is arguably the most successful in launching management trends is the best-seller book (e.g., Furusten, 1999; Kieser, 1997). Best-selling management books follow a certain well-worn formula: focus on a single factor or idea; contrast the "old" and "new" ideas; create a sense of urgency and inevitability; link the idea to highly treasured values; provide case studies of outstanding success; and stress the

idea's universal applicability (Clark & Greatbatch, 2004, pp. 401–402). Thus, for example, we have "a new yardstick" (Goleman, 2000, p. 3) for judging and predicting the success of people (emotional intelligence, or EQ), "not just how smart we are" (the old idea, IQ). Moreover, we need to pay attention to this new yardstick because there is "a coming crisis: rising IQ, dropping EQ" (p. 11). As for universality, anyone can acquire EQ because "our level of emotional intelligence is not fixed genetically, nor does it develop only in early childhood. Unlike IQ, which changes little after our teen years, emotional intelligence seems to be largely learned" (p. 7). Moreover, EQ is on the side of virtue, being readily attainable to all who seek it: "there is an old-fashioned word for this growth in emotional intelligence: maturity" (p. 7). And so on.

But simply writing a best seller is not enough to get managers to implement its ideas. Rather, it is often difficult for managers to put research into practice without an explicit "road map" and/or personal support on the part of the researcher, author, or some other change agent (Argyris, 1985; Mohrman, Gibson, & Mohrman, 2001). Thus, in order to turn book sales into additional sources of revenue, the best-seller book industry has responded to this need in a number of ways—for example, by turning certain authors into "brands" (e.g., Jim Collins, Daniel Goleman, John Kotter, or Tom Peters), promoting their ideas across a variety of media (e.g., video and audio-tapes, CD-ROMs, web sites, and book-affiliated consulting groups), and adapting general messages to specific audiences (e.g., sequentially retargeting Goleman's original 1995 book on emotional intelligence toward managers, educators, and parents, or re-focusing Christensen's original (1997) book on disruptive innovations in technology to other industries such as health care and education).

In addition, Clark and Greatbatch (2004) document how, once a writer becomes a brand, his or her future books may emerge from an editor's idea (rather than the writer's) and may even be written primarily by ghost writers. Furthermore, in the service of higher sales, authors, editors, and publishers sometimes create artificial sales figures (e.g., buying thousands of early copies of books to make them "appear" to be popular). According to Clark and Greatbatch (2004):

> The popularity (of best-sellers) with readers cannot be attributed to "real" sales. The writer of the book and the named author on the cover are not necessarily the same individual. Finally, the data or observations that underpin the ideas being presented cannot be assumed to exist. Thus, the assumption that the books themselves and the ideas they contain are grounded in terms of the authenticity of a referent point does not necessarily hold. They therefore represent a form of pseudoknowledge. (p. 399)

The distortion of information in best-selling books may even apply to best-selling *text*books, which are generally assumed to be credible purveyors of the knowledge base underlying the disciplines they represent (Stambaugh & Trank, 2009). However, this assumption, too, seems questionable. For example, in an article by several best-selling textbook authors (Cameron, Ireland, Lussier, New, & Robbins, 2003), OB textbook writer Stephen Robbins said, "Publishers are in the business of selling books. They'll sell anything if they think people might want it. They don't care about integrity or quality. Moreover, they firmly believe that *they* create books. They have little respect for authors" (p. 716).

In other words, the world of the management best seller is a universe away from that of top-tier academic journals, with "whole industries" often being brought into existence in order to push a best-selling idea (Abrahamson, 1996; Clark & Greatbatch, 2004; Furusten, 1999). Because such practices are generally anathema to academic researchers under current cultural norms, it is perhaps not surprising that few recent management innovations appear to be coming from academia (Pfeffer & Fong, 2002).

Potential Solutions

To this point, I have argued that research-practice gaps occur for (at least) three basic reasons: lack of awareness, lack of belief, and lack of implementation. Below, I present potential solutions to each of these three components of the gap.[7] For an overview of all solutions, see Table 13.2.

Increasing Awareness

At least four general approaches can be used to increase awareness of research findings: (a) make better use of existing practitioner outlets; (b) create new outlets and formats; (c) investigate topics of greater interest to practitioners; and (d) improve social relations and communication skills between academics and practitioners.

MAKE BETTER USE OF EXISTING OUTLETS

The quickest and most direct way to make research more accessible to practitioners is to get findings into outlets and venues that are already

Table 13.2 Proposed Solutions to the Three Components of the Gap

Objective	Potential Solutions
Increase awareness of research	Make better use of existing outlets Create new outlets and formats Investigate topics of greater interest to practitioners Improve social relations and communication skills between academics and practitioners
Increase believability of findings	Communicate more persuasively and empathetically Coproduce research with practitioners or consultants Improve rewards and recognition for research that benefits practice Increase flexibility of top-tier journals Conduct research on persuasiveness of research findings Continue to strengthen our research base Become more effective teachers of research methods, statistics, and critical thinking
Increase implementation	Present advice in form of principles plus examples Improve "implications for practice" sections in primary research studies Increase applications research and research contextualization Provide specific support for implementation

widely used by those who practice. This requires knowing where different types of practitioners go to get their information. For example, the recent SIOP Practice Survey (Silzer & Cober, 2008) found that among full-time practitioners, the sources most frequently used to gain professional knowledge and skills were web sites and other online sources (approximately 95%), followed by (at roughly 80% each) professional conferences; articles, publications, and books about business management or HR; networks of professional colleagues and trailed by non-research publications in I/O psychology (75%). Only 55% indicated that they read research articles to keep informed, despite the fact that most (78%) had doctoral degrees. By way of contrast, Rynes et al. (2002) found that HR managers almost never (< 1%) read research articles, while almost all of them read *HR Magazine,* which has a circulation of more than 250,000.

Thus, the most effective ways of reaching I/O practitioners would appear to be through the SIOP web site and conferences, and through non-research-oriented publications in business management and HR. In contrast, the most effective ways to reach HR practitioners would appear to be through the SHRM web site and *HR Magazine* (Cohen, 2007; Rynes et al., 2002).

This knowledge is now being put to good use. For example, since 2008, *HR Magazine* has been increasing research coverage, adding regular research updates to its "Executive Briefing" section, and

soliciting academic opinions in feature articles. In addition, SIOP has collaborated with SHRM to create a "SIOP Science for SHRM" board, one of whose first projects will be to create joint-authored (one academic, one practitioner) summaries of research findings and their implications for distribution to more than a quarter of a million SHRM members. In the longer term, the board is exploring the production not only of written content, but also of alternative formats such as web casts, DVDs, and educational sessions that carry certification credits. In another innovation, SIOP is planning an annual publication, *Science You Can Use: Managing People Effectively*, which will produce somewhat longer summaries of research findings as well as implications for practice. In addition, the SHRM Foundation has created a downloadable *Practice Guidelines* monograph series on the SHRM web site, providing guidance on such topics as compensation and performance management by researchers such as David Allen, David Day, Rob Heneman, Elaine Pulakos, and Robert Vance. Academic professional associations can also hire public relations consultants to place research findings in the popular press. This can be beneficial in at least three ways: by increasing the general visibility of academic disciplines, by reaching much broader audiences beyond those of specialist professional associations, and by bringing the topics that academics study to public attention (McHenry, 2007). Both SIOP and the AOM employ such publicists, who have received considerable popular press attention

for such research as Judge and Hurst's (2007) study of the role of self-evaluations and socioeconomic status on mid-career income levels, Trevor and Nyberg's (2008) study of the role of downsizings on subsequent turnover in organizations, and the role of gender and gender role orientation on earnings (Judge & Livingston, 2008).

Of course, all of the above suggestions share the optimistic assumption that "pushing" research out to practitioners will result in increased uptake of research-based recommendations. However, that will not be the case to the extent that practitioners do not see the relevance of the research to their own situations, regardless of how relevant *researchers* think it is.

A third way of making better use of existing venues for reaching practitioners (or future practitioners) is through evidence-based teaching (EBT). According to Rousseau and McCarthy (2007), EBT encompasses the following four principles: (1) focus on principles where the science is clear; (2) develop decision awareness in professional practice; (3) diagnose underlying factors related to decisions; and (4) contextualize knowledge related to evidence use. Although EBT may be standard practice in psychology classes, it is a minority practice in business schools (see Charlier, Brown, & Rynes, 2011), where many managers (and some HR managers) are educated.

