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ASQ Forum

What Theory is *Not*

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This essay describes differences between papers that contain some theory rather than no theory. There is little agreement about what constitutes strong versus weak theory in the social sciences, but there is more consensus that references, data, variables, diagrams, and hypotheses are not theory. Despite this consensus, however, authors routinely use these five elements in lieu of theory. We explain how each of these five elements can be confused with theory and how to avoid such confusion. By making this consensus explicit, we hope to help authors avoid some of the most common and easily averted problems that lead readers to view papers as having inadequate theory. We then discuss how journals might facilitate the publication of stronger theory. We suggest that if the field is serious about producing stronger theory, journals need to reconsider their empirical requirements. We argue that journals ought to be more receptive to papers that test part rather than all of a theory and use illustrative rather than definitive data.

The authors, reviewers, readers, and editors who shape what is published in *ASQ* insist, perhaps above all else, that articles contain strong organizational theory. *ASQ*'s Notice to Contributors states, "If manuscripts contain no theory, their value is suspect." A primary reason, sometimes the primary reason, that reviewers and editors decide not to publish a submitted paper is that it contains inadequate theory. This paper draws on our editorial experiences at *ASQ* and *Research in Organizational Behavior* (*ROB*) to identify some common reasons why papers are viewed as having weak theory.

Authors who wish to write strong theory might start by reading the diverse literature that seeks to define theory and distinguish weak from strong theory. The *Academy of Management Review* published a forum on theory building in October 1989. Detailed descriptions of what theory is and the distinctions between strong and weak theory in the social sciences can be found, for example, in Dubin's (1976) analysis of theory building in applied areas, Freese's (1980) review of formal theorizing, Kaplan's (1964) philosophical inquiry into the behavioral sciences, Merton's (1967) writings on theoretical sociology, and Weick's (1989) ideas about theory construction as disciplined imagination.

Unfortunately, the literature on theory building can leave a reader more rather than less confused about how to write a paper that contains strong theory (Freese, 1980). There is lack of agreement about whether a model and a theory can be distinguished, whether a typology is properly labeled a theory or not, whether the strength of a theory depends on how interesting it is, and whether falsifiability is a prerequisite for the very existence of a theory. As Merton (1967: 39) put it:

> Like so many words that are bandied about, the word theory threatens to become meaningless. Because its referents are so diverse—including everything from minor working hypotheses, through comprehensive but vague and unordered speculations, to axiomatic systems of thought—use of the word often obscures rather than creates understanding.

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Lack of consensus on exactly what theory is may explain why it is so difficult to develop strong theory in the behavioral sciences. Reviewers, editors, and other audiences may hold inconsistent beliefs about what constitutes theory and what constitutes strong versus weak theory. Aspiring organizational theorists face further obstacles because there is little consensus about which theoretical perspectives (and associated jargon) are best suited for describing organizations and their members (Pfeffer, 1993). Even when a paper contains a well-articulated theory that fits the data, editors or reviewers may reject it or insist the theory be replaced simply because it clashes with their particular conceptual tastes. Finally, the process of building theory is itself full of internal conflicts and contradictions. Organizational scholars, like those in other social science fields, are forced to make tradeoffs between generality, simplicity, and accuracy (Weick, 1979) and are challenged by having to write logically consistent and integrated arguments. These difficulties may help explain why organizational research journals have such high rejection rates. Writing strong theory is time consuming and fraught with trial and error for even the most skilled organizational scholars. This is also why there is such great appreciation for those few people, like James March, Jeffrey Pfeffer, and Karl Weick, who are able to do it consistently.

We don't have any magic ideas about how to construct important organizational theory. We will not present a set of algorithms or logical steps for building strong theory. The aim of this essay is more modest. We explain why some papers, or parts of papers, are viewed as containing no theory at all rather than containing some theory. Though there is conflict about what theory is and should be, there is more consensus about what theory is *not.* We consider five features of a scholarly article that, while important in their own right, do not constitute theory. Reviewers and editors seem to agree, albeit implicitly, that these five features should not be construed as part of the theoretical argument. By making this consensus explicit we hope to help authors avoid some of the most frequent reasons that their manuscripts are viewed as having inadequate theory.

