

Editorial

The *Journal of Applied Psychology* is the oldest and largest top-tier journal publishing theory and research relevant to industrial and organizational psychology, organizational behavior, and human resources management. It has been a source of scholarship in applied psychology for nearly a century (the first volume published in 1917), and it is the largest journal in these fields in terms of published articles (138 in 2007) and pages (1,800 in 2007). It is also among the most influential of journals in these areas, garnering the most citations (11,182 in 2007) and a high impact factor (a useful but controversial indicator of journal quality; Brumback, 2008). By any reasonable metric, the *Journal of Applied Psychology* is a recognized, respected, and influential source of scholarship for the organizational sciences.

My editorial team began receiving manuscripts on January 1, 2008, and I project (as I write this editorial in October 2008) that we will have received close to 900 manuscript submissions by the end of the year. I am fortunate to have enlisted the assistance of 10 exceptional associate editors who represent a wide range of expertise around the globe (Tammy D. Allen, *University of South Florida*; Neil R. Anderson, *University of Amsterdam*; Gilad Chen, *University of Maryland*; David V. Day, *University of Western Australia*; Richard P. DeShon, *Michigan State University*; Sharon K. Parker, *University of Sheffield*; Robert E. Ployhart, *University of South Carolina*; Quinetta M. Roberson, *Villanova University*; Eduardo Salas, *University of Central Florida*; and Jing Zhou, *Rice University*). With the exception of Richard P. DeShon, who joined our team in August, all have been reviewing manuscripts since the start of our operation. Manuscript submissions, reviews, and revisions are ably coordinated by my editorial assistant, Jennifer Wood, via the Journal Back Office (JBO) system. Reviews are conducted by our board of consulting editors (150 members to start and growing to 200 by January 2009) and many issue consultants (i.e., ad hoc reviewers), who represent a wide range of diverse expertise.

Before taking on the position of editor, I served as an ad hoc reviewer for many years, a consulting editor for nearly a decade, and an associate editor (under the editorship of Shelly Zedeck) for the last 6 years. That is a large span of a career, and one does not accumulate that much experience without developing some definite ideas about the journal. My editorial team is also highly experienced, and together we have developed plans for what we would like the journal to publish and how we would like to see it evolve as it continues to grow in size and influence. In this editorial, I would like to share with you our vision of (a) the mission and scope of the journal, (b) our expectations for quality, and (c) the nature of the review process we would like to enact.

The Mission and Scope of the *Journal of Applied Psychology*

The primary emphasis of the *Journal of Applied Psychology* is the publication of original investigations that advance theoretical understanding and create new knowledge for applied psychology¹

within the broad scope of the organizational sciences. We are primarily interested in publishing empirical research and conceptual articles that enhance understanding of psychological phenomena in human and organizational systems. Phenomena of interest include the interplay of cognition, motivation, affect, emotion, and action with respect to behavior and performance in work and organizational contexts. We are interested in investigations that consider one or, even better, cut across multiple levels—individuals, groups, and organizations—of organizational systems. Most of the theory and research published in the *Journal of Applied Psychology* addresses the micro (i.e., individual) and meso (i.e., teams and groups) levels of organizational systems. However, we are interested in macro research (i.e., higher level units and organizations) as long as it contributes to an understanding of psychological phenomena (e.g., Van Iddekinge et al., in press). Most of the research the journal publishes will likely be grounded in a single cultural context, but we seek work that develops theory and examines its boundaries, generality, and contingencies across different cultural settings. We are primarily interested in theory and research with application potential in contexts such as business, education, training, health, service, government, or military institutions (public or private sector; profit or nonprofit).