Although I am not aware of any evidence regarding the extent to which EBT is practiced in educating I/O psychologists, there are several factors that work against extensive practice of EBT in business schools. One is that business students tend not to like either theories or empirical research (Rousseau & McCarthy, 2007; Trank & Rynes, 2003). Although this might also be somewhat true of psychology students (although I know of no evidence to indicate that this is so), student preferences have a far stronger impact on what gets taught in business schools than in the social sciences because of the role that student opinion plays in popular press rankings of business schools (e.g., *Business Week* or *Forbes*; Gioia & Corley, 2002). Because student opinions count heavily in business school rankings, curricular and pedagogical choices—as well as grades—tend to move in directions favored by students. These trends detract from business schools' efforts to provide a truly professional education in which abstract knowledge or generalizable principles that can be applied to many different situations are taught (Trank & Rynes, 2003).

Another potential deterrent may be the common use of the case method in business schools. The traditional case method focuses on analysis and discussion of the specific problems and context in each case, with students being encouraged to express their own opinions and instructors often emphasizing that there is "no right answer" (Ellet, 2007). According to Greiner, Bhambri, and Cummings (2003), "the Harvard Business School case method advocated that every company situation was unique and not easily amenable to generalization. The learning emphasis was on inductive reasoning as students were expected to learn through Socratic debate and exchange in sharpening not only their analytical abilities, but also their intuition, judgment, and behavioral skills" (p. 403). Greiner et al. (2003) contend that, in recent years, this traditional case method has increasingly given way to one in which students are encouraged to analyze cases in light of theory (i.e., with "right" and "wrong" answers) or empirically based principles—a trend that they (Greiner et al.) find disturbing but others do not (e.g., Hambrick, 1997; Locke, 2002). In any event, analyzing cases vis à vis theory is not the same as analyzing them in light of empirical evidence (Locke, 2007; Russell, 2009, as some theories that continue to be popular have not held up well to empirical evidence (Davis, 1971; Miner, 1984; Rynes, Gerhart, & Parks, 2005).

Thus, it should not be assumed that research findings occupy a major place in business schools or other management education venues. Nor, as Stambaugh and Trank (2009) illustrate in the case of business strategy textbooks, should it be assumed that new research findings easily make their way into management textbooks, especially if they are inconsistent with the "established wisdom." Unfortunately, there are presently few models for EBT in either management or psychology, although some excellent exceptions can be found in Burke and Rau (2010), Charlier et al. (2011), Latham (2007a, 2007b), Pearce (2006), and Rousseau and McCarthy (2007). One particularly effective method may be to integrate inductive or empirically derived principles (Locke, 2002, 2009) with cases or other experiential teaching methods (such as role playing), although this method, too, would require abandoning the HBS assumption that "every situation is unique" and accepting at least the general applicability of meta-analytic results and empirically derived decision tools such as artificial intelligence. (Examples of the latter have emerged in medicine, such as the Isabel system, which suggests the most likely diagnoses for an individual patient's combination of symptoms, or DynaMed, which carries a

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"Level of Evidence" rating that reflects the quality of evidence underlying each study examining treatments for particular diagnoses; Ayers, 2008).

CREATE NEW OUTLETS AND FORMATS

In addition to disseminating research through existing practitioner channels, researchers and/ or practitioners can also attempt to create new outlets that are designed to be appealing to both groups. One such venture is the Collaborative for Evidence-Based Management, which is modeled on the Cochrane Collaboration (www.cochrane. org) in medicine. The CEBMA was established as "a community of practice to make evidence-informed management a reality. Its mission is to close the gap between management research and the ways practitioners make managerial and organizational decisions and educators teach organizational behavior, theory, strategy, and human resource management" (EBM Conference, 2007). Its major long-term goal is to build a web site containing systematic research syntheses on various management and organizational science topics, summarized in ways that are easy to use by both practitioners and educators (Rousseau, Manning, & Denyer, 2008; see www.cebma.org). Systematic research syntheses are based on "comprehensive accumulation, transparent analysis, and reflective interpretation of all empirical studies pertinent to a specific question" (Rousseau et al., p. 479). By making such syntheses available in a single place, the collaborative should make it far easier for practitioners to search for the latest research findings. Similarly, the SHRM (www.SHRM.org), SIOP (www.siop.org), and AOM (www.aomonline. org) web sites provide access to research findings in a variety of formats, including online versions of academic journals in the latter two cases.

Another tactic that has been employed to attract practitioners to research-based ideas is the creation of new journals or media. For example, AOM created the *Academy of Management Executive* (*AME*)*,* INFORMS (Institute for Operations Research and Management Sciences) created *Analytics*, and SIOP created *I/O Psychology: Perspectives on Science and Practice*. One valuable feature of the last journal is that it also contains articles by practitioners that inform academics about the state of the art in practice (e.g., Tippins's 2009 article on Internet testing). Such articles are important because they can help academics keep up to date in their teaching, while at the same time making them aware of interesting practical research needs.

However, the success of new journals can be highly variable. For example, AOM recently changed the mission (and name) of *AME (to Academy of Management Perspectives)* because an internal study showed that it was not being used by executives. Similarly, an earlier academic journal designed to focus on a practical area, *Journal of Quality Management*, was discontinued after only a few years due to lack of readership. On the other hand, *Analytics*—whose goal is "to provide readers with a better understanding of how data, modeling and mathematical analysis is [*sic*] used to drive better business decisions and provide concrete competitive advantage"—garnered 1,800 subscribers in its initial year of publication, two-thirds of whom were not current members of INFORMS. Subscriptions have been increased by surveying new subscribers to learn about their needs, analyzing hits to individual articles or issues on the web site, and asking INFORMS members to forward articles or entire issues of *Analytics* to practitioners they know (Bennett, 2008). To the extent that organizations like SIOP and AOM move forward with new publications or other methods of bridging the gap, they would benefit from similar marketing and evaluation strategies to increase the effectiveness of their efforts.

INVESTIGATE TOPICS OF GREATER INTEREST TO PRACTITIONERS

Although creating new outlets and making better use of existing outlets might help, these steps will not make much difference to practitioners if current research topics do not interest or help them. In fact, given serious time constraints, many practitioners (and academics) only seek information in response to immediate problems. Therefore, if a practitioner enters relevant keywords into Google Search and no academic references pop up, the ability of academics to influence him or her may be lost for a very long time.

For this reason, the gaps mentioned earlier between the content interests of academics and practitioners represent serious impediments to research-based knowledge applications. Combining the findings of Cascio (2008), Cascio and Aguinis (2008), Deadrick and Gibson (2007), Heath and Sitkin (2001), McGrath (2007), Offermann and Spiros, (2001) and Silzer and Cober (2008), the following topics appear to be under-researched relative to practitioner interest: compensation and benefits; role of technology and the Internet in HR management; HR as a functional area; macro and strategic HR; consulting and executive coaching; globalization; workforce planning; diversity and cross-cultural issues; change management; communication; trust;

interdependence; sustainable growth; leading in adversity, and employment branding.

In a perverse sort of self-fulfilling cycle, the fact that relatively little academic research currently exists on most of these topics may make it *less* (rather than more) likely that such research will emerge in the future. Although it might seem a "no-brainer" that topics that are both important in the real world and under-researched by academics should be highly attractive to academic researchers, in reality this is often not the case. This is because current practices in top-tier academic publishing place priority on building upon previous research (e.g., Sackett & Larson, 1990) and, more specifically, building on previous *theory* (e.g., Daft & Lewin, 2008; Hambrick, 2007; Locke, 2007). In this way, the lack of prior research (and/or theory) can become a self-perpetuating condition (although perhaps raising issues in well-regarded bridge journals such as *I/O Psychology* will pave the way for future publications in top-tier academic journals as well).

Thus, barring some major changes in editorial policies and/or academic incentives (to be discussed in greater detail shortly), many researchers will not be willing to devote the time or accept the risk involved in building important new research areas from the ground up. Nevertheless, that is precisely what some of the very best researchers do (see, for example, Eisenhardt's work on technological change and speeding products to market, e.g., Brown & Eisenhardt, 1997, or Eisenhardt & Tabrizi, 1995; or Locke and Latham's excellent research program on goal setting, e.g., Locke & Latham, 1984; Locke, 2007). We need to create incentives for more researchers to make these kinds of investments.

