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STAW

TOWARD A SOCIAL PSYCHOLOGY OF ORGANIZATIONS

STUMBLING TOWARD A SOCIAL PSYCHOLOGY OF ORGANIZATIONS: An Autobiographical Look at the Direction of Organizational Research

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■ **Abstract** I recount some of my early experiences in the field and how they shaped my views about conducting research. As I describe it, my entry into organizational behavior was not at all seamless, requiring a series of adjustments along the way. Like many of my colleagues who had moved into the field of organizational behavior, I had to find a source of valued added---a new perspective or set of alternative ideas to contribute to the field. This process of adjustment, I fear, is no longer so prevalent in the field today. Although many social psychologists have migrated to business schools, they are still by and large doing social psychological rather than organizational research. They often extend social psychological theories to the business context, but they rarely seek to reframe and reformulate core organizational issues and problems. For this to change, I argue that future research needs to become more contextual and phenomenon-driven. My hope is that, with the recent upsurge in talent entering the field, we can find a way to harvest more of its creativity, moving from the application of social psychology to a genuine social psychology of organizations.

Keywords social psychology of organizations, applied social psychology, organizational behavior, contextualism, phenomenon-driven research, organizational problems

INTRODUCTION

As with the *Annual Review of Organizational Psychology and Organizational Behavior's* first two perspectives articles ([Porter & Schneider 2014](#), [Schein 2015](#)), I offer some background about our field as well as some observations about how it has evolved over the past decades. But I do this from a very personal standpoint. That is, I provide a brief narrative about how I entered the field and what awaited me as I began my academic career. The reason I start with such a personal narrative is not because my entry into the field was so special or idiosyncratic. It was probably fairly typical at the time for someone moving from a background in both social psychological research and business into the field of organizational behavior. However, what was important about my early experiences and those of many of my colleagues was the necessity for us to adapt as we migrated into organizational behavior. We entered the field with a set of skills and perspectives that required many alterations if we were to succeed in this new environment. In addition to our changing, we also worked hard to influence the field, with the product being one of mutual adaptation. Unfortunately, as I argue here, I do not think this same level of mutual adjustment is now occurring in organizational behavior, and as a result the field has not advanced as far as it could.

In this article, I mostly address the field of organizational behavior rather than industrial and organizational (I/O) psychology, given that was my port of entry and has been my working environment ever since. I focus especially on how the field of organizational behavior has changed over the years, mainly due to the increasing number of social

psychologists joining the area. The migration of social psychologists to organizational behavior has presented certain challenges as well as opportunities. Our field has become increasingly microscopic and theory driven, with the goal of extending social psychological theory to the organizational context rather than understanding the organizational context itself. As a result, I describe how we might redirect our efforts toward a more contextual and phenomenon-driven agenda. I close by offering some tips on studying negative rather than positive outcomes and some suggestions for steering the field away from the perspective of the corporation toward the welfare of the employee and the common good. My hope is that, with some substantive changes to our field, we can move closer to a social psychology of organizations rather than drifting toward an application of disciplinary research.

HOW I ENTERED THE FIELD

Graduate School

My introduction to the field of organizational behavior began during graduate school at the University of Michigan. I was enrolled in Michigan's MBA program hoping to someday become a top executive with a Fortune 500 company. However, soon after my arrival on campus I discovered that I enjoyed discussions of social science with friends studying political science and anthropology much more than any of my course material in accounting and finance. Because of my changing interests, I capitalized on Michigan's flexibility in its MBA curriculum, taking several psychology courses as electives. I managed to attend classes taught by some famed organizational researchers (e.g., Stanley Seashore, Arnold Tannenbaum, Basil Geogopolis), but I still wasn't sure I wanted to be a real psychologist. Luckily, I found what I believed to be the perfect compromise in an emerging field called organizational behavior. The only complication was that it was 1968, and the Vietnam

War was raging. First, I needed to find a way to avoid being drafted into a war I firmly opposed.

Northwestern had circulated a flyer advertising a new program in what it called, Organization Theory, boasting an interdisciplinary faculty drawn from business, engineering, psychology, sociology, political science, and anthropology. This sounded perfect. It was an ideal blend for someone like me who was unsure of his intellectual and professional leanings. And, to make matters even better, the founder and coordinator of the program, Michael Radnor, assured me that I would be able to get a draft deferment if I worked as a research assistant on his NASA grant. So, off to Northwestern I went, excited about learning everything I could about this budding field of organization theory.

Soon I discovered that prospective students should be a bit more skeptical when reading flyers about new academic programs. Most of the faculty associated with this interdisciplinary program had no plans to teach any courses related to organizations, nor take on students specializing in the area. In fact, the existing program consisted only of a couple of seminars on macro-organizational behavior, held during students' second year. For our first year, we were basically on our own, advised to take the graduate core in sociology, psychology, and economics. After some initial panic and feelings of abandonment, each of the new students gravitated to either the psychology or sociology department. But we were migrant scholars, and as such we had to perform well enough for the disciplinary faculty to pay some attention to us.

I was fortunate to work with Tom Cook, Phil Brickman, and Don Campbell in Northwestern's psychology department. My promised draft deferment did not pan out, but I managed to avoid the Vietnam War through a series of appeals and legal maneuvers. I was especially fortunate in being able to combine my concern about the draft with an appreciation of field experimentation gleaned from courses with Cook

and Campbell. In fact, my first true introduction to social science research was a joint project with a fellow student (Bill Notz) that used the draft lottery as a natural field experiment to predict students' attitudes toward the Vietnam War. Tom Cook helped us publish this research in the *Journal of Psychology and Social Psychology* (Notz et al. 1971), and we meticulously studied each of his alterations and additions to begin learning the craft of publishing psychological research.