We are interested in publishing empirical research, theory development and conceptual analyses, and descriptive research that advances psychological understanding. First, the touchstone of the reputation of the *Journal of Applied Psychology* is its emphasis on publishing theoretically driven and rigorously conducted empirical investigations that extend conceptual understanding in applied psychology. We are interested in investigations that generate original data to create new knowledge. Research can be conducted in the laboratory or field; selection of a research setting should be commensurate with the intended contribution and state of the literature. Laboratory investigations are better for demonstrating that an effect can occur and for examining a phenomenon with precision. Field settings are better suited for showing that an effect does occur and for building evidence that a lab phenomenon generalizes. Cross-sectional, single-source, survey-based studies are not encouraged. We are also very receptive to meta-analyses that summarize what is known from the research base in an area of inquiry and that identifies boundary conditions and directions where new research is needed. We are open to methodological articles, as long as they provide a clear conceptual contribution to research in applied psychology (i.e., new techniques or method demonstrations that do not make a conceptual contribution are more appropriate for *Psychological Methods*). This has been the tradition of the *Journal of Applied Psychology* and it will continue to be our foundation.

Second, although empirical research is the primary emphasis of the journal, we strongly encourage conceptual submissions. Good

¹ Other than clinical and applied experimental or human factors, for which there are more appropriate American Psychological Association journals.

theory synthesizes literature, extends or creates new theories of psychological phenomena, and stimulates new lines of research. The *Journal of Applied Psychology* has a long history of publishing theoretical monographs. Monographs are broad and deep meta-theoretical treatments of a topic area. They develop new theories that push research forward in an area of inquiry (e.g., Kanfer & Ackerman, 1989). Monographs can also synthesize research to develop new perspectives that open new avenues of research (e.g., Kraiger, Ford, & Salas, 1993). Or, monographs may represent the “systematic accumulation, analysis, and reflective interpretation of the full body of relevant empirical evidence related to a question” (Rousseau, Manning, & Denyer, 2008, p. 475). Good examples of this sort of monograph can be found in any issue of *Psychological Science in the Public Interest*. Monographs have been very influential publications for shaping research. Although the journal has not published a monograph in over a decade (meta-analyses, competing journals, and high-quality book series are deflecting such submissions, possibly), we are very supportive of monographs.

In addition to monographs, we are also very receptive to tighter and more focused theory development efforts. The prior editorial team encouraged submissions for a special section on *Theoretical Models and Conceptual Analysis* that are consistent with this more focused conceptual contribution (see Volume 89, Issue 6 in 2004 and Volume 90, Issue 6 in 2005). Klein and Zedeck (2004) provided a clear and succinct discussion of what constitutes good theory. A good theoretical contribution integrates the literature, providing insights about commonalities that can be exploited in future research. It identifies areas of disagreement and contradiction, finding ways to bridge them or pointing toward ways to competitively test them. It synthesizes different domains, creating novel predictions from their combination. A good theory explains and establishes principles for how a phenomenon works. It then shapes research as investigators translate principles or propositions into hypotheses that are examined in a particular context. A good theory generates many such research opportunities. It is not tested all at once. Rather, it takes an accumulation of evidence across many evaluations to map support for the theory. A good theory generates many midrange theories (Weick, 1995) that can be evaluated with data.

Note that we are not interested in extended reviews of the literature that do not advance understanding. We are also not interested in what I would describe as small theory or, essentially, highly focused models without data. Many times a theoretical submission posits a “boxes and arrows” model and derives a set of propositions from it. Just having boxes and arrows does not constitute a good theory (Sutton & Staw, 1995). To the extent that the propositions translate directly into hypotheses if the model is contextualized, our consulting editors have typically responded that the author should collect the necessary data to test the model. A good conceptual contribution has to go a step beyond.