BUILD SOCIAL RELATIONSHIPS BETWEEN ACADEMICS AND PRACTITIONERS

In addition to increasing direct access to research findings, some argue that it is also crucial to work indirectly—that is, by building better social relationships between academics and practitioners. For example, Bartunek (2007) has called for "a relational scholarship of integration" (p. 1328) that is based, at least initially, on social (rather than instrumental) relationships between academics and practitioners. March (2005) made much the same point:

> [The facilitation of cooperation across boundaries is] augmented by appropriate attention to the role of wine, flirtation and play. Among strangers, wine can often usefully antedate talk, play can often usefully antedate work, and meetings of the heart can often usefully antedate meetings of the mind. The grim Puritanism of scholarly work has often been intolerant of such frivolity. As a result, scholarly institutions are often designed incorrectly. Conferences are filled with research papers and commentaries, to the exclusion of bottles of wine and opportunities for casual interaction.... And stories of successful collaboration are filled with rationalizations for it, to the exclusion of records of the vintages consumed. These "irrelevant" facilitators of association provide bases for warm social interactions and thus, ultimately, for scholarly exchange and collaboration. (p. 17)

Once social relationships have been cemented, both the transfer of existing knowledge and the joint creation of new knowledge become easier (Brown & Duguid, 2000; Nonaka & Konno, 1998; Rogers, 2003; Wenger, 1999). Thus, social relationships between academics and practitioners should be pursued alongside more instrumentally oriented ones.

Increasing Belief in Research Findings

The task of increasing belief in research findings is, essentially, one of increasing the credibility and perceived relevance of research to practitioners. This might be accomplished in several ways.

COMMUNICATE MORE PERSUASIVELY AND EMPATHETICALLY

> "The best research is as much at home in *Business Week* or *HBR* as it is in *Administrative Science Quarterly* (*ASQ*) or *AMR*. Of course, the jargon is different. But the fundamental ideas in the best research translate into meaningful implications for both researchers and managers" (Eisenhardt, 1998).

According to a recent review by Podsakoff, MacKenzie, Podsakoff, and Bachrach (2008), Kathleen Eisenhardt has been the most influential management scholar among academics (as assessed by citations in 30 journals, including I/O psychology-based journals such as *JAP*, *PPsych*, and *OBHDP*) over the past 25 years. Eisenhardt is known not only for her many outstanding academic publications, but also for the frequency with which she translates her work for practitioner audiences via books (e.g., Brown & Eisenhardt, 1998) and articles in bridge journals such as *HBR*. Her experience shows that it is possible for very high-quality research to be successfully translated, even if it does not happen very often.

Earlier, I indicated that book publishers use templates or "formulae" for creating best-selling books (e.g., Clark & Greatbatch, 2004). Similarly, there are templates for how academic research might be "translated" to make it more relevant, credible, and actionable for practitioners (e.g., Gruber, 2006; Sommer, 2006). To give one example, Kelemen and Bansal (2002) analyzed eight ways (research orientation, focus, attitude, data collection/analysis, data aggregation, referential system, rhetorical devices, criteria of goodness) in which Brown and Eisenhardt translated their 34-page *ASQ* article on change in high-velocity environments (Brown & Eisenhardt, 1997) into an 11-page article for *HBR* (Eisenhardt & Brown, 1998). For example, for *ASQ*, Brown and Eisenhardt's objective was to challenge existing theoretical orthodoxy, while for *HBR* it was to describe how various companies handled change well; for *ASQ*, the authors expressed an attitude of tentativeness toward their findings, while for *HBR*, they encouraged managers to use time-based pacing in high-velocity environments; for *ASQ*, company names were disguised and charts were constructed to show the same variables for all firms, while for *HBR*, firm names were included and only successful practices were discussed; data from all six firms were aggregated for *ASQ*, while for *HBR*, each firm represented a separate anecdote; for *ASQ*, the paper was organized around problem definition, link to prior theory, methods, results, discussion, and conclusion, while the *HBR* article used anecdotes, illustrations, text boxes providing more detail, few references to other research, and call-out quotes.

Kelemen and Bansal (2002) argue that "a great deal of research is simply being 'wasted,' either because academics are not skilled at translating their findings in a language that appeals to practitioners or because there are no institutional incentives to do so" (p. 204). This suggests the need for multiple translation strategies that include, but also go beyond, translations by academic authors. For example, professional organizations such as SIOP or SHRM might employ their own research translators; researchers might coauthor with practitioners or get practitioner comments before submitting research translations; or a small number of senior researchers who are committed to translation (such as Walter Borman, Wayne Cascio, Gary Latham, Edwin Locke, Eduardo Salas, and Scott Tannenbaum) might serve on boards such as SIOP's "Science for SHRM" to make sure that the job gets done (in fact, these things are already happening; Latham, 2009a). After all, it sometimes only takes

a small number of individuals to create a tipping point (Gladwell, 2000).

In addition to using a template such as the one above or other exemplary research translations as models, a second way in which academics can more effectively translate their work is by becoming, in Latham's (2007a) words, "bilingual":

As a consultant to clients, I don't "do research"—I get involved in projects and interventions. Included in the projects and interventions is a "framework" or "strategy" rather than a method or procedure for seeking answers to questions of importance to the clients, not only to me....Rather than point to the need for a control group, I point to the necessity of being able to show senior management what happened in cases in which we did versus did not implement our proposal....After analyzing the results, rather than discussing an F-test, let alone structural equation modeling, I show managers one or more graphs that make explicit what happened where we did, versus where we did not, implement our ideas. (p. 1029)

A third way in which academics might increase the interest of practitioners is by incorporating emotion, as well as logic, into their communications (Bartunek, 2007). According to Heath, Bell, and Sternberg (2001), ideas that "stick" are "selected and retained in part based on their ability to tap emotions that are common across individuals" (p. 1029). Moreover, arguments that generate *positive* emotions are more likely to inspire serious consideration than those that induce negative ones (Bartunek, 2007), at least in voluntary situations such as practitioners considering whether or not to apply research findings.[8]

If Bartunek's notions are correct, articles that reveal gaps between research findings and practitioner beliefs are more likely to engender defensiveness than motivation to change, unless they are accompanied by empathetic statements as to why such beliefs might seem reasonable, as well as specific implementation steps that provide a sense of positive self-efficacy. As a case in point, the article by Rynes et al. (2002)—which highlighted differences between practitioner beliefs and research findings—did give specific advice, but probably did not do enough to acknowledge that the gaps were understandable and were due, in part, to shortcomings on the academic side. Perhaps it is not too surprising, then, that an executive commenting on the articles said, "As a practitioner, I feel somewhat defensive in commenting upon what is a direct criticism of what we do" (Hansen, 2002, p. 103).

A final way in which academics can make research communications more persuasive is by applying the well-known power of anecdotes or "cases of one" to their own writings. Although anecdotes *alone* constitute very weak forms of evidence (Locke & Latham, 2009; Rousseau et al., 2008), when used in conjunction with large-sample evidence they can make empirical findings more interesting and relevant to practitioners, as well as more useful to academics in their role as teachers.[9] A related tactic would be to introduce selected materials from popular business and news media to show either the importance of a problem or its complexity in practice. Although some academic journals discourage the use of any "popular" citations, I believe that certain uses (such as large-sample descriptive statistics, powerful case examples, or information about the costs of a particular problem) can play an enlightening role and make articles more interesting to both academics and practitioners.

CO-PRODUCE RESEARCH WITH PRACTITIONERS OR CONSULTANTS

Co-production of research with non-academics is likely to enhance the believability of findings for several reasons. First, academics often lack credibility with practitioners, particularly if they do not have significant managerial or consulting experience, because people are more likely to listen to someone who has "walked in their shoes." Thus, co-authored articles (such as the ones in the December 2004 issue of *HRM* or those being planned by the *SIOP Science for SHRM* series) are likely to increase receptivity to research-based ideas.

In addition to boosting credibility, co-production can also result in more novel, creative, and important research insights (e.g., Campbell et al., 1982; Cohen, 2007; Hakel, Sorcher, Beer, & Moses, 1982; Lawler et al., 1985; Rynes et al., 1999; Shapiro et al., 2007; Starbuck, 2006). Unfortunately, however, production and coproduction of published research by practitioners has decreased dramatically in I/O psychology over the past 40 years. For example, between 1963 and 1967, 41.3% of authors in *PPsych* and 31.5% in *JAP* had non-academic affiliations (Cascio & Aguinis, 2008). By 2003–2007, these percentages had fallen to only 14.0% in *PPsych* and 4.7% in *JAP*. This implies that finding practitioner coresearchers is likely to be difficult.