From our training at Northwestern, Bill Notz and I also appreciated how rare it was to have a naturally randomized treatment---and therefore we both wanted to use the draft lottery in our dissertations. But doing so would require a "joint dissertation," something that had never been tried before, at least at Northwestern. Several faculty members from psychology and business advocated for us to be a test case for the idea, but, alas, it was turned down by the graduate school. As a result, Bill and I divided up our proposed studies. He took the education sample and looked at the effects of the draft lottery on young men's decisions to stay in college (Notz 1977). I used the lottery to test self-justification versus self-interest effects on the motivation of ROTC cadets. I wanted to see how men who had joined ROTC primarily to avoid being drafted would cope with the program once they had received a safe draft number, one that invalidated their original reason for joining the organization. In this context, self-justification prevailed, with cadets' attitudes and performance showing improvement after receiving high lottery numbers (Staw 1974).

Getting a Job

Armed with what I thought was an interesting dissertation, I started searching for an academic job. I soon learned that my background and interests were not preferred for incoming faculty in organizational behavior, and as a consequence, several job interviews were disastrous. One campus visit began with a heated argument with a business school

dean who thought social science research was something that young faculty should avoid. Another interview with a representative of a top business school ended with the observation, "Of course we won't be interested in hiring you, but would you like to know what I think of your dissertation." I said, "Sure," feeling confident in the value of my work. His response was, "It's like pissing in the breeze. That's what I think of your dissertation...an utter waste of time." From these two interviews alone, I might have concluded that organizational behavior was not a favorite topic for business school faculty. But it was more than that. The major stumbling block was that I was not addressing an important problem in a business context, with members of the business community as my respondents. It didn't matter that the research had a unique data set or capitalized on an unusual event (i.e., the draft lottery). What mattered was that I had not attacked a real business problem, nor gathered my data from real employees at an actual business organization.

Fortunately, there were a few departments interested in hiring me. Indiana University was objectively the best option, given that it had a seasoned group of organizational behavior faculty (led by Bill Scott) who often conducted experimental research. But I opted for the University of Illinois' business school, which was trying to build a new group in organizational behavior. A year earlier, Jeff Pfeffer had arrived from Stanford Business School with an exciting agenda of sociological research on organizations. Jerry Salancik and Bobby Calder were also new arrivals. They were both creative social psychologists who had crossed over to organizational research from Yale and North Carolina's psychology departments. The next year we added Greg Oldham from Yale's Department of Administrative Science and Lou Pondy who was already on the faculty at Duke. This was soon followed by Keith Murnighan from Purdue's social psychology program and Dave Whetten from Cornell's program in industrial relations.

As strong as this group of faculty now appears in retrospect, it was not an easy beginning for most of us. We were not led by a group of established scholars, especially on the micro side, and we had little contact with the much better known and respected I/O psychology program within Illinois' psychology department. Not surprisingly, during my first year at Illinois only Pfeffer was having success at publishing in organizational research journals. The rest of us were receiving rejections that read more or less like the following: "This might be fine social psychological research, but it is not relevant to the fields of organizational behavior and I/O psychology." Journals dismissed our arguments that an understanding of organizational behavior required a deeper understanding of behavior in general, as well as any claim that there may be principles of behavior that travel across contexts, content, and samples of subjects. So we had to adapt. Salancik started conducting research with Pfeffer on power in organizations. Calder moved into marketing research. And I started a project with Greg Oldham, hoping to glean some insights from his extensive training with Richard Hackman, an established leader in the field I was earnestly trying to join.

At this early point in my career I was very frustrated not only with the internal workings of the field (i.e., the publication process), but also its finished product. My training in experimental methodology left me entirely skeptical of the correlational findings that then dominated organizational behavior and I/O psychology. So I tried a frontal attack. I wrote a paper with the somewhat grandiose title, "Attribution of the 'Causes' of Performance: A General Alternative Interpretation of Cross-Sectional Research on Organizations." I argued that many of the field's most accepted findings could be explained by reverse causation, which I tried to demonstrate with a simple group experiment. I showed that when subjects were fed false information about their workgroup's performance, they would rate the group's characteristics (e.g.,

cohesiveness, conflict) in accordance with its performance. This seemed natural enough and in line with developments in the psychology of causal inference. Nonetheless, reviewers at two major journals rejected the paper because it was based on artificial groups in a laboratory experiment. I was naturally dejected, but decided to give the paper another try with a newer outlet, then called *Organizational Behavior and Human Performance*. The piece again received uniformly negative reviews. Still, Karl Weick, then the journal's associate editor, encouraged me to add a second experiment and to resubmit the paper. Although the reviewers thought the revision only made the paper worse, Weick accepted the piece over their unanimous objections. To this day, I have never observed such a brave editorial decision. It probably saved my career, given that I was having grave doubts about whether I would ever be allowed to play a useful role in this field.

The reason I go into such detail about these early career experiences is not that I had a set of encounters that were entirely unique. Others suffered a similar fate. My point is simply to emphasize that a certain amount of adaptation was needed to survive as a young scholar moving into the field of organizational behavior. My prior training did not fit the basic orientation or skill set that was dominant in the field at that time. I had arrived to the field with reasonable training in social psychology and macro-organizational behavior (Northwestern's organization theory program was largely macro at that time), although I knew little of I/O psychology or micro-organizational behavior. So, like others who had migrated into our field during the 1970s and 1980s, I had to make some serious adjustments. In essence, each of us had to figure out what value added we could bring to the field, so that we might use our particular backgrounds as a resource in reformulating theory and method. Unfortunately, I do not believe such a process of adaptation is happening (at least to the same extent) nowadays. In fact, I would argue that, as the number of social psychologists affiliated with departments of

organizational behavior and management has grown over the past decade, fewer of these new entrants are seeking to change the field. Instead, most seek to advance their careers as social psychologists rather than challenge and change the study of organizational behavior. I will elaborate on this somewhat harsh observation, as I believe it is so consequential to our field today.

THE INFLUX OF SOCIAL PSYCHOLOGISTS TO THE FIELD OF ORGANIZATIONAL BEHAVIOR

Over the past decade, a number of prominent senior scholars have either migrated entirely from psychology departments to business schools or have chosen to split their time equally between the two fields. At the junior level, the influx has been even more pronounced. As a consequence, the ranks of micro-organizational behavior faculty at many of the top US business schools are now filled primarily with social psychologists rather than people trained by other organizational behavior groups. This is generally the case at Stanford, Chicago, Northwestern, Berkeley, Columbia, Duke, Yale, and UCLA, to name a few of the leading programs in organizational behavior and management. How did this happen?

