

RESPONSE

Applying Organizational Justice: Questionable Claims and Promising Suggestions

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Abstract

With an eye toward promoting applications of organizational justice, I respond to commentaries on my focal article (Greenberg, 2009a). Specifically, I challenge questionable claims regarding (a) characterization of applied research, (b) why we don't conduct more applied justice research, (c) moral versus instrumental rationales for promoting justice, (d) the validity of intervention studies, and (e) interpretations of Lewin's classic observation about the practical value of theory. I also identify and comment upon two suggestions for promoting applied justice research: (a) promoting cooperation between researchers and practitioners and (b) conducting comprehensive, integrative interventions.

My essay lamenting the paucity of applied justice research (Greenberg, 2009a) generated a set of very thoughtful commentaries, and I'm confident that I–O psychologists will benefit from the ideas their authors expressed. Although I intended to be appropriately provocative in my role as the author of the focal article in this journal, I was somewhat surprised to find that several responses were based on claims I believe to be questionable. I welcome this opportunity to challenge these assertions.

To my delight, the commentaries also included a number of very worthwhile suggestions that I am pleased to highlight, endorse, and extend. In offering these remarks, both positive and negative, I hope to satisfy my overarching objective: to promote applications of organizational justice. I begin here by identifying and responding

to five areas where my perspective differs from that of the commentators.

What Constitutes Applied Justice Research?

Systematically cataloging the literature using the categories I described in my focal article, Bauer et al. (2009) confirmed my observation that implication studies dominate the field of organizational justice and that application studies have been few and far between. Several commentary authors (Gilliland, 2009; Lopez, 2009; Rupp & Aquino, 2009) suggest, however, that my approach to identifying applied justice research was too restrictive and that there have been more applied justice studies than I have implied. They reach this conclusion by using different definitional criteria.

Unpublished Managerial Interventions

Lopez (2009) notes that published journal articles contain only some of the

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intervention studies that are conducted and that, although they don't label them as such, many managers conduct justice interventions as part of their ongoing efforts at organizational improvement. I wonder about these interventions, however. Do they incorporate control groups against which improvements may be gauged and other desiderata of good research? In fact, I wonder if these interventions are incorporated into research (designed to test hypotheses) at all, so much as they are demonstrations (designed to apply principles). After all, research is not managers' business (an end in itself), but a tool to enhance their business (a means to an end), and one with which they may be unfamiliar (at least, absent is the collaboration with researchers, as Lopez wisely recommends). On the basis of this—and, certainly without knowing more of the specifics—I believe it may be imprudent to consider some of the things that managers do in the name of promoting justice to be intervention studies.

This is not to say, of course, that managers are not well informed and that their practices are ill advised. Far from it! I claim simply that the bases for some managerial insight about justice and the practices managers use in this regard may be improved by incorporating fundamental principles revealed by decades of research on organizational justice (Greenberg, 2009b).

Studies Focusing on Other Topics

Like Lopez (2009), Gilliland (2009) also notes that there are applications of organizational justice that are not being counted because they haven't been labeled as such. Specifically, Gilliland calls our attention to studies on participation, information sharing, and respectful treatment of workers, which reveal findings consistent with conceptualizations of justice. These are good examples, and one can identify additional areas of organizational functioning in which justice principles may be operating although they are not identified as such. In fact, in the late 1980s, I organized a symposium at a meeting of the Academy of

Management showcasing research in which justice concepts were camouflaged in studies of other topics without being identified as such. Like the managers of which Lopez speaks, the scientists conducting these studies were unaware of a set of conceptual tools that promised to shed light on their efforts.

That many of the topics we study from a justice perspective already have been researched but not given a justice moniker is not surprising. After all, when Rob Folger and I (Folger & Greenberg, 1985) introduced the concept of procedural justice to the I-O psychology literature a quarter century ago, we did so by touting its applicability to a variety of already well-researched phenomena. What we did then, and what still appears to be happening, is that justice concepts are being used retrospectively to shed light on existing phenomena. Then, in some cases, justice concepts are imported to those areas of study in which they are being applied. For example, this has been ongoing in the field of compensation for some time (Milkovich & Newman, 2007).