PARTS OF AN ARTICLE THAT ARE NOT THEORY

1. References Are Not Theory

References to theory developed in prior work help set the stage for new conceptual arguments. Authors need to acknowledge the stream of logic on which they are drawing and to which they are contributing. But listing references to existing theories and mentioning the names of such theories is not the same as explicating the causal logic they contain. To illustrate, this sentence from Sutton's (1991: 262) article on bill collectors contains three references but no theory: "This pattern is consistent with findings that aggression provokes the 'fight' response (Frijda, 1986) and that anger is a contagious emotion (Schacter and Singer, 1962; Baron, 1977)." This sentence lists publications that contain conceptual arguments (and some findings). But there is no theory because no logic is presented to explain why aggression provokes "fight" or why anger is contagious.

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Calls for "more theory" by reviewers and editors are often met with a flurry of citations. Rather than presenting more detailed and compelling arguments, authors may list the names of prevailing theories or schools of thought, without even providing an explanation of why the theory or approach leads to a new or unanswered theoretical question. A manuscript that Robert Sutton edited had strong data, but all three reviewers emphasized that it had "weak theory" and "poorly motivated hypotheses." The author responded to these concerns by writing a new introduction that added citations to many papers containing theory and many terms like "psycho-social theory," "identity theory," and "social comparison theory." But it still contained no discussion of what these theories were about and no discussion of the logical arguments why these theories led to the author's predictions. The result was that this paper contained almost no theory, despite the author's assertion that much had been added.

References are sometimes used like a smoke screen to hide the absence of theory. Both of us can think of instances in which we have used a string of references to hide the fact that we really didn't understand the phenomenon in question. This obfuscation can unfortunately be successful when references are made to widely known and cited works like Kanter (1977), Katz and Kahn (1978), March and Simon (1958), Thompson (1967), and Williamson (1975). Mark Twain defined a classic as "A book which people praise but don't read." Papers for organizational research journals typically include a set of such throw-away references. These citations may show that the author is a qualified member of the profession, but they don't demonstrate that a theoretical case has been built.

Authors need to explicate which concepts and causal arguments are adopted from cited sources and how they are linked to the theory being developed or tested. This suggestion does not mean that a paper needs to review every nuance of every theory cited. Rather, it means that enough of the pertinent logic from past theoretical work should be included so that the reader can grasp the author's logical arguments. For example, Weick (1993: 644) acknowledged his conceptual debt to Perrow's work and presented the aspects he needed to maintain logical flow in this sentence from his article on the collapse of sensemaking: "Because there is so little communication within the crew and because it operates largely through obtrusive controls like rules and supervision (Perrow, 1986), it acts more like a large formal group with mediated communication than a small informal group with direct communication." Note how there is no need for the reader to know about or read Perrow's work in order to follow the logic in this sentence.

2. Data Are Not Theory

Much of organizational theory is based on data. Empirical evidence plays an important role in confirming, revising, or discrediting existing theory and in guiding the development of new theory. But observed patterns like beta weights, factor loadings, or consistent statements by informants

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rarely constitute causal explanations. Kaplan (1964) asserted that theory and data each play a distinct role in behavioral science research: Data describe *which* empirical patterns were observed and theory explains *why* empirical patterns were observed or are expected to be observed.