There have been several catalysts for this sea-change in the composition of organizational researchers. One driving force has been the disparity in resources between business schools and the disciplinary social sciences. While university budgets have continued to be strained, with few position openings in the areas of social psychology or I/O psychology, business schools have expanded rapidly with positions regularly available in the areas of organizational behavior and management. Salary differences have also widened between business schools and psychology departments, making fields such as organizational behavior, management, and marketing destinations of choice rather than necessity.

Although differences in job openings and salaries have existed for decades, until recently business schools had been quite reluctant to hire social psychologists, especially at the junior level. Even though newly minted social psychologists generally have had a longer list of publications than those receiving doctorates from business schools (especially when coming from a prolific lab), there were always fears about whether these young psychologists could handle MBA teaching. Northwestern's postdoc program was instrumental in dispelling that fear. Northwestern was one of the first business schools to popularize the teaching of negotiation, and their approach to the subject fit well with the talent pool of social psychologists intending to move to the business school environment. Northwestern's approach to negotiation research relied heavily on lab experimentation, and its teaching pedagogy involved the extensive use of exercises and simulations of business negotiations. Teaching negotiation courses was therefore an easy transition for young psychologists who had little background in business. After spending two years in Northwestern's postdoc program (or in several other programs modeled after it), social psychologists were then positioned to dominate the entry-level market for assistant professorships in organizational behavior. Their resumes were longer. Their teaching experience was now relevant to the business school environment. And, given an extra two years for conducting research, these postdocs appeared to be much stronger job candidates than newly minted graduates from organizational behavior doctoral programs.

A third factor underlying the influx of social psychologists into organizational behavior has probably been the evolution of the field itself. When I entered the field there was an emphasis on interdisciplinary research, where concepts were routinely borrowed from psychology, sociology, and economics to explain how people behaved in the organizational context. Organizational behavior groups were a place where people came together from many disciplinary backgrounds to

address some common properties and problems of organizations. However, over the past decades the field has bifurcated into distinctly micro and macro branches. Whereas doctoral students in organizational behavior were expected to take theory courses in both sociology and psychology, current students stick primarily to one core discipline. Whereas dissertation committees would routinely include micro and macro scholars, committees are now more uniformly psychological or sociological in their composition. Currently, faculty in micro- and macro-organizational behavior go to different annual meetings (except for the Academy of Management), read vastly different literatures, and use methodologies that specifically reflect their core disciplines. In fact, it is now rare for an organizational behavior group to seek a generalist in organizational research; instead, they search for a candidate in micro- or macro-organizational behavior, where the micro-macro split means specialization in psychological or sociological research. Young scholars who might choose to take a multidisciplinary approach, perhaps using multiple methodologies, often face more difficult job prospects than those taking a more orthodox, disciplinary approach.

In summary, a confluence of forces have worked to increase the number of social psychologists in the field of organizational behavior. What may have started with the lure of greater resources (salary and positions) provided by business schools only took hold as postdoc opportunities provided the mechanism for crossing from psychology to business. And, as this migration started to pick up steam, it also made increasing sense from a research perspective, as the field of organizational behavior became specialized into exclusively micro and macro branches. In recent years, these trends have become self-reinforcing, with those doing the hiring implicitly (and explicitly) replicating themselves through the recruitment and selection process.

WHAT ARE THE CONSEQUENCES OF THIS SHIFT?

The migration of social psychologists to the field of organizational behavior could be expected to bring new impetus to our understanding of behavior in organizations. Perhaps the ambitions of classic works such as *The Social Psychology of Organizations* (Katz & Kahn 1966) and *The Social Psychology of Organizing* (Weick 1969) might finally be realized with this dramatic increase in talent coming into the field. The new wave of immigrants, like prior generations of social psychologists, might reexamine the constructs and theories that currently prevail in the field of organizational behavior and reshape them in more productive ways. And, given the changing demographics of the field (moving from minority to majority status), this transformation might be a smoother task than what my colleagues and I faced during the 1970s and 1980s. In other words, the new majority could seamlessly reshape what now constitutes micro-organizational behavior.

Unfortunately, this is not what is happening in our field today. Although social psychologists from previous generations had to adapt their research to succeed in the business school environment, this is not required of the current group of transplants. The current generation continues to publish in disciplinary journals such as *Journal of Personality and Social Psychology* and *Psychological Science* rather than adapt their wares to outlets such as *Administrative Science Quarterly* and the *Academy of Management Journal*. Their style of research has also not been altered much in the transition. It is still largely aimed at validating psychological processes rather than understanding organizational phenomena or problems. It is still largely based on laboratory experimentation using student samples rather than field observations of organizational members, analyses of archival data, or even field experiments. It is still largely based on short-term manipulations (e.g., “recall the last time...”) rather than ongoing experiences in the organizational context. And, the focus of research is still mainly concerned with individual perception and behavior rather than the

dynamics of behavior as they play out in organizations across multiple levels of analysis. In short, the research of social psychologists working in business schools is not very different from that of social psychologists working within their home discipline. Such limitations in focus and method have served to unduly restrict organizational research, at least from what it could potentially be. I provide some examples of these limitations by briefly examining two topics of research that have been most heavily influenced by the influx of social psychologists, that of power and decision making.

POWER IN ORGANIZATIONS

One of the areas of real progress over the past 15 years is the social psychology of power. We have learned that the experience of power can lead to a broad increase in personal agency, including one's action-orientation ([Galinsky et al. 2003](#)), self-assertion ([Anderson & Berdhal 2002](#)), risk taking ([Anderson & Galinsky 2006](#)), and creativity ([Galinsky et al. 2006](#)). However, an increased focus on one's own desires and the means to accomplish them also seems to come at the expense of the needs of others. The experience of power has been found to lead to rude, selfish, and even unethical behavior (~~[Ward & Keltner 1998](#)~~[G. Ward, D. Keltner, unpublished manuscript; Keltner et al. 2003](#)). Not only are those with power less inhibited in the expression of their emotions ([Keltner et al. 2003](#)), but the expression of certain emotions such as anger may actually lead others to attribute more power to the actor ([Tiedens 2001](#)). These are just a few of the findings coming out of this productive stream of research (see [Anderson & Brion 2014](#), [Anderson & Brown 2010](#) for reviews).

