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The Case for Behavioral Decision Research in Organizational Behavior

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Abstract

We argue that the field of organizational behavior (OB) is well positioned to adopt some of the strengths of behavioral decision research (BDR). Doing so would enable the field to gain in influence, scholarly stature, paradigm strength, and practical relevance. In the course of making this argument, we review recent advances in BDR and highlight its relevance for OB. In particular, our discussion focuses on how BDR can inform topics of longstanding interest to OB researchers.

Introduction

The field of organizational behavior (OB) is uniquely positioned to take advantage of a tremendous opportunity. That opportunity is embodied in behavioral decision research (BDR), a field prospering at the fertile intersection of

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economics and psychology. In this paper, we aim to identify how this opportunity manifests itself, as well as some of the impediments to fully exploiting it. Along the way, we will review recent developments in BDR that we believe can help chart a course for future research, and what we hope will be further advances in OB.

OB research endeavors to understand people in organizations—their motives, their decisions, their interpersonal relations, and the outcomes of their choices. To this end, OB scholars have incorporated theory and research from several disciplines, most notably psychology and sociology. As a field, we have generally neglected the relevance of economics. Economists for their part, have not made the mistake of ignoring our work. In fact, several economists have been adept at borrowing useful ideas from other disciplines, including psychology and OB (Loewenstein, O'Donoghue, & Rabin, 2003; Odean, 1998b; Rabin, 1993; Thaler, 1991). The result has been a rapid growth in the importance and influence of behavioral economics—a nascent field whose research goals are strikingly similar to the goals of OB research. A small group of scholars in OB have attempted to return the favor—borrowing useful research approaches from economics. Their work generally falls in the category of BDR.

BDR is the study of human judgment and decision making. The BDR approach is distinct from other decision-making research in that it, like most economics, relies on a normative backdrop (for reviews, see Dawes, 1998; Payne, Bettman, & Johnson, 1992; Shafir & LeBoeuf, 2002). That is, BDR work can specify what rational decision makers should have done, and the degree to which actual decisions deviate from the optimal choice. The use of a normative standard represents BDR's defining feature.

BDR's Traditional Core Topics

BDR's traditional core is what has been referred to as the "heuristics and biases" research program. Research in this tradition examines the cognitive heuristics that people employ to help them deal with the fact that their brains are constrained in their cognitive processing capacity, while the complexity of the social worlds in which they live is not similarly constrained (Bazerman & Moore, 2008; Hastie & Dawes, 2001; Newell, Lagnado, & Shanks, 2007). For instance, when trying to estimate the likelihood of an event's occurrence, people rely on the information most available in their minds (Tversky & Kahneman, 1973). That is why they tend to worry too much about vivid but low-probability risks, such as an airplane crash or a terrorist attack, and why people worry less about more common but mundane risks, such as obesity or skin cancer (Tversky & Kahneman, 1974).

However, mental accessibility can play tricks on us. The evidence suggests that systems of mental recall are better at performing positive than negative hypothesis tests (Klayman & Ha, 1987). For instance, when considering the question of whether George W. Bush is a scoundrel, it is easier to think of

instances of false claims and unfulfilled promises than examples of principled successes. When considering the question of whether George W. Bush is trying his best to do a good job as President, the opposite is true. This so-called confirmation bias (Nickerson, 1998) has widespread consequences. It leads to anchoring, in which one's ultimate conclusion is anchored too closely to one's initial hunch (Mussweiler & Strack, 1999; Strack & Mussweiler, 1997). It also leads to the hindsight bias, which leads people to believe, incorrectly, that they would have correctly predicted what was going to happen (Fischhoff, 1975; Koriat, Fiedler, & Bjork, 2006). And it enables people to sustain kooky or irrational beliefs, because it is usually possible to generate some supportive evidence for even the most implausible hypotheses (Gilovich, 1991).

For many years, BDR researchers gained fame by identifying their own biases and naming them (Krueger & Funder, 2004). The result was a tremendous proliferation of different biases and effects. This is partly a result of the fact that researchers in BDR, as elsewhere, are rewarded more for blazing new terrain than for integrative work that explains the common causes between disparate prior results. Nevertheless, it has become harder for BDR researchers to stake credible claims to new bias territory because so many biases have been identified that they are beginning to run together. Consequently, their commonalities are becoming easier to see. For instance, recent work on anchoring demonstrates its common origins with other biases (Mussweiler, 2003). And recent work on overconfidence reconciles the different ways it has been studied, accounting for both commonalities and discrepancies across studies (Moore & Healy, 2008).

For our purposes here, it is important to reiterate what is distinctive about the BDR approach: BDR always seeks to compare what *is* with what *ought to be*. Intuitive human judgment is far from perfect. Understanding exactly how it is imperfect sheds light on underlying psychological processes and suggests ways in which people can learn to do better (Kahneman, 2003). This emphasis on optimality represents BDR's distinct advantage and one of its potential contributions to research in OB.

What BDR Offers OB

BDR's prescriptive focus aligns well with the interests of OB scholars. Research on central OB topics such as motivation, goal setting, emotion, and employee selection has generally sought to identify ways to maximize individual and organizational performance, but the overwhelming majority of this work fails to specify the optimal choice in a specific situation (given the necessary trade-offs). Adopting the BDR approach would enable OB researchers to explore this tantalizing possibility.

Having a precise normative standard offers several notable benefits to OB scholars. First, the use of a standard could boost the field's paradigmatic consensus because it delineates which research questions are interesting and worth

pursuing (namely, interesting research documents and explains deviations from the rational benchmark). Second, this perspective might enable researchers to engage with more normative or prescriptive disciplines, including economics, decision science, operations research, marketing science, accounting, engineering, and statistics. Third, the normative benchmark endows research with more applied value because it clarifies how people's judgments deviate from optimality and what they can do to improve the quality of their choices and decisions. Indeed, the lessons derived from this work refer to a domain that is directly under the control of individual managers: their own thoughts, judgments, and decisions.

The BDR approach played a fundamental role in some of OB's greatest intellectual achievements, including Simon's (1947) theory of bounded rationality and the Carnegie School of organizational scholarship (Cyert & March, 1963; March & Simon, 1958). In both cases, the normative backdrop of optimal rationality was the foil against which the researchers developed theories of OB. It is our view that BDR continues to have much to say about topics of central interest to OB researchers, despite the fact that the BDR approach has lost some of its early footing in the field of OB.