The distinction between the amount and kind of evidence supporting a theory and the theory itself may seem obvious to most readers. Yet in the papers we have reviewed and edited over the years, this is a common source of confusion. We see it in papers by both experienced and inexperienced authors. We also see it our own papers. Authors try to develop a theoretical foundation by describing empirical findings from past research and then quickly move from this basis to a discussion of the current results. Using a series of findings, instead of a blend of findings and logical reasoning, to justify hypotheses is especially common. Empirical results can certainly provide useful support for a theory. But they should not be construed as theory themselves. Prior findings cannot by themselves motivate hypotheses, and the reporting of results cannot substitute for causal reasoning.

One of Sutton's early papers tried to motivate five hypotheses about the relationship between union effectiveness and union members' well-being with the following paragraph:

> Recent empirical evidence suggests that the collective bargaining process (Kochan, Lipsky, and Deyer, 1974; Peterson, 1972), the union-management contract (Davis and Sullivan, 1980), and union-management relations in general (Koch and Fox, 1978) all have important consequences for the quality of worklife of unionized workers. Moreover, Hammer (1978) has investigated the relationship between union strength and construction workers' reactions to their work. She found that union strength (operationalized in terms of workers' relative wages) was positively related to both pay satisfaction and perceived job security. Finally, the union's ability to formally increase members' participation in job-related decisions has been frequently cited as contributing to the unionization of teachers and other professionals (e.g., Bass and Mitchell, 1976; Belasco and Alutto, 1969; Chamot, 1976). (Carillon and Sutton, 1982: 172–173).

There is no attempt in this paragraph to explain the logical reasons *why* particular findings occurred in the past or *why* certain empirical relationships are anticipated in the future. We only learn from the paragraph that others had reported certain findings, and so similar patterns would be expected from the data. This is an example of brute empiricism, where hypotheses are motivated by prior data rather than theory.

Although our examples focus on using past quantitative data to motivate theory and hypotheses, qualitative papers are not immune to such problems. Quotes from informants or detailed observations may get a bit closer to the underlying causal forces than, say, mean job satisfaction scores or organizational size, but qualitative evidence, by itself, cannot convey causal arguments that are abstract and simple enough to be applied to other settings. Just like theorists who use quantitative data, those who use qualitative data must develop causal arguments to explain *why* persistent

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findings have been observed if they wish to write papers that contain theory (Glaser and Strauss, 1967).

In comparing self-managing teams to traditional teams with supervisors, Barker (1993: 408) quoted an informant, " 'Now the whole team is around me and the whole team is observing what I'm doing'." This quote doesn't contain causal logic and isn't abstract enough to be generalized to other settings. But these data helped guide and support Barker's inference that because every team member has legitimate authority over every other, and because the surveillance of multiple coworkers is harder to avoid than that of a single boss, self-managing teams constrain members quite powerfully. So, although qualitative data inspired Barker's inferences, they are distinct from his theoretical analysis. Mintzberg (1979: 584) summarized this distinction succinctly: "The data do not generate theory—only researchers do that."

3. Lists of Variables or Constructs Are Not Theory

Pages 249 to 253 of March and Simon's (1958) *Organizations* present a "numerical index" to 206 variables discussed in the classic book. This list of variables and the definitions that March and Simon present of these variables are important parts of their theory but do not, alone, constitute theory. A theory must also explain why variables or constructs come about or why they are connected. Weick (1989: 517) quoted Homans to make this point:

> Of particular interest is Homan's irritation with theorists who equate theory with conceptual definitions; he stated that "much official sociological theory consists in fact of concepts and their definitions; it provides a dictionary of a language that possesses no sentences."

Papers submitted to organizational journals often are written as if well-defined variables or constructs, by themselves, are enough to make theory. Sometimes the list of variables represents a logical attempt to cover all or most of the determinants of a given outcome or process. Such lists may be useful catalogs of variables that can be entered as predictors or controls in multiple regression equations or LISREL models, but they do not constitute theory. Listing the demographic characteristics of people associated with a given behavior is not theory. Dividing the world into personality versus situational determinants does not, by itself, constitute a theory of behavior. Nor does developing a categorical scheme to cover the determinants of a dependent variable such as escalation (Staw and Ross, 1987) constitute an explanation of that variable.