The practical application of research on power to the business world often involves the teaching of techniques for gaining power, such as seeking and allocating valued resources, participation in decision-making

bodies, and the formation of strategic alliances. However, more recently, with the influx of social psychologists into business schools, there has also been an emphasis on increasing the perception of power. For example, MBA students can be empowered through the recall of past experiences of influence ([Galinsky et al. 2003](#)) and even the repositioning of one's body into a wide posture ([Carney et al. 2010](#)), with the goal of helping those displaying behaviors associated with power to move up the hierarchy. Unfortunately, given the many downsides of the self-perception of power (e.g., self-centered and even unethical behavior), training others to enact power moves could come with some serious side effects. As a result, we may need research on ways to restrain those with power as well as ways to mitigate the collateral damage to others. Some of this may also require the bolstering of the power of followers. We already know that increasing a leader's sense of power can inhibit the expression of opinions and ideas by those interacting with the high-power person ([Locke & Anderson 2015](#), [Tost et al. 2013](#)). We may therefore need more research on how to get the voices of those without power to be effectively heard. When tasks are highly interdependent and based on dispersed knowledge, effective mechanisms for sharing that knowledge are needed. But, unfortunately, power is often confused with competence, such that those with expertise but low in power are not heeded. Thus, at a minimum, there should be greater emphasis on the antecedents and techniques of upward influence, so that those with less power can play a more significant role in group and organizational interactions.

A related line of inquiry might focus on how leaders can not only possess and exercise power, but at the same time maintain some understanding and compassion for others. Some research has already found that under certain circumstances power can foster a sense of responsibility rather than just entitlement ([Tost 2016](#)). Somewhat ironically, when empowered leaders see others' contributions as

important for their own (and their group's) achievement, they tend to be more responsive and inclusive in their behavior toward others ([Tost et al. 2013](#)). Thus, it may be time to revisit alternative forms of organization such as flatter and more interdependent forms of hierarchy, where groups function in a collaborative rather than hierarchical fashion. We may also want to re-examine the selection of organizational leaders. I suspect there are personality characteristics that allow some leaders to exercise power without the usual bluster and disdain for others.

However, these same personality characteristics may now serve as a handicap in rising to the highest levels of the organization. Thus, we should not just study the consequences of decent and wise leadership, but how wise and decent people can attain positions of leadership.

As [Pfeffer \(1981\)](#) noted long ago, the exercise of power is the way things get done in organizations. Grand strategies are nothing but rhetoric if they are not actually implemented into budgets and action plans. Corporate visions do not often move beyond the boardroom when they are subtly resisted by lower levels in the organization---when people nod their heads in agreement but drag their feet during implementation. Thus, defining power as the capacity to alter another's behavior may not be as useful as thinking of power as the ability to get others on board with enthusiasm and zeal ([Grant 2012](#)). Logically, this may be where the construct of leadership starts to have meaning beyond the simple ascription of power or the exercise of specific forms of managerial behavior such as structuring work and setting goals.

At this juncture, it may be most useful to link research on the psychology of power to more macro research on control structures (see [Tost 2016](#) for a start in this effort). Power norms in the organization should certainly be part of this endeavor, especially as they relate to the exercise of power in interpersonal interactions. It may also be helpful, however, to examine the psychological effects of organizational structures designed to broaden the influence of employees (e.g., via

worker's councils, board representation, and other forms of participation) beyond what is currently practiced in most business firms. We might also reexamine the flexibility of power structures over time, going back to the question of whether organizations with more fluid power structures are more adaptive over time, given that they may be more responsive to new demands and opportunities in the environment. Finally, we might welcome a return to [Tannenbaum's \(1968\)](#) classic work suggesting power to be an additive rather than relative concept, where perceptions of *total* rather than *relative* power are associated with greater organizational effectiveness.

DECISION MAKING

Arguably, one of the greatest achievements in the social sciences over the past few decades has been the development of theory and research on judgment and decision making (or JDM; see, e.g., [Kahneman et al. 1982](#), [Kahneman & Tversky 2000](#)). This work not only resulted in a Nobel Prize for Daniel Kahneman, but with it a belated appreciation of cognitive shortcomings by the field of economics, which had previously assumed that human behavior was the product of fully rational self-interest. Although the field of organizational behavior has long valued the concept of bounded rationality (e.g., [Simon 1957](#), [March & Olsen 1976](#)) and understood the prevalence of decision errors (e.g., [Bazerman 1994](#)), it was recently argued that the fields of I/O psychology and organizational behavior would be greatly enriched by paying closer attention to JDM research ([Dalal et al. 2010](#)). I'm not so sure, at least as JDM research is currently practiced.

One of the beauties of JDM's research findings is their simplicity. We have learned that people generally fall victim to common biases and heuristics, using rather straightforward comparisons to objective facts or by drawing clear contrasts with economic rationality. The results of these

experimental tests have been incorporated into a litany of cognitive shortcomings such as overconfidence, anchoring, insensitivity to sample size, hindsight bias, sunk cost effects, etc. Despite this impressive list of findings, however, some scholars have persuasively noted limits to JDM's effects. People may not be so error prone if they are placed in a more accountable state, where their outcomes or processes can be observed by others and where there are incentives to be careful and accurate (e.g., [Tetlock 1985](#), [Lerner & Tetlock 1999](#)). Moreover, patterns of causal attribution and perception typically found in Western psychological research may not always extend to other contexts (e.g., [Nisbett 2003](#)). Even the very act of choice has been found to be culturally dependent. European Americans not only value independent choice to a greater extent than Asians, but they are also more likely to perceive their environments as containing a greater number of discrete choices or decisions ([Markus & Schwartz 2010](#)). Thus, when attempting to extend JDM research to organizational settings we need to refrain from academic hubris. What may appear to be universal biases and heuristics may instead be tendencies that are a product of culture and context.