Still, several methods are possible. First, as suggested earlier, professional organizations such as the AOM or SIOP can schedule more sessions at which the sole purpose is for academics and practitioners to network and discuss topics of joint interest (Bartunek, 2007; Cohen, 2007; Shapiro et al., 2007). Second, joint activities can be pursued in the context of executive or evening courses in management or psychology, where students are likely to be full-time employees in some organization or another (Latham, 2007a). Tushman and O'Reilly (2007) suggest that this tactic is likely to prove particularly fruitful in evening or executive programs that are designed for specific (usually large) companies with which the university has a special relationship. Alternatively, researchers might gain access to companies when graduates from full-time programs recommend faculty members as consultants to their new employers (for examples of studies that started this way, see Russell, 2001, or Sutton, 1991). Third, long-term relationships can be developed between colleges and universities, private-sector organizations, and governmental agencies through advisory councils or centers (such as the Center for Effective Organizations [CEO] at the University of Southern California or the Center for Advanced Human Resource Studies [CAHRS] at Cornell). Fourth, individual researchers can offer their technical and analytical skills to consulting firms in return for access to large-sample databases and joint publications in top-tier journals and other venues (e.g., see Harter, Schmidt, & Hayes, 2002, or Schneider, Smith, Taylor, & Fleenor, 1998).

Finally, academics might approach potential coauthors by asking what kinds of problems they are having, right now, in a domain of interest (e.g., recruiting).[10] A conversation might then be built around academic findings that have relevance to those problems, followed by discussions of areas for which neither side currently has good evidence or answers. From that starting point, arrangements might be made for the academic partner to take the lead on writing in consultation with the practitioner, who can provide examples and contextual nuances that improve the paper's relevance, interest, and "stickiness." In the best of cases, this step might be followed up with joint research projects in the "unsolved" areas of the domain.

IMPROVE REWARDS AND RECOGNITION FOR RESEARCH THAT BENEFITS PRACTICE

Another way in which researchers can be enticed to move in different directions is to improve incentives and recognition for applied research or research on topics of interest to practice. One potentially effective method of changing research trajectories is to offer funding for researchers willing to conduct

specific types of research. For example, the SHRM Foundation and the Management Education Research Institute (the research arm of the Graduate Management Admissions Council) have both sponsored research projects in areas deemed important to practice and education. In times of declining public funding (particularly in the social sciences), such programs can induce researchers and doctoral students to address areas that might not naturally arise as extensions of the research currently appearing in top-tier journals, which is presently the stimulus for nearly all published research in I/O psychology (Sackett & Larson, 1990). A closely related tactic is to create professional awards for research with clear practical implications (e.g., SIOP's M. Scott Myers Award for Applied Research) or for researchers whose overall body of work benefits practice (e.g., SHRM's Michael R. Losey Human Resource Research Award or the Management Education Research Institute's Fellowship program). The number of such awards has increased noticeably over the past ten years and may well be part of the reason for increased academic attention to bridging the gap.

INCREASE FLEXIBILITY OF TOP-TIER JOURNALS

Although prizes and professional recognition are nice, under the present academic reward system the strongest predictor of academic salaries is the number of publications in top-tier journals. To the extent that top-tier publications continue to dominate academic rewards in the future (and some authors fervently hope that they do not; e.g., Adler & Harzing, 2009), one of the most effective ways of changing academic research would be to increase the range of content and methods found in top-tier journals. Of course, this is much easier said than done, as illustrated by the trajectory of *OS* over its first 18 years (Daft & Lewin, 2008).

Some researchers believe that one of the most important changes journals could make would be to reduce the increasingly prominent role of theory (versus empirical findings) in determining whether or not an article gets published (e.g., Hambrick, 2007; Starbuck, 2006). Over time, some top-tier journals have moved from merely favoring articles that *use* theory to requiring that articles *contribute to* theory (e.g., Colquitt & Zapata-Phelan, 2007; Sutton & Staw, 1995).[11] Both Hambrick (2007) and Starbuck (2006, pp. 107–113) have argued that imposing rigid standards for theoretical contribution impedes, rather than furthers, scientific progress. Their views are also shared by Locke (2007):

Everyone who publishes in professional journals in the social sciences knows that you are supposed to start your article with a theory, then make deductions from it, then test it, and then revise the theory. . . . In practice, however, I believe that this policy encourages—in fact demands, premature theorizing and often leads to making up hypotheses after the fact—which is contrary to the intent of the hypothetico-deductive method. (p. 867)

The challenge that confronts journal editors is how to loosen theoretical requirements without lowering standards (or, to be frank, status within the academic community). Hambrick (2007) suggested the following alternative standard:

Does the paper have a high likelihood of stimulating future research that will substantially alter managerial theory and/or practice? This new standard would require papers to be—by all appearances—*important.* (p. 1350; emphasis added)

Locke (2007) offered this recommendation: Instead of demanding a theory to start with, the introduction to a research paper could summarize what is known about the phenomenon in question and state the purpose of the proposed study: how it will go beyond what is known. . . . Introductions would be much shorter than they are now, because the author would not need to write pages and pages of justification for hypotheses, so long as it was made clear that something new is being done. Then in the discussion section, the author would do the work of inductive integration—tying together the new findings with what was previously known. This means that much of the material formerly in the introduction, if not discarded, would be moved here. . . . The author could also identify how far along the field is in developing a theory and what more needs to be done (e.g., identify causal mechanisms, identify moderators). (pp. 886–887)

Placing less emphasis on theory – especially in new areas - would almost certainly increase practitioner interest in our findings, not only because different kinds of questions could be asked, but also because there would be more room for reporting the "rich detail about interesting phenomena" (Hambrick, 2007) that is of interest to practitioners and researchers alike (Bamberger, 2008; Bartunek, Rynes, & Ireland, 2006; Cohen, 2007; Guest, 2007; Rousseau & Fried, 2001). Other types of research that might qualify under such revised

standards include first descriptions of new phenomena (Hambrick, 2007) and renewed interest in surveys of current practice (e.g., Bretz et al., 1992; Rynes & Boudreau, 1986; Saari, Johnston, McLaughlin, & Zimmerle, 1988). Although these types of studies are of great interest to practitioners (and, I believe, to many researchers as well), because of the difficulty of publishing them in top-tier outlets, academics have all but abandoned practice surveys to consulting firms that often treat the data as proprietary and/or charge large sums of money for access to their findings. Thus, surveying is perhaps another area in which the strong research skills of academics might be allied with the substantive needs of practitioners to create mutual scientific and practical benefits.[12]

Less emphasis on theory might also result in top journals publishing more articles that "mine" huge databases to spot regularities that have not previously been predicted or detected (Anderson, 2008). Although data mining is pejoratively referred to as "fishing" in most methods classes, the huge amounts of information now available on web sites or in company or consulting databases give us a much better chance than before of revealing patterns that will hold up in subsequent analyses (Ayres, 2008; Baker, 2008). Equally important, the regularities uncovered in this way can provoke speculation about possible causes and, in so doing, may aid future theoretical development and insights about human behavior as well. Still, even in the area of medicine, which is believed to be considerably ahead of management in terms of evidence-based practice, it is very difficult to publish results showing even large practical treatment successes if either the basic science behind the treatment has already been published elsewhere or the underlying reason (i.e., theory) for the effectiveness has not yet been discovered (Begley, 2009b). Among other problems, the fact that such studies are unlikely to show up in top-tier journals decreases the likelihood that medical researchers will learn of the results and start searching for the underlying "whys" of successful practice.

CONDUCT RESEARCH ON PERSUASIVENESS OF RESEARCH FINDINGS

One thing that might be very helpful at this juncture is a stronger research base illuminating what causes practitioners to believe, or not believe, our research. For example, although it is clear that the use of personal anecdotes has been successful in selling the popular ideas of emotional intelligence and the alleged unimportance of *g* (e.g., Goleman,

2000; Gladwell, 2008), it is not clear whether the same strategy would work for selling the *un*popular idea of the *importance* of *g*. If not, such studies would provide additional evidence that the importance of intelligence is a fundamentally aversive idea to most people and, more generally, might begin to illuminate the boundary conditions around effective presentation of research ideas.

