Title
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Journal
Academy of Management Learning and Education, 11(2)

ISSN
1537-260X

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Publication Date
2012-06-01

DOI
10.5465/amle.2011.0554

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The Decreasing Value of Our Research to Management Education

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For centuries we have expected the best teachers also to be scholars. *Scholar* comes from the Old English word for student, someone who is committed to learning. This tells us how scholars are expected to approach their teaching: Scholars know the latest knowledge, seek to contribute new knowledge, and are always learning. Despite worries that good scholarship and good teaching vie for faculty members' time and attention (e.g., Clark, 1997; Massy & Zemsky, 1994), for centuries the best universities have expected their teachers to be scholars and their scholars to be teachers. Echoing this expectation, Becker and Kennedy (2005) argued that creating knowledge and imparting knowledge are complementary activities. This is based on the belief that their students should be taught what is current, and that those who are actively learning are seemingly less prey to the arrogance of assuming they already know everything about a subject. Doing scholarship should make us humble about how limited our understandings are, and in this ideal educational setting, teachers and students work together to explore new ideas and accept that their knowledge is always tentative. Of course, this ideal (or what Marsh & Hattie, 2002, called "this myth") too often fails, and those failings have been extensively discussed (e.g., Burke & Rau, 2010). Here we do not question whether scholars make better teachers or offer suggestions about how to improve scholarship (e.g., Loyd, Kern, & Thompson, 2005), or teaching (e.g., Christensen & Carlile, 2009), but focus on the decreasing usefulness of management scholarship to management teaching.

The practice of scholarship should do more than make scholars more humble teachers; scholarship is expected to be more than an activity done for its own sake. Society pays for scholarship in the hope that it will produce new knowledge, some of which teachers can share and discuss with their management students and with the practicing managers in their executive education sessions. While not all knowledge need have immediate practical value, we expect that some management research knowledge, as an applied social science, can be shared with our students. As we detail below, there is a role for research that increases other scholars' understanding of sometimes esoteric issues. However, management is not molecular biology, and our editors' demands for implications-for-practice sections in the scholarly articles they publish reflect a general expectation for useable knowledge in much of management research. Here we present evidence that our scholarship is increasingly failing us as teachers: The research published in our best scholarly journals has become less actionable over time, and so, less useful to our students. We ask, "Has management research become less useful to our students and practitioners? If so, what might we do as individual scholar/teachers and as a community of educators to respond to this trend?"

THE RELEVANCE OF MANAGEMENT RESEARCH

Our argument follows a long history of complaints that management research lacks relevance to practicing managers. More than 50 years ago the Ford (Gordon & Howell, 1959) and Carnegie Foundations (Pierson, 1959) argued that American university business schools should shift from experienced executives as teachers toward more emphasis on the scholar-teacher model of education prevalent in the other social sciences. Aided by the substantial funds provided by those foundations, American management schools made that shift beginning in the 1960s. Yet, ever since, there have been questions about

The authors wish to thank Ann Clark for her research assistance; and Jonathan Doh, Jean Bartunek, and the anonymous reviewers for their feedback and encouragement.

whether management research is relevant to practicing managers and our management students.

We cite just a few of those concerns over "the lack of relevance": Porter and McKibbin (1988) questioned whether much of the research produced by business school faculty was useful to practice. Hambrick (1994) lamented that there was a "closed incestuous loop" among scholars whereby they were both the producers and consumers of scholarship that did not address managers' needs. Aldag (1997) and Beyer (1997) suggested several ways management research could be made more relevant to management practice. The editor of the *Academy of Management Journal* hosted a forum to encourage more research relevant to practice (Rynes, 2007a). More recently, Bartunek and Rynes (2010) reported that a student-assistant with managerial experience coded only 8% of the scholarly articles proposing the practical implications of management articles as actually useful to managers.

If too little of our research is relevant to practicing managers, it will not be of use in our teaching, as has been argued by the first author (Pearce, 2004), who had to rely on folk wisdom to teach experienced managers (see also Pearce, 2006, 2009). Most students enroll in universities expecting to learn something relevant to their lives. This is especially the case for management students—many of whom are experienced or working MBA students looking for practical solutions.

ADDRESSING THE GAP BETWEEN MANAGEMENT RESEARCH AND PRACTICAL RELEVANCE

Those decrying the relevance of management research have proposed many excellent practical ideas to address the problem. For example, McGahan (2007) proposed that research can be interesting to both scholars and practicing managers if theory can provide answers to a current practical problem, and Tushman and O'Reilly (2007) suggested that executive education courses can be settings where scholars and practitioners can create such virtuous cycles of knowing and doing. Aldag (1997) offered several ideas for bridging the gap: Ask experienced students to write critiques of management theories, survey practitioners about what research they would like to see, and establish research advisory boards made up of practitioners for our scholarly journals (see also Aldag & Fuller, 1995). Bartunek and Rynes (2010) proposed that the implications-for-practice sections in empirical research papers could be more effective in making clearer the practical usefulness of the research. Although Bartunek and Rynes (2010) found an increase in implications-for-practice statements in empirical organizational-behavior research articles after the calls to make research more relevant in the 1990s, they also found that such statements usually were not written in a way that could be immediately actionable. They suggested that implications-for-practice statements could more usefully communicate the relevance of management research (1) by making clear to what practitioners should attend and analyze; (2) by clarifying what general principles are implied by the results; (3) by explicitly encouraging practicing readers to use the study as a prompt to their own analyses; (4) by providing more information about the context of the study so practitioners can evaluate how it might apply in their own settings; and (5) by noting if the findings are consistent with current practice. Over the years, there have been many suggestions for specific changes in the way scholars conduct and report research, yet despite these proposals, we will show that management research has become even less useful to our students. As insightful as they have been, these proposals have primarily focused on structural changes to how we conduct and communicate management research. Here we propose that there has been a shift toward less actionable research published in our most prestigious journals, not just how we communicate management research.

That is, despite the decades of laments about the lack of relevance of our research, the many thoughtful practical suggestions for addressing the problem, editors' requirements to include statements about the implications of the research for practice, and an increasing proportion of experienced students in our classrooms, we investigated our hunch that the practical relevance of our research actually has gotten worse.

Despite the decades of laments about the lack of relevance of our research, the many thoughtful practical suggestions for addressing the problem, editors' requirements to include statements about the implications of the research for practice, and an increasing proportion of experienced students in our classrooms, we investigated our hunch that the practical relevance of our research actually has gotten worse.