Because JDM research is generally laboratory based, it is often an exercise in parsimony. The goal is to isolate particular psychological forces, holding extraneous influences constant so that main effects (and interactions) can be demonstrated clearly and convincingly. In organizational settings, however, there may be a multiplicity of forces creating background noise that obscures all but the strongest JDM effects. The context may also facilitate multiple decision errors that contradict each other. For example, investment decisions could be influenced by some forces that promote conservatism (e.g., a status quo bias) at the same time that other forces stimulate risk seeking (e.g., prospect theory's predictions for gambling in a loss situation). Another complication is that well-known (or easily observed) biases may give rise to efforts to compensate for their effects. For example, consider the

commercial real estate industry where it is commonly understood that developers (those who propose construction projects) generally exaggerate the positive outcomes of their proposals. Experienced investors are thus quite wary of the estimates provided by developers, discounting their optimistic promises. The situation is similar within corporate settings, where managers routinely promote projects that enhance their own power and influence. Because data are expected to be tilted in a self-serving manner, executives in charge of allocating resources are thus likely to deflate managers' overconfident judgments and excessively optimistic projections. Adding to this complexity, sponsors of competing projects may also work hard to find and publicize flaws in the estimates of their rivals, helping to squash their rosy predictions.

Thus, a key question for researchers on topics such as overconfidence is whether biases will survive in an organizational arena that naturally includes a variety of checks and balances. Just because a decision bias has been repeatedly validated in the lab does not mean that it will be manifested in the same way within an organizational environment. Perhaps some biases are so ingrained that they will survive countervailing and confounding forces, so that their effects can be found in archival data over long periods of time (e.g., [Hayward & Hambrick 1997](#), [Staw & Hoang 1995](#)). Some biases may even be heightened by a multiplicity of facilitating forces, as [Janis' \(1972\)](#) found in his early investigations of groupthink. Still others may not survive at all in a naturalistic situation. Therefore, as researchers, we must ask why some decision biases develop and grow in the wild, whereas other errors are more constrained in the organizational environment (see [Zsombok & Klein 2014](#) for some empirical leads).

APPRECIATING THE ORGANIZATIONAL CONTEXT

My comments about recent research on power and decision making can be broadly interpreted as a call for greater contextualism. As [McGuire \(1983\)](#) noted more than 30 years ago, experimentation involves the subtle art of set design, where central issues or forces are made salient to subjects whereas others are relegated to the background. In organizational experimentation, greater contextualism may therefore mean less of an emphasis on artificial tasks (such as spinning dials and estimating the weight of others) and more attention to the simulation of work experiences (e.g., through in-basket exercises and more realistic scenarios). However, contextualism is more than just trying to make experiments somewhat realistic. It also involves greater appreciation for the phenomenon under study, and this may be a demanding chore for the current crop of social psychologists working in business schools.

To get an accurate fix on a given phenomenon one has to understand the environment in which it takes place. Often such an understanding results from personal experience in the field. For example, Arlie Hochschild had worked as a flight attendant before becoming a sociologist and turning her attention to emotional labor. John VanMaanen had spent summers as a young Disneyland employee before later examining the Magic Kingdom from a research perspective. Rod Kramer had a short career as a screen writer before becoming an organizational behavior professor conducting research on Hollywood pitches. Even my own research on escalation of commitment benefited from some important vicarious experience. As a young man I watched my father attempt to turn around a failing business. He was one of the early entrepreneurs in discount retailing, growing his company from a single store to a thriving regional chain. Once large national corporations entered the industry, however, most of the independent operators were forced to drop out. They either sold their businesses or gradually withdrew their personal resources from their firms. Instead, my father moved more and more of his own funds into the business as it declined---

until it finally failed and he had depleted most of his assets. His was a classic case of escalation of commitment.

Obviously, not all researchers have the benefit of personal experience to elucidate their object of study. They do, however, have the opportunity to conduct interviews with the actors they intend to study and the possibility of observing the environment they seek to simulate. They also can capitalize on alternative methods in their pursuit of a phenomenon. For example, in my early research on escalation I relied on some simple scenario studies to demonstrate that individuals have a tendency to throw good money after bad ([Staw 1976](#)). This preliminary finding was replicated, extended, and moderated through further laboratory research (see [Brockner 1992](#), [Sleesman et al. 2012](#) for reviews). I was pleased, on the one hand, that my early work was being followed up. On the other hand, I was uncomfortable with the near total reliance on laboratory research to test the phenomenon. I kept wondering why no one had yet conducted a case study to see how escalation takes place in a real organization, and why no one had yet conducted archival research to demonstrate escalation's effects across multiple settings over time.

I was not a qualitative researcher, but I let Jerry Ross convince me that we should conduct an in-depth case study on the decision to hold the World's Fair, Expo 86. At the time, Jerry was a professor at Simon Fraser University located near Vancouver, British Columbia, the prospective site for the fair. He had access to a wealth of information and was enthusiastic about sharing it with me. Still, I was a bit hesitant. Although Jerry and I had conducted many lab studies on escalation, neither of us had much experience in analyzing qualitative and historical data. Luckily I listened to Jerry, since we learned a great deal about escalation through this case study. The events leading to Expo 86 illuminated the importance of social and organizational processes for understanding how escalation operates. We learned that commitment to a course of action

might be influenced by a leader's identity being tied to a particular project, by departmental or subunit power being based on the survival of a venture, and by a project or product becoming so institutionalized that withdrawal becomes almost unthinkable (Ross & Staw 1986). In addition, from this case study we began to appreciate how multilevel forces can often occur sequentially, where individual decision errors give rise to social and organizational processes bolstering commitment (Staw 1997). Such observations would not have been possible if our research had been confined to the laboratory. Only by moving from the lab to the field, and by moving from controlled experiments to more qualitative case studies, was it possible to understand the range and sequencing of forces that hold organizations to losing courses of action (see also Ross & Staw 1993).