In particular, we need much more information about how to successfully present evidence that challenges peoples' prior beliefs, such as those pertaining to the importance of *g* or the greater predictive efficacy of actuarial over clinical decision models. This is a very intractable problem that has been shown to exist even in the hard sciences and medicine (Begley, 2009a), where one would think that disbelief would be less likely to arouse emotional reactions than in the social sciences. For example, Halloun and Hestenes (1985) reported on attempts to change the beliefs of students who had completed a physics class, but who still were not convinced that Newtonian physics was more correct than Aristotelian beliefs about motion. They began by asking the students questions that required them to rely on their theories about motion to predict what would happen in a simple physics experiment. After the students had made their projections, the researchers performed an experiment that demonstrated that their assumptions were wrong. They then asked the students to explain the discrepancies between their ideas and the outcome of the experiment:

> What they heard astonished them; many of the students still refused to give up their mistaken ideas about motion. Instead, they argued that the experiment they had just witnessed did not exactly apply to the law of motion in question; it was a special case, or it didn't quite fit the mistaken theory or law that they held as true. . . . The students performed all kinds of mental gymnastics to avoid confronting and revising the fundamental underlying principles that guided their understanding of the physical universe. Perhaps most disturbing, some of these students had received high grades in the class. (Bain, 2004, p. 23)

That these results were obtained with a far less "hot" topic than the importance of intelligence suggests that it is very difficult to change fundamental underlying beliefs in a wide variety of domains. Therefore, getting people to use behavioral science research involves far more than simply making it available and giving people guidelines for how to apply it. Making research findings more believable

to a general audience is very important to the success of EBM and EBT, particularly since we already know that many people tune out to large-sample empirical evidence. Perhaps I/O psychology, HR, and management researchers can adopt ideas from social psychological research on persuasion and attitude change (e.g., Goldstein, Martin, & Cialdini, 2008; Petty & Cacioppo, 1986), from behavior decision making's emphasis on framing effects (Kahneman, Slovic, & Tversky, 1982) or from medicine's experience with trying to persuade the general public to avoid unsafe behaviors such as smoking or overeating (e.g., Fishbein & Yzer, 2003; Shen & Dillard, 2007). Learning more about why people accept or reject our research findings—and ways to overcome rejection—should be a very high priority for future research.

CONTINUE TO STRENGTHEN OUR RESEARCH BASE

One of the biggest methodological advances over the past 30 years has been the extensive application of meta-analysis (Hunter & Schmidt, 2004) to important questions in I/O psychology and related fields. By combining the results of multiple quantitative studies and correcting for measurement and sampling errors, meta-analyses have been extremely helpful in revealing reliable relationships that formerly seemed to be situation-specific (e.g., the generality of g as a predictor of performance), while at the same time identifying variables that modify the strength or direction of those relationships (e.g., job complexity).

Nevertheless, despite the advances of meta-analysis, there are still conflicting results in many areas, preventing practitioners who might like to use available research from drawing clear implications. To illustrate the problem, I offer an example from my own research. When Amy Colbert, Ken Brown, and I first initiated the project that eventually led to Rynes et al. (2002), our first step was to identify clear, generalizable findings in the seven areas of HR tested by the HR Certification Institute. All three of us were surprised at just how difficult it was to do this. Eventually, we generated a list of nearly 50 such statements, which we then pre-tested for broad consensus using members of the editorial boards of *JAP* and *PPsych*. This pre-test whittled the list of agreed-upon items to 39, which were then responded to by a practitioner sample. By the time the article went to press a year later, another four items had to be removed due to new findings or new critiques of previous work that threw the validity of earlier findings into question.

In short, when we tried to answer Oprah's trademark question, "What do we know for sure?," it seemed to add up to a rather thin list of findings—and not always about terribly important issues. Similar sentiments have been expressed by other academics who suddenly became managers and tried to apply research findings (e.g., Billsberry, 2008; Pearce, 2004).[13]

Some of the conflicting findings that prevent us from being able to draw strong conclusions continue to stem from methodological shortcomings, such as small sample sizes, low response rates, convenience sampling, self-report data, violations of methodological assumptions, common method variance, and varying levels of range restriction (e.g., Edwards, 2008; Starbuck, 2006). As indicated earlier, one of the most effective solutions to sample size and response rate problems would be increased collaboration between academics and other entities such as government, professional associations, consulting firms, or corporations (e.g., Cascio, 2008; Van de Ven & Johnson, 2006; Wall & Wood, 2005). Another helpful change would be to increase the number of studies employing either crucial experiments (Platt, 1964) or triangulated methods (Jick, 1979; Starbuck, 2006), both of which can help in resolving discrepant findings or revealing how statistical relationships "play out" in field settings (e.g., Edmondson, 1996; Latham et al., 1988; Sutton & Rafaeli, 1988).

However, in addition to methodological shortcomings, other factors (such as clashes of ideologies or selective sampling of prior literature) also lead to lack of consensus in the field (Tranfield, Denyer, & Smart, 2003). This has caused some to argue that we need new forms of research cumulation, in addition to narrative reviews and meta-analyses. For example, the management and medical literatures (particularly in the UK) have been experimenting with various types of *systematic reviews*, such as realist synthesis and meta-ethnography (e.g., Tranfield et al., 2003; Rousseau et al., 2008).

Although a full discussion of these methods is beyond the scope of this chapter, in general these additional forms of synthesis aim to review more comprehensive collections of research than meta-analyses (e.g., both quantitative and qualitative studies of varying levels of evidentiary quality) and to provide highly transparent, detailed information about how all methodological decisions were made. In addition, emphasis is placed more on reviewing evidence from an operational perspective (i.e., "What works?") than is usually the case in meta-

analysis and narrative reviews, where the primary goal is often to understand underlying processes more than to improve operational effectiveness. Finally, the emphasis on including studies with very diverse methods in amassing the original sample is designed to ensure that controversial areas and critiques of dominant views are not glossed over. Two potential challenges associated with these types of reviews are: (a) the considerable time and effort required to generate them, and (b) the "translation" of such massive documents into formats likely to encourage implementation.

BECOME MORE EFFECTIVE TEACHERS OF RESEARCH METHODS, STATISTICS, AND CRITICAL THINKING

To this point, my suggestions have focused on how academics might modify their research to make it more relevant and believable to practitioners. However, it is also worth considering whether academics, in their dual role as teachers, might become better at helping students become more informed consumers of research (e.g., Burke & Rau, 2010; Rousseau & McCarthy, 2007; Trank & Rynes, 2003). As Abrahamson and Eisenman (2001) and Highhouse (2008) have noted, one reason that practitioners often are not persuaded by academic research is that they have little understanding of why some types of evidence are stronger than others. Until more practitioners truly understand the power of large samples vis-à-vis anecdotes, case studies, and clinical opinions, academic research will continue to fall mostly on deaf ears.

There is considerable evidence that many people distrust statistics because they do not understand them and, indeed, are quite "afraid" of them (Ayres, 2008; Paulos, 2001). Moreover, this lack of understanding produces a sense of threat that can cause defensive or self-protective reactions when confronted with statistical evidence (Bain, 2004). However, many educational experts believe that widespread math anxiety and statistics phobias are not inevitable, but rather have been created by ineffective textbooks and teaching methods (McDonald, 1987; Tobias, 1995).

Given that most students (at least in the United States) enter college with math phobia, it seems highly likely that many students (i.e., future managers or other practitioners) also leave college without a solid grasp of research methods, statistics, and analysis. And, as is true of lower educational levels, ineffective teaching may again be a large part of the problem. For example, the typical statistics or methods text is filled with equations and problems that have little to do with substantive content areas of interest to students. In addition, much is made of issues that, in practice, are often irrelevant (or worse still, misleading), such as the difference between a sample and a population (since most of our data come from convenience samples rather than random or even representative subsets of identifiable populations) and whether or not a difference is "statistically significant" (since the assumptions underlying such tests are almost never met and the results depend heavily on sample size). Finally, there is an overemphasis on what Bain (2004) calls "chug and plug"—memorizing formulae and plugging numbers into equations, rather than problem solving or applying the material to real-world contexts.

The idea that there might be better ways of helping students learn—and that research on teaching and learning can help us answer such questions—has led to the creation of new journals (such as *Academy of Management Learning and Education* [*AMLE*] and *Decision Sciences Journal of Innovative Education* [*DSJIE*]) that may do much to help us improve the effectiveness of our teaching, particularly in technical areas. For example, Aguinis and Branstetter (2007) recently published an article about an effective, empirically validated way of teaching the concept of the sampling distribution of the mean, while Corner (2002) offered a hands-on way to teach research design.

Another way to improve student learning in statistics might be to use popular press books such as *Super Crunchers* (Ayres, 2008) or *Innumeracy* (Paulos, 2001) in conjunction with more formal statistical texts. These books do not delve into the details of the math behind statistical tools, but rather illuminate a variety of ways in which these tools can (and are) being used to make decisions in many areas of life. As such, they richly contextualize the abstract material found in more formal texts. Although formal evaluative research needs to be conducted, anecdotal reviews of these books suggest that they might go a long way toward contextualizing and motivating the study of statistics:

> [*Super Crunchers*] is really interesting because of how it relates to quantitative tools that can be put to use in amazing ways. I read the book in conjunction with a textbook for a MBA level statistics class. I highly recommend it for anyone as a way of seeing the numerous ways that numbers are put to use, most ways (*sic*) I have never even thought of. (review on Amazon.com)

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Another possibility is to think about whether, at educational levels below the Ph.D., the goal should be as much (or more) to produce critical thinkers and informed *consumers* of research (e.g., Abrahamson & Eisenman, 2001; Burke & Rau, 2010, rather than would-be elementary statisticians. Much progress might be made, for example, by teaching students or practitioners how to generate and interpret different types of evidence claims (e.g., Billsberry, 2008; Lehman, Lempert, & Nisbett, 1988), even if they do not learn how to conduct formal research. For example, exercises might involve discussing and critically evaluating the validity and reliability evidence provided by a test vendor, or creating structured interview questions to determine the qualifications of consultants seeking to design an attitude survey.