In order to investigate our suspicion that less management research was actionable by those we teach today, and by extension, any other practitioners, we collected some data. First, we sought to track the proportion of articles that could be actionable by practitioners in our best scholarly journals over time. Second, we examined the distribution of subfields in the most recent volumes to see if changes in the mix of subfields in management may have contributed to the decline in the usefulness of management research. Finally, we selected a management publication intended for an audience of sophisticated, practicing managers (*The Economist*) to see how often its journalists and editors write about our management-focused research compared to original scholarly research from the related discipline of psychology. After reporting the results of our modest data-collection effort, we speculate about why management research seems to have become less useful to the students we teach.

IDENTIFYING ACTIONABLE RESEARCH

To answer our question, we needed to define what we judged as research useful in the education of executives, managers, and students of management. Beyer (1997) described Pelz's (1978) three classifications of ways in which research could be useful: symbolic, instrumental, and conceptual utilization of research. *Symbolic utilization* is the use of research to legitimate a preexisting position or course of action. Certainly our research can be used to justify a manager's bias or preferred course of action, but few scholars go to the great trouble of conducting research and getting it published solely to produce such justifications. Rather, we assume most of us would want to rely on instrumental and conceptual knowledge in our teaching.

Instrumental utilization is when research findings are applied in a direct way to address a specific practical problem. Beyer (1997) notes that the major barriers to the instrumental utilization of research are the myriad particular circumstances and constraints that managers face. She suggests that managers' greater implementation of qualitative research (see Van de Vall, Bolas, & Kang, 1976,) stems from the description of context in such research reports, something that enables managers to evaluate whether the context of the research is similar enough to their own to allow application there. Nevertheless, whether managers evaluate instrumental management research, or a celebrity CEO's ghost-written autobiography, they do so as one of Roethlisberger's (1977) temporary walking sticks, to aid them as they go along, useful until they can find a better walking stick. Practitioners know that circumstances always vary, and so we suggest this is not the primary reason management research may have become proportionally less instrumentally useful over time.

Beyer (1997) contrasts the more direct usefulness of instrumental utilization with conceptual utilization of research. Practicing managers use research conceptually when it aids in their general understanding of a phenomenon, even if that understanding does not immediately influence their actions. Bartunek (2007) makes a strong case for the value of research that aids practitioners' conceptual understanding, particularly because it can draw those practitioners' attention to important phenomena that they would otherwise not notice. Drawing on Beyer's (1997) categorization, our focus is on research that teachers and their students can understand as having implications for their actions, that is, both conceptual and instrumental utilization. For this reason we will call useful research *actionable*.

All research knowledge is conceptual in nature and can range from a conceptual understanding (if I do A, B usually is likely to follow), to highly abstract understandings of human nature, social, or economic systems. Since we expected all the research published in our best journals to contribute in some way to students' potential conceptual understandings, we chose to focus only on conceptual understandings that had implications for their actions. Those implications need not be immediate, but for us to consider the research as actionable, it had to have some relevance for students' potential actions. That is, we decided that research was potentially useful if any person could possibly take some kind of organizational action based on knowing its results. Detailed descriptions of examples of articles we coded as actionable and not actionable appear below.

That management research be actionable to be useful to our students is important. Davis (1971) proposed that a particular theory or study becomes "interesting" if it violates some (but not all) of the readers' assumptions, and if it has relevance for action. That action need not be practitioner action, for example, the study may open a new line of research, but if there is no relevance for action the reader's response is, "Who cares?" All experienced teachers know that the quickest way to lose students' interest is to talk about something they already know (what Davis, 1971, labeled, "That's obvious"), or something with no possible use for them now or in some imagined future. Of course, our research may be actionable for other scholars rather than for managers; scholarship must settle methodological or theoretical disputes important to its good conduct. No one would expect all our research to be actionable by managers, or by any other organizational practitioner. As Beyer (1997) notes, only a small fraction of research conducted in industry, which is wholly focused on practical applications, is actually ever used. Whether the proportion of practitioner-actionable research reported here is too high (or too low) we will leave the readers to judge; our focus is on whether the proportion of actionable research has changed over time.

We focused on research that can be used either conceptually or instrumentally as a basis for action, by anyone taking a management course or executive session of any type. We call such research *actionable* for several reasons. First, because our criteria do not map directly onto the categories of research utilization identified by Beyer (1997), we could not use one of her terms. Second, the term *relevant* is too vague. It means connected or pertaining to the matter at hand, but connected in what way? Something may be connected but not actionable, as when someone tells an amusing story about another manager's quirky eating habits. Only when a research produces an understanding, or knowledge, relevant to some action an organizational participant might take is it scholarship useful to teaching, and so the focus of our modest study.

JUDGING MANAGEMENT RESEARCH AS ACTIONABLE

Sample Research Articles

To see if our research has become less useful over time, we selected the two most prestigious management journals that have been published the longest, allowing us to test the changes. These are *Administrative Science Quarterly* and *Academy of Management Journal*. Without question, these are among the handful of most prestigious scholarly journals publishing empirical research in management. They have the highest impact factors as of this writing (3.842 and 9.263, respectively), and their articles are used by *The Financial Times* to rank business schools by the quality of their scholarship.

We took the most recent complete volume (2010) of both journals, and sampled the articles in each of the preceding 10 years through 1960, the first year both were in print using our once-a-decade sampling frame. That is, the sample consists of the articles from the entire volume (year) for these two journals for 1960, 1970, 1980, 1990, 2000, and 2010. In this way we hoped to keep the number of articles to be coded down to a reasonable number (the total number of coded empirical articles = 420), as well as to avoid any possible sampling bias from selecting any other year. This is a sample, not a census of all articles, so we conducted statistical tests on the changes over time.

To create the sample of articles to evaluate as possibly actionable, we eliminated all editors' introductions, instructions to contributors, editors' reports, review articles, essays, and books reviews, focusing only on articles reporting empirical research. We considered a study's findings actionable if any person who might take a management or organizations course or an executive-education session could possibly take action based on the study's findings. We included studies addressing possible actions by board members, chief executive officers, entrepreneurs, those who fund entrepreneurs, middle managers, supervisors, and those who work in or with organizations of any type. We also included research that could suggest action to influence public policy, as long as an individual could conceivably act based on what was learned from the study. In short, we purposely defined "our students" in the most comprehensive way possible.

