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THE FIELD OF MANAGEMENT'S DEVOTION TO THEORY: TOO MUCH OF A GOOD THING?

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Many nice things can be said about theory. Theories help us organize our thoughts, generate coherent explanations, and improve our predictions. In short, theories help us achieve understanding. But theories are not ends in themselves, and members of the academic field of management should keep in mind that 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: 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.

It is well known that the top journals in management require that all manuscripts contribute to theory (Colquitt & Zapata-Phelan, 2007; Rynes, 2005; Sutton & Staw, 1995).[1] The current editorial statement of *AMJ* (its "Information for Contributors") mirrors those of our other top journals and illustrates this insistence explicitly: "All articles published in the *Academy of Management Journal* must also make strong theoretical contributions." And, believe me, there is no breaching or skirting this policy. After years of comparing notes with colleagues about the rejection letters we have received, it seems the most annoying passage—which I am sure editors have preprogrammed for handy one-click insertion—is this one: "The reviewers all agree that your paper addresses an important topic and is well argued; moreover, they find your empirical results convincing and interesting. At the same time, however, the reviewers believe the paper falls short in making a theoretical contribution. Therefore, I'm sorry . . . etc., etc., etc."

One might ask whether our top journals are really as doctrinaire about theory as I am suggesting. After all, editors sometimes refer to a "lack of theoretical contribution" as a polite brush-off for papers with various kinds of shortcomings. And, granted, the formal editorial statements of top journals try to convey a "big tent" philosophy as to what constitutes a theoretical contribution. But still, after years of writing reviews, reading the reviews of fellow referees, reading editors' decision letters, and seeing what shows up in print, I find it is exceedingly clear that the gatekeepers for the top journals in management first screen manuscripts for basic readability and technical adequacy, and then they apply one pivotal test, above all others: Where's the theory? As someone who regularly reads the journals of sister fields (including those of higher stature than management), I am not aware of any other field in which theory is viewed with such religious fervor.

I don't want my point to be mistaken. First, no personal motives underlie my thesis. I have had more than my share of papers accepted by our journals, and my biggest successes, I guess, have been in theory development. Second, I'm not pointing fingers, as I have been fully complicit in building up our current approach. I've served as an editor, as a member of multiple editorial boards, as an officer of the Academy, and am in every other way part of the establishment. But my unease has been growing in recent years, and now I want to elbow the powers that be. Third, I am not proposing that we abandon our commitment to theory. Theory is essential, and the field of management will not advance without it. It's just that we've gone overboard in our obsession with theory. The requirement that every paper must contribute to theory is not very sensible; it is probably a sign of our academic insecurity; and it is costing us in multiple ways.

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[1] I refer specifically to the *Academy of Management Journal*, the *Academy of Management Review*, *Administrative Science Quarterly*, and *Organization Science*. Of course, theory is the entire mission of *Academy of Management Review*. Top-tier specialty journals such as *Strategic Management Journal*, the *Journal of Applied Psychology*, and *Organizational Behavior and Human Decision Processes* vary in their degree of insistence on theory.

A COMPARATIVE PERSPECTIVE

Management's idolization of theory began, harmlessly enough, as an outgrowth of the field's efforts to demonstrate academic worthiness. In the late 1950s, blue-ribbon Carnegie Foundation and Ford Foundation reports levied withering attacks on business schools for their lack of academic sophistication (Porter & McKibbin, 1988). As a result, in the 1960s and 1970s all fields of business adopted a new commitment to drawing from basic disciplines (e.g., economics and psychology), to analytic rigor, to the virtues of normal science and, above all, to theory. A scan of the top journals in marketing, accounting, finance, and management for the mid 1970s reveals a pervasive incorporation of theory.[2] Since then, however, the other fields have relaxed their single-mindedness about theory. Confident in their academic standing, other business fields regularly publish—in their top journals, no less—papers that are not particularly theory-based or theory-oriented. Management, however, is stuck. Like insecure adolescents who are deathly afraid of not looking the part, we don't dare let up on our showy devotion to theory.

And it *is* a showy devotion. It's not enough that all our papers must invoke an overarching explanation for any expected or observed empirical results, which of course is what a theory is.[3] Beyond that, we are obligated to pepper our papers with as many mentions of "theory" or "theoretical" or "theorizing" as possible. And we get extra points for banner headings that cry out, "Hey, I'm being theoretical here."