Because justice does not operate in a vacuum and we study "the fairness of X," it's commonplace for us to overlap with the literature on X, as we should. In such cases, the value added of a justice framework comes in the form of shedding new insight into existing phenomena. Unlike Gilliland (2009), I would refrain from referring to studies that are not informed by the justice literature as justice studies. Rather than quibbling about what to count, the important thing pointed out by Gilliland is that there can be no mistaking the explanatory power of applying justice concepts to such efforts on a post hoc basis, as he and his associates have done so eloquently recently (Cropanzano, Bowen, & Gilliland, 2007) and as Sashkin and Williams (1990) did 2 decades ago. Such efforts not only introduce justice concepts to broader applied audiences, they also promise to promote research in those areas.

Implication Versus Application

With respect to delineating applied research, I disagree with Rupp and Aquino (2009), who ask us to count implication research—investigations that lead us to the brink of application but fail to take the plunge. Unlike application studies, implication studies do *not* provide direct and immediately useful answers to managerial questions. “Our field is rich with empirically supported theory ripe for integrative application” (p. 208) Rupp and Aquino tell us, and I cannot agree more. However, until those ripe possibilities become real examinations, we will not have conducted applied research.

In this connection, I agree with Landy (2008), who cautions that implication research may ultimately lead to application research but that implication studies themselves should not be considered application studies. This is not to say that implication research is not valuable. To the contrary, there have been many studies of this type (Bauer et al., 2009), and they have benefited our field greatly. And this justice researcher, for one, plans to add to this count in his personal research agenda. When I do, however, I will *not* refer to the work as tests of justice applications, and I would encourage others to be similarly careful about over-representing the applied nature of their own investigations. Doing otherwise threatens to mislead scholars and practitioners.

Why Don't We Do More Applied Justice Research?

Three sets of authors (Lefkowitz, 2009; Rupp & Aquino, 2009; Somers, 2009) identified culprits for the paucity of applied justice research to supplement the ones I offered. I acknowledge that additional explanations surely are likely and I welcome these, but I question the logic and assumptions of some of the ones offered in the commentaries.

Inattention to Distributive Justice

Lefkowitz (2009) argues that the literature's heavy focus on interactional justice and

procedural justice comes at the expense of distributive justice and that this is one of the sources of our intention to application. How so? Lefkowitz (2009) does not tell us. Is there something inherently more applied about distributive justice than other forms of justice? If so, it evades me. In fact, efforts to apply justice principles in the workplace have focused on interactional justice (e.g., Greenberg, 2006a) largely because of its applied potential as a form of justice that managers can promote by virtue of their individual efforts. Moreover, workers have been found to be acutely aware of both distributive and procedural determinants of justice on their jobs (e.g., Greenberg, 1986) and to respond adversely to violations of these forms of justice (Conlon, Meyer, & Nowakowski, 2005). Together, such findings suggest that all forms of justice—not just distributive justice, as claimed (Lefkowitz)—have considerable potential for application.

Lefkowitz (2009) is correct in observing that as a field we've been paying less attention to distributive justice than to other forms of justice—and, I might add, for the past 2 decades. This is due in part to the natural tendency for scientists to jump on the bandwagon by studying promising new concepts (as procedural justice and interactional justice were when the trend first emerged), especially when fueled by intriguing conceptual and empirical findings. In the case of the shift from distributive justice to procedural justice, momentum was sparked by arguments regarding the limitations of equity theory (Leventhal, 1980) and evidence revealing significantly greater variance in key outcomes accounted for by procedural justice relative to distributive justice (e.g., Alexander & Ruderman, 1987; Tyler & Caine, 1981). In the case of the shift from procedural justice to interactional justice, momentum was sparked by efforts to differentiate the distinct qualities of these two constructs (for a review, see Bies, 2005) and by evidence attesting the greater proportion of variance accounted for by interactional justice relative to both distributive

justice and procedural justice (Ambrose & Schminke, 2003).