Third, we are receptive to descriptive research on applied psychological phenomena for which the literature lacks basic knowledge that will provide a foundation for building new knowledge and theory (see Hambrick, 2007). This does not mean we are receptive to any kind of descriptive research, much of which can be trivial. Let me also emphasize that good descriptive research will have to pay very careful attention to explicating a replicable methodology for the descriptive data that are reported. Descriptive

research of interest should be directed at providing data on important and unknown phenomena, particularly answering those questions for which theory alone cannot (or is very unlikely to) yield solutions. For example, the literature has very little knowledge to offer regarding the time scales for a variety of phenomena that evolve and emerge over time. Theory alone is very unlikely to guide researchers in determining the time frames for phases of team development (and the contingencies that might influence them). Gersick (1988, 1989) generated descriptive data that guided theory building for the temporal developmental phases for one type of work teams—project teams. It is a start. Team development time lines for other types of teams still need to be mapped. Newcomer socialization is another research area where researchers can gain insights on temporal pacing with descriptive data. Much socialization research is longitudinal, but the time scale for changes that occur in the newcomer during the process has not been well researched. There is much longitudinal research on socialization, but researchers are guessing at the time line. In addition to time scales for linear developmental processes, phenomena can also vary considerably over time (e.g., dynamics of affect, performance, or other behaviors). These examples relate to my interests and are merely meant to be illustrative, not to define the limits of what we will consider. We are receptive to descriptive research that helps to elaborate important temporal phenomena. Finally, we are interested in the discovery and documentation of new, important, and meaningful phenomena that are not known, are poorly understood, or are newly emergent. For example, some observers suggest that the latest generation of employees entering the workforce have a distinctly different perspective on the importance of work and career relative to prior generations. If so, that has implications that can cascade through many areas of research. We need some good descriptive data to define the profile of this new generation of workers. Finally, good descriptive data will often (though not always) necessitate rigorously conducted qualitative research on phenomena that are difficult to capture with quantitative methods. A recently published article on team conflict and conflict resolution strategies described qualitative methods to generate the primary data and quantitative analyses to structure and interpret it as a basis for developing a conceptual framework (Behfar, Peterson, Mannix, & Trochim, 2008). It is a good illustration of how useful descriptive data can be for advancing a theoretical contribution.

Expectations for Manuscripts

This is not intended to be an exhaustive tutorial covering everything that we consider in the evaluation of a manuscript for publication. However, since I assumed the role of incoming editor, I estimate that I (and, on occasion, the associate editors) have desk rejected approximately 10%–15% of manuscript submissions to the journal. This suggests a lack of calibration on the part of some authors with respect to our expectations. I want to highlight some basic considerations in an effort to sensitize authors to issues that are critical if a manuscript is to have a reasonable prospect to be published. First and foremost is unique theoretical contribution. How your manuscript builds on and extends theory has to be explicit, clear, and stated early on in the manuscript. Most manuscripts are rejected for failing to meet this standard. If there is no theory, it is poorly developed, it is solely based on prior empirical

findings (but with no rationale), or it lacks a coherent rationale, then the manuscript is challenged. If the relations under investigation have already been substantially examined, that limits the conceptual contribution. If “no one has examined this before,” there may be good reasons for that; yet you still need to develop a unique theoretical contribution. Second, hypotheses have to be derived, and they have to be consistent with the rationale. Remarkably, it is often the case that hypotheses are disconnected from or inconsistent with the theoretical rationale. Third, the methodology has to be appropriate for evaluating the hypotheses and it has to be rigorous. Aside from a theoretical contribution, this is the other area where many manuscripts crash and burn. Most desk-rejected manuscripts (aside from posing a trivial research question) are single-shot, cross-sectional, self-report survey designs. There are very rare occasions where such a design may be warranted and defensible. In such situations, authors need to make every effort to address the concerns of common source variance (see Podsakoff, MacKenzie, Podsakoff, & Lee, 2003). Fourth, analyses have to be commensurate with the hypotheses and data. It is not just a matter of using the latest and greatest technique. Simple analyses can suffice so long as they are effective for evaluating the hypotheses. The effect sizes or confidence intervals (where warranted) should be reported. Finally, a good discussion goes beyond restating the findings. It explores implications for theory extension, additional research directions, and carefully calibrated suggestions for practice. A good discussion also sticks close to the data and does not go off on wild speculation.