How far has management's strutting of theory gone? I analyzed the 120 articles published in the 2005 volumes of *AMJ*, *ASQ*, and *OS* and found that 100 percent of the articles—every single one—contained some variation of the word "theory" in the text. By comparison, only 78 percent of the 178 articles published in 2005 in the *Journal of Marketing*, *Journal of Finance*, and *Accounting Review* contained any such words. These felicitous words appeared 18 times, on average, in each management article, but only 8 times, on average, in each nonmanagement article.[4] The management and nonmanagement articles did not differ in their average lengths, so it can accurately be said that the sacred words were more than doubly abundant in the management articles. Moreover, 65 percent of the articles in the management journals had section headings that trumpeted "theory," while just 20 percent of the nonmanagement journals had such headings.[5]

Is it possible that the other fields of business have the same zeal about theory as management but just call it something else? The answer is a bit yes but mostly no. The other fields tend to use less pretentious terms to describe a line of thought or an argument. They talk more in terms of *logic, concepts, premises,* and *ideas*. At the same time, though, the statistics above clearly indicate that "theory" is in the vocabulary of scholars in other fields. The other fields are simply not as hung up on theory. Sure, they have their theories, and plenty of their papers are expressly theory-driven. But authors writing for these fields don't feel the need to sprinkle mentions of theory on every page, like so much aromatic incense or holy water, in quite the way we do. And a number of their papers are not theory-based at all.

A look at the top journals of other fields readily uncovers papers that do not purport to contribute to theory. For instance, a 2006 *Journal of Marketing* article introduced the phenomenon of the "doppelgänger brand image," which is the disparaging backlash that often befalls emotion-laden brands (Thompson, Rindfleisch, & Arsel, 2006). Demonstrating their ideas through an in-depth analysis of Starbucks, the authors neither propped themselves up with any theories nor claimed to have generated any theories. They simply documented and dis-

[2] I omit management science and operations management from consideration because their orientations are primarily toward mathematical modeling, rather than theory per se.

[3] For further discussion of what constitutes theory (and what does not), see Sutton and Staw (1995), Rynes (2005), and Colquitt and Zapata-Phelan (2007).

[4] Three notes about my sample: First, in conducting this analysis, I omitted editors' notes, commentaries, and other nonrefereed pieces. Second, I used 2005 volumes because it was the latest year that all the journals were available in HTML format, which allows convenient text analysis. Third, I limited my analysis to just three nonmanagement journals as a matter of convenience, and I cannot be sure of how the results would differ if I included all top-tier journals from the other fields. In the case of marketing, we can reasonably expect that the omitted journals (the *Journal of Consumer Research*, the *Journal of Marketing Research*, *Marketing Science*) would exhibit even less mention of theory because they emphasize methodological and modeling issues.

[5] Interestingly, there were no significant differences among the three management journals on any of the statistics examined; nor were there any significant differences among the three nonmanagement journals, even though they are in various fields. It appears that the academic field of management is an outlier, differing systemically, and in a wholesale manner, from these other fields—at least on the dimensions examined.

sected a fascinating, important phenomenon, in a way that would be a remote prospect in our top journals.

Similarly, a 2006 paper in the *Journal of Finance* rigorously explored a series of profoundly important questions about government bailouts of corporations, including, Do political connections lead to bailouts? and Is the financial performance of politically connected bailed-out firms different from that of nonconnected bailed-out firms? (Faccio, Masulis, & McConnell, 2006). The authors presented this entire paper without invoking any theoretical trappings, relying instead on the prima facie importance of the topic and the strength of their empirical work to hook the reader.

Indeed, a recent article in *Fortune* highlighted the instrumental role of finance scholars as fact-finding sleuths who often report momentous empirical patterns in their top journals (Fox, 2006). The article described, for instance, how scholarly hunches and careful data analysis led to the uncovering of two major financial scams: backdating stock options and after-hours trading in mutual funds. Says the article, "Want to know what the next big corporate scandal will be? Get yourself a subscription to the *Journal of Finance*" (Fox, 2006: 96).