I caution, however, that the relatively greater attention to procedural justice and interactional justice does not mean that we have forgotten about distributive justice. Just because newborns entering a family may receive more attention than older children does not suggest that the older children siblings are rejected. They simply may require less attention at the time. And just as the addition of new children to a family alters patterns of interaction with older children, the emergence of new scientific concepts changes the way we look at older ones. Today, for example, we are inclined to focus on distributive justice as part of an integrated approach to justice that incorporates procedural justice (Brockner & Weisenfeld, 1996) and even all forms together in conceptualizations of "overall justice" (Ambrose & Arnaud, 2005). This is reasonable insofar as the three forms of justice are not only highly intercorrelated when measured but also because they are conceptually nested. To wit, distributive justice is the perceived fairness of outcome distributions; procedural justice is the fairness of the procedures used to determine those procedures; and interactional justice is the perceived fairness of explanations of those procedures. With this in mind, attention to any one particular form of justice may be understood as referencing another either directly or indirectly.

There is another aspect of Lefkowitz's (2009) commentary that troubles me. Specifically, he claims that unlike other forms of justice, distributive justice is objective in nature and that people's perceptions of it may be incorrect. By what standards? There are considerable cross-national differences in justice perceptions (Miles & Greenberg, 1993), and I would be uncomfortable claiming that the perceptions of people in one nation are more correct than those in another. If anything, because perceptions of procedural justice and interactional justice are considerably more similar across cultures (Greenberg, 2001), it would be easier (but still unwise) to make a case for their

objectivity relative to distributive justice. Additionally, by nature, many elements of procedural justice are objectively verifiable. For example, the existence of voice-giving procedures may be objectively identified (e.g., the presence of suggestion systems), as many procedures for correcting erroneous decisions (e.g., use of appeals procedures in court or the instant replay rule in American football). Thus, I think Lefkowitz might have made a stronger case for the objective nature of procedural justice than distributive justice.

Regardless, the fact that there may be objective aspects of some justice phenomena should not deter us from studying "softer" issues, such as people's awareness of justice practices (which may be even more important than the mere existence of these practices when it comes to triggering responses) and their phenomenological reactions to them, such as emotional responses to injustice, which have profound effects on people (Greenberg & Ganegoda, 2007). This is our stock in trade as psychologists. Although we surely can, and do, study objective phenomena, some of us may prefer leaving questions about broader, systemic aspects of justice to labor economists, lawyers, and sociologists. Better yet, we should collaborate with such professionals in an effort to gain a more thorough, cross-disciplinary understanding of applied justice issues across multiple levels of analysis. (Indeed, the initial foray into procedural justice was the result of one such multidisciplinary effort: the collaboration between Thibaut, a psychologist, and Walker, an attorney; Thibaut & Walker, 1975.)

Attending to the Sources of Justice and Injustice

Like Lefkowitz (2009), Rupp and Aquino (2009) also suggest that our inattention to applications of justice is based on the particular focus of our research. Although Lefkowitz suggests that the problem lies with attention to one particular form of justice compared with others, Rupp and Aquino argue that the culprit is our focus

on forms of justice altogether. As an alternative, they suggest that the multi-foci approach to justice (Cropanzano, Rupp, Mohler, & Schminke, 2001), which examines sources of justice and injustice (e.g., supervisors, peers), is inherently more application oriented.

I agree that the multi-foci approach is valuable but I don't follow their reasoning about its capacity to encourage application. Specifically, Rupp and Aquino (2009) claim that, "A multi-foci approach also shifts our conceptualization of justice from the study of employee perceptions to the study of justice as a performance competency" (p. 206). As I see it, developing competence in behaving fairly requires *not* overlooking the various forms of justice but being hyper-aware of how to foster them (e.g., what procedure to use to make a compensation system fair in terms of both internal policies and market standards). Moreover, it may be argued that although focusing on specific sources of justice may encourage idiosyncratic considerations that vary across organizations, focusing on ways to promote specific forms of justice is more generalizable.