Established wisdom on research methodology teaches us that different methods possess distinctive strengths and weaknesses in terms of internal and external validity (Campbell & Stanley 1966). Moreover, a quick survey of the social sciences would lead us to conclude that particular methods tend to be disproportionately associated with certain academic disciplines (e.g., lab experimentation with psychology, archival research with economics and sociology, qualitative methods with anthropology and sociology). Therefore, given that all research is flawed in some fundamental way (McGrath et al. 1982), the only route to achieving a better understanding of a phenomenon is through the use of multiple methodologies. Unfortunately, as Chatman & Flynn (2005) have noted, few topics in organizational research have gone through such a "full cycle" of research. More typically, topics are explored almost exclusively by a particular methodology, and this is especially the case when most of the researchers addressing the topic have come from a common disciplinary background. Thus, my plea to young scholars who have recently entered our field is to try some alternative methodologies,

even if they are only a means to enrich the methods on which one normally depends.

PHENOMENON-DRIVEN RESEARCH

When I argue for the need to contextualize research, I am not necessarily saying that we need to avoid laboratory research. We do need to make lab research more reflective of the contexts in which organizational phenomena operate. However, beyond these typical pleas for greater external validity, I am also arguing for research that is designed to understand *organizational phenomena* rather than more general psychological theory. The purpose of most psychological research is to test fundamental theory and/or extend that theory to new contexts. For example, some excellent research on perspective taking recently extended that psychological process to intergroup relations, negotiations, and ethical behavior (see [Ku et al. 2015](#) for a review). Although this program of research has been fruitful, it is primarily the extension of social psychological theory to the organizational context rather than an effort to generate new theory *about that context*. Alternatively, if we were to construct new theory explaining some aspect of organizational life (e.g., intergroup conflict), perspective taking would be included only if we deemed it to be central to that issue.

In my view, constructing theory in organizational behavior should primarily be about understanding organizational problems and/or phenomena. Problem-driven research has long been the central mission of I/O psychology, and this has been a source of strength as well as weakness. It is a strength in that attention is focused on outcome variables that are considered important by one or more constituencies. However, it can also be a weakness when the field focuses on a very limited set of variables and from a very limited perspective. Early in my career, conversations with colleagues would often include jokes about

publishing, noting that a study would only be publishable if it addressed one of four dependent variables: work performance, absenteeism, turnover, or job satisfaction. Even when I wrote an annual review chapter on the field ([Staw 1984](#)), I argued for a reformulation of research on these same four variables rather than a wholesale scuttling of the list. I did try to stretch the list of outcomes to include creativity and innovation, but that was the limit of my bravery.

By proposing that social psychologists become more grounded in their organizational research, I am not advocating an unfamiliar role. Remember that much of the research on attitude change originated from efforts to sell bonds during World War II as well as concerns over the impact of propaganda. Similarly, the examination of the bystander effect ([Darley & Latane 1968](#)) started with questions about the murder of Kitty Genovese, when numerous witnesses observed a brutal crime but did not call the police for help. Therefore, as important organizational events occur in the world (e.g., a corporation's admission of financial fraud, a severe industrial accident, or a major product breakthrough), they should spark our interest in building theory that might explain these outcomes. Even everyday contradictions and anomalies should wet our appetite for research. In a sense, my study of dispositional sources of job attitudes ([Staw & Ross 1985](#), [Staw et al. 1986](#)) was a product of anomalous events. It resulted from the observation of a close relative continuing to be disgruntled as her work and life situation were both improving. This led me to challenge prevailing theories of job attitudes that had emphasized either the objective nature of the work ([Hackman & Oldham 1976](#)) or the role of situational influence ([Salancik & Pfeffer 1978](#)), and prompted me to test a more dispositional theory of job attitudes.

In advocating contextualism, I also want to make the case that psychological research on organizational phenomena can reach upward and outward rather than just in an inward direction. That is, we can use psychological insights to explain organization-level phenomena, as

[Chatterjee & Hambrick \(2007\)](#) have done in constructing measures of CEO narcissism to explain corporate acquisitions and erratic financial performance. As Bob Sutton and I argued in a piece called “Macro Organizational Psychology” ([Staw & Sutton 1993](#)), there are at least three major ways that psychological forces can influence the behavior of organizations: (a) Autonomous agents can represent the organization to outside publics; (b) powerful members can influence organizational structures, reputations, and performance; and (c) the aggregation of individual traits, emotional states, and beliefs can shape organizational attributes and behavior.

The examination of upward and outward influence can also be more than an inventory of which micro features (such as individual characteristics) are associated with macro variables (e.g., group and organizational structure). As recently elaborated by [Morgeson et al. \(2015\)](#), important events may initially confront individuals and then subsequently induce changes in the work group and larger organization. An event can also initially hit the organization and ~~their~~ its impact then filter down to the individual member. Regardless of the direction and scope of such events, however, the major point of Morgeson et al.’s model is that events can be usefully traced through the organizational system. By following the impact of particularly important and disruptive events, we may begin to understand processes that extend from the individual actor to other entities in the organizational system.

At present, most research in micro-organizational behavior is not particularly concerned with how individual behavior can influence the social context in which people work, let alone the larger scale actions of the firm. Instead, most of the energy in current research is devoted toward more microscopic explanations of the person’s behavior. It has, for example, become standard practice to include tests of mediation in most psychological research papers. The idea is to isolate the process underlying an empirical finding so as to “prove” that a particular

theoretical mechanism explains a given effect. However, often these tests of mediation don't delve much deeper than the older practice of conducting a manipulation check, especially when they entail measures that are simple acknowledgments of having received the treatment rather than an assessment of the underlying process. And, even when they are informative, we should keep in mind that mediation analysis contributes to the trend toward delving inward rather than searching in an outward or upward direction to explain a person's influence on other people and/or entities in the organization.