Perhaps we believe that the other fields of business have lost their way, while we in management continue to adhere to the one true path and that our reverence for theory is the superior approach. If so, what would we point to as evidence that we are right and the others are wrong? Do we develop knowledge better or faster than the others? Do we have higher stature within business schools than the others? Do we have a greater impact on professionals and the world of practical affairs? My strong sense, albeit buoyed only by impressionistic data, is that the field of management actually *lags behind* sister fields in all these respects. And our hang-up about theory is not incidental to our shortcomings, but rather is a central cause.

THE COSTS TO OUR FIELD

The fact that the major journals in management require a theoretical contribution in every paper takes an array of subtle, but significant, tolls on our field. The most substantial cost is the absence of certain forms of research that other fields find highly valuable. But there are additional costs too.

Facts Must Await Theories

Imagine it's the 1930s, and you are an epidemiologist who has a hunch that cigarette smoking does bad things to people. Smoking is stylish and has even been portrayed as healthful, so your nagging suspicions to the contrary make you a bit of a crackpot. But you persevere, and in a series of matched-sample studies, you find recurring evidence that smoking is associated with an array of serious maladies. As an epidemiologist, rather than a biologist, you have no clear insights about the central mechanisms at work; in fact, you even acknowledge that unobserved covariates may be driving the relationship. But you feel a strong need to get your findings reported, so you send your manuscript to a prominent journal.[6]

You see where I'm going. If the epidemiologist's paper went to a journal like one of ours, it would be rejected. No matter how important the topic or persuasive the analysis, the message would be: Go away and don't come back until you have a theory. Fortunately, the epidemiologist's intended outlet was more receptive, and the reporting of Dr. Franz Müller's findings paved the way for a long series of studies that verified his results and confirmed why and how smoking is harmful (Brandt, 2007).

There are multiple ways for knowledge to advance. One of the most efficient ways, seemingly comprehended in all academic fields except management, is for important or interesting facts to be reported, so that subsequent researchers can then direct their efforts at understanding why and how those facts came to be (Helfat, 2007; Miller, 2007). The field of economics, the most prestigious of the social sciences, adheres to the merits of this approach. For example, the mission of the distinguished National Bureau of Economic Research (NBER) is all about facts: "The object of the NBER is to ascertain and present to the economics profession, and to the public more generally, important economic facts and their interpretation in a scientific manner" (Jaffe, Lerner, & Stern, 2006: vii).

This willingness to consider facts without theory carries over to the top journals in economics, and any number of examples could be identified. For instance, Schmalensee published a paper in the *American Economic Review* in 1985 that was a straightforward, unvarnished exercise in fact-finding, but one that spawned an immensely important and influential stream of research in economics and strategy. Using data on a large sample of companies, Schmalensee set out to identify the degree to which variance in business-unit profitability is

[6] I've used this example before (Hambrick, 2004), but as I've learned more about early research on smoking, the more convinced I am of its aptness. See Brandt (2007) for this fascinating story about facts preceding theory.

due to the industry a unit is in, to the unit's market share, or to its parent company. Although it could be said that prior theory drew him to these potential explanations of performance, Schmalensee's approach was unabashedly atheoretical: "The analysis reported here is fundamentally descriptive; it does not attempt directly to estimate or to test hypotheses. . . . Cross-section data can yield interesting stylized facts to guide both general theorizing and empirical analysis of specific industries" (1985: 341). Here is a paper, one that we in management would describe as "a fishing expedition" or "brute empiricism," that ends up making a very big difference. I say let's get the facts out and then direct our efforts at understanding the nuances, the whys, and the hows that lie behind the facts. Baker and Pollock (2007) made the same point by noting that a given piece of research might not be "theory-driven" but still be "theoretically interesting"—if it stimulates subsequent development or revision of theory.

The field of management has a prevailing wisdom, to simplify a bit, that theory is ideally built from qualitative in-depth case studies and then subsequently tested on large samples or in controlled experiments. But this approach omits a crucial first step: the identification of the phenomenon or pattern that we need a theory to explain. I propose that we should be willing to start with the generation of facts, most typically from large-sample analysis, that can inform us as to what we need a theory for (an approach also proposed by Helfat [2007]). Then, as we get into exploring the whys and hows, a combination of quantitative and qualitative studies will be fruitful.