Most importantly, I don't see this as an either-or situation. We should be clear about the referents involved in our research, such as by asking questions about the fairness of, say, a boss or a colleague, although we have not always done so. And, as Rupp and Aquino (2009) note, this can—and I add, in many cases, should—be crosscut with questions about various forms of justice (e.g., the procedural justice of the organization's performance appraisal system, or the interactional justice of one's immediate supervisor). The more focused we can be about the questions we ask survey respondents the greater insight we can derive from their answers.

Given that our measures follow from our conceptualizations, it's not difficult to see why we so often ignore various sources of justice and injustice. Today's overwhelmingly dominant measure of organizational justice, Colquitt's (2001) justice scale, fails to reference any particular sources

when asking about distributive justice and procedural justice and references only an "authority figure" when asking about interactional justice (here, separately, with respect to its interpersonal and informational components). I believe that for the promise of the multi-foci approach to be realized fully, it would be helpful for a new measure of organizational justice to be developed that incorporates this particular perspective.

Focus on Justice or Injustice?

In presenting a third, and allegedly simpler explanation for the paucity of applied justice research, Somers (2009) asks us to believe that our applied research focuses on injustice as opposed to justice, which "makes people uncomfortable" (p. 215), and allegedly keeps scientists from venturing into this territory as a result. Basic research, in contrast, he says, focuses on the presumably more approachable topic of justice. This is an interesting claim because underlying it is the assumption that justice and injustice are not opposite sides of the same coin, as they generally are conceived, but discrete. (Coincidentally, I am currently researching this issue.)

Even if we are to accept the notion that a glass can be half empty without also being half full, I challenge Somers' (2009) observation about the particular distribution of emphasis he claims. In fact, I believe that Somers has things reversed. My read (admittedly informal and anecdotal, like his) is that basic research appears to have focused primarily on responding to injustice (which has dominated the study of organizational justice; Barclay, Skarlicki, & Latham, 2009; Greenberg, 2006b), whereas applied research has focused on the promotion of justice.

The focus on injustice may be seen in basic research and implication research on all three types of organizational justice. Going back to the early tests of equity theory (Adams, 1965), scientists focused on how people sought to redress the injustices of underpayment inequity and overpayment

inequity (for a review, see Greenberg, 1982). Basic research (e.g., Greenberg, 1987) and implication studies (e.g., Alexander & Ruderman, 1987) of procedural justice also focused on injustice—namely, how people respond to unfair procedures. Finally, studies of interactional justice have focused on the behavioral (e.g., Greenberg, 1993) and emotionally charged (e.g., Aquino, Tripp, & Bies, 2006) ways in which people respond to interactional injustices.

In contrast, our intervention studies have assessed the efficacy of conditions introduced to promote justice (Greenberg, 1990, 2006a). Of note, because they involve individual-level interventions, which are easier to affect than system-wide changes, these efforts have focused only on interactional justice at this point. Indeed, as Lopez (2009) correctly points out, promoting interactional justice is likely to be regarded as an exercise in managerial skill development. This focus on the promotion of justice counters Somers' (2009) suggestion that application efforts are focused primarily on injustice rather than justice.

Although intervention studies have focused primarily on promoting interactional justice, an individual-level variable, we may also consider broader, system-level interventions. On the basis of the implication research by Schminke, Ambrose, and Cropanzano (2000), for example, there's reason to believe that the introduction of certain systemic changes in organizational structure (enhancing decentralization) may promote procedural justice. Given that decentralization gives employees voice in organizational procedures, this isn't surprising, but it would be naïve of us to expect top leaders to change the designs of their organizations solely to bring this about. After all, there are likely to be complex, non-justice-based reasons for structural decisions that take precedent.