In recent years, the trend toward inward discovery (or reductionism) has been heightened by the use of neurological and other forms of physiological measurement (e.g., [Akinola 2010](#)). Although past research occasionally used blood pressure and heart rate monitoring as an indicator of stress, it is now more common for organizational researchers to include saliva tests for levels of cortisol and brain imaging in studies of decision making, attraction, and attitudes. Whereas traditional research might measure a person's affect with paper and pencil scales, modern neuroimaging provides objective validation by demonstrating activity in areas of the brain generally associated with pleasure or pain. However, do such measures really provide an advance, and if so, are they worth the added difficulty and expense? Old-fashioned measures carry the liability that they are generally reactive instruments. Respondents may act differently when they know they are being observed, perhaps tailoring their answers to the tastes and dictates of the questioner. For example, being asked a question about job satisfaction may be interpreted (and answered) quite differently depending on whether the question comes from someone associated with management (the usual case with organizational behavior research), from someone representing a union, or from a sympathetic person sitting next to the respondent on a commuter train. Physiological measures may make it more difficult for the respondent to hide his/her true emotions. However, given that

physiological measurement is rarely unobtrusive (at least not with our current technology and ethical standards), what we think are objective responses may still be colored (perhaps in a complex way) by the motives and fears of respondents. And how should we interpret significant differences between physiological data and more traditional scores? Which do we believe? If I think I am happy, but show physiological distress, am I still happier than when I think I am unhappy, yet show physiological contentment? Such a philosophical dilemma should not only perplex us but serve as an impetus toward greater understanding of the meaning of psychological states.

MOVING FROM APPLIED SOCIAL PSYCHOLOGY TO A SOCIAL PSYCHOLOGY OF ORGANIZATIONS

Much of my discussion, to this point, could be characterized as a lament that our understanding of organizational behavior may have been unexpectedly sidetracked by the influx of social psychologists. I have made the case that, with their increased numbers and greater legitimacy within the business school environment, social psychologists have not had to make the painful adjustments necessary to really understand organizational phenomena. As consequence, many of these scholars have continued to pursue basic psychological research, while others have sought to extend social psychological theory to the organizational context. Much of my own work probably falls into this second category of research, given that my lab studies of escalation could be interpreted as a simple extension of dissonance and self-justification theory to the organizational world. Although useful, this applied social psychology falls short of the ambitions of a true social psychology of organizations.

[Table 1](#) summarizes some of the key differences between social psychology applied to organizations and a more far-reaching social psychology of organizations. As noted in the table, most applied social psychology uses laboratory or field experimentation as a way of

extending existing psychological theory to organizational settings. In contrast, a social psychology of organizations seeks to understand the phenomena and problems of organizational life, and this goal is pursued with multiple methods, multiple levels of analysis, and a search for causal influences that may travel in an upward, outward, as well as downward direction. With applied social psychology, the context is generally seen as a factor that limits the external validity of findings, something that must be accounted for in the design of more realistic lab settings and through a broader sampling of field sites. With the social psychology of organizations, understanding the context is the main focus of inquiry. It is why new theory must often be built to explain processes that go beyond or in a different direction from more basic psychological research.

<COMP: PLEASE INSERT TABLE 1 HERE>

ORGANIZATIONAL BEHAVIOR FOR WHOM?

So far I have been advocating changes to our style of research that might help us better understand organizational behavior. However, I would be remiss if I did not also acknowledge the role that various constituencies play in our research. As [Becker \(1967\)](#) warned long ago in his discussion of sociological research, we should regularly ask why and especially for whom ~~are~~-we are investigating certain phenomena.

When I first entered our field I imagined that I was joining the holy pursuit of knowledge, endeavoring to understand how and why people (and collectivities) behaved as they did. Soon I realized that such noble goals were not the only reasons for our research. As a business school professor I felt pressure to investigate whether certain actions or processes could make a difference in the performance of individuals and outcomes for the firm. Later I came to appreciate that the vantage point of business schools, however narrow it might be, was still somewhat

broader than the purview of I/O psychology. Whereas the audience (and often the sponsor) of research in organizational behavior has been the management of organizations, the audience of I/O psychology research can be interpreted as firms' HR department or various associations of HR professionals. Thus, while management is concerned with the overall well-being of the organization (its performance, efficiency, adaptability), the I/O perspective is more focused on specific organizational issues and functions such as selection, training, retention, safety, and work performance.

Probably the only area of organizational research that does not (at least implicitly) take the employer's point of view is that of behavioral ethics. No doubt there is a stream of research on employee theft that is primarily concerned with the employer's cost of "shrinkage" (e.g., [Greenberg 1990](#)). The main thread of organizational research on ethics, however, is what prompts employees (and firms) to lose their moral compass, to commit illegal and/or unethical acts ([Moore & Gino 2013](#)). In this regard, it should be noted that many illegal acts may not actually begin as self-centered or selfish attempts to cheat. A substantial number of ethical violations probably start with an effort to serve others in the corporation---as an effort not only to meet personal goals, but also as a way to help one's supervisor and workgroup make their numbers, thereby allowing bonuses to be distributed, layoffs to be avoided, and the possibility of bankruptcy to be eliminated. Unfortunately, what may start as an effort to help one's colleagues and the larger organization can morph into wholesale fraud as sales are "borrowed" from upcoming quarters, channels stuffed, and earnings engineered through opaque financial transactions. Only recently has some attention shifted from the reasons individuals succumb to the temptation to cheat to the social and organizational processes that can sustain unethical behavior over time (see, e.g., [Ashforth & Anan 2003](#), [Brief & Smith-Crowe 2016](#)).