Of course, the question of what constitutes an "interesting fact" is open to debate. To me, a "fact" (or what is sometimes called a "stylized fact" [Helfat, 2007] or an "empirical regularity") becomes more intriguing, more worthy of investigating, in proportion to the presence of these conditions: the fact is surprising and previously undocumented; it amounts to an associational pattern rather than just a univariate tendency; the temporal order of the involved variables is clear; the outcome variable is important; the sample is large and carefully constructed (multiple samples are a bonus); all obvious covariates and endogenous relationships have been controlled for; and the effect size is big. Thus, there is no clear dividing line between what constitutes a momentous fact and an incidental fact, but reviewers and readers should be able to recognize the extremes.

As a hypothetical illustration, let's envision a competently executed large-sample study that provides strong evidence that when German or Italian companies adopt American-style governance processes, their financial performance improves, but when Singaporean or Thai companies adopt such processes, their performance declines. Now those would be interesting facts, even in the absence of a clear explanation. And once those facts were reported, researchers could embark on a combination of targeted quantitative and qualitative studies to ascertain what's really going on. The result could be substantial advances in theories about governance, institutions, stakeholder relations, or cultural values—advances triggered by the reporting of facts. We should relax our requirement that facts be reported only with theories.

Contorted, Ponderous Prose

I was recently at a brown-bag seminar where a pair of management colleagues were seeking advice about a preliminary research idea. It took just a few minutes for us all to agree that their research question was fascinating. It addressed an extremely interesting issue that both academics and practicing managers would like to learn more about. The only problem: the presenters had no theory. So, we spent the entire session going through our collective mental catalogues of theories that might be invoked so that the project could proceed and have some prospect of publication. People were mentioning theories I'd never heard of. We became frenzied, nearly desperate: "Good god, there must be a theory out there that we can latch onto." Because the researchers are savvy at the publishing game, I'm pretty sure their project will eventually appear as an article in one of our journals. And I can further predict that the straightforward beauty of the original research idea will be largely lost. In its place will be what we too often see in our journals: a contorted, misshapen, inelegant product, in which an inherently interesting phenomenon has been subjugated by an ill-fitting theoretical framework.

Our insistence on theory in every article has caused a lot of bad writing. In every paper, we must have the obligatory section about the origins and current state of the theory we are invoking—again, no matter how strained its relevance. We must adopt the conceptual nomenclature of the theory, instead of just referring directly to the phenomena or variables we are examining. And, above all, we must go to lengths to say how the paper contributes to theory. It's not enough to say how the paper contributes to our knowledge or understanding. Instead, we must do a lot of elaborate hand-waving to assert that some theory or another is better off because of our paper. In a recent essay, Danny Miller groaned that management researchers must "pretend to be developing theory or contrive an explanation when merely trying to advance a question or call out a pattern of consequence" (2007: 3). No wonder a finance colleague, who makes a point of carefully reading the papers of all tenure candidates, once said to me, "Can't you folks in management just go ahead and say what's on your mind?"

Too Little Regard for Simple Tests

In assessing whether a manuscript "contributes to theory," reviewers almost always apply the criterion that the paper must set forth new theoretical ideas, i.e., a new theory or an elaboration of an existing theory. Unfortunately, straightforward tests of existing theories usually don't qualify, and this means our field has an absurdly high ratio of ideas to tests of ideas. In turn, this means we suppose much more than we know.

It should come as little surprise that Kacmar and Whitfield (2000) found that only 9 percent of the theoretical presentations in *AMR* articles are ever tested. In a colossal catch-22, the Academy of Management has set things up so that we publish theories in *AMR*, but we cannot then later publish direct, straightforward tests of those theories in *AMJ* (or any other top journal).[7] Take it from someone who has tried: Your paper will not meet with a warm reception if you claim merely to be testing a previously proposed line of thought. And reviewers will come right out and laugh at you if you claim to be replicating a prior test of a theory. As a result, the vast majority of published ideas in management are submitted to no tests at all, a handful are submitted to one test, and only a minuscule few are tested over and over or in multiple ways. Again, we don't really know much for sure.

If we aspire to develop a reliable body of knowledge that managers can use for "evidence-based" decisions, as called for by Pfeffer and Sutton (2006) and Rousseau (2006), we must allow an accumulation of the requisite evidence. The only way to do this, of course, is to allow ample testing and replication. All other academic fields I am aware of—especially those that have professional constituencies that rely on a formal body of knowledge—attach significant value to straightforward tests of previously proposed theories, ideas, and operating mechanisms. We in management, however, are so riveted on new and revised theories, and so dismissive of simple generation of facts and evidence, that our revealed ethos is that we care much more about what's fresh and novel than about what's right.