We will argue that OB has much to gain from a more widespread acceptance of BDR approaches—increased influence, scholarly stature, paradigm strength, and practical relevance. We begin by discussing some of the distinctive features of BDR. We then review how BDR can enhance our understanding of a sample of core topics within OB. As our review will highlight, BDR research has built on some of the foundations laid by research in OB, appropriating important insights. We feel that OB as a field is well positioned to build further upon the progress BDR has made, but that many OB researchers have been reluctant to exploit this opportunity. We address some of the key concerns that OB researchers have about adopting BDR approaches. We close by highlighting some of the benefits to OB researchers of incorporating BDR approaches in their own work.

BDR's Influence in OB and Beyond

Herbert Simon's (1947, 1967, 1978, 1997) work on bounded rationality—the notion that people are limited in formulating and solving complex problems and in processing information—provided an important underpinning for BDR. Simon went on to develop these ideas, through his collaboration with James March, in ways that helped lay the foundations for modern organization theory (March & Simon, 1958). Both Simon and March's work on decision making provided keen insight on topics relating to psychology, OB, and economics. In 1978, Simon became the first non-economist to win the Nobel Prize in economics, a distinction later matched by Daniel Kahneman, whose work with Amos Tversky remains central to BDR. The broad appeal of their work has helped BDR gain a foothold in several disciplines.

BDR scholars have been influential in law schools, examining issues such as how juries determine the size of legal penalties (Kahneman, Schkade, & Sunstein, 1998) or how judges determine verdicts (Englich, Mussweiler, & Strack, 2006). BDR has also contributed to the study of medical decision making, investigating issues such as whether physicians are overconfident in their diagnoses (Oskamp, 1965) and the degree to which physician judgments are biased by gifts from pharmaceutical manufacturers (Dana & Loewenstein, 2003; Ubel, 2005). BDR is well represented in marketing, particularly among those who study consumer behavior. It has even become influential among accounting scholars, spawning the sub-field of behavioral accounting (Birnberg & Sutton, 1989; Nelson, Bloomfield, Hales, & Libby, 2001). Likewise, BDR has played a central role in developing the field of behavioral finance, which examines how people make decisions about financial matters (Malmendier & Tate, 2005; Odean, 1998a; Thaler, 1993).

Outside of academia, BDR has influenced a number of policy decisions. For instance, Thaler and Benartzi's (2004) Save More Tomorrow program has helped increase rates of saving in retirement programs quite substantially at hundreds of American corporations. Using principles drawn from BDR, the program invites workers to commit themselves to increasing their savings rate in the future. After employees join, they can remain in the program until they choose to opt out. The success of Save More Tomorrow has been remarkable, with average saving rates for plan participants increasing from 3.5 to 11.6% over a period of just 28 months (Thaler & Benartzi, 2004). Other work has relied on BDR in order to understand how simple policy changes can help promote an increase in organ donation (Johnson & Goldstein, 2003), better police work (Wells & Olson, 2003), and consumer spending (Epley, Mak, & Idson, 2006; Milkman, Beshears, Rogers, & Bazerman, 2008).

BDR's influence in economics and policy decisions has dramatically increased its potential impact. In terms of clout, economics stands apart from other social sciences. In addition to the economists who are employed within academia, economists work in key positions for governments, Federal Reserve banks, the Bureau of Labor Statistics, and the Council of Economic Advisors. They advise presidents and help set monetary policy. In the corporate world, many businesses employ economists who advise the firm's leadership on various matters, including strategy and human resources. Clearly, these are domains in which OB would have a great deal to say (Bazerman & Malhotra, 2007). BDR approaches represent a viable avenue by which our field could exercise such influence.

What BDR Has to Say about Core Topics in OB

BDR is an interdisciplinary field with contributions from psychology, economics, marketing, and neuroscience in addition to OB. Noting this, some scholars may question whether BDR is core to the field of OB. We believe it is. The

issues that are most fundamental to BDR are central issues in OB (Bazerman, 2005), including how people (including managers) make decisions under uncertainty, how they value the outcomes of their decisions, and how they search for and interpret information. The BDR approach can apply to other traditional areas of OB scholarship, including motivation and employee selection, among others. In this section, we highlight some of the connections we see between the BDR approach and a few topics of interest to organizational scholars. We begin by discussing traditional micro-OB topics of motivation, goal setting, fairness, workplace emotion, and employee selection. We then address a couple of topics that may be of more interest to macro researchers: institutional inertia and social networks.

The list of OB topics we have chosen here is not exhaustive, nor even comprehensive. It is merely a sample of topics that we feel relate to some of the more insightful findings in BDR, particularly some of the more recent discoveries. In covering these topics, our aim is not to instruct OB scholars on what they should be studying. Rather, our simple goal is to provide a summary of relevant BDR studies that might stimulate readers' imaginations about the potential for future work in each of these areas.

Motivation

What makes employees willing to exert more effort, particularly in ways that improve their performance? Research on employee motivation often takes the implicit perspective of the employer in examining the rewards that lead people to work harder (Latham & Pinder, 2005). Along this vein, some BDR studies have found that employers are likely to overestimate the motivating influence of extrinsic rewards (Miller, 1999). For instance, Heath (1999) showed that managers overestimate the degree to which their employees are motivated by money. However, most of the work in BDR relevant to the issue of work motivation takes the implicit perspective of the individual in asking whether people are motivated in ways that will benefit themselves. Do people correctly understand their own preferences and work to achieve those things that will truly increase their happiness and welfare? A variety of work suggests the answer is no.

Are People Motivated to Pursue the Right Outcomes?

People make systematic errors in predicting what will make them happy. They assume that living in California will make them happy (Kahneman et al., 1998). Junior professors think that getting tenure will make them happy (Gilbert, Pinel, Wilson, Blumberg, & Wheatley, 1998). And people overestimate the degree to which greater wealth will increase their own happiness (Kahneman, Krueger, Schkade, Schwarz, & Stone, 2006). Sadly, the evidence suggests that better weather, tenure, and more money do not lead to enduring changes in life satisfaction. Fortunately, people recover from negative

outcomes such as being denied tenure or becoming paraplegic far more rapidly than they expect (Brickman, Coates, & Janoff-Bulman, 1978; Gilbert et al., 1998). Unfortunately, people also get used to tenure, good weather, and wealth.

BDR studies have consistently found that reference points and framing are integral to understanding people's motivation to achieve and their satisfaction with subsequent outcomes. For instance, living near wealthier neighbors leaves people feeling worse off because the neighbors' wealth provides a higher reference point that makes them feel poor by comparison (Luttmer, 2005). By the same token, negotiators are happier when they get more than the other side because the other side represents a reference point against which they compare their outcomes (Loewenstein, Thompson, & Bazerman, 1989; Novemsky & Schweitzer, 2004). These results are, of course, ironic because they have counter-intuitive implications—people are happier when they have poorer neighbors, and negotiators may derive more satisfaction from seeing their opponents unhappy than from getting better prices.