As an empirically based field, organizational research is often enticed by tests showing the relative strength of one set of variables versus others on particular outcomes. We are attracted to procedures that show the most important influence on dependent variables, as though the contest will show *who the winner is.* Comparative tests of variables should not be confused with comparative tests of theory, however, because a predicted relationship must be explained to provide theory; simply listing a set of antecedents (or even a causal ordering of variables as in LISREL models) does not make a theoretical argument. The

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key issue is *why* a particular set of variables are expected to be strong predictors.

4. Diagrams Are Not Theory

Diagrams or figures can be a valuable part of a research paper but also, by themselves, rarely constitute theory. Probably the least theoretical representations are ones that simply list categories of variables such as "personality," "environmental determinants," or "demographics." More helpful are figures that show causal relationships in a logical ordering, so that readers can see a chain of causation or how a third variable intervenes in or moderates a relationship. Also useful are temporal diagrams showing how a particular process unfolds over time. On occasion, diagrams can be a useful aid in building theory. For researchers who are not good writers, a set of diagrams can provide structure to otherwise rambling or amorphous arguments. For those researchers who are talented writers, having a concrete model may prevent obsfuscation of specious or inconsistent arguments.

Regardless of their merits, diagrams and figures should be considered as stage props rather than the performance itself. As Whetten (1989) suggested, while boxes and arrows can add order to a conception by explicitly delineating patterns and causal connections, they rarely explain *why* the proposed connections will be observed. Some verbal explication is almost always necessary. The logic underlying the portrayed relationships needs to be spelled out. Text about the reasons why a phenomenon occurs, or why it unfolds in a particular manner, is difficult to replace by references to a diagram. A clearly written argument should also preclude the need for the most complicated figures we see in articles—those more closely resembling a complex wiring diagram than a comprehensible theory.

Good theory is often representational *and* verbal. The arguments are clear enough that they can be represented in graphical form. But the arguments are also rich enough that processes have to be described with sentences and paragraphs so as to convey the logical nuances behind the causal arrow. One indication that a strong theory has been proposed is that it is possible to discern conditions in which the major proposition or hypothesis is most and least likely to hold. Pfeffer and Salancik (1978), for example, argued that power is a stronger predictor of resource allocations under conditions of uncertainty. House (1988), likewise, made the case that individuals high in power needs are likely to gain control when organizations are in a state of flux. The reasoning underlying these predictions (even their direction) is not apparent by just showing the existence of moderating variables in a causal diagram. Logical explanations are required.

5. Hypotheses (or Predictions) Are Not Theory

Hypotheses can be an important part of a well-crafted conceptual argument. They serve as crucial bridges between theory and data, making explicit how the variables and relationships that follow from a logical argument will be operationalized. But, as Dubin (1976: 26) noted, "A

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theoretical model is not simply a statement of hypothesis." Hypotheses do not (and should not) contain logical arguments about why empirical relationships are expected to occur. Hypotheses are concise statements about *what* is expected to occur, not *why* it is expected to occur.

We cannot find a single source that asserts that hypotheses, or other specific predictions, alone constitute theory. As Kaplan (1964: 350) put it, "An explanation rests on a nomological or theoretical generalization, or an intelligible pattern, but a prediction need not have such a basis. . . . We can give a reason for making some specific prediction rather than another, but we may be able to give no reason other than past successes for expecting the reason to come true." Homans (1964), Merton (1967), and Weick (1989) are just a few of the authors who made clear that predictions presented without underlying causal logic do not constitute theory.