The number of empirical papers in each issue of all of the volumes we sampled appears in Table 1. What we find most remarkable in this sample is the substantial increase in number of articles in each volume, and hence, potentially actionable papers in *Academy of Management Journal* over the decades. *Administrative Science Quarterly* also seemed to be following this trend toward greater numbers of articles in each volume, but then reversed it in the 2010 volume.

Coding Procedure

We coded the empirical articles summarized in Table 1 as either actionable or not actionable. Some articles *clearly* had implications for action, while others required an assessment. We coded even the most tenuously actionable articles as actionable. The research reported was considered actionable if any individual could take or alter his or her own actions based on its findings. If the results could be combined with other knowledge,

TABLE 1
Number of Empirical Articles in Each Journal

	Administrative Science Quarterly	*Academy of Management Journal*
1960	17	21
1970	32	23
1980	31	57
1990	23	39
2000	24	74
2010	16	63

Note. Table articles exclude editors' introductions, instructions to contributors, editors' reports, review articles, essays, and book reviews.

whether that was informal personal knowledge or research knowledge, we coded the article as actionable. This coding required a great deal of subjective judgment. The challenge of making such judgments systematically and without bias is no doubt one reason why those who have decried the lack of relevance of management research have relied on their impressions, or a textual coding of language used in research articles. As coauthors, we were not blind coders. Our method falls between unsystematic impressions and a more rigorous blind coding with interrater reliability coefficients. We consider our coding to be an imperfect preliminary first step, which is more systematic than impressions and more content-focused than a word count, but nevertheless quite limited. We hope that others can build upon our work.

The coding proceeded as follows. Initially we discussed the distinction between actionable and not actionable research results. Next, the first author coded the articles from one volume year from each journal, writing out the reasons for the coding decision for each article. In the next step, the second author then coded the articles from a different volume year for both journals, and the two reviewed and discussed the reasons for their decisions, clarifying and articulating the coding rules. The second coauthor then coded the remaining articles in the sample, and the two met to discuss and make final decisions on difficult-to-classify cases and finalize the rules used for the final coding (see the Appendix).

The coding rules consisted of three questions. If all three could be answered as "yes" the article was coded as actionable, if any one was answered as "no" the article was coded as not actionable.

1. Are the research findings more than purely descriptive accounts of uncontrollable circumstances for observing a particular outcome? For example, some articles document market or institutional forces in an industrial context, or construct a cognitive map of people in a setting; these do not address what individuals could usefully do in such circumstances. Following Davis (1971), if the new knowledge reported had no implications for anyone's practical understanding or action, we coded it as not actionable.[1]
2. Can a causal conclusion (however tentative) be made as a result of the research? If an article reports associations that cannot be reasonably causally ordered, then it is not clear what action an individual could take. We eliminated articles reporting one-shot correlational studies (more commonly found in the earlier decades) as not allowing confident causal conclusions.
3. Does the causal conclusion translate into a practical action that can be taken? For example, some research documents individual differences in processes that it would be illegitimate or illegal to act upon without evidence that these processes affect job performance. For example, gender differences in language used when working on a team project would not be the basis for team formation (which would usually depend on expertise) and would be a fraught subject for mentoring given the risks to individuals of gender-inconsistent actions.

DECLINE IN THE USEFULNESS OF MANAGEMENT RESEARCH FOR ACTION

As can be seen in Tables 2 and 3, we found a significant decrease in the proportion of research that we as teachers can use because our students would find the results actionable. *Administrative Science Quarterly* had a majority of its articles

TABLE 2
Percentage of Actionable Articles: *Administrative Science Quarterly*

	Qualifying articles	Actionable articles	% Actionable
1960			65%[b]
Issue 4	5	3	
Issue 3	4	3	
Issue 2	3	3	
Issue 1	5	2	
1970			63%[b]
Issue 4	9	7	
Issue 3	8	5	
Issue 2	7	4	
Issue 1	8	4	
1980			52%[b]
Issue 4	7	4	
Issue 3	7	4	
Issue 2	8	4	
Issue 1	9	4	
1990			35%[a]
Issue 4	5	1	
Issue 3	5	2	
Issue 2	6	4	
Issue 1	7	1	
2000			33%[a]
Issue 4	6	2	
Issue 3	6	3	
Issue 2	6	2	
Issue 1	6	1	
2010			19%
Issue 4	4	1	
Issue 3	4	0	
Issue 2	4	1	
Issue 1	4	1	

Note. Percentages with the same superscript are not different from each other.

[1] A complete list of how each of the 420 articles was coded is available from either author.

TABLE 3
Percentage of Actionable Articles: *Academy of Management Journal*

	Qualifying articles	Actionable articles	% Actionable
1960			43%[a]
Issue 3	8	2	
Issue 2	7	6	
Issue 1	6	1	
1970			35%[a]
Issue 4	6	2	
Issue 3	6	3	
Issue 2	6	2	
Issue 1	5	1	
1980			40%[a]
Issue 4	15	4	
Issue 3	16	8	
Issue 2	14	7	
Issue 1	12	4	
1990			36%[a]
Issue 4	11	6	
Issue 3	9	3	
Issue 2	9	2	
Issue 1	10	3	
2000			36%[a]
Issue 6	18	6	
Issue 5	13	4	
Issue 4	13	3	
Issue 3	14	7	
Issue 2	8	4	
Issue 1	8	3	
2010			24%
Issue 6	12	4	
Issue 5	13	4	
Issue 4	11	3	
Issue 3	9	1	
Issue 2	9	1	
Issue 1	9	2	

Note. Percentages with the same superscript are not different from each other.

categorized as actionable in its early decades, but then between 1980 and 1990 we see a substantial decline to less than a third of its articles coded as actionable, moving from 65% actionable in 1960 to only 19% actionable in 2010. The *Academy of Management Journal* never had a majority of its articles categorized as actionable, but still we find the percentage of articles our students or practitioners could use from the *Academy of Management Journal* dropping by almost half, to 25% in 2010. Despite the low statistical power from these small sample sizes, we see statistically significant drops in the percentage of actionable research in both *Administrative Science Quarterly* and *Academy of Management Journal* from 1960 to 2010. In the 2010 volume of the *Academy of Management Journal*, we counted only 16 empirical papers as having results suggesting practical action or understanding that could be the basis for action to one of our students—only about double the actionable articles it published in 1960 and 1970 despite the *Academy of Management Journal's* editors publishing three times more articles in recent decades.