Research on incentives, affective forecasting, and reward schedules fits in well with the BDR approach. The same approach, on similar topics, might also suit research on motivation and job satisfaction in OB. For example, work by Hsee and Abelson (1991) suggests that people strongly prefer an improving series of outcomes to a declining series, which intuitively makes sense. However, the authors also demonstrate that people prefer an increasing series *even when the declining series offers greater benefit*, because the decline is perceived as a loss. The same pattern may be apparent in employees' perceptions of age–wage profiles—their satisfaction may depend more on the slope of an age–wage profile, rather than its absolute value at any particular point in time. As for errors in affective forecasting, people may wrongly assume that promotions or raises will lead to increases in job satisfaction.

For organizations, it would be helpful to know the factors that underlie such miscalibration and who is more likely to commit these errors in judgment. For example, if people quickly habituate to changes in income but never quite get used to annoyances like a bad commute, as BDR research suggests (Gilbert, Lieberman, Morewedge, & Wilson, 2004), employers may do a better job of hanging on to valued employees by subsidizing housing close to work than by increasing employees' pay. Moreover, BDR approaches suggest another way to reframe research on work motivation: by explicitly considering the necessary trade-offs. For instance, is it possible to weigh increased productivity produced by stronger work motivation against the increased rate of employee burnout? To what degree does work motivation crowd out or undermine the pursuit of other goals?

Goal Setting

One of the most prominent and well supported theories in OB is goal setting theory. To stylize this sizable literature, research has found that specific goals

increase effort over more general instructions to "do your best" (Locke & Latham, 1990). Again, this work often implicitly adopts the perspective of the employer in assuming that more effort and higher productivity are necessarily positive.

Heath, Larrick, and Wu (1999) highlighted the value of thinking about goals as reference points from a BDR perspective. Reference points and framing effects in judgment have been at the core of BDR since the 1979 publication of Tversky and Kahneman's prospect theory, which posits that people judge outcomes relative to reference points, and that losses loom larger than gains. In other words, a loss of a given size is more painful than a gain of the same size is pleasurable. For this reason, people will work harder to avoid a loss by falling short of a goal-established reference point than they will to achieve a gain in performance (Heath et al., 1999).

This is, of course, not an unalloyed positive. Working harder need not always be in the interests of the individual worker or even the organization that employs them, as Schweitzer, Ordoñez, and Duouma (2004) point out. One of prospect theory's key tenets is that when people are seeking to avoid a loss, they make riskier choices than when they are pursuing a gain. Consistent with this principle, Schweitzer and his colleagues show that reliance on performance goals can increase the prevalence of unethical behavior because those who anticipate falling short of a goal are more likely to engage in risky strategies, including cheating, to clear the hurdle. Those who set high goals are also more likely to fall short of them than are people who set modest goals, so although they may achieve more, those who set high goals are frequently less satisfied with their outcomes (Galinsky, Mussweiler, & Medvec, 2002; McGraw, Mellers, & Ritov, 2004).

BDR research has also uncovered some perverse effects of goal setting on workers' decision making. Camerer, Babcock, Loewenstein, and Thaler (1997) examined work patterns of New York City cab drivers. Because taxis are in greater demand on rainy days than on sunny days, cabbies who set goals for how much money they will make in a work shift find it is easier to achieve their goals when it is raining. Consequently, Camerer et al. find that cabbies do not work as long on rainy days—after they achieve their goals, they turn in their cabs and go home. The ironic result is that cabbies work less on days when their hourly income is higher. This is a bad outcome for those who need cabs because the supply of cabs is lower when they are in greater demand. It is also a bad outcome for the cabbies because they wind up spending more time working when they could be engaging in leisure activities—perhaps outside enjoying the good weather.

While the performance benefits of goals has been a long-standing research topic within OB (with some notable exceptions such as Staw & Boettger, 1990), the downside of goal setting represents a natural line of inquiry for BDR researchers. There are many provocative research questions that remain

open. Are ambitious fiscal goals more likely to produce unethical behavior, as Schweitzer et al.'s (2004) work suggests, or just higher levels of performance? Further, when does goal setting energize people to work against their self-interest, perhaps in support of a collective cause or against the interest of the group (e.g., Camerer et al., 1997)? Finally, how good are people at setting goals in ambiguous circumstances? Answering this question requires us to investigate what people instinctively do and compare it against what they might optimally do, a goal that should appeal to BDR and OB researchers alike.

Fairness

Organizational justice and fairness have long been of interest to researchers in OB (Bies, 1987; Brockner, Tyler, & Cooper-Schneider, 1992; Greenberg, 1987). But because this research usually lacks a clear normative backdrop, it cannot answer crucial questions about whether a preference for fairness is efficient, rational, or desirable. A parallel literature on fairness judgments exists in BDR, and some of this work examines how fairness judgments clash with economic imperatives (Kahneman, Knetsch, & Thaler, 1986). For instance, people regard it as unfair when an employer cuts employee pay by five percent. However, people find it more acceptable for the same employer to provide a modest seven percent raise in a year when inflation is running at 12%. The buying power of employee salaries is equal in the two conditions, but the two situations produce very different judgments regarding fairness.

Compensation and Morale

A normative standard can strengthen some OB theories regarding employee compensation. Take, for example, the issue of downward wage stickiness. Economic models would prescribe that when the supply of labor goes up or demand for it goes down, the wages paid to laborers should drop. But evidence suggests that employers very rarely reduce the wages paid to incumbent workers (Thaler, 1991). This anomaly can be accounted for by justice research, which shows how motivation and effort are undermined when workers are paid less than what they believe they deserve. One way to make an individual feel underpaid is by reducing their rewards when their effort and productivity remain constant (Brockner, O'Malley, Hite, & Davies, 1987; Goodman & Friedman, 1969).

Another interesting economic anomaly has to do with pay differentials across different industries. Highly profitable industries tend to pay all their employees more than do less profitable industries (Thaler, 1991). In other words, secretaries performing the same sort of work are likely to be paid more at an investment bank than at a grocery store. Efficient labor markets should not produce such disparities. Perhaps even more noteworthy, companies pay their employees more in profitable years, even when the profitability is the result of market conditions over which the employees have no control

(Bertrand & Mullainathan, 2001a). While theories of fairness can easily explain why firms would feel the need to share wealth with their employees, standard neoclassical economic theory struggles to explain these outcomes.

Strategic Interaction

Probably the biggest BDR literature on fairness examines experimental games such as the ultimatum game. These games have proven to be of such lasting interest to BDR researchers because economic theory provides a clear and unambiguous prediction for what rational actors ought to do and these predictions are so frequently contradicted by people's actual behavior. In the ultimatum game, two people are offered money (say $10) if only they can agree on how to divide it between them. One person must propose how to split the money and the other person must respond by choosing whether to accept the offer (in which case the money is divided accordingly) or reject it (in which case neither of them gets anything).