Thus, until we can make the kind of compelling cases about the promotion of justice that Lopez (2009) indicates, such as by showing how financial indices are enhanced by the promotion of justice (as was reported by Simons & Roberson,

2003, for example), it's unrealistic to hope that companies will knock down our doors with requests to introduce systemic, macro-level justice interventions. For now, I believe it's important to continue conducting implication studies that focus on system-wide variables, such as Schminke et al. (2000) have done, because the cumulative weight of such efforts may inspire and pave the way for subsequent application studies (hence, subsequent applications).

Moral Versus Instrumental Rationales for Promoting Justice

My recommendation that I–O psychologists focus on the instrumental benefits of promoting justice in organizations although eschewing moral arguments when communicating with clients about promoting justice appears to have struck a nerve. Rupp and Aquino (2009), in particular, disagree and suggest that the widespread adoption of various organizational practices (e.g., codes of ethics and ethics audits) has made today's managers more sharply attuned to moral issues than I acknowledge.

I respectfully counter their argument on two grounds. First, although there can be no arguing that legal regulations (e.g., the Sarbanes-Oxley Act of 2002) have made various ethical practices commonplace in today's organizations, this does not ensure that managers are prepared to address the "coverage of subjective realities and moral dilemmas" that Rupp and Aquino (2009, p. 208) acknowledge to be involved in moral discussions. Indeed, ethical behavior (with its social system emphasis) and moral arguments (stemming from personal character) may be related only in indirect and complex ways.

Second, even if our moral arguments are understood and accepted, I suspect that they would be less effective than a more practical, instrumentally oriented approach that speaks managers' more practical, native language. In this regard I agree wholeheartedly with Lopez (2009), who advises, "When trying to recruit managers, justice

researchers need to tailor their approach so that the managers clearly understand the return on investment (ROI) for the research, especially how the intervention will solve their business/organizational challenges" (p. 226). This is not accomplished through moral arguments.

Barclay et al. (2009) also object to my suggestion to solicit research partners by using an instrumentally framed argument. However, their argument attacks a straw man by countering a point that I didn't make. Specifically, they claim that my "stance on the importance of a self-interested motive above other motives (e.g., morality) is one that has already been heavily criticized in the literature" (p. 203). Following this statement with particular citations to Folger and his associates indicates that they believe I was discussing the role of moral versus instrumental motives for people's behavior. This is a different matter. Although I have discussed this elsewhere (Gillespie & Greenberg, 2005), I did not address it in my focal article because it is far removed from the more pragmatic point I was making—that is, the wisdom of highlighting the established instrumental benefits of promoting justice in organizations when discussing the matter with managers. I caution readers not to be misled by their criticism of an unrelated issue.

Validity of Intervention Studies

For I–O psychologists to understand how to interpret the findings of justice intervention studies, it is essential for the operational definitions of constructs used to be linked closely to conceptualizations that shed light on them. Byrne (2009) claims I violated this requirement in my interactional justice training intervention study (Greenberg, 2006a). Specifically, she asserts, "Greenberg (2006a) indicates that he *guessed* what he thought would be interactionally just behavior: demonstrating emotional support (Isn't that perceived supervisor or social support?) and avoiding intimidation (Where is that described in justice literature and how do we do that?)" (Byrne, 2009, p. 217, emphasis added).

In fact, however, I did *not* rely on an idiosyncratic and speculative operationalization of interactional justice. Instead, as noted clearly, training content was based on six deliberately chosen elements derived from suggestions in the literature on interactional justice. To highlight this, I mapped each element to its conceptual sources in Table 3 of the article reporting that research. There was no guessing involved whatsoever. In fact, the word "guess" appears nowhere in that article.

My primary concern in setting the record straight extends to the broader issue identified by Byrne (2009): the need for us to agree upon the particular behaviors and properties necessary to define operationally various forms of justice in practice. I agree that is the ideal we seek ultimately, but I wonder if this is either practical or wise today. Because the field of organizational justice is reaching maturity, scientists are now only beginning to coalesce around the meaning of key justice concepts (Colquitt, Greenberg, & Scott, 2005). And because practice follows theory in our world a lag in standardizing practice protocol is to be expected. Furthermore, because so little systematic applied justice research has been conducted to date it is unrealistic to expect scientists to agree about how to put certain concepts into action. We simply lack the experience to know what's best.