These results are in contrast to what Palmer, Dick, and Freiburger (2009) report. As part of a larger study of whether rigor had contributed to a decline in the relevance of organization-science research published in *ASQ* from 1956 to 2004, they reported a reduction in relevance of organization-science articles from 80% of the articles "of interest and subject to the control of managers" to 30% in 1976, but then relevance increasing to 75% in 2004. They do not provide more details about how they judged a topic to be controllable by managers, but it appears to be close enough to what we call actionable that this difference in findings of these two studies gave us pause. Because we considered a broader range of articles actionable, focusing on research that would be actionable by anyone (not just managers), we would have expected to find a higher percentage of articles that were actionable compared to their controllable ones. Alternatively, they categorized a study as relevant if any of the studied concepts were potentially controllable by managers, whereas we looked at the totality of the research conclusions as suggesting action that could be useful to educators (an illustration of a possible difference is reflected in the reasons why the Stewart & Barrick, 2000, article was categorized as not actionable below). Alternatively, it could be because they report 3 years (1956, 1976, and 2004) that were not included in our sample, and there is a wide variation from year to year (or editor to editor) in the proportion of articles that are actionable or managerially controllable. We look forward to comparing our data year-to-year to see if some difference in our coding judgments or wide fluctuations in relevant/actionable research, or something else, best accounts for these differences. In the meantime, we illustrate with some examples of articles categorized as actionable and not actionable.

Examples of Actionable Research

We classified Lee, Ashford, and Bobko's (1990) *Academy of Management Journal* article as actionable. This study consisted of questionnaires distributed in a variety of occupational settings with the employees providing information about their personalities and job perceptions, and their supervisors rating their employees' job performance. These authors found that those individuals with a particular personality characteristic ("Type A," or excessive achievement striving, competitiveness, time urgency, and aggressiveness) had better job performance and were more satisfied with their jobs when they had a greater feeling of personal control at work. We categorized this research as actionable because most supervisors probably could identify subordinates with Type A personalities, and then take actions, such as reducing role ambiguity and increasing delegation and job autonomy for these employees, and so facilitate their job performance and satisfaction.

The other example of actionable research comes from Castilla and Benard's (2010) *Administrative Science Quarterly* article. In a series of experiments the authors found that if an organizational culture is meritocratic, experienced managers give significantly larger bonuses to men than to equally meritorious women, but when neither employee was meritorious, women received larger bonuses than did the men. Castilla and Benard (2010) then test various possible reasons for this bias, for example the visibility of the performance, the language used to characterize the performance, and so forth. This research is actionable because the authors identify features that exacerbated gender bias, features that could be eliminated from instructions to supervisors in bonus allocations. This research is also particularly useful in the classroom because it is interesting: Few students (or anyone else) would assume that meritocracy, which by design is expected to be blind to performance-irrelevant demographic characteristics, would lead to pay decisions favoring men over equally well-performing women. This research could be summarized in the classroom and used as the basis for potentially powerful conversations in which experienced and inexperienced students alike could explore why meritocracy has this decidedly nonmeritocratic outcome, and what managers could do to reduce gender bias in their own organizations.

Examples of Not Actionable Research

We selected two examples of research we categorized as not actionable to illustrate two features of research more recently favored by editors and reviewers, features that might be contributing to the decline in useable research in management. First, we categorized Stewart and Barrick's (2000) article in *Academy of Management Journal* as not actionable. It is a questionnaire-based study in which members of production teams provided information on the independent, moderator, and mediator variables, and their supervisors provided performance data on the teams. This combination of mediator and moderator tests is so complicated that we reproduce the findings the authors report in their abstract below, rather than try to restate them in our own words:

> For teams engaged primarily in conceptual tasks, interdependence exhibited a ∪-shaped relationship with team performance, whereas team self-leadership exhibited a positive, linear relationship with performance. For teams engaged primarily in behavioral tasks, we found a ∩-shaped relationship between interdependence and performance and a negative, linear relationship between team self-leadership and performance. Intrateam process mediation was found for relationships with interdependence but not for relationships with team self-leadership (Stewart & Barrick, 2000: 135).

Such complicated moderator–mediator results make it difficult to know what action any individual could take based on this study. We were unable to convert these findings to any statement or group of statements that we could comfortably share in a classroom. While team self-leadership could possibly be under the control of someone creating teams, we found it difficult to decide how we would advise those creating teams to vary the team's interdependence, something that is usually driven by the task, and here the self-leadership is only actionable in certain complex combinations with task interdependence. Further, it would be very difficult to try to explain this complex set of moderator–mediator relationships to students, who presumably would struggle to understand the abstract concepts as well as what a moderator–mediator relationship means in practice. It would take a great deal of valuable classroom time to help students and executive education participants understand complex interaction effects among a multitude of variables, some of which are out of individuals' control. Even if we could clearly explain these findings, we doubt our students or executive education participants would have any sense of what they could do about them.

This study reflects an organizational behavior research design that has become increasingly popular in recent decades, and so could be one reason why the proportion of actionable research has declined over that time. It is well known that research that is based on correlations among self-reported data do not allow confident conclusions about causality (and even establishing that they represent distinct concepts can be a challenge, cf. Podsakoff, MacKenzie, Lee, & Podsakoff, 2003). Without confident conclusions about what the research means, the research results cannot be useful for action. Of course, our best scholarly journals no longer publish such same-instrument self-report correlational studies. However, at the same time that same-source bivariate relationships are no longer acceptable, moderator–mediator studies among self-report variables taken from the same questionnaire have become more common. We suppose that this is for two reasons. The first is because the results from moderator and mediator studies cannot be explained away as coming from common-method variance, but still allow the researchers to take their measures from the same self-report questionnaire, making data collection easier than collecting data from multiple independent sources. The second reason might be that, on the surface, these studies identify different situational conditions, something important to anyone contemplating practical action. However, if the situations are not comprehensible or controllable by individuals, such knowledge is not actionable by our students or practitioners. The sheer complexity of the different logical possibilities (for variables that may or may not be the most powerful ones driving the effects) requires more cognitive processing than most of us can muster, and that few students would tolerate.