The solution offered by economic theory is simple: the proposer should offer the smallest possible amount to the responder. The responder, rationally preferring something to nothing, should accept. But this is not what people do. Instead, an even split of the money is the most common proposal, and when proposers offer less, their offers are routinely rejected (Roth, 1995). Many scholars have speculated that proposers split the money evenly because they are concerned about fairness, and this same preference for fairness leads responders to reject profitable offers. In general, research on the ultimatum game suggests that people care about fairness and are willing to forego real money in order to punish unfair behavior by others.

The paradigm of the ultimatum game has enabled researchers to ask basic questions about why it is that people seem to care about fairness. One fascinating set of questions surrounds the behavior of the proposer. Are proposers more generous than economic theory predicts because they really care about fairness or because they expect that responders will reject stingy offers? One way to assess this question is to eliminate the responder's option to reject the proposal. In this so-called "dictator game", the proposer alone determines how the money should be divided. Compared with the standard ultimatum game, offers go down in this version of the game, but they still stay significantly above zero, suggesting that fear of rejection is not the only motive driving proposers' generosity (Camerer & Thaler, 1995; Rabin, 1993).

Some have suggested that the reason people give money to others in the dictator game is not that they want to be fair to others, but instead that they are concerned with appearances. If they could escape with the money and without anyone being the wiser, they would. Indeed, when the experimental setup gives dictators the assurance that the experimenter will never know what they chose, their generosity declines (Hoffman, McCabe, Shachat, &

Smith, 1994). And when dictators are given the choice of keeping the other player in the dark, their generosity declines still further (Dana, Cain, & Dawes, 2006). Dana and colleagues gave participants in their study a choice: they could either: (1) play a standard dictator game in which they could allocate $10 between themselves and another person; or (2) exit the game silently, receive $9, and the other person would never know about even the existence of the game. Roughly one-third of participants took the silent exit option, although this choice is difficult to justify as rational, since a self-interested person should play the standard dictator game and just keep the entire $10.

These experimental studies of fairness preferences offer important insight on how people think about fairness and how fairness norms influence their decisions. Studies of the proposers' decisions in the ultimatum game shed light on the ways that organizational leaders can most effectively allocate the bounty generated by a profitable business venture. Similar approaches can help elucidate the motives underlying the fairness judgments of workers who depend on those allocation decisions (see Blount, 1995).

BDR approaches also allow researchers to ask interesting questions about how people value fairness. Specifically, how much is fairness worth to them compared with objectively valued rewards? Weighing trade-offs between being perceived as fair and being perceived as selfish is a common dilemma that managers in organizations face, and the normative benchmark is not always clear. However, much of the research on justice and fairness in OB focuses on the relative effectiveness of different strategies that aim to promote fairness, assuming that fairness is valued above all else. In contrast, from the employee's perspective, justice research in BDR considers the trade-offs between perceived fairness (e.g., procedural justice) and instrumental rewards (e.g., outcome favorability). Future research in OB might consider different factors that lead managers to value acting in a fair manner rather than acting on one's self-interest, drawing on findings from previous BDR work on the same topic.

Emotion in the Workplace

The study of emotion in the workplace has seen a substantial increase in recent years (Elfenbein, 2007), but as Brief and Weiss (2002) highlight in their review, this research literature faces a number of challenges. One of these challenges is that in field settings it is difficult—if not impossible—to examine the effects of emotion itself. In naturalistic settings, emotions are integral to the situation, and are therefore bound up with thoughts and experiences that produced the emotions. How can OB researchers determine the relative influences of emotions and the cognitions with which they are bound up?

The solution to this problem in the BDR literature has been to study incidental emotion—emotion that people happen to be feeling when they form a judgment or make a decision. Typically, BDR researchers will induce

an emotion and examine its carryover to another, unrelated decision. Normatively, incidental emotion should have no effect on a new, unrelated situation. Any influences it does have are therefore of interest to BDR researchers (as well as emotion researchers).

For instance, Baron (1990) showed that positive mood, induced with pleasant scents, increased cooperativeness in negotiation. Forgas (1998) found that negotiators in good moods were also happier with their outcomes. Baron's and Forgas's research, like much of the research on affect, examines generalized positive or negative mood. More recent work, however, suggest that grouping all negative emotions together is misleading. Lerner and Keltner (2001), for example, showed that different negative emotions had very different effects on perceptions of risk. Fear increased people's perception of risk and anger decreased it (see also Lerner, Gonzalez, Small, & Fischhoff, 2003; Tiedens & Linton, 2001). Happiness, for its part, tends to increase reliance on judgmental heuristics, such as racial stereotypes (Bodenhausen, Kramer, & Suesser, 1994). These results provide useful insights that can help us understand how the emotions expressed and experienced within an organization may bias the judgments of its members.

Research on job mood, which often suffers from endogeneity concerns, might benefit from adopting similar BDR approaches. For example, one might predict a relationship between workplace emotion and expense account abuse. Research on emotion suggests that both angry and happy people are more willing to make risky self-interested choices than fearful people. If an employee is fearful at work when selecting a hotel for an upcoming business trip, she might pick a more cautious option (e.g., Holiday Inn), but if she feels happy or angry, she might be more inclined to throw caution to the wind and go with a more luxurious option (e.g., the Plaza). Such a hypothesis would be difficult to test with field data in which workplace emotions are inextricably linked with expense account activity. However, inducing emotions at work (either negative or positive) and then evaluating whether a change in emotion affects employees' expense account decisions would provide a more interpretable test. Similar examples abound, where engendering emotions under controlled circumstances can help provide clarity to OB research on emotions in the workplace.

Employee Selection

Hiring decisions are among the most important in organizations. Many firms perform this critical task by relying on managers' intuitive assessments following face-to-face unstructured employment interviews. This practice remains the norm, despite strong evidence that interviews are not particularly useful in predicting future job performance (Conway, Jako, & Goodman, 1995; Hunter & Hunter, 1984) and that linear models outperform managers' intuitive judgments (Dawes, 1972, 1979). Managers' enduring faith in the

employment interview and in their own intuitive judgments may be driven by several different cognitive biases that have been documented in past BDR research.