Finally, it may be argued that the preliminary state of our knowledge about putting justice principles into practice makes it premature to settle into any single way of operationalizing our constructs. I fear that scientist–practitioners may become prematurely comfortable with the practice of using certain protocols without question simply because these already have been established, overlooking their flaws. Just as reliability means little without validity, agreement about how to manipulate variables may be misleading (and make things worse) if we're doing it incorrectly and perseverating errors as a result. (Without meaning to open a can of worms, this is analogous to the practice of using various questionnaires to tap constructs because

they have been widely used, even if they may be less than completely appropriate in certain instances.) Specifically, my concern is that premature standardization risks freezing critical thinking by making it convenient to reuse existing protocols. If those standards are flawed, then agreement comes at too high a cost.

Ultimately, though, should it come to pass that we do agree on the best way to manipulate constructs in keeping with theory, Byrne's (2009) plea for consistency in practice should be heeded. Until then, I think it's best to think through (i.e., not guess at) and specify the conceptual bases for manipulating variables in practice to ensure their appropriateness. After all, this is the same matter of internal validity over which we fuss (and properly so!) in laboratory experiments. The internal validity of our manipulations (i.e., interventions, here) in field experiments and quasi-experiments is at least as important, if not more so.

There's Nothing So Valuable as a Good Interpretation of Lewin

When Lewin (1951/1997) proclaimed "there is nothing so practical as a good theory" (p. 288), his message was that "theorists should strive to create theories that can be used to solve social or practical problems, and practitioners and researchers in applied psychology should make use of available scientific theory" (Vansteenkiste & Sheldon, 2006, p. 63). In other words, Lewin advocated bridging the scientist-practitioner gap, adumbrating a philosophy that has become the hallmark of contemporary applied psychology (Gelso, 2006).

Rupp and Aquino (2009) note correctly that I agree with Lewin's sentiment. However, I disagree with Rupp and Aquino about how to derive practical utility in our work. I argued that it is most useful to conduct research designed to test the impact of justice interventions in organizations and that this work should be guided by theory. Rupp and Aquino, however, take a broader approach. They are satisfied to consider as

applied research theory-based studies that have implications for application, the type that is most prevalent in the justice literature (Bauer et al., 2009).

Specifically, they claim that research on Cropanzano's multifoci approach to justice (Cropanzano, Rupp, Mohler, & Schminke, 2001) and his "deonance," or morality-focused approach (Cropanzano, Goldman, & Folger, 2003) has applied value. Ultimately, this may be so, but applied potential is insufficient as a basis for claiming applied value. To date, that applied value has yet to be established. What research inspired by these approaches has offered, however, are promising points of departure that beg to be taken to the next step (as I outline in Greenberg & Lind, 2000). By claiming that these approaches "could also offer more systematic guidance for managers about how organizations can structure themselves to promote fairness" (p. 205, emphasis mine), it appears that Rupp and Aquino (2009) are mistaking *implied psychology* (which uses "could" and "can") for *applied psychology* (which uses "does" and "do") (cf. Landy, 2008). And this, I'm afraid, was not the focus of Lewin's (1951/1997) dictum.

Barclay et al. (2009) also invoked Lewin (1951/1997) as a basis for challenging my ideas. But instead of recognizing my agreement with Lewin, however, they claim—quite incorrectly—that I levied an "attack on theory" (p. 203) and they cite as evidence my remark, "if studying theory is good, then studying application is bad" (Greenberg, 2009a, p. 190). I am sorry that Barclay et al. mistook this statement of the "anti-application corollary (to a focus on theory)" (Greenberg, 2009a, p. 190) as the sentiment I was endorsing instead of the one I was challenging. Indeed, I point them to the sentence that immediately precedes the one in question: "I surely count myself as among those acknowledging the value of this orientation (a strong focus on theory), but I am concerned about what has emerged as its anti-application corollary" (Greenberg, 2009a, pp. 180–190). Having clarified this, Barclay et al. were indeed

correct in noting that I “cannot be serious” (p. xx) about this anti-application bias. Indeed, I never espoused it.