Our second not actionable example reflects the growing proportion since 1960 of strategy and organization theory research in our most prestigious scholarly journals. We begin by acknowledging that much organization theory research is not intended to address actions that individuals might take, as reflects its origins in sociology, a field addressing social and structural influences on organizations. Our example of this type of not actionable research is Greve's (2000) article in *Academy of Management Journal*. Using an archival sample of greater Tokyo banks from 1894 to 1936 taken from Japan's Census of Banks, he finds that firms' decisions to enter market niches are driven by density dependence, mimetic isomorphism, and mutual forbearance. This article seeks to capture what strategic decision makers actually do (in contrast to what certain management theories predict) and to translate those actions into academic terminology. There is no uncovering of actions that the decision makers might not already know, as was the case for Castilla and Benard's (2010) study. This research corrects previous theorists' overly narrow focus on transportation costs alone in describing how branch locations decisions are made, and so is a contribution to our scholarly knowledge. Pruning away inaccurate and misleading theories is valuable to other scholars. However, since the author reports actual location decisions that were driven by factors other than transportation costs, it does not seem that our previously erroneous transportation-driven theories of branch location decisions have had much influence on practitioners' actions anyway. There are many studies in recent decades like this: research demonstrating that a particular theory does not accurately describe reality. While such corrective studies may set our scholarly theories straight, if they simply describe what practitioners are already doing anyway it is hard to see how they have implications for anyone's practical actions.

Does a Shift in Management Research Subfields Account for the Decline?

We decided to explore whether a possible shift in the mix of management subfields since 1960 may have contributed to a decline in the proportion of actionable research. Van de Vall, Bolas, and Kang, 1976, suggested that qualitative research was more actionable because it provided more contextual information for practitioners to evaluate, and Palmer and colleagues (2009) reported that articles published in *Administrative Science Quarterly* focused on groups or individuals were more likely to study variables controllable by managers than were articles focused on organizations or larger aggregates. In Table 4, we report the actionable and not actionable articles for the 2010 volumes by whether the research was based on qualitative or quantitative research and by whether it was from the management subfield of organizational behavior, organizational theory, strategy, human resources management, or another subfield (such as public policy). Note that if an article could be categorized as using more than one method or subfield, we included it in both (so the article totals are slightly larger than the number of 2010 articles listed in Tables 2 and 3). Although the numbers are too small to conduct statistical tests, Table 4 clearly shows that the relatively newer management subfields of organizational theory and strategy were more likely to be categorized as not actionable. Perhaps not as surprisingly for such an applied subfield, the lone human resources article (published in *Academy of Management Journal* in 2010) was categorized as actionable. The ratio of actionable to not actionable articles in organizational behavior is 1:1.8, nearly twice the proportion as organization theory (1 actionable to every 3.4 not actionable article). Further, only one strategy article was categorized as actionable to 20 strategy studies categorized as not actionable. Note that this is in spite of our categorization of any article that practitioners addressed in strategy research on which a chief executive officer or board members could act as actionable.

In addition to the preceding note that organization theory research focuses on social aggregates, the lower proportion of actionable strategy and organization theory articles may also result from the long

TABLE 4
2010 Article Counts by Method and Management Subfield: *Administrative Science Quarterly* and *Academy of Management Journal*

		Actionable		Not actionable	
		ASQ	*AMJ*	*ASQ*	*AMJ*
Method[a]	Quantitative	2	10	12	41
	Qualitative	1	4	6	8
Management subfield[b]	Organizational behavior	1	10	4	16
	Organization Theory	3	6	10	21
	Strategy	0	1	5	14
	Human Resources	0	1	0	0
	Other (e.g., Public Policy)	0	1	1	1

[a] Articles that used mixed methods (with both qualitative and quantitative components) were doubled counted.
[b] Articles that crossed subdisciplines (e.g., OB and HRM) were double counted.

shadow economics casts over these management subfields. The field of economics is noted for its simplifying assumptions about organizations and human motivation. A considerable proportion of the recent research from these management subfields holds economists' simplifying assumptions to empirical test and finds them wanting, much as Greve (2000) did in the example discussed above. Again, if data indicate that managers (or their supervisory bodies) have ignored economics-based theories, and the article simply describes what executives and other strategic decision makers are already doing, such research doesn't have any action or understanding implications for use in our classrooms. Such research may be important for intellectual disputes within the subfields of organization theory and strategy, but this research does not do managers or board members much good.

Further, over the decades organization theory research has shifted away from problems of organization structure and design, research that was actionable for managers (see, e.g., Lawrence & Lorsch, 1967; Woodward, 1965). In recent decades the greater focus has been on the elaboration of theories that are not actionable and descriptions of structures, such as populations of organizations or institutions, that provide fewer opportunities for individual action. Because such descriptive theory-debunking research accounts for an increasing percentage of articles in the *Academy of Management Journal* and *Administrative Science Quarterly* since 1960, we suggest this could be one factor contributing to the declining proportion of management research that can be used in our teaching.

WHY THE PROPORTION OF ACTIONABLE MANAGEMENT RESEARCH HAS DECLINED

Of course, we can only speculate about why there appears to be a decline in the proportion of management research that is potentially actionable. We have already suggested two possible reasons—that editors and reviewers increasingly favor uncovering complex moderator–mediator relationships that often include variables too convoluted or uncontrollable to be clearly explained or serve as the basis for action, and the increasing proportion of management articles addressing highly abstract theories from sociology and economics that have not been applied by practicing managers or their boards. Here we offer additional possibilities. First, we provide evidence that the decline is not a matter of the greater rigor of recent management research. Next, we propose three possible changes over time that may be contributing to this decline in the usefulness of our research in our classrooms: The mismatch between the ability to do actionable research and the demands of business-school rankings, changes in the journal-review process over the decades, and the lack of a powerful constituency pressing for actionable management research.

A Question of Relevance Not of Rigor

It could be argued that recent research is more rigorous; that is, it uses research designs that reduce or eliminate alternative explanations of the results. If so, more rigorous controls may make recent management research less relevant to management students and other practitioners. A complaint that rigorous research is not relevant to management practice is as old as the entry of research-focused scholars into business schools, and shows no sign of abating with time (e.g., Behrman & Levin, 1984). Bartunek (2007) characterized the debate as akin to tribal warfare. Like Staw (1995) and Aldag (1997), among others, we disagree that more rigorous management research is inherently less actionable. It is not a question of rigorous research being less translatable into action or actionable insight, but appears to be a shift in the choices of research questions. Staw (1995) made a persuasive argument that research that is rigorous allows us to be confident in what we have learned, and research that is not rigorous is meaningless, and so useless.