The first issue has to do with structured versus unstructured interviews. Structured employment interviews are generally superior to unstructured employment interviews with respect to their reliability, predictive validity, and legal defensibility (Campion, Palmer, & Campion, 1997). However, organizations rely far more heavily on unstructured employment interviews (Graves & Karren, 1996), and there are at least three reasons for this. First, people are overconfident in their own interviewing abilities (Dawes, 1996; Dawes & Dana, 2007) and their ability to predict the behavior of others (Griffin, Dunning, & Ross, 1990). Consequently, they underestimate the value in doing the work necessary to organize and structure their interviews (Dipboye, 1997). Interviews are also susceptible to self-enhancement and similar-to-me bias. They tend to be swayed by ingratiation, even when it seems transparent (Ferris & King, 1991; Gilmore & Ferris, 1989; Gordon, 1996), and tend to favor applicants who are similar to them with respect to personality, race, or gender (Sears & Rowe, 2003).

The second reason for the popularity of unstructured interviews is that structuring interviews takes time and effort. Personnel selection is so important that even small increases in the quality of employees selected can pay off handsomely in the long run (Schmidt & Hunter, 1998). Nevertheless, most managers and organizations never bother to figure out whether it is worth the effort. As in other domains where BDR has documented flaws in intuitive judgment, when people do not understand how their intuitive judgments are impairing their decisions, they lack motivation to change how they make decisions (Bazerman, 2005; Kahneman, 2003). Third, unstructured employment interviews are standard practice, and as research on *status quo* effects suggests (as we discuss in a subsequent section), people routinely fail to question standard operating procedures.

In addition to addressing organizations' failure to use structured job interviews, there is another way in which BDR research suggests that hiring decisions could be improved. Any sensible hiring process should include a number of considerations in addition to the interview, such as the applicants' educational backgrounds, recommendations, work experience, and performance on ability tests, assessment centers, or job tryouts (Hough & Oswald, 2000). When it comes to combining and weighting all these considerations to arrive at a final decision, organizations rely almost exclusively on unaided human judgments, despite the fact that evidence from BDR clearly and indisputably shows the superiority of linear models for making these sorts of structured decisions (Dawes, 1979, 2005).

The BDR approach might provide some new direction for research on interviewing. Work on person perception has demonstrated the reliability of "thin

slices"—that people tend to make snap judgments of others after interacting with, or observing, a person for just a few minutes, or maybe even a few seconds—and that these snap judgments correlate fairly well with more complex performance outcomes (Ambady & Rosenthal, 1992). People find it difficult to revise these quick impressions even when they are presented with inconsistent information. Numerous studies have documented this effect in employment interviews, suggesting that interviewers make up their minds regarding candidates in the first moments of the interview (Arvey & Campion, 1982; Schmidt & Hunter, 1998; Wright, 1969).

These findings are remarkably insightful, but a BDR perspective would ask an entirely different question. Specifically, are these snap judgments better than judgments made using more complete data? If researchers evaluate the objective performance of people who make "thin slice" judgments, do these judges perform poorly relative to their colleagues with more substantial periods of evaluation, or are they comparable in terms of predicting job performance? For example, given that extraversion is an excellent predictor of success in sales positions and an easy trait to detect in interviews, how long should interviews be for sales positions? Five minutes? Perhaps just a handshake and some chit chat? A research program with a normative benchmark would help answer these questions convincingly.

Institutional Inertia

Institutional theory holds that established modes of thought and traditional assumptions are built into the organization. Compliance with institutional rules and norms occurs because they are taken for granted as "the way we do these things" (Scott, 2001, p. 57). One up-side of the resulting institutionalization of standard practices is that it helps give organizations some of their stability and inertia. This inertia is, of course, essential for the delivery of predictability and reliability in firm performance (Hannan & Freeman, 1984). On the other hand, inertia is the bane of every CEO who wants to introduce meaningful organizational change. Whereas the normative and regulatory pillars of institutional theory rely on organizational structures and rules, the cognitive pillar is the one with the strongest implications for individual cognition (DiMaggio, 1997).

Although there has been little empirical study of the psychological underpinnings of a cognitive pillar of institutionalism (for an exception, see Zucker, 1977), there is some work in BDR that bears directly on the issue. Samuelson and Zeckhauser (1988) documented what they called the *status quo* bias: people's reluctance to change the way things have been done. While there have been numerous demonstrations of the *status quo* bias in the experimental laboratory (Kahneman, Knetsch, & Thaler, 1991), the neighboring states of New Jersey and Pennsylvania unwittingly carried out a natural experiment on the *status quo* bias when they selected default options

for their state's drivers when it came to selecting types of automobile insurance in the early 1990s. Both states offered their drivers two very similar options, one of which included a full right to sue (the default option in Pennsylvania) and another, less expensive option, which restricted the right to sue (and was the default in New Jersey). According to Johnson, Hershey, Meszaros, and Kunreuther (1993), 79% of New Jersey drivers selected the limited right to sue, but only 30% of Pennsylvania drivers did so. Normatively, the default option ought to have been irrelevant to drivers' decisions about which type of insurance they should buy. Nevertheless, it had a powerful influence—they generally chose the default option (see also Johnson & Goldstein, 2003).

Ritov and Baron (1992) showed how the *status quo* bias led people to think differently about action versus inaction. Their participants were asked to decide whether to inoculate 10,000 children against a disease that was expected to kill around 10 of them. The problem was that the vaccination itself had unpleasant side effects that were likely to kill 5 of the 10,000. The possibility that their action (the vaccination) might kill children led to what Ritov and Baron called an omission bias: participants preferred the *status quo* even though it meant more children would die because change would mean action that would cause harm.

One thing that distinguishes studies of *status quo* biases from work in the tradition of institutional theory is that in BDR there is a normative benchmark. Institutional theory points out the many ways in which organizations are resistant to change, but usually cannot answer the question of whether such inertia is good or bad for the organization because it cannot specify how much inertia the organization *should* have. Researchers can argue this question until they are blue in the face, but empirical data cannot provide a resolution until the researchers can identify a normative standard for how much inertia is optimal for the organization. BDR perspectives can help establish this normative standard.

BDR work on the *status quo* bias should interest OB researchers in multiple areas. For example, a crucial issue in change management is employee resistance to major change efforts. According to Kahneman and Tversky's (1979) reasoning, such resistance comes from a combination of two factors that relate to the *status quo* bias: (1) employees valuing certain outcomes more than uncertain outcomes, which makes change less attractive; and (2) asymmetrically valuing the losses and gains derived from changing the *status quo* (e.g., overvaluing the losses associated with changing compared with those associated with staying put). Using the BDR approach, OB researchers might investigate whether resistance to change can be mitigated by manipulating the framing of perceived "gains and losses". This approach might also benefit research on turnover decisions (e.g., why do people choose to stay when it would be rational for them to leave?) or leadership (e.g., do charismatic leaders

overcome the *status quo* bias by altering followers' evaluations of potential gains and losses?).