Although it may seem obvious that a listing of hypotheses cannot substitute for a set of logical explanations, this is exactly what is done in many papers. We have noticed two telltale signs that a paper has presented hypotheses in lieu of theory. First, there may be so many hypotheses that none can be adequately explained or motivated. A second tip-off is when the introduction of a paper ends with a long list of hypotheses, a table of predictions, or a summarizing figure. Often, such lists, tables, or figures are only tenuously linked to causal explanations scattered throughout the introduction, or there may be no linkage at all. In one extreme but by no means uncommon example, Tetrick and LaRocco (1987) tested 21 hypotheses about job stress without presenting the causal logic for any of these predictions. The 21 hypotheses were portrayed in a figure and not otherwise discussed or even listed in the five paragraphs constituting the introduction. Readers were referred to another source for the conceptual logic.

Sometimes authors use a long list of hypotheses to "spread the risk" of empirical research. So much time and effort is invested in research projects that authors naturally want to show something for their labor. They may use a buckshot approach to theory testing, posing a wide range of hypotheses and empirical tests. While this may increase one's publication record, it does not make good theory. Strong theory usually stems from a single or small set of research ideas. Some famous examples have been statements that people are motivated to resolve inconsistencies (Festinger, 1957), that social systems are subject to evolutionary forces (Campbell, 1969; Hannan and Freeman, 1989), and that there can be "normal accidents" (Perrow, 1984). These assertions were simple, though their implications have been widespread. From such simple theoretical arguments have come a set of interrelated propositions and hypotheses that explicated the logical and empirical implications of each theory. Papers with strong theory thus often start with one or two conceptual statements and build a logically detailed case; they have both simplicity and interconnectedness.

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IDENTIFYING STRONG THEORY

Though we have noted that it is easier to identify features of manuscripts that are not theory than it is to specify exactly what good theory is, our own prejudices about the matter are already evident. We agree with scholars like Kaplan (1964) and Merton (1967) who assert that theory is the answer to queries of *why*. Theory is about the connections among phenomena, a story about why acts, events, structure, and thoughts occur. Theory emphasizes the nature of causal relationships, identifying what comes first as well as the timing of such events. Strong theory, in our view, delves into underlying processes so as to understand the systematic reasons for a particular occurrence or nonoccurrence. It often burrows deeply into microprocesses, laterally into neighboring concepts, or in an upward direction, tying itself to broader social phenomena. It usually is laced with a set of convincing and logically interconnected arguments. It can have implications that we have not seen with our naked (or theoretically unassisted) eye. It may have implications that run counter to our common sense. As Weick (1995) put it succinctly, a good theory explains, predicts, and delights.

Like other descriptions of strong theory, the prior paragraph reads more like a wish list than a set of realistic expectations. This may be why pleas for better theory fall on receptive ears but recalcitrant hands. Everyone agrees that our theories should be stronger, so long as it does not require us to do anything differently. This is the main reason we decided to write something on what theory is not. Perhaps erecting our five "Wrong Way" signs will help change behavior in ways that more eloquent road maps have not.

THE CASE AGAINST THEORY

So far, we have made the assumption that theory is good. We have assumed that a stronger theoretical section will help a paper have more impact on the literature and more fully inform the reader. We have also assumed that most researchers would strive to write better theory if they had more knowledge about how to do so or more time and energy to put into their manuscripts. But these assumptions may not be universally shared.

Some prominent researchers have argued the case against theory. John Van Maanen (1989), for example, has stressed that the field first needs more descriptive narratives about organizational life, presumably based on intensive ethnographic work. He called for a ten-year moratorium on theoretical (and methodological) papers. The happy result of such a moratorium, Van Maanen suggested, would be a temporary halt to the proliferation of mediocre writing and theory, a broader audience (attracted by better writing), and better theory—after the moratorium had passed, both old and new models would be grounded in a well-crafted set of organizational narratives. Van Maanen's argument is reminiscent of logic contained in *Zen in the Art of Archery* (Herrigel, 1989). If we avoid aiming at the target for a long while and first develop more fundamental knowledge, we

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will do a better job of hitting the bull's-eye when we finally do take aim.