In addition to these basics, there are a few considerations to which this editorial team will pay special attention: context, task, levels, and time. I have addressed these issues in detail elsewhere (Kozlowski & Bell, 2003; Kozlowski & Ilgen, 2006; Kozlowski & Klein, 2000) and will simply highlight their importance here. First, most phenomena of interest transcend many settings or contexts, but most research is grounded in a particular setting; context is a constant. It is a selection of one context among many. Clearly, this sets limits on generalizability. Thus, it is important that even if investigators do not vary the context or measure it directly that they do provide sufficient descriptive information about it so its role as a boundary condition can be considered conceptually and in to-be-conducted meta-analyses. Second, what people are doing, the task they are performing, whether in the lab or field, defines some of the most salient and potent features of their proximal experience. Much like the context, whether the task is central to the research question or not, it needs to be carefully considered and described well (especially if it is a constant) so that readers can classify it in a task typology (e.g., Thompson, 1967; Van de Ven, Delbecq, & Koenig, 1976). Third, if the context or task is varied (deliberately) or sampled broadly, then it becomes a level of analysis and should be incorporated substantively (if possible) in the theory and evaluation. I am not suggesting that all research we publish has to encompass more than one level of conceptualization and analysis. However, I am suggesting that it is desirable for it to do so. Multilevel theory, methods, and analyses have developed substantially (Kozlowski & Klein, 2000). They provide a foundation for developing more integrative, effective, and useful theories, and I would like research published in this journal to continue to push that envelope. Finally, in addition to incorporating multiple levels of the organizational system (individual, team, organization), the other important theoretical and methodological frontier

for research is incorporating time in theory and research. Time is a level. We have many, many theories that are process based but studied almost exclusively with cross-sectional designs that—while suggestive—cannot adequately evaluate such theories. Advancing theories that address the dynamics of how important phenomena emerge, evolve, and change over time is the next frontier. We encourage theory and research that explore this frontier.

Review Process

I examine all manuscripts submitted to the journal for appropriateness of the topic, research methods, and basic standards of rigor. I then assign the manuscript to the member of the editorial team (one of the associate editors or me as action editor) on the basis of the best expertise match. The action editor selects reviewers on the basis of expertise for the topic and methods, and the manuscript is sent for review. When reviews are returned, the action editor makes a determination based on her or his evaluation of the manuscript, the substance of the reviews, and reviewer recommendations; reviewers advise, the action editor decides. This process typically takes about 6–8 weeks to complete. Manuscripts that show potential will usually go through one or more revisions. Let me emphasize to authors that should you receive a decision letter that offers the possibility of revision—no matter how remote a possibility success may seem—it is an opportunity to revise. Action editors strive to be as explicit and realistic regarding what issues need to be resolved as possible so authors can make an appropriate decision. The review process we seek to enact is intended to be a dialogue among scholars. We strive to orchestrate a respectful and rational exchange that is intended to sharpen and improve the manuscript under consideration. Manuscripts that succeed are responsive to the revision requests and are clear and explicit in how they did so. Revisions that are not responsive to points highlighted by the action editor and the reviewers, are ambiguous about how they changed, or that mostly attempt to argue away the criticisms are not likely to succeed.

To close, my editorial team and I are excited to be officially taking the helm of the *Journal of Applied Psychology*. I owe special thanks to Shelly Zedeck, who provided the associate editors under his tenure with an excellent model of collaborative and empowering leadership—a model I have been striving to emulate. We believe that we can contribute to the increasing reputation, growth, and breadth of the journal. But we can only help guide the process. We need you to make this happen. We need authors to do their best work and to submit it to the *Journal of Applied Psychology*. We need reviewers to be incisive and constructive. We need the collective help of the community of scholars in industrial and organizational psychology, organizational behavior, and human resources management to succeed. Join us in continuing to build the *Journal of Applied Psychology*.—Steve W. J. Kozlowski, Editor.

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