Social Networks

OB scholars are fascinated by social networks, and for good reason. Social ties serve as a critical means by which things get done in the workplace and provide social support in times of stress. Recently, research on social networks has taken a more cognitive view, examining people's mental representations of their social networks and others' networks too. This is an important topic in part because it can help us understand whether people's social network positions are a consequence of strategic behavior or of happenstance (Flynn, Reagans, Amanatullah, & Ames, 2006; Janicik & Larrick, 2005). After all, if people are unaware of the position they occupy in the social network, then it is unlikely they arrived there on purpose. Occasionally, this line of research asks normative questions regarding the extent to which people's perceptions of social networks are consistent with reality. When it does so, it becomes relevant to BDR.

Krackhardt and Kilduff (1999) examined whether people accurately perceive the social networks in which they are embedded. Their answer was that it depends on the proximity to the individual making the judgment. Beliefs about ties between those close by, such as friends, were biased because people were motivated to believe that their friends liked the same people they liked and disliked the same people they disliked. Beliefs about those far away in the network were colored by simplifying assumptions of balance in positive and negative relationships because people did not have enough information to make accurate judgments about those remote individuals. But in between, people had more accurate perceptions of social ties: motivational biases were weaker, yet they had enough information about colleagues who were moderately close to them to make an informed judgment.

The inaccuracies present in people's beliefs about social networks represent a profound challenge for some of the common approaches to the study of social networks (Krackhardt, 1996). These studies often rely on individuals' reports of their relations with others for establishing the structure of the network. However, as Krackhardt (1987) details, people's perceptions of their social networks correspond only weakly with reality. This fact does not imply that cognitive social networks ought to be dismissed as worthless. Perceptions can, under some circumstances, be more important than reality (Kilduff & Krackhardt, 1994). However, it suggests that researchers would do well to take self-reports with a grain of salt and obtain objective measures of important variables whenever possible.

Current work on cognitive networks raises several questions that bear implications for research in OB. Specifically, are patterns of information sharing in an organization suboptimal because those employees who possess valuable

information lack accurate cognitive maps of their social networks? Further, are attempts at social influence susceptible to the same problem? For example, consider a senior manager who is trying to influence informally the thoughts and feelings of a sizable set of employees. The manager may rely on close contacts to act as agents of influence on his behalf, but are these primary contacts as well connected as the manager hopes, or will the manager's perception of their connections be biased in some systematic way? If, in fact, cognitive networks among coworkers are biased, it would be useful for organizations and their members to know how they are biased in order to improve their effectiveness at keeping tabs on the thoughts and feelings of their constituents or influencing those individuals in a meaningful way.

The ideas presented in this section represent a small number of ways in which BDR can inform topics of interest to OB scholars. This not an exhaustive set—we see numerous other domains within OB that could be strengthened by considering BDR's normative question, including ethical decision making in organizations, helping behavior, commitment, group dynamics, and work–life balance, among others. To be clear, much of the research we have discussed and that we believe qualifies as BDR is not done by people who regularly attend the meetings of the Society for Judgment and Decision Making or who regard themselves as part of the BDR crowd. Nevertheless, when the research considers an explicit normative standard that allows the researchers to specify how and how much judgments or decisions deviate from the optimum, by our definition that research qualifies as BDR.

The BDR Approach

As our review suggests, BDR is more of a research approach than a specific topic. BDR pushes researchers to think about what the null hypothesis ought to be. Sometimes, as when the experimental manipulation is normatively irrelevant, it makes sense to have a null hypothesis that predicts no difference between conditions. But other times, normative theories provide more specific guidance. Indeed, normative theories may provide a point prediction regarding what a rational person ought to do, and researchers can compare actual behavior to this clear standard. Purely descriptive research without a normative backdrop hardly ever offers theory clear enough to specify a point prediction.

At this point, we should address some concerns that OB researchers frequently express about BDR, including the work that we have reviewed here. Concerns about BDR's empirical evidence converge on two overarching issues: context and generalizability. Some critics wonder if BDR studies can account for the richness of organizational contexts or if the findings derived from BDR work apply to most organizations. In addition, some researchers question whether BDR's focus on decision making errors can be useful in developing practical theories and whether OB researchers can easily identify

normative benchmarks in studying complex phenomena. We address each of these concerns in the following sections.

Gathering Empirical Evidence for BDR

BDR is not necessarily limited to a particular source of empirical data. Most BDR research takes place in laboratory environments, but a significant amount of work takes place in the field. In addition to those studies we have cited, there are many examples of the powerful and important consequences of gain/loss framing and reference points on everything from labor markets (Camerer et al., 1997; Falk, Fehr, & Zehnder, 2006) to racetrack betting (Camerer, 2000) and investing (Odean, 1998a, 1998b). For instance, DellaVigna and Malmendier (2006) found that people's unrealistically virtuous intentions, which have been highlighted in laboratory studies of self-control, are also manifest in the behavior of health club patrons. According to data collected from thousands of patrons, people intend to use health clubs more than they actually do, as shown by their willingness to pay monthly fees that exceed the cost of paying by the visit, based on their actual usage rates. DellaVigna and Malmendier (2004) also show how corporations capitalize on people's self-control problems in a number of different ways (e.g., credit card agreements).

Still, the majority of BDR research comes out of the lab. The reason for that is two-fold. First, manipulation of the hypothesized independent variable is indispensable for establishing causality—a criterion as critical in organizational research as in other sciences. For instance, many people assume that group cohesion leads to higher group performance, but field data showing a correlation between cohesion and performance cannot prove causality. The same factors that lead a group to be productive may also produce group cohesion. This possibility turns out to be true: Staw (1975) showed that when groups are led to believe that they have succeeded, they come to believe that they have been more cohesive. Putting research to use successfully depends on correctly identifying causal relationships and therefore understanding what causes what.

Second, the control afforded by the laboratory setting allows researchers to specify more easily what the optimal decision by participants would be. This second reason represents what is distinctive about BDR. Many interesting and influential field studies cannot specify what an optimal decision would be. Consider managers who attempt to find employees by relying on their social networks (Granovetter, 1995). These managers are making a trade-off of convenience over thoroughness of search. Are they making this trade-off optimally? Field data cannot answer this question for multiple reasons: (1) we cannot know how good the un-hired people would have been; (2) it is difficult evaluate precisely how good the actual employees are; and (3) we cannot compare their quality with the cost of searching for better employees. Using a BDR approach need not assume that managers are rational, but without being able

to specify what a rational person ought to do, research is relegated to being descriptive rather than prescriptive (Bazerman, 1999).

While it is tempting to think that it ought to be possible to study managerial decision making in the field by simply asking managers to report what they do or by observing their decisions, these approaches have proven problematic for two reasons. First, people often are deeply and profoundly unaware of how they go about making many decisions (Bertrand & Mullainathan, 2001b; Nisbett & Wilson, 1977; Schwarz, 1999), which makes self-report data unreliable. Second, although observing decisions made in the field can yield rich data, this approach raises concerns about sample size. The BDR field studies that have proven most enlightening are those in which the same decision is made many times by many people, such as betting, hiring, or investment decisions.