More direct arguments against theory can also be mustered from those who rely on quantitative methods. Some evaluation researchers, such as Thomas Cook, have noted that it is more important to isolate a few successful change efforts (those that show consistent positive results) than it is to understand the causal nuances underlying any particular outcome. Likewise, many advocates of meta-analysis view the mission of social science to be an accumulation of empirical findings rather than an ebb and flow of theoretical paradigms (Kuhn, 1970). They tend to see research publications as having value simply because they serve as storage devices for obtained correlations, not because they elaborate a set of theoretical ideas.

An array of organizational research publications have evolved to serve these disparate views of the merits of theory. At the most empirical end of the spectrum are journals such as the *Journal of Applied Psychology* and *Personnel Psychology.* These outlets typically present brief reviews of the literature along with a simple listing of hypotheses. The front end of these journal articles is typically short; the hypotheses are often replications or offshoots of previous work. More attention is paid to describing the methods, variables, data analysis techniques, and findings. Accordingly, the usual reason for rejecting a manuscript at these outlets is that the data do not adequately fit the hypotheses or there is a fatal flaw in the study design. The originality of the hypotheses and the strength of the theoretical arguments are less likely to constitute the major reason for acceptance or rejection.

An outlet such as *Research in Organizational Behavior* resides at the other extreme. The editors of *ROB* view theory development as its primary contribution. If data are presented, they are used for illustrating rather than testing a theory (e.g., Meyer and Gupta, 1994). The philosophy of *ROB* is not antagonistic to data collection and analysis; it simply relegates the role of empirical research to more traditional journal outlets.

Attempting to span the space between theory testing and theory building are journals like *ASQ, Academy of Management Journal,* and *Organization Science.* In the organizational research community, *ASQ* stands as perhaps the most concerned about theoretical issues, with the goal that empirical papers should also make a conceptual contribution. This bridging role is difficult to fulfill, since there are inevitable tradeoffs between theory and empirical research. On the one hand, *ASQ* asks authors to engage in creative, imaginative acts. On the other hand, *ASQ* wants these same authors to be precise, systematic, and follow accepted procedures for quantitative or qualitative analysis. These contradictory requirements can only be captured by phrases such as "disciplined imagination" (Weick, 1989), "wild thoroughness," or "accepted deviance."

Unfortunately, contributors to our field's research journals are rarely skilled at both theory building and theory testing. Most contributors seem to be adept at one or another parts

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of the trade; either being a good theorist with incomplete empirical skills or a good empiricist with halting theoretical abilities. Northcraft and Neale (1993) have noted that such shortcomings can sometimes be resolved by building research teams with complementary skills. But we suspect that there may not be enough strong theorists to go around. Organizational researchers are primarily trained in data collection techniques and the latest analytical tools, not the nuances of theory building. Our doctoral programs tend to skip over theory building, perhaps because it is not a step-by-step process that can be taught like LISREL or event-history analysis. Reading major theorists and writing literature review papers is often passed off as training in theory building, even though such assignments really don't teach one how to craft conceptual arguments.

Given our field's likely imbalance of theoretical and empirical skills, is the goal of providing strong theory *and* research a quixotic venture? Should journals make a decision—either to become a home for data or theory, but not both? So far, *ASQ*'s answer to the above quandary is "compensatory education." *ASQ* has tried to fill this breach through the review process, in which authors' attempts to write theory are scrutinized in detail by reviewers and editors. Pages of pointed criticism are conveyed to authors in hopes of "educating them." The product is usually an author who either dutifully complies with whatever theoretical ideas are suggested or who becomes so angered that he or she simply sends the paper elsewhere. By going through rounds of revision, a manuscript may end up with stronger theory, but this is not the same as saying that the authors have actually learned to write better theory. Learning to write theory may or may not occur, and when it does occur, it is almost an accidental byproduct of the system.

ARE WE EXPECTING TOO MUCH?