We conducted another modest study to see if the proportion of actionable management research has declined because practitioners cannot understand or do not value rigorous research. We collected data from *The Economist*, a publication that often translates research (in the social, physical, and biological sciences) that journalists believe is interesting to their manager and investor readers. Each print issue of *The Economist* contains several articles translating scholarly research (identified by author and journal) for practitioners. Some publications for practitioners (e.g., *The Wall Street Journal, Harvard Business Review*) mix models and advice from consultants and others, drawing on their personal experiences with articles that appear to be based on scholarly research. However, only *The Economist* cites the original scholarly journal, eliminating any need for more subjective categorizing by the authors.

To make the task manageable, we sampled the most recent five complete years, 2006 through 2010, of the print version of *The Economist*. We compared mentions of research from our sampled *Administrative Science Quarterly* and *Academy of Management Journal* with *Psychological Science*, the flagship journal of the Association for Psychological Science. The Association for Psychological Science broke away from the American Psychological Association because many research psychologists thought the dominance of practicing clinical and counseling psychologists in the American Psychological Association detracted from rigorous scientific psychology. It is a scholarly association created to recognize rigorous research using random assignment and similar controls, often with student subjects. We selected its flagship journal, *Psychological Science*, which publishes primarily laboratory experiments. *Psychological Science* has a mandate to publish the most rigorous and theory-leading scientific psychology, and has no mandate to publish research actionable by managers or investors. In addition, we searched for discussions of research from the American Psychological Association's *Journal of Applied Psychology*, since that journal has long served as a bridge between management and psychology.

In Table 5 we find substantially more references to research published in *Psychological Science* (18 discussions) than in *Administrative Science Quarterly* (none), *Academy of Management Journal* (1 discussion), or *Journal of Applied Psychology* (none) in *The Economist* from 2006 through 2010. For example, in the February 13, 2010 issue in an article entitled "Fair Dues," *The Economist* reports the results of a study published in *Psychological Science* finding that people are less likely to view equally distributed bonuses as fair if money is distributed rather than goods of equal value. The journalist appeared to judge this research as interesting to their readers, and we would have categorized it as actionable because managers who want to distribute a bonus pool equally would be more likely to be seen as fair if they gave presents or threw a party rather than distributed money. Like Castilla and Benard's (2010) research, this study would make for a lively classroom discussion.

TABLE 5
Articles from *Academy of Management Journal, Administrative Science Quarterly* and *Psychological Sciences* Mentioned in *The Economist* 2006–2010

Academy of Management Journal
1. (February 6, 2010)
Administrative Science Quarterly
0
Journal of Applied Psychology
0
Psychological Science
18. (September 8, 2007, December 8, 2007, January 26, 2008, March 29, 2008, May 3, 2008, May 24, 2008, June 28, 2008, November 22, 2008, December 20, 2008, January 24, 2009, April 11, 2009, June 13, 2009, July 11, 2009, August 1, 2009, January 23, 2010, February 13, 2010, June 26, 2010, November 13, 2010)

No one could argue that the research published in *Psychological Science* is less rigorous than that published in our leading scholarly management journals or the bridge journal of *Journal of Applied Psychology*. The editors of *The Economist* write for an educated practitioner audience, exactly the audience we seek to reach as scholar-teachers. Apparently, these journalists also find our recent research uninteresting, reporting on only one article from our two most prestigious scholarly journals in the most recent 5 years. While research can be interesting to practitioners because it questions some of their assumptions, we suspect that a lack of any actionable knowledge in so many of our articles may be contributing to the virtual absence of research as identified as coming from our leading management journals discussed over the 5 years in *The Economist*. Journalists who have to work too hard to find that one nugget of interesting research may not bother to consult that journal at all. Clearly, the decline in the proportion of research useful in our teaching is not the result of any possible increase in the rigor of our research, but appears to come from a shift in the questions management scholars choose to address.

The Difficulty of Actionable Research and Demands for More Publications

Previous calls for more relevant management research have implied that the research is not relevant because scholars don't know what is relevant (hence the calls for more practitioner feedback and reviews) or that scholars have too few incentives to do relevant research (admonitions to promotion committees and editors). In contrast, we suspect that the difficulty of doing good quality, theory-driven, actionable research has been underappreciated in previous discussions of the lack of relevance of much management research. It isn't that scholars are not trying hard enough; rather, we suspect that such research is more difficult to do than is research that addresses a gap or absence of implementation of an abstract theory. There are many challenges involved in getting such research published in our most demanding scholarly journals. For example, researchers may start with a practical management problem, but then find that they can develop no valid way to measure the constructs or accurately identify which of the myriad of forces involved are the critical ones. What is more, doctoral students are taught to join the scholarly conversation (cf. Huff, 1999) and so immerse themselves in the literature for the first years of their training, and they are admonished to find research questions that address gaps in that literature. This produces research designed to address a suspicion that a particular theory is wrong or narrower in scope than represented in the literature. This practice can result in dissertations addressing a question that never had any practical import, thus producing articles that are not actionable. A lot of things have to go right to produce a theoretically important actionable research article.

A lot of things have to go right to produce a theoretically important actionable research article.

Further, doctoral students face increasing pressure to begin to publish while still in their early years of training, and may lack a sophistication that may be developed with more scholarly experience, to do research that is both actionable and theoretically strong. In its stead, the most salient message is to publish in top journals, and that is equated with strong theoretical contributions, rather than practical management problems that are addressed with theoretically strong research. Students are taught that meaningful new implications or insights for theory must be present, above all, and although articles must also include a section on implications for practice, practical relevance may be rather indirect (cf. Bartunek & Rynes, 2010), and lack of actionable research findings is not a cause for rejection. Therefore, with all of the challenges of publishing, many scholars find themselves beginning with questions that are theoretically important rather than first identifying a compelling management issue and following with a strong theoretical framework for addressing that issue.

Previous calls for more relevant management research focused on reward systems or editorial policies, and have ignored how difficult such research is, treating the irrelevance of so much management research as primarily an incentives problem. Too many seem to assume that if only incentives such as promotion criteria or editorial policies were reformed, we would have more and better actionable research. Rather, we think the rewards are strong and clear—good quality actionable research does get published, and those who publish it go on to the most glorious careers in our most prestigious universities, as is reflected in even a cursory glance at the many actionable articles published by faculty in these elite institutions. High quality research that addresses theoretically important actionable questions is hard to do, and more of us find that we can do good quality research that is not actionable than research that can do both. As an analogy, it is not that the incentives for a successful professional major- or premier-league sports career are weak, it is that few really have the ability to play at that demanding level: Many more can play in minor leagues or work in the team's front office. We suspect that producing good quality actionable research is like playing sports in major or premier leagues, it is not done often because it is very difficult to do, not because there are inadequate rewards for it. Despite all of the doctoral consortia, reformed doctoral training, and junior faculty mentoring available, very few are going to be able to do this difficult work.