Aside from the study of behavioral ethics, few research topics in organizational behavior are explicitly oriented toward the collective good. Actions that improve corporate profits may or may not aid the general public, depending on how the profits are achieved and whether they are achieved at the expense of other competing interests. For example, Google is a firm that is celebrated as the creator of many innovative products and was founded with the credo “Don’t Be Evil.” However, this imperative has not been extended to the payment of federal and state taxes. It has not prevented the company from parking much of its earnings overseas so as to minimize tax liabilities, even though its operations benefit at least indirectly from federal and state expenditures (e.g., through the education of its employees). My goal is not to single out Google for criticism, given nearly every major technology company has a similar tax avoidance strategy, but to point out that our field primarily takes corporate well-being as the end-state to be achieved rather than the perspective of the larger community. To date, there has been little or no research on determinants of corporate resistance to layoffs, although there was great variance in such actions during the last major recession. Similarly, there has been little research on what leads corporations to embrace or resist environmental regulations, although there are certainly wide differences in the stance of firms and industries on this issue. Finally, our field rarely takes the exclusive perspective of individual workers---especially if their welfare comes at the expense of organizational well-being. For example, when we address issues such as job satisfaction and workplace stress we make the implicit assumption that what is good for the employee is also good for the employer. We avert our attention from employee-centric topics such as how one might get promoted (when being no better than others on the job), how not to get fired (when making a minimal contribution or working on an unsuccessful project), and how to maximize one’s time with family rather than at the corporation (without jeopardizing one’s

career). Whimsical answers to these questions can of course be gleaned from traditional research (e.g., bias in performance evaluation and promotion decisions), but few of us have had the nerve to take such an extremely employee-centric perspective.

STUDYING POSITIVE VERSUS NEGATIVE EFFECTS

Although most of our research and teaching has been devoted to improving the functioning of organizations, I'm not sure we have always taken the most practical route toward this end. Generally we try to analyze actions associated with high performance and attempt to instill these practices among a wide range of individuals and organizations. However, we may have a greater impact if we concentrated on behaviors and practices to avoid. For example, rather than studying the sources of good leadership, we might actually get more out of the analysis of really bad bosses, those whose toxic behavior drives others out of the organization ([Sutton 2010](#)) or leads the firm in a strategically disastrous direction ([Chatterjee & Hambrick 2007](#)). Similarly, we might examine truly dysfunctional incentive schemes, the kind that cause adverse reactions and extremely negative side effects ([Ordóñez et al. 2009](#)). Another fruitful candidate could be the mechanisms that move ethical lapses at the individual level to more widespread organizational wrongdoing ([Darley 1992](#), [Brief & Smith-Crowe 2016](#)). Finally, my favorite candidate for study may be catastrophic decision making, where small decision errors can grow into major organizational blunders ([Bazerman & Watkins 2004](#)).

By having greater understanding of the origins of adverse consequences we may be able to avoid the most extreme instances of negative outcomes rather than just moving forward the average level of the curve. At the individual level, such a focus might involve isolating the organization's worst performers and discovering what has driven them to

such a negative level. The same could be argued in terms of group and organizational performance. Regardless, my advice would not be to mimic the dictates of Jack Welch, who advocated the yearly dismissal of the organization's bottom 10% as well as the removal of any product line that is not among the top two in its industry. Instead, my recommendation would be to learn what has led to being on the negative tail, so that these behavioral paths can be avoided in the future. Such an inquiry might be somewhat similar to the postmortem examination of medical accidents used by hospitals or the lessons learned by the military in studying its losing battles.

Of course, studying adverse consequences is not easy, given that it often entails the in-depth analyses of people and units who prefer to avoid the exposure of their errors. For instance, I once offered to conduct research at a large national bank to see how factors used in the loan approval process affected subsequent performance of their loans. Even a small improvement in loan procedures could make a large impact on the bank's profitability. Nonetheless, the executive in charge of corporate lending resisted all aspects of this research, saying that it would be too much trouble and too costly for the bank. After much prodding, he was finally willing to tell me the real reason for his resistance. Essentially he said, "Your research is designed to show areas for improvement, but it can also be interpreted as an indictment of our loan policies. Since I am the author of the current policies, I will not risk any exposure of their shortcomings." Although I tried to convince him that he could also take credit for any improvements, he felt the risk was just too great. So I thanked him for his honesty and went on my way.

Because few organizations are likely to welcome behavioral researchers in a search for errors, abuses, violations, or other shortcomings, investigators need to be more creative in their approach to the subject. Rather than entering the firm through the front door, it is often more feasible to study mistakes and immorality from the outside---

from interviews in employees' homes, from anonymous websites, and from [the confines of](#) churches and country clubs. In one of the most creative inquiries on illegal corporate behavior, [Clinard \(1983\)](#) interviewed former executives who had retired to Arizona and New Mexico, where they were much more open about the misdeeds of their former employers and even themselves. We need to finally follow Clinard's lead with research in which behavior is either unobtrusively observed or reported in a more candid manner.

CONCLUSION

This article recounts some of my early experiences in the field and how they shaped my views about conducting research. As I described, my entry into organizational research was not seamless. Although I arrived with publications in social psychology and believed I could publish additional work in that venue, I had to make a series of adjustments to succeed in my chosen field of organizational behavior. As with many of my colleagues who ~~had~~ moved from psychology to organizational behavior, I had to find some value added, be it a new perspective or an alternative set of ideas for the field. This was, I believe, a worthwhile endeavor---one that enriched us as migrant scholars and one that helped move the field forward. Unfortunately, as I outline here, this process of mutual adjustment has largely ceased. Although an increasing number of social psychologists are now housed in business schools, they are still doing social psychological rather than organizational research. For this to change I argue that future research needs to become more contextual and phenomenon driven. My hope is that, with the upsurge in talent entering the field we can find a way to harvest more of its creativity, moving from the application of social psychology to a genuine social psychology of organizations.

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Table 1 Alternative paths for organizational research

	Social psychology applied to organizations	Social psychology of organizations
Goals of the research	Application of social psychological theory to behavior in organizations	Understanding organizational phenomena, problems, and issues
Levels of analysis	Individual and group levels of analysis	Multiple levels of analysis, including cross-level inquiries
Research methodology	Lab and field experimentation	Multiple methods, including qualitative and quantitative research, and use of real-time, observational, and historical data
Direction of inquiry	Search for underlying processes (reductionism)	Search for upward, outward, and downward influences
Form of contextualism	Context as a limitation to external validity	Context as a primary focus of investigation