At this point in the essay we are forced to ask whether we have been naive. Perhaps there are enduring individual differences and preferences that explain why good theory is so hard to find in organizational research papers. Perhaps people who are driven more by data than ideas are enticed to join an empirically based field such as organizational behavior. Perhaps the applied nature of the field attracts practical, no-nonsense types rather than the more dreamy misfits who might naturally be good at theoretical pursuits. If this is so, then the importance of training should become an even larger issue. Without constant pressure for theory building, the field would surely slide to its natural resting place in dust-bowl empiricism.

The problem with theory building may also be structural. Journals could be placing authors in a double bind. On the one hand, editors and reviewers plead for creative and interesting ideas, for there to be an important contribution to organizational theory. On the other hand, authors are skewered for apparent mismatches between their theory and data. Providing a broad theory, in which a given phenomenon is located in a network of interorganizational or cultural influences, will usually lead to complaints that the author did not measure all the variables in his or her model.

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Providing a deep theory, in which intervening mechanisms or processes are spelled out in graphic detail, may likewise lead to objections that only the antecedents and consequences of the model are measured. Reviewers will typically say, "If a contextual variable or intervening process is so important, why wasn't it operationalized?"

Contradictory demands for both strong theory and precise measurement are often satisfied only by hypocritical writing. Theory is crafted around the data. The author is careful to avoid mentioning any variables or processes that might tip off the reviewers and editors that something is missing in the article. Peripheral and intervening processes are left out of the theory so as not to expose a gap in the empirical design. We are guilty of these crimes of omission. We have even counseled our graduate students to leave out portions of their theory that are not measured well and to delete otherwise interesting data that did not directly relate to their theoretical argument. The result of these omissions is that the craft of manuscript writing becomes an art of fitting concepts and arguments around what has been measured and discovered. If widely shared, as we suspect they are, these practices mean that our publications have little resemblance to what methodology texts preach as the proper sequence of theory building, design, measurement, and analysis.

So what should journals do to address the inherent difficulties of having strong theory and method in a single research paper? Should these outlets guard even more zealously the scientific sequencing of hypothesis-testing research, for example, by requiring that a list of all variables measured in the study (and their intercorrelations) be included with each submitted manuscript? Should journals spend even more time and energy on the review process, hoping to educate rather than just select manuscripts from the field's constituents? Or might our journals be best served by letting down their guard just a bit?

SOME RECOMMENDATIONS

When research manuscripts are divided on the dimensions of theory and method, it is easy to see where the bulk of our contributions lie. Papers with weak theory and method are routinely rejected. Their authors are sent back to the drawing board or on to another journal. At the other end of the spectrum are those few papers with both strong theory and method. These are the exceptional pieces that can become "instant classics," as they are hurriedly passed among scholars and discussed with twinges of jealousy. There are few controversies in the high-high and low-low cells of this matrix.

It is when we turn to the "mixed" cells of the theory-method matrix that we see conflicts of taste and value. Because so few papers are considered strong in both theory and method, journals are forced to make implicit tradeoffs on these dimensions to fill their pages. Even though journals may boldly espouse the goal of theory building, the review process usually works the other way. In practice, it is much easier for a set of reviewers and editors

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to agree on a carefully crafted empirical piece that has little or no theory than it is for them to go along with a weak test of a new theoretical idea. The author of this second type of manuscript can expect to receive a set of reviews stating, "although some interesting and well-motivated hypotheses were proposed, the author failed to . . . ."

Journals specializing in theory testing can live comfortably with the manuscript selection process as it now stands. They can reach consensus on publishing a set of papers that follow strict methodological guidelines to test existing theories. The problem is much greater with journals like *ASQ*. In trying to build theory as well as a database for organizational research, these journals push authors to their limits and beyond. A key difficulty is that papers chosen for revision tend to be those with acceptable methods and undeveloped theory. Extracting theory from those who could not (or would not) initially provide it can be a grueling and unpleasant process.