If it was only that publishing high-quality actionable research was difficult, we would simply have fewer articles published, and the proportion of actionable research would not have declined. Rather, it is the combination of the difficulty of successfully completing such research *and* the increasing global demand for business education, fueling increasing pressure to publish. The doctoral and junior faculty training improvements over the past decades seem to have improved the raw number of actionable management research articles, just as the average quality of professional sports play has improved over the same decades. However, the supply of management research superstars has not kept proportional pace with the demand. More business schools now compete globally for students, making the media-based business school rankings more important to deans throughout the world when compared to the early decades covered in this study. Many deans believe that one way to improve their rankings is to put more pressure on their faculty (usually junior faculty over whom they have more leverage) to publish in the management journals used for business-school rankings. These deans care much less about whether such research is actionable. Rather, deans speak about whether their faculty members are "getting hits in 'A' journals." This has led to an explosion of manuscript submissions to our most prestigious scholarly journals in the past decades. Now junior faculty members throughout the world are under greater pressure to publish in the most prestigious journals, so many will search for anything that will increase their chances of publication; and rarely does this advantage come from the publication of actionable research. Journal editors can only publish the papers that are submitted to them, and if there is a scarcity of good actionable research in a sea of solid research that is not useful to practitioners or our teaching, so be it.

Changes in the Journal Review Processes

There has been a change in the editorial review process over the decades that may also have contributed to the declining proportion of actionable research. Until about the mid-1980s, the best scholarly journals had all-powerful editors. These editors would send submissions out for review, but they were informed by reviewers and did not delegate their publication decisions to reviewers. For example, the first author remembers receiving a request for a revision and resubmission from the then-editor of the *Journal of Applied Psychology* in 1981. The editor wrote a detailed letter summarizing the changes he thought a revision should address, and then directed the author to the reviewers' suggestions, only for possible consideration. The editor reviewed the resubmitted revision for whether it addressed the concerns he listed in his letter, and made his decision without further consultation with the reviewers. The editor decided what was necessary in a revision and then evaluated whether the revision met those criteria. In contrast, today, revisions must address (in documented detail) all editor and *reviewer* suggestions, or make a very persuasive case for not doing so. This places sometimes irreconcilable demands on prospective authors.

We all expect that journal reviewers are selected for their diverse expertise and perspectives—editors need this heterogeneity of viewpoints to feel confident in their decisions. But this very heterogeneity becomes a burden on authors when every individual reviewer must be satisfied (often pulling the paper in several different theoretical and empirical directions). Not all reviewers are as professional as they should be in helping the authors to produce and communicate their own research more effectively. Instead, today all multiple reviewer theoretical preferences and antipathies are demanded in revisions, with editors avoiding telling authors which issues can be safely ignored (no matter what they may personally think). When combined with the increasing diversity of international and disciplinary approaches reflected in management today, those submitting and revising manuscripts become overwhelmed by the need to please reviewers with divergent demands and tastes.

In another example of the weakened power of journal editors, when Sara Rynes became editor of the *Academy of Management Journal* she streamlined the review process (Rynes, 2006) and called for more qualitative research (Rynes, 2007b). However, these changes were reversed 2 years later by her successor. Experienced researchers know that it takes more than 2 years to conceive, carry out, and then draft a manuscript reporting qualitative research. Clearly, this journal's policy of changing editors every 3 years further undermines the influence of any one editor.

We don't know why these shifts in editorial practices have occurred: What problem was this editorial abdication of responsibility supposed to solve? However, we do suspect that having to cover so many theoretical bases and appease diverse empirical tastes without any clear understanding of who the authors need to please leads hopeful authors to try to include more theoretical perspectives and use the latest esoteric empirical approaches, leaving any possible utility to practitioners the last of their concerns.

No Constituency Demands Actionable Management Research

Finally, drawing on Ghoshal's (2005) work, we wonder: Where is the powerful constituency pressing for actionable research? Ghoshal (2005) detailed the ways in which economics-based theories in management have been used by the powerful to rationalize and justify their exploitation of others. He persuasively argued that there were powerful constituencies who stood to benefit from theories that claimed people were opportunistic and that all relationships should be treated as one-off transactions where exploitation was ever-present. As Beyer (1997) noted, symbolic utilization justifies and supports what practitioners already think. Scholarly researchers face an array of powerful and conflicting demands and must bow to the most salient ones. Who, then, is the powerful constituency for actionable management research?

Others lamenting the irrelevance of so much management research have identified the powerful constituents for the present state of affairs, and we have added to their lists. Journal editors are judged by citation counts, which favor articles using new methods and theories, not knowledge useable by students and practitioners (who do not cite the research they use). Senior management scholars value research that looks highly erudite or statistically complex, to build their discipline's credibility in the eyes of their university colleagues. Deans want their faculty to publish in journals that will improve their schools' rankings. Decisions about what is published in our scholarly journals have been dissipated to unaccountable anonymous reviewers. Alternatively, we could identify only one potentially powerful constituency—our fee-paying experienced students—who could press for more actionable research. Yet our experienced students and other practitioners don't expect more from us. After all, harsh as it may seem, we have convinced them "that's academic" often means "that's pointless."

CONCLUSIONS

We collected data to test our suspicion that the scholarly research published in our best management journals has become proportionally less conceptually and instrumentally useful to executives, managers, and others who want to participate in and run organizations more effectively, and so, less useful to us as teachers of management. We found a significant decrease in the proportion of journal articles that generated actionable knowledge from 1960 to 2010. We then examined the previous 5 years of reports of research from a practitioner-focused publication, finding that rigorous student-subject laboratory studies from a journal with no pretense to producing research useful to managers was discussed much more often than research from either *Academy of Management Journal*, *Administrative Science Quarterly*, or *Journal of Applied Psychology*. This decline in the proportion of actionable research studies matters. Rousseau and McCarthy (2007) suggest that one of the barriers to a more research evidence-based teaching is the difficulty harried teachers have in finding research they can use in their classrooms. Journalists, teachers, and practitioners alike are less likely to turn to our best scholarly journals for insights and understanding if they expect that they have a less than one in four chance of finding something they can use.