A further limitation of field research is that it fails to capture what *could* have happened. Field studies restrict our attention to the variables at play in the field rather than introducing other variables that might have greater impact. The potential advantages of some decision-making strategies may go unnoticed if the fuller range of possible strategies was not explored thoroughly in an experimental setting. Although pre-commitment savings programs such as Save More Tomorrow are relatively rare in organizations, we now know that they *could* exist and that they are superior in many ways to traditional retirement savings programs. We know this because of carefully conducted laboratory research. The BDR approach prompts researchers to focus more attention on how to encourage managers to adopt better decision making practices, perhaps by testing the effectiveness of interventions in a controlled setting.

The Importance of Field Research

Field research, including descriptive research and case studies, can deeply inform our theories by shedding light on possible relationships between variables. In particular, qualitative research is useful in specifying which variables may be critical, but at the same time, may escape our attention (Fine & Elsbach, 2000). In addition, field research can help researchers refine and clarify their theories. Applying abstract and general theories to particular contexts is often far from straightforward, but such application can be invaluable for noting boundary conditions, identifying catalysts, and specifying conceptualizations of key variables.

Of course, field research can also inspire questions that move to the lab for further testing. Moore, Swift, Sharek, and Gino (2007) found that undergraduates from institutions with lenient grading were more likely to be admitted to graduate school than were their peers from institutions with tougher grading policies. In an attempt to understand this result and to explore possible explanations for it, the researchers carried out a series of laboratory experiments that eventually offered a new perspective on the basic psychological phenomenon

known as the fundamental attribution error (L. Ross, 1977). To pick another example, the common failure to save enough for retirement has inspired insightful research on impatience and intertemporal choice (Loewenstein & Elster, 1992). And organizations' persistent use of unstructured employment interviews by organizations, despite the damning evidence on their value, has inspired research to attempt to understand this puzzling behavior (Dawes & Dana, 2007).

Many organizational researchers rightly express concern that laboratory research omits the organizational context. In any organizational decision, there are many factors interacting at once, and laboratory designs cannot account for all the important contextual variables simultaneously operating (Mowday & Sutton, 1993). This criticism is entirely correct. But field research does not necessarily solve the problem. It is usually impractical—often impossible—to measure all the key influences on an individual decision in an organizational context. If the context matters, researchers should figure out which aspects of context matter and study those aspects in rigorous ways. Researchers who argue that they cannot specify the key features of the context that matter are, in a sense, admitting that they do not understand what they are studying.

Generalizability of Laboratory Results

Some OB researchers worry that laboratory results will not generalize to field contexts. However, the literature is replete with examples of basic phenomena first studied in the laboratory that were then replicated in the field (Camerer, 2000; Locke, 1986). Escalation of commitment was originally studied in the lab (Staw, 1976, 1981) but has been documented in several field settings including professional basketball teams (Staw & Hoang, 1995), bank loan officers (Staw, Barsade, & Koput, 1997), and government budgeting decisions (J. M. Ross & Staw, 1986). The power of gain/loss framing to influence risk preferences has been extensively studied in the lab (Kahneman & Tversky, 1979, 2000), but has also proven useful for explaining actual investment behavior (Benartzi & Thaler, 1995; Odean, 1998a), labor markets (Camerer et al., 1997), choices to save or spend (Thaler & Benartzi, 2004), and more (see Camerer, 2000). The self-serving nature of fairness judgments in negotiation was originally demonstrated in laboratory studies (Babcock, Loewenstein, Issacharoff, & Camerer, 1995; Thompson & Loewenstein, 1992) but was subsequently documented in the judgments and rhetoric of parties to a labor/management dispute (Babcock, Wang, & Loewenstein, 1996). BDR researchers agree that "field testing" a theory is essential if the theory is to be of practical value.

We enthusiastically endorse the use of a variety of research methods (see Chatman & Flynn, 2005). Qualitative research can be useful for getting familiar with a phenomenon and identifying the key variables. Experiments are essential for being able to establish causality, and experimental research designs are often most feasible to implement in the laboratory. Field studies are essential

for demonstrating the generalizability of a particular phenomenon outside the laboratory, and for clarifying how causal relationships play out. These insights often lead to more questions that deserve further study, both in the lab and the field.

Is BDR Simply Showing that People are Stupid?

Several observers have complained that BDR work is overly negative, especially work growing out of the "heuristics and biases" tradition (Gigerenzer, 1996; Juslin, Winman, & Olsson, 2000; Krueger & Funder, 2004). According to Kihlstrom (2004), decision research essentially demonstrates that "people are stupid", highlighting their shortcomings and their weaknesses rather than their successes and strengths. Indeed, the positive psychology (Seligman & Csikszentmihalyi, 2000) and positive organizational scholarship (Cameron, Dutton, & Quinn, 2003; Luthans & Youssef, 2007) movements have arisen, in part, as a response to this perceived negativity.

There are two primary reasons for BDR's focus on failures and irrationalities. The first is the same reason why physicists smash atoms. Physicists learned about the structure of atoms by observing how they broke apart or failed when they were smashed with high-velocity particles (Kahneman & Tversky, 1983). Just so, the inner mechanisms of human cognition are often revealed most clearly in their failures (Epley, Van Boven, & Caruso, 2004). In our attempts to understand human behavior, we are much in the same position as physicists trying to infer the properties of atoms by observing them from the outside, or of someone who is trying to study a complex machine by observing it in action. Those who study human cognition have not made much progress disassembling the machine and seeing how it works from the inside. Instead, the most promising approach has been to give the machine tasks to perform and then observe its performance.

When the machine works perfectly, solving every problem we give it, we learn little about the human machinery other than the fact that it is impressive. And it is indeed impressive. The modest three-pound human brain can solve complex problems of recognition, perception, and coordination that lie beyond the abilities of even the most sophisticated computers. For instance, our abilities to effortlessly recognize faces and comprehend language are beyond the capabilities of any computer on earth. However, psychologists learn the most about how we accomplish these successes not by observing successes but by taking account of failures. When do we confuse one face with another? When do we confuse one word for another? Answers to these questions have helped us understand how our minds process visual and auditory information (Holt & Lotto, 2008; Yovel & Kanwisher, 2005). Likewise, studying how organizations fail can provide useful lessons about what helps them succeed (Perrow, 1984; J. M. Ross & Staw, 1986; Sitkin, 1992; Weick, 1993).