Our recommendation is to rebalance the selection process between theory and method. People's natural inclination is to require greater proof of a new or provocative idea than one they already believe to be true (Nisbett and Ross, 1980). Therefore, if a theory is particularly interesting, the standards used to evaluate how well it is tested or grounded need to be relaxed, not strengthened. We need to recognize that major contributions can be made when data are more illustrative than definitive.

We also think journals like *ASQ* need to revise their norms about the linkage between theory and data. Not everything discussed in the introduction of a manuscript need be operationalized in the method section nor show up in a set of regression equations. If theory building is a valid goal, then journals should be willing to publish papers that *really are* stronger in theory than method. Authors should be rewarded rather than punished for developing strong conceptual arguments that dig deeper and extend more broadly than the data will justify. We are not advocating long, rambling introductions that are entirely divorced from empirical analyses. Rather, we believe there is room for sharper discussion of processes underlying a phenomenon as well as grounding of causal forces in the broader social system.

In many ways, our journals have already been imposing these proposed standards on qualitative as opposed to quantitative research. The prevailing wisdom has been that qualitative research is more useful for theory building than theory testing. Rarely are qualitative studies accepted for publication when they simply provide data that validate an existing theory. Seldom are ethnographic descriptions published when they are not also a source of new concepts or ideas. It is even difficult to publish qualitative studies that provide in-depth analysis of a localized phenomenon if reviewers cannot be convinced that such knowledge is applicable to more general social processes.

Perhaps the standards used to judge qualitative papers have the opposite drawback of those used for quantitative papers, with theory emphasized too much and data not emphasized

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enough. Authors of qualitative studies are often asked to drop much of the description of characters and events, so as to make room for greater theoretical development. The resulting description may end up as little more than a small sequence of vignettes or a summary table of quotations, illustrating those concepts or hypotheses formulated in a paper. Such paring can deplete a manuscript of much of its value. Lost may be the rich description that Van Maanen (1989) said is necessary for researchers to build strong theory over time. Lost also may be the chance to build cumulative theory from small but comprehensible events. Weick (1992: 177) noted that much of his own work constitutes "knowledge growth by extension," which "occurs when a relatively full explanation of a small region is carried over to an explanation of an adjoining region." We may need to be as careful in not overweighting the theoretical criteria for qualitative papers as in underweighting the theoretical contributions of quantitative research.

CONCLUSION

We began this essay with the general complaint that many manuscripts we see as reviewers and editors are devoid of theory. In our experience, authors seem to fool themselves into thinking that at least five otherwise worthy features of a research paper can be theory when they are not. So we put up and explained five "Wrong Way" signs for authors. We hope these guidelines will help authors avoid writing manuscripts that contain little or no theory. But we are not so naive as to think that these few signposts will create a rush of new theory in organizational research. The problem is more complex and the solutions more complicated. We explored several structural reasons for the current imbalance between theory and method in organizational research, noting how the problem may stem from both the way we run journals as well as the nature and training of researchers who make up our field. Our conclusions, though sometimes oblique and contradictory, can be read as pleas for more balance in weighing the theoretical versus empirical sides of research. We argue for greater theoretical emphasis in quantitative research, along with more appreciation of the empiricism of qualitative endeavors.

In closing, we ask the reader to consider whether the evidence provided by people such as Freud, Marx, or Darwin would meet the empirical standards of the top journals in organizational research. Would their work be rejected outright, or would they be given the opportunity to go through several rounds of revision? Just thinking about such a question brings forth the essential role of balance (or tolerance) in evaluating research. When theories are particularly interesting or important, there should be greater leeway in terms of empirical support. A small set of interviews, a demonstration experiment, a pilot survey, a bit of archival data may be all that is needed to show why a particular process *might* be true. Subsequent research will of course be necessary to sort out whether the theoretical statements hold up under scrutiny, or whether they will join the long list of theories that only deserve to be true.

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