To the extent that courses also cover the actual conduct of research, students at sub-doctoral levels may benefit more from assignments that ask them to construct an opinion survey or run a focus group than how to test for statistical significance or correct for measurement error (see also Billsberry, 2008). With careful planning, these types of learning goals could be embedded in the context of specific functional-area courses (e.g., employee selection, compensation, or performance management), in addition to (or perhaps in place of) dedicated statistics or analysis courses.

Increasing Implementation of Research Findings

Many of the suggestions made to this point will not only help with accessibility, interest, and believability, but also are likely to improve the chances that more practitioners will actually implement research-consistent practices. For example, as part of the e-mail conversations among members of the SIOP Science for SHRM board, Jeff McHenry offered the following observations:[14]

I was thinking about how HR policy and practice gets set in a large company, based on my experience (which is limited to N = 2 organizations).
Typically, there are program managers working in centers of excellence who do external research and benchmarking as they start to think about design principles. What are the types of information that they find useful?

Magazine articles—especially those that include both design principles and case studies

Summaries of research and best practices—white papers are OK, but most tend to like PowerPoint decks better (probably because they're more concise,

the info is more user-friendly, and the slides can be reused by the program managers in presentations they make to their senior management)[15]

Webinars and podcasts—particularly if the speaker knows his/her facts, is somewhat entertaining, and has lots of good examples/case studies

Conferences—similar speaker attributes.

Thus, previous suggestions to publish in practitioner outlets, to consider alternative formats, to include case studies, to describe processes, and to communicate in more interesting ways may increase use of research findings at the same time that they improve accessibility, interest, and credibility.

Once again, coproduction strategies are likely to be useful for improving the odds of implementation (Amabile et al., 2001; Hakel et al., 1982; Jelinek, Romme & Boland, 2008; Mohrman et al., 2001). Indeed, Amabile et al. (2001) suggest that some types of research are unlikely to be accomplished at all without coproduction. Coproduction ensures, at the very least, that practitioners are interested in a topic and are potentially willing to act on what is discovered. However, both Amabile et al. (2001) and Mohrman et al. (2001) indicate that initial interest is insufficient to ensure action. Rather, moving to actual implementation requires trust between academics and practitioners, attention to group process, true two-way dialogue (rather than the top-down discourse embedded in the "principal investigator" model), and considerable "joint sense-making" regarding the data. Moreover, implementation may not extend far beyond the organizations that are initially involved in the research because of the "not invented here" syndrome. As such, other methods also need to be pursued.

PRESENT ADVICE IN THE FORM OF PRINCIPLES PLUS EXAMPLES

Many academics are profoundly uncomfortable with moving from description to prescription (e.g., Bazerman, 2005; Kelman, 2005; Mohrman et al., 2001). The kind of bold pronouncements offered in some management best sellers make academics cringe, realizing that social and organizational worlds are highly complex and difficult to either change or predict.

Still, if academics hope to increase the extent to which managers and other practitioners act on the basis of evidence rather than hunch or myth, they must be willing to offer suggestions based on the best available evidence. Furthermore, those suggestions must be neither too complex (Locke, 2009), nor too simple (Bartunek, 2007). Thus, academics who

wish to encourage evidence-based practice must find a way to simplify complexity enough that practitioners will be encouraged to act, but without promising certainty or making unsupportable claims.

Locke (2009) argues that one good way to do this is by offering "principles." Principles are "general truths on which other truths depend…a principle may be described as a fundamental reached by induction" (Peikoff, 1991 p. 218, quoted in Locke). Among the principles offered in Locke's *Handbook of Principles of Organizational Behavior* 2009) are the following: (1) select on intelligence because it is the single best predictor of differences in individual productivity (particularly for high-skilled and knowledge-based jobs); (2) job satisfaction is an important predictor of life satisfaction, and mental challenge is a key factor in job satisfaction; and (3) setting specific, challenging goals for employees is a very effective motivational technique, but only if certain procedures are followed (Rousseau, 2009.

Attempting to elevate the concept of principles to a higher level of abstraction, Pfeffer and Fong (2005) suggested we use "*first* principles" to build general theories that help people (both practitioners and students) "see the connection among diverse, apparently unrelated, topics," because "there are enormous benefits for memory and understanding from coherent, integrated theoretical structures of thought" (p. 373). As an example, they show how the first principle of "self-enhancement" can be used to explain a wide variety of power and influence-related phenomena, including escalation of commitment, similarity attraction, in-group favoritism, the disinhibiting effects of power, and the persistence of hierarchical structures. They also reference other researchers' attempts to build unified explanations from first principles, such as Lawrence and Nohria's (2002) attempt to explain wide swaths of human behavior in relation to "four innate drives—the drives to acquire, to bond, to learn, and to defend" (p. 5). The main point is that, in addition to explaining particular phenomena or solving particular problems one at a time (i.e., an emphasis on dependent variables), Pfeffer & Fong (2005) believe that we should also look for unifying causes or principles (i.e., independent variables) that are associated with multiple outcomes or effects.

Having said this, however, with respect to increasing implementation of research-supported practices, it is probably more useful to frame articles around particular problems (such as turnover or theft) or specific HR functions (such as recruitment or compensation). This is because practitioners are more

likely to search for information using problem- or function-based terms than terms associated with principles (e.g., self-enhancement) or first principles (innate drives). As such, perhaps the most promising use of "first principles" will be in teaching or training venues, where they can be introduced at the beginning of a course or session and then integrated into discussions of multiple applications to problems or functional areas.

IMPROVE "IMPLICATIONS FOR PRACTICE" SECTIONS IN PRIMARY RESEARCH STUDIES

The strongest implications for practice flow from aggregations of many research studies, such as meta-analyses and systemic reviews (Hunter & Schmidt, 2004; Locke, 2009; Rousseau et al., 2008). Still, there is also room for improving the way in which researchers formulate implications for practice in the context of single studies. Although it may not be appropriate to require implications for practice in all articles (since that might introduce further rigidity in publishing practices and cause premature prescription; see Locke, 2007), it would be good to think about how these sections might be made more useful, where appropriate.

At present, implications for practice—where offered—are often both "thin" and quite generic. For example, based on a review of articles in *AMJ* for the year 2006, Bartunek (2007) found that 36% had no implications for practice, even though improving practice is part of *AMJ*'s mission. Of the articles that did contain implications for practice, the most common recommendation (in 38% of the articles) was for practitioners to become "more aware" of some phenomenon (examples included "monitoring" demographics, "understanding" how to make governance decisions, or "being cognizant" that HR practices can be used to build human capital). The other three most common types of recommendations were to provide training (21%), to influence others' interactions in some way (17%), or to either increase or decrease employee heterogeneity (17%). Overall, Bartunek (2007) concluded:

> When considered as a whole, much of the advice given in the 2006 *AMJ* articles is not easy for managers or other practitioners to apply. Recommendations to pay special attention to a phenomenon do not help a manager know what to do in response to it. Moreover, little of the advice includes rationales for intended actions, even though there are extensive conceptual rationales for the studies whose findings lead to the proposed

actions. That is, implications are typically suggested in a decontextualized, distant way. Some of the advice would appear to many readers to be contradictory, and some of it is simply hortatory. (pp. 1325–1326)

AQ1

A recent extension of Bartunek's study (Bartunek & Rynes, 2009) to five journals (*AMJ*, *JAP*, *JOB*, *OS*, and *PPsych*) over two different time periods (1992–1993 and 2003–2007) showed that the number of articles containing "implications for practice" has grown over time in all of these journals, although they increased much more in some journals than others (specifically, *PPsych* increased from 34% in 1992–1993 to 79% in 2003–2007; *JAP* increased from 29% to 58%; *AMJ* from 27% to 55%; *JOB* from 40% to 58%, and *OS* from 46% to 47%). In this broader sample of journals, the top four implications for practice were also a bit different. Specifically, although "increasing awareness" and "training" were still the top two recommendations, the next two most common pieces of advice were to change the design or structure of something (e.g., an organization, a workgroup, or a career) and to change selection or hiring procedures. On the less encouraging side, the grade level required to read "implications for practice" sections increased by nearly a full grade (from 16.6 to 17.5) between 1992–1993 and 2003–2007.