The second reason for BDR's focus on failures and irrationalities is that the field prospers by exploiting these anomalies. When people behave in ways that are consistent with normative models—or at least not highly inconsistent with them—BDR researchers have little advice to offer on how to improve. BDR thrives in the productive tension between what *is* and what *ought to be*. This does not mean that BDR is interested only in mistakes and failures. People can also deviate from the normative benchmark by outperforming it. These instances have been of great interest to behavioral decision researchers. One example is our ability to avoid the so-called "winner's curse." Although game theory predicts that markets will collapse when the seller knows more than the buyer about the item being sold (Akerlof, 1970), real people manage to sustain markets even under asymmetric information and BDR researchers have investigated how they do this (Bazerman, Gibbons, Thompson, & Valley, 1998). Another example comes from the well-known prisoner's dilemma game, in which universal competition and mutual destruction is the rational equilibrium, but BDR studies have shown how people manage to avoid this dismal outcome with impressive frequency (Dawes, 1980; Ledyard, 1995).

Lastly, it is worth noting that BDR is interested only in generalizable human tendencies that result from cognitive processes. Mistakes that are due to carelessness, lack of training, and inexperience are not nearly as interesting as those errors whose universality suggests they derive from fundamental cognitive biases that all people share. Further, cognitive processes are, according to at least some scholars (Fischhoff, 1982; Larrick, 2004), more malleable than dispositional traits or resource constraints, which opens up opportunities for meaningful interventions (see also Heath, Larrick, & Klayman, 1998).

What's the Right Normative Standard?

We have thus far not addressed the question of where the normative standard comes from in BDR. Usually, the answer to this question is fairly straightforward. The answer comes from decision analysis, the quantitative analytical approach to decision making against which BDR contrasts itself. Decision analysis is usually consistent with Bayesian principles of belief updating, statistical principles of expected value, and economic notions of rationality, including game theory. It is tremendously valuable prescriptively, because it guides the way to optimal strategies and stable equilibria. It forms the foundation upon which many fields are built, including neoclassical economics, operations research, accounting, finance, quantitative marketing, and statistics. On the other hand, it is routinely inaccurate as a description of human behavior. That is, of course, where BDR thrives: in examining the contrasts between the optimal and the actual.

We should note, however, that there is room for researchers to disagree about what the right normative standard is in a particular situation. It is not

fair to accuse people of being irrational just because they are not omniscient. Many important phenomena in social psychology and in BDR can be explained by simply considering the selective information that people have at their disposal when they form judgments (Fiedler, 1991, 2000, 2007). For instance, Denrell (2005) has pointed out one such reason why people evaluate outgroups and enemies more negatively than ingroups and friends—once people form a negative evaluation of an outgroup or an enemy, they avoid interactions that might allow them to update (and potentially correct) their negative perceptions. To pick another example, Moore and Small (2007), show how key results from the overconfidence literature can be explained by the simple fact that people do not know how good they are, either in absolute terms or compared with others.

We should also acknowledge that, under some circumstances, it is impossible to specify a normative standard because it is hard to specify the decision maker's incentives, interests, or constraints. We readily concede that this limitation constrains the domains in which it is possible to specify a normative standard on which BDR depends. When it is impossible to specify a normative criterion, it is not possible to do BDR. On the one hand, this represents a real constraint. Yet on the other hand, we must be realistic about what is worth studying. There are many fascinating phenomena that elude scientific investigation because we cannot figure out how to examine, measure, or make sense of them. If we do not understand the situation well enough to specify a normative standard, we believe there is room to question whether we, as social scientists, should be spending our limited time examining it, or whether we should be making more substantial progress on problems to which we can make more useful contributions.

Conclusions

This paper began with the assertion that the field of OB is uniquely positioned to take advantage of BDR's growing strength, status, and influence. What we mean by this is that OB is already comfortable with its status as an interdisciplinary field, situated between basic disciplines, and interested in opportunities for applying basic research to real problems. Furthermore, BDR shares with OB a deep interest in the judgments, decisions, and behaviors of people in organizations. OB and BDR scholars are both interested in research with prescriptive implications. OB scholars can deliver the practical insights and pedagogical content of BDR research to MBA students and working managers who are eager to receive it and who can put it to direct and valuable use in their professional lives. Furthermore, many of the best BDR researchers are already in business schools.

In 1993, Jeffrey Pfeffer pointed out many ways in which OB's lack of paradigmatic consensus weakened the field's potential impact. When we, as a field, cannot agree on which research questions are worth asking, let alone which

research methods are appropriate for attempting to answer those questions, it undermines our ability to make theoretical progress in our research and to wield intellectual influence in academe and beyond. Progress is made more difficult by the vague nature of our theories and our resistance to formalization. As Meehl (1978) pointed out in his critique of "soft" social science, our theories are never truly confirmed or refuted—they just sort of hang around until they pass out of fashion. One direct consequence of our field's weakness has been that other fields—most notably economics—have claimed territory that ought rightfully to belong to us.

BDR does not share these weaknesses. It enjoys a clearer consensus about what is worth studying and how to study it. BDR theories are more amenable to formalization (for example, see Kahneman & Tversky, 1979; Loewenstein et al., 2003; Moore & Healy, 2008; Rabin, 1993). In addition, BDR lends itself to prescription and practical application because it tells managers the ways in which their decisions are likely to deviate from optimality and informs them about how they can do better. Perhaps these are some of the reasons for BDR's growing influence in economics departments, medical schools, law schools, schools of engineering, and schools of public policy. Indeed, BDR offers the possibility of connecting OB with scholars doing related work in other fields.

Can BDR provide the paradigm that can unify the disparate camps within OB? We think it might, but not easily. For many scholars within OB, what makes their work "organizational" is that they conduct field research on working professionals in real organizations. While it is possible to do BDR in field settings, it is often difficult to specify the relevant normative standard because it is difficult to get measures of key variables in the field. For instance, understanding whether entrepreneurs are making a mistake when they gamble their life savings on their ventures depends on understanding how much they enjoy running their own businesses. Understanding whether workers are making a mistake when their commitment to the organizations for which they work leads them to sacrifice time with their families depends on being able to compare the quality of these differing forms of fulfillment. Obviously, it is not easy to obtain credible measures of these important constructs.

Nevertheless, OB researchers need not accept BDR as the dominant research paradigm to enjoy the benefits it has to offer. For many research topics, all it takes is a little thought and some refinement of the experimental design in order to clarify what people or organizations *ought* to be doing. There are many benefits of being able to specify the normative backdrop against the actual behavior of organizations and their members. The benefits of BDR approaches have been too long neglected by OB researchers. As it happens, OB, more than other disciplines, can capitalize on BDR's strengths and learn from its successes. Our field has only to seize the opportunity laid before it.

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