With respect to the existence of such an anti-application bias in our field’s journals, Barclay et al. (2009) and I also disagree. I find this surprising because I’m sure they are well aware that editors of top I–O psychology journals require that the articles accepted for publication make theoretical advances. The research also may—and hopefully, will—have applied value, but unless it also has theoretical value, it may not be published in one of those outlets. I would speculate that with other characteristics being equally positive, an article reporting research that makes a strong theoretical contribution but has limited applied value stands a better chance of being accepted in one of our field’s most prestigious journals than an article that makes a limited theoretical contribution but has strong applied value. I realize, of course, that there are other journals in which this bias might not apply, but such “practitioner journals” are not held in the same high esteem in the academy as theoretically oriented, “scholarly journals.” This is the anti-application bias of which I speak and I still contend, given our rational nature as scientists, it plays a key role in determining the nature of the research we do.

At the risk of continuing to “protest too much” (of which I was accused by Barclay et al., 2009), I advocate that instead of convincing ourselves that most of our justice research really is more applied than I claim, we would be better off conducting theory-based applications—thereby moving from potentially applied research to actually applied research. As Calder, Phillips, and Tybout (1981) note, what makes theoretical research *potentially* applied is the fact that it lays out arguments for application that subsequently may be tested (I apply this approach to justice research in Greenberg & Lind, 2000). These tests are required to establish the *actual applicability* of a theory and it is these that are lacking and for which I am calling.

Doing this would be in keeping with Lopez’s (2009) suggestion about the kind of research that would have the greatest value to practitioners. Does primarily theory-oriented research have immediate value to practitioners? I doubt it. Eventually, it may have value but only if the practical utility of that work is established empirically. And at that point, it’s not only practitioners who find value in the research but theoreticians as well. Until then, I think there is strong value in continuing to conduct the implication studies that have gained hegemony in our field (Bauer et al., 2009), but that we also should add to our repertoire by conducting applied studies as well.

Promising Suggestions for Promoting Applied Justice Research

I was pleased to see within the commentary articles several recommendations for promoting justice research. I now highlight and comment on the most promising ones.

Cooperation Between Researchers and Practitioners

Lopez (2009) offered the interesting recommendation that academicians and practitioners join forces in conducting applied justice research. Specifically, she advises scholars to help managers understand the benefits of research and managers to help scholars understand the practical problems of greatest concern to them. I agree that such collaboration promises to be mutually beneficial although I suspect that this will not come naturally to either party. Among academic researchers, reluctance may stem from the issues I raised in my focal article (Greenberg, 2009a), particularly the general bias against applied research. Among practitioners, Lopez (2009) explains that practical constraints and a lack of awareness of justice concepts keep them from engaging in (or also, presumably, contracting for) research.

I understand and appreciate these powerful constraints, but I believe that we can identify possible workarounds by looking

at how scientists and practitioners work in the more mature field of medicine. At one professional extreme, there are medical researchers who don't see patients and at the other there are clinicians, practitioners who generally don't conduct research. Yet, some clinicians may be inclined to submit case studies to journals when they have particularly valuable incidents to report. In general, there is greater professional incentive for clinicians to publish case studies in their fields' journals than there is for managers to publish case studies in their fields' journals. In fact, given the proprietary nature of many managerial practices (which is not surprising given the highly competitive nature of business), there may be strong disincentives for managers to be equally forthcoming with their successful experiences. This is unfortunate insofar as it denies us the benefits of what they have learned.

A model used in the field of medicine points to a potential mechanism for allowing greater public dissemination of managerial efforts. Between the researcher and clinician extremes, there is another group of professionals, clinical researchers, who function at the interface by seeing patients and conducting research. Usually working at university-affiliated medical centers, such individuals are well situated to combining the best features of both worlds by conducting research on issues confronted while seeing patients and then applying what they have learned from these efforts when treating other patients. If this sounds familiar, it's likely because this arrangement formalizes our highly vaunted scientist-practitioner model.