Another (less formal) finding by Bartunek and Rynes (2009) was that when they presented the results of this paper in various research seminars, many management academics admitted discomfort and uncertainty about whether, and how, to write good "implications for practice" sections. Especially for those academics who conducted lab studies and/ or were not interacting very much with practitioners, there was a reluctance to claim that they had discovered anything that could really be "of use" to practitioners. Some academics who read earlier versions of this chapter also commented that implementation is a weak spot for many academics.

Given these reactions, several actions might be useful. For example, it might be helpful to produce a book or a special journal issue of essays by true "scientist-practitioners" about how they apply research findings in practice (i.e., application exemplars). A good example of this type of essay with respect to performance measurement can be found in Tannenbaum (2006). (A similar effort with respect to how academics use research findings in the classroom can be found in some of the essays in André and Frost's [1997] *Researchers Hooked on Teaching*.)

In addition, journals might encourage authors to include implications for teaching as well as implications for theory and practice (Rynes & Trank, 1997). In her presidential address to the AOM, Rousseau (2006) said:

> (T)he most important reason evidence-based management is still a hope and not a reality is not due to managers themselves or their organizations. Rather, professors like me and the programs in which we teach must accept a large measure of blame. *We typically do not educate managers to know or use scientific evidence.* (p. 262; emphasis in the original)

Providing academic readers with guidance as to how research findings might be used in their teaching (perhaps including one or two examples of firms that seem to do such things well) would constitute one step toward EBT. Of course, professors with primarily teaching (as opposed to research) interests would still have to *read* research findings for this to have any impact. At present, many apparently do not (Rousseau, 2006), which suggests that academic research often fails to reach professors as well as managers and other practitioners.

Given this situation, a different solution would be to combine popular case and experiential learning methods with texts based explicitly on research principles (e.g., Latham, 2009b; Locke, 2009; Pearce, 2006). In this approach, the review, selection, and integration of research findings would be delegated to textbook authors. However, since there is wide variability in the extent to which textbooks are truly research-based (Stambaugh & Trank, 2009), successful implementation of this strategy will require both that (a) instructors value research findings and (b) have the means to assess the research-based credibility of textbook authors.

INCREASE APPLICATIONS RESEARCH AND RESEARCH CONTEXTUALIZATION

The fact that many researchers seem to be uncomfortable writing implications for practice reflects a deeper problem as well. Specifically, the most common methods employed in many areas of I/O and management research are relatively weak in terms of either internal validity, generalizability, or both. For example, Bartunek and Rynes (2009) found that authors who conduct lab studies are particularly uncertain about how generalizable their results are to the field—a concern that is shared by practitioners (Boehm, 1980). In addition, using the widely researched area of organizational justice as an example, Greenberg (2009) argues that

"implications" studies—that is, those that show correlations between justice perceptions and some other variable—are weak sources of true implications because they neither show practitioners how to change perceptions nor convincingly demonstrate that it will matter to organizational outcomes if they do (p. 183).

As such, Greenberg (2009) argues that in order for practitioners to "*do* something" with our research, we need far more "application" or intervention studies in which researchers "introduce organizational practices believed to promote justice and then assess the effectiveness of those practices" (p. 184). Applying this distinction, Bauer et al. (2009) examined 545 justice studies published over the past 15 years and found that implication studies outnumbered applications by a factor of 25 to 1.

Of course, Greenberg (2009) is hardly alone in his call for more applications or intervention studies. Indeed, studies for intervention or "action" research go back more than half a century (e.g., Lewin, 1946; French & Bell, 1973). Many who have pondered the need for such studies grapple with the fact that pure experimental designs are rarely possible in field settings, challenging researchers to find alternative designs that nevertheless reduce various threats to internal validity (e.g., Cook & Campbell, 1979; Sackett & Mullen, 1993). Others place less value on purity of design and more on engaging with organizational participants in a series of steps involving pragmatic experimentation, implementation, and evaluation, followed by further experimentation and implementation based on what is learned in the process (e.g., Cooperrider & Srivastva, 1987; Romme & Endenburg, 2006; Van Aken, 2004).

Even in the absence of intervention studies, I/O and related research would be more appealing to practitioners (and more useful to future researchers) if it were less decontextualized (e.g., Bamberger, 2008; Johns, 2001; Porter, 2008; Roberts, Hulin, & Rousseau, 1978; Rousseau & Fried, 2001). Greater contextualization is likely to improve the prospects for implementation in several ways. First, contextualized studies are usually conducted in real organizational settings, which inherently give them more credibility with practitioners than convenience samples. (Convenience samples require "averaging across" or "controlling for" contextual factors, rather than treating them as potentially important pieces of information). Second, in contextualized studies, authors are more likely to tell what happened, how, why, and whether it worked or not (Gephart, 2004). Given this concrete information, readers can decide whether the context seems relevant to their own setting and, if not, whether the "what or how" might be adapted to fit their own situation (Collins, 2004). In other words, contextualization provides a concrete baseline for practitioner reflection, modification, and, potentially, action (e.g., Czarniawska & Sevon, 2005; Schön, 1995).[16]

Relatedly, the importance of context to practitioner receptivity suggests that in order to increase the odds of implementation, we need to match our research contexts more closely to the current distributions of industries, organizational sizes and structures, and so on. Not surprisingly, practitioners are more likely to notice, read, and act on ideas that are presented in a context that reflects their own working environment (e.g., Guest, 2007). Thus, for example, public sector managers prefer to read articles that are tailored specifically to their unique context (Feldman, 2005; Kelman, 2005), as do managers in health care (e.g., Greenhalgh, 2006) and other fields. Closer to home, Weimer (2006) revealed the same preference among academics in their search for literature on the "practice" of teaching. For example, academics in business schools are far more likely to read *JME* or *AMLE* than general education journals, even though many of the findings from the education discipline have relevance for management and psychology.

Given this well-established preference for context-specific research, a recent study by O'Leary and Almond (2009) suggests that managers in certain sectors of the economy are likely to be "underserved" by recent organizational research. Specifically, based on a sample of 914 field studies published in *AMJ, ASQ, JOB* and *OS,* they found "striking, persistent, and growing discrepancies between the industries that are economically important and the industries that have served as settings for organizational research. For example, education and manufacturing are oversampled in relation to their economic importance, while real estate, construction, wholesale, and retail are undersampled" (p. 1). Management of the public sector also appears to be dramatically undersampled, as are small businesses (e.g., Kelman, 2005; Pettigrew, 2005). More generally, Walsh, Meyer, and Schoonhoven (2006) argue that we have over-studied old organizational forms and under-studied new ones (e.g., global, disaggregated, organizations with ever-increasing reach into public and private lives). Thus, situating more research in new-style organizations is likely to enhance practitioner interest in I/O and related research.

PROVIDE SPECIFIC SUPPORT FOR IMPLEMENTATION

Other than coproduction of research, perhaps few things would help more to increase implementation of research ideas than some sort of "help line" or chat room for those who are considering a change but have specific questions or problems with respect to implementation. The popularity of such help lines can be seen at SHRM, where their Knowledge Advisors handled 114,458 inquiries by phone, e-mail, and live chat in 2008 and provided 447,000 Express Request e-mail responses for the most common types of inquiries (D. Cohen, personal communication, March 10, 2009). Another useful site for benchmarking, chats, and interest groups is HRM the Journal (http://www.hrmthejournal.com), which at the time of this writing has nearly 1,700 members and 29 active discussion groups.

Although neither academics nor consultants can be expected to provide free advice indefinitely, it might be very helpful if the authors of SIOP's research reports for practice would make themselves available for subsequent questioning for at least some minimal period of time after publication on the SIOP, SHRM, and HRM the Journal web sites.

Future Directions

A wide variety of suggestions for future research and practice have already been offered in the previous sections on "solutions." Here, I highlight some of the areas that I consider to be most important for bridging the academic-practice gap:

• One important area for future research is to find the most effective ways to communicate research findings, particularly when those findings run counter to what people currently practice or believe. Given the well-known tendency for people to filter out non-self-affirming information, it is important for researchers to explore ways of breaking through self-protective defenses. Previous research suggests that this is extremely difficult to do, but because it is so crucial, research that sheds light on this issue would make a very valuable contribution.