We offered several speculations for the decline in the proportion of actionable research despite the continuing calls for more relevant management research and numerous concrete suggestions intended to foster more useable research over the years. These include the favoring of complex moderator–mediator analyses, and studies demonstrating that abstract economic theories have not been implemented in practice. We reminded readers that this decline is not just an incentives problem, but that producing theoretically important actionable research is difficult and so more likely an ability problem, rather than incentives one. This, when combined with growing demand for faculty to publish only in journals used in business school rankings has increased pressure to produce more publications without a proportional increase in actionable research. We proposed that the decline in actionable research may also be the result of an abdication of editors' own judgments to demanding that all reviewers' requests be met. Finally, we speculated that the absence of a sufficiently powerful constituency for actionable management research also played a part in this decline.

Despite these reasons, we do not think that finding actionable research questions, per se, is very difficult. As Tushman and O'Reilly (2007) suggested, we can work with the executives we teach and meet in school-sponsored events to learn what they see as questions they would like to see addressed. Just a few of such research questions might include the following:

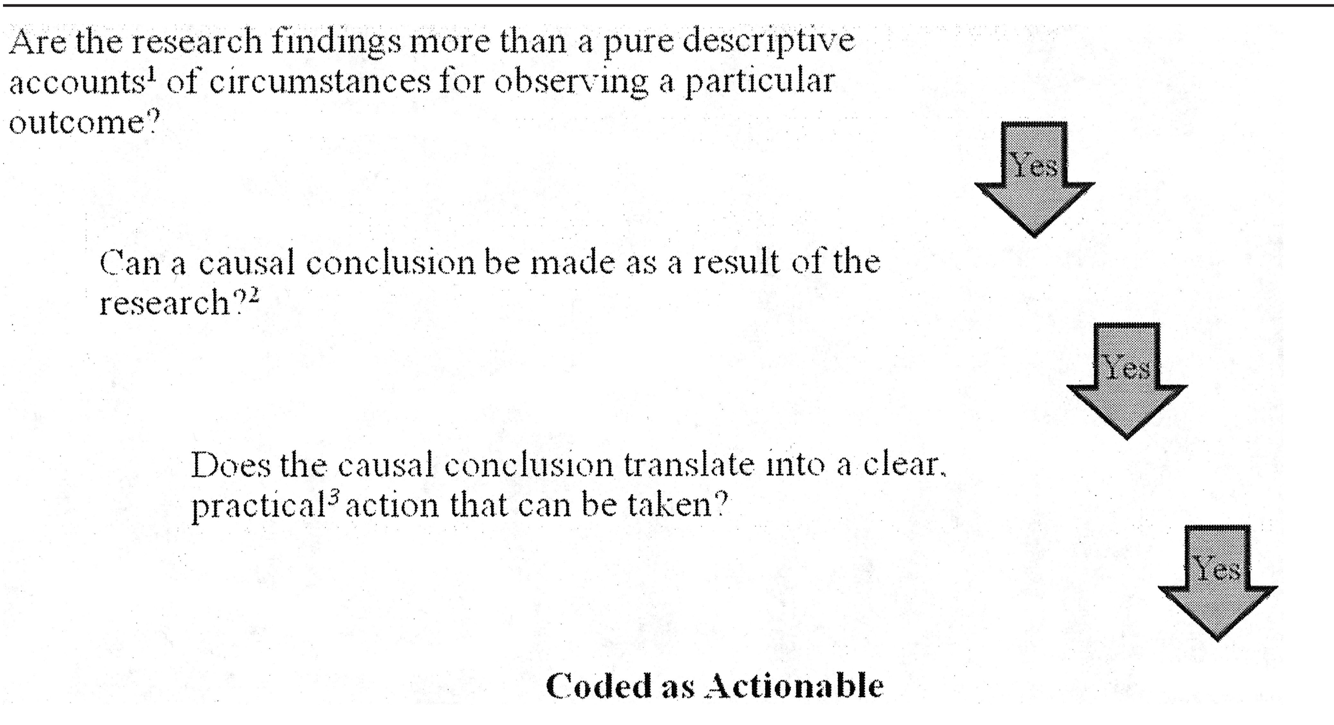
- What can individuals do to become a visionary leader?
- When and how have individuals succeeded in changing institutions?
- What actions can entrepreneurs take to persuade venture funders that they are worthy of venture funding?
- In building support for your change program, is it more important to develop the support of as wide a set of people as possible or identify the minimum necessary coalition and direct your energy to deepening the support of that smaller set of supporters?

This initial attempt to test our sense that management research is becoming less useful to our students may be flawed: Different journals or volumes may produce different results. Some may argue that we have been too stringent, and our analysis required more subjective judgment than we would have preferred. We hope this preliminary analysis will spur others to evaluate other journals, or other samples of these journals, using decision rules they believe to be more appropriate.

Nevertheless, if this initial work is supported, it is worrisome. It cannot be good for any field to move further and further away from generating new knowledge that those outside ivory towers might use—particularly so for an applied field like management. Teachers who have to work so hard to find the one or two studies they can use in their teaching may find it easier to teach about actionable research from classic studies from the 1960s and 1970s, rather than the research from the century in which they live. What is more, the data reported here (and in previous laments about the irrelevance of so much management research) could provide support for business school ranking publications to dispense with any use of scholarship in their business school rankings (if research is useless to managers, why should it matter to the quality of a school's education?). If supported by additional work, this apparent decline in the proportion of useful management research to others makes it easier for financially strapped deans to replace expensive full-time research faculty with part-time adjuncts (if research doesn't contribute to their educational mission). And, of course, we are paid by people outside of academia to produce knowledge useful to them, creating moral obligations, as well as self-interested reasons to do better.

APPENDIX

Decision Rules for Categorizing Articles as Actionable



[1]If the research merely gives description, for example, of market and institutional forces, or if it outlines how cognition produces preferences then these papers were considered to be "not actionable" because they do not address what individuals could usefully do with such forces or cognitions. For example, research focused on firm-level management policies that are not actionable by even a CEO alone are categorized as not actionable, but research useable by any individual to more effectively influence public or firm policy was coded actionable.[2] If the research does not address actions that can have an effect it is not actionable.[3] For example, research documenting individual differences that produced team-process effects that it would be illegal or illegitimate to address because they are not performance-relevant is coded "not actionable."

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