Although clinical researcher positions are prescribed jobs among some medical school faculty, only a few I-O psychologists follow this approach and in most cases they probably have done so by blazing paths for themselves. Given the considerable good that clinical researchers do in the field of medicine, I see merit in establishing similar formal arrangements among I-O scientists and practitioners. This would not only help us conduct research that establishes effective ways of promoting organizational

justice but also to realize the many tangible benefits to individuals and organizations our field has to offer.

Comprehensive, Integrated Interventions

I was pleased to see Rupp and Aquino's (2009) call for comprehensive interventions that are integrated into human resources systems. Although implementing these would be challenging, I agree that there is considerable merit in conducting research that focuses on justice as both a core competency (a macro-approach) and a leadership competency (a micro-approach), as it may pertain to such essential human resource processes as recruitment, selection, training, and the like. Implementing such a shift to broader, macro-level issues would be no small feat. Yet, efforts in this regard would be worthwhile given the potential benefit: allowing us to focus on issues of justice that cut across organizational systems (e.g., a gain in justice in one area may be offset by injustices in other areas).

Illustrating such an integrative approach, Rupp and Aquino (2009) note the managerial development program of Rupp, Baldwin, and Bashshur (2005), in which justice was treated as a leadership competency. Using behavioral simulation exercises as indicators, they found positive post-feedback changes in justice behavior. As in other programs of this type, it's possible that changes found during training might not be observed on the job. Additionally, the complex nature of the development protocol precludes identifying the unique effects of justice relative to other managerial practices.

Such concerns go with the territory, however, and may be expected whenever complex development programs are employed. They should not deter us, however. I agree with Rupp and Aquino (2009) that there is merit in implementing and systematically studying additional macro-level programs of this type.

I would add that these should be supplemented by micro-level investigations that shed light on the particular development

tools that are most effective. All along the way, these studies should be guided carefully by the theoretically based literature on organizational justice. It is because Rupp et al. (2005) did precisely this that their work serves as a good example for future researchers to follow.

Conclusion

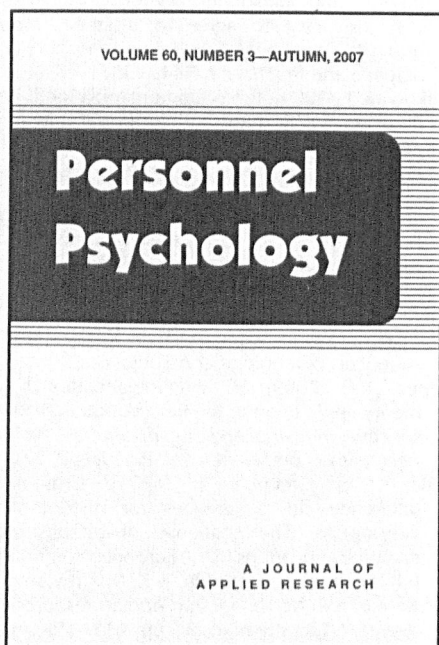
Almost a decade has passed since I first noted that research on organizational justice has focused more on explaining organizational justice than on applying it (Greenberg, 2001). I repeat this claim now, noting that only a handful of studies directly testing the efficacy of organizational justice applications have appeared in our field's top journals in the years between my observations. Knowledge must precede practice, of course, and I am convinced that sufficiently well-established principles of organizational justice exist (Greenberg, 2009b) to justify advancing to practice.

Despite some contentiousness in places, the dialogue that unraveled on these pages reflects all parties' mutual commitment to promoting organizational justice. We may disagree about the specific nature and form of these efforts to date, but we agree strongly that attention to promoting organizational justice, in one way or another, is valuable and eminently worthy of our attention as scientists and practitioners. I hope that the passion of our commitment will encourage readers to focus on the applied potential of organizational justice in their own future research.

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