HOPES AND RECOMMENDATIONS

I have two main recommendations for how we might overcome the problems I've noted. First, the leading journals in management should broaden their scope to include papers that do not directly contribute to theory but are nonetheless of great potential consequence. These might be papers that identify compelling empirical patterns that cry out for future research and theorizing. They might be rich qualitative descriptions of important but unexplored phenomena that, once described, could stimulate the development of theory and other insights. Or they might be of other types altogether.

I am not proposing that our top journals should lower their standards, only that they should shift them. Reviewers would still be asked to apply stringent requirements, in terms of argumentation, acknowledgement of relevant literature, technical adequacy, and readability. But the requirement for a "contribution to theory" would be replaced with this test: *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. Reviewers would apply their best judgments as to whether a given paper would make a difference to our field, perhaps applying Miller's definition of what should count as valuable research:

> . . . the discovery of new arguments, facts, patterns or relationships that, in a convincing way, help us to better understand some phenomenon that is of consequence to a social or scientific constituency. Such research may bear little or no connection to preexisting or future theory, span many theories, or give rise to understanding that only eventually will form the basis of new theories. (Miller, 2007: 6)

With this criterion in place, some pieces that would be published under current standards would no longer qualify, leaving space in our most elite journals for new types of more consequential articles.

I don't expect journal editors to change their policies just on the basis of this essay. But perhaps they will at least explore my assertion that the top journals in other fields do not have nearly the theory fetish that ours do and will objectively examine

[7] The stated policies of the top journals in management, including *AMJ*, allow for direct tests of theories, but the reality is that reviewers want to see new ideas or angles. Consider this thought experiment: What do you suppose would be the reaction of *AMJ* reviewers if you submitted a paper that started out, in essence, by saying, "My purpose is to exactly test the ideas laid out in a recent *AMR* article"?

the pros and cons of our approach. I further encourage the leaders of our professional societies, members of editorial review boards, and indeed all of us to support the editors of our top journals in undertaking the reassessment I am proposing.

My second idea is that we need at least one journal, and perhaps more than one, that is largely devoted to straightforward tests of theories, including replications and extensions. Many other fields have such journals. For example, *Marketing Letters* and *Economic Letters* publish short articles of various types, most notably tests, replications, and minor extensions of theories. Although these journals are not among the premier outlets in their fields, they are at the very tops of the second tiers; their editorial boards are lustrous; and they often publish pieces by some of the most distinguished scholars in their fields. These are highly valued and influential outlets that allow knowledge to accumulate through idea generation, testing, retesting, and refinement—and all relatively quickly, because these journals purposely have very short reviewing and publication cycles.

Perhaps the editors of one of our existing second-tier journals will consider a reconfiguration of the type I am proposing—as a sort of *Management Letters*. Such a mission would be truly distinctive and might allow that journal to enhance its stature and impact, emerging above a crowded set of outlets that, for the most part, are locked in battle as faint replicas of our top-tier journals. Another alternative, of course, is for the Academy of Management to consider the addition of a *Management Letters* to its portfolio of journals. Such a journal would clearly be in keeping with the mission of the Academy, and it might greatly serve the advancement of management knowledge and the needs of Academy members.

SUMMARY

I suspect that many members of our field, including those in leadership positions, believe that our hypercommitment to theory—and particularly the requirement that every article must contribute to theory—is somehow on the side of the angels. They may believe that this is a hallmark of a serious field. They may believe that theory is good and that the "mere" description of phenomena and generation of facts are bad. Worse yet, they may have given no thought to these matters, accepting our field's zeal about theory as simply part of the cosmos. My aim has been to promote a rethinking of these positions.

Theory is critically important for our field, and we should remain committed to it. And, for sure, the greatest acclaim will always go to those who develop breakthrough theories. So there is plenty of incentive to keep working on theory. But it takes much more than theory for an academic field to advance. Indeed, various types of atheoretical or pretheoretical work can be instrumental in allowing theory to emerge or develop. Thus, our insistence in the field of management that all papers contribute to theory may actually have the unintended perverse effect of stymying the discovery of important theories. More broadly, this norm—or policy, really—is holding back our field.

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