• Means need to be found to reward, or at a minimum, not to punish, academic-practitioner boundary spanning. Although it is commonly assumed that interactions with practitioners are likely to reduce academics' research productivity, Podsakoff et al.'s (2008) list of the most-cited management scholars—in combination with

autobiographical evidence regarding the origins of their research programs (e.g., Bedeian, 2002)—makes it clear that most researchers with the very highest citation rates regularly engage with practitioners (e.g., Kathleen Eisenhardt, Donald Hambrick, Charles O'Reilly, Jay Barney, Michael Hitt, Jane Dutton, and Frank Schmidt). Based on real or imagined pressures from their universities, many academics may be spending too much time protecting themselves from "intrusions" on their research time while missing out on truly exceptional research opportunities (Campbell et al., 1982). Given that relationship building is very important to successful collaborations, boundary-spanning activities should be given more weight in academic reward systems than is generally the case. Senior faculty need to take the lead in pushing for increased value on boundary-spanning activities and research with practical implications (McGrath, 2007) so that doctoral students and junior faculty—typically the source of innovation in most fields—are not discouraged from engaging with the broader community.

• Doctoral programs need to make better use of Ph.D. students' pre-doctoral program experiences. At present, many students feel that Ph.D. programs try to "stomp out" their previous experiences and interests, rather than incorporating or building on them (e.g., Bartunek et al., 2003; Dutton, 2003; Empson, 2007; Vermeulen, 2007). It is important to honor and encourage the passionate interests and big questions with which many Ph.D. students enter our programs.

• It is crucial to build a research base examining processes and outcomes of various forms of evidence-based teaching. Academics also need to empirically evaluate the effectiveness of attempts to teach basic principles of research methodology or critical thinking skills to on-campus students so that they become better consumers of information after graduation (for an early example, see Lehman, Lempert, & Nisbett, 1988). It would also be valuable to examine the extent to which academic coursework changes students' beliefs, attitudes, or mental models, since much of the behavioral science curriculum is designed to influence these variables (Rynes & Brown, 2011).

• Future research should also examine the role of positive versus traditional (i.e., gap- or deficiency-based) approaches in changing behaviors: Is one more effective than the other in reaching across academic-practice boundaries? Both academics and practitioners are increasingly

experimenting with positive approaches to change (such as appreciative inquiry and positive psychology), which use self-affirming techniques designed to reduce defensiveness and resistance to change (Austin & Bartunek, 2003). Assuming that positive approaches are indeed successful in reducing defenses, does this reduced defensiveness translate into more, or less, eventual change? What happens when appreciative inquiry bumps into traditional management? Evidence on this point would be very helpful, since positive approaches are very common in ODC consulting but have rarely been evaluated in terms of subsequent quantitative outcomes (Yaeger et al., 2005).

• We need more research on how managers make decisions about program adoption or non-adoption. Without a clearer understanding of how I/O, HR, and related practitioners decide which interventions to pursue, there will be much more guesswork than necessary in trying to find effective ways to share information across academic-practice boundaries. Latham (2007a) suggests that we "conduct research on the adoption and diffusion of human resource research findings in the workforce"…because…"through myriad laboratory experiments, simulations, and field studies, ways to transfer HRM knowledge to HRM practice are likely to be discovered" (p. 1028). I agree.

Conclusion

Here in my adoptive home state of Iowa (in the midwestern United States), people like to say: "If we build it, they will come."[17] The general idea is that if someone produces a valuable service or product, people will buy it. This is akin to the "push" model of research dissemination, which is the primary strategy that I/O and related academics have been using to try to influence practitioners through their research. At the present point in time, this strategy seems to be meeting with rather limited success (e.g., Cohen, 2007; Guest, 2007; Johns, 1993; Lapointe, 1990; Rynes et al., 2002; Silzer & Cober, 2008). At a very minimum, the present push needs to be combined with a "pull" strategy, studying the issues that interest practitioners (e.g., Cascio & Aguinis, 2008; Deadrick & Gibson, 2007; Rynes et al., 2007) and providing more inputs in the form of problem-solving or decision aids (in addition to the more customary topical reviews).

Perhaps a more useful operating principle for narrowing the gap would be "It takes a village." First, there is an emerging consensus that we need to tackle the gap on multiple fronts—building

personal relationships, using multiple outlets and creating new ones as needed, producing various types of translations and forums for sharing (Cohen, 2007; Latham, 2007a; Rousseau, 2006; Rynes, 2007). Given these myriad needs, many individuals need to get involved. Second, a village implies "community"—not two separate communities, but two "blended" communities with at least some overlapping interests. The role of community and relationships in producing trust, sharing ideas, and generating new knowledge and processes should not be underestimated (Bartunek, 2007; Dutton & Dukerich, 2006; March, 2005).

Notes

1. Whether these discrepancies between practitioner perceptions and research findings are due to lack of *awareness* (as opposed to lack of belief, despite awareness) is not entirely clear. However, I reserve discussions of gaps due to differences in *beliefs* for the next section, which reviews research that directly assesses practitioners' beliefs following direct exposure to research findings.

2. One important limitation of this study is that the *JOB* board consists mostly of academics. As such, their notion of the "ideal" research portfolio may diverge considerably from that of practitioners.

3. The scientist-practitioner model is a training model for graduate programs that focuses on creating a foundation for both scientific research and practice. It was developed primarily to train clinical psychologists, but has also been the inspiration behind many I/O psychology programs. Under this model, graduate students are trained to be both scientists (i.e., researchers) and practitioners who apply their knowledge and techniques to solve organizational or client problems (e.g., Dunnette, 1990; Fleishman, 1990; Shapiro, 2002).

4. Interested readers are encouraged to read Pinker's entire book, as its arguments are complex and cannot be adequately explained in this space-limited forum.

5. Abrahamson (1991) defines a "fad" as an imitation or diffusion process that is determined primarily by organizations themselves, in contrast to "fashions," where external organizations (e.g., consultants or government agencies) strongly influence imitation and diffusion. For purposes of this paper, I will use the terms interchangeably.

6. Not everyone agrees with this point. For example, Kieser (1997) and Clark & Greatbatch (2004) suggest that performance "gaps" and management "needs" are so thoroughly "manufactured" that no separate theory (beyond a theory of aesthetic fads and fashions) is needed to explain management fads.

7. Placement of particular "solutions" under one component of the gap rather than another is somewhat subjective, since neither the components of the gap nor their solutions are independent of one another.

8. In authoritarian contexts such as work, receiving negative feedback about one's performance (which, at least in the short run, can produce negative emotions and cognitive defenses) generally produces more subsequent improvement than receiving positive feedback (e.g., Atwater, Roush, & Fischthal, 1995; Johnson & Ferstl, 1999; Rynes et al., 2005). However, many of the incentives to improve performance in the face of negative feedback from supervisors or peers are not operative in the case

of practitioners simply reading research accounts that suggest adopting alternative practices.

9. The "power of one" or anecdotes has also been shown in the context of charitable giving. For example, people give more money to charities that highlight a specific child in need than to appeals emphasizing that millions of children are starving in some region of the world (Singer, 2009).

10. Thanks to Craig Russell and Alison Eyring for these ideas.

11. In the interest of full disclosure, I formerly edited a top-tier journal, *AMJ*, which also included "contribution to theory" as a core requirement for publication. Although our editorial team broadened the range of articles that were considered to contribute to theory (see Rynes, 2005), in retrospect I believe we should have gone even further toward reducing the emphasis on theory, along the lines suggested by Hambrick (2007).

12. However, one possible impediment to this cooperation might be that due to top-tier journals' emphasis on issues such as construct validity and reliability, academic-designed surveys might be longer and more complex than those produced by professional organizations or consulting firms, thus reducing response rates (see, for example, Tannenbaum, 2006).

13. Others are more optimistic about the number of implications we can draw for practice from our research (e.g., Locke, 2009; Miner, 2007; Rousseau, 2009). The difference may be that it is easier to draw some "general principles" (which nevertheless are not always true) than to come up with statements that are either true or false under all known conditions.

14. It would be helpful to have the insights of other practitioners as to how new practices get adopted in their own organizations, perhaps using qualitative methods (e.g., structured interviews to track specific instances of implementation) to obtain greater detail than is currently available.

15. Support for this notion can be seen in a recent study (Haynes et al., 2009) published in the online version of the *New England Journal of Medicine,* which embedded links to Power Point slides to encourage the use of a 19-item checklist that the authors found to reduce surgical deaths by 40%.

16. Two excellent examples of highly contextualized implementation studies in the area of reward systems are Petty, Singleton, & Connell (1992) and Wageman (1995).

17. This is a reference to the 1989 movie, *Field of Dreams*, voted the sixth-best fantasy film of all time. A plot synopsis can be found at http://en.wikipedia.org/wiki/Field_of_Dreams.

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