

What does a great meta-analysis look like?

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In the first issue of *Organizational Psychology Review*, Daan van Knippenberg stated that I would be elaborating on the issue of what makes a good meta-analysis (van Knippenberg, 2011). Given this call to action (or one could say burden of responsibility), I have taken it upon myself to pull together some thoughts on what I believe authors should be striving for in producing meta-analyses, as well as trying to provide a roadmap for authors who are considering submitting a meta-analysis to *OPR*. What I will not be doing is providing a treatise on the mechanics of conducting a meta-analysis, as there are a multitude of books, articles, and other training materials that do a better job than I could ever hope to do.

Meta-analyses: Not your grandfather's review papers

Whenever I read a new article employing meta-analytic techniques, I am amazed at the whiz-bang gizmos and the newfangled statistical processes being employed. This reminds me in many ways of watching movies that are

promoted as having the best, most realistic special effects *ever seen in the history of the world!* My purpose in watching a movie is not to be awed by what a group of people were able to create on a computer, but to instead enjoy the story. If I notice the special effects, my belief is that the director is not doing his/her job. The same goes for a meta-analysis: the purpose of a meta-analysis is not to promote the cool new techniques you used, but to tell a story. As an aside, this applies to any paper that involves statistics, not just meta-analyses.

What authors (and reviewers) often forget is that a meta-analysis is at its core a review paper. When Glass (1976) first introduced the “analysis of analyses,” he was trying to address an explosion on research in the education field. He believed that scholars were failing to integrate all of this research in traditional qualitative reviews, which had fallen into a rut of producing “verbal synopses of studies [that] are strung out in dizzying lists” (p. 4). He put this rather succinctly:

We face an abundance of information. Our problem is to find the knowledge in the

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information. We need methods for the orderly summarization of studies so that knowledge can be extracted from the myriad individual researches.

This simple perspective on the problems in the field, coupled with the statistical advances most notably pioneered by Jack Hunter and Frank Schmidt (e.g., Hunter & Schmidt, 1990) led to the evolution of what is one of the most popular routes to publication in the field of organizational psychology. Many of the early publications utilizing meta-analyses were both extremely important from an applied (e.g., Hunter & Hunter's [1984] work on cognitive ability for selection) and research perspective (e.g., Barrick & Mount's [1991] work was the push needed to revitalize personality in the organizational psychology literature). Yet, it seems that the initial goal of extracting knowledge from a large literature was lost in the rush to produce more publications; today, more and more meta-analyses are narrow in scope and size, as the large, obvious, and highly studied areas of organizational psychology have already been meta-analyzed (sometimes many times).

What's a great review?

Looking back on the classic meta-analyses can give insight into what we should strive for in the field when producing meta-analyses. One obvious statement that I can make is for researchers to conduct a meta-analysis on a topic that is studied heavily, but has yet to be analyzed. Good luck with that endeavor. Some of those topic areas still exist, but they are disappearing quickly. If you already know the big idea the literature is missing, stop reading this and get to producing those papers before someone beats you to it!

Are you still here? Ok, good. That means I am not just typing this sentence so that the other editors and I can read it. Let us take a step back and think about what the rest of us can do to have an impact utilizing meta-analyses.

Let me answer this question by first posing two other questions: what are the great qualitative review papers, and why did you like them so much? For me, great qualitative review papers are great because they do not just review the literature. They organize it, they frame it, and they provide a roadmap for the future.

Consider the review of the social-support literature by Cohen and Wills (1985). This article is one of the 10 most highly cited papers in the history of *Psychological Bulletin* (with over 3,000 citations), and the most highly cited non-methodological article in that journal (with almost 25% more citations than the next review article). Clearly, this is an article that has had an impact on the field. What did they do right?

It is clear that they tackled a subject that was popular. However, many authors were investigating the topic at that time, and it is unlikely that this was the only review on social support published (either in journals or book chapters) around that time period. What is more striking is that this article looked backwards and forwards in addressing the literature. They started by identifying the two major (conflicting) theories in the social-support literature (i.e., social support either affects well-being directly, or by buffering against the harmful effect of stress). They continued by documenting methodological issues that could affect the relationships. They then moved into the specific review, documenting the support for (and against) each model. They concluded this paper by moving through two sections.

First, they discussed the issues that were clarified through the specific article (i.e., what we now know as a function of them doing this work). This was not highlighted by a statement such as "social support is important." Instead, they really focused on what the field should know about social support. Second, they focused on where the field should go as a function of the conclusions reached in their review. We have all read (and perhaps produced ourselves) articles in which authors state boilerplate future research such as "pay

attention to context” or “do more longitudinal research.” Those statements do not provide insight into literature in a meaningful way. Instead, Cohen and Wills (1985) focused on bigger issues (e.g., whether social support or an alternative unmeasured variable were driving results; how social support works; the relationship between social support and serious health outcomes). Beyond this, though, was that they posed some solutions to the questions they raised. For example, to their question about “How are perceptions of support formed and maintained,” they suggested that scholars integrate ideas from “social exchange theory, coping theory, and formulations of interpersonal relationships” (p. 352). This is a departure from many articles that metaphorically throw a multitude of ideas at the wall and see what sticks (thereby drumming up citations). Instead, these authors actually thought through the processes involved and helped launch research streams (rather than just label future research streams). In the end, this review spurred a large volume of research on social support, prompting a series of subsequent quantitative reviews (e.g., Chiaburu & Harrison, 2008; Halbesleben, 2006).

What should I be doing?

This moves us to the question of what you should be focusing on while preparing your manuscript. I cannot tell you there is a single correct way to write a meta-analysis, but I will pose several questions that you can think about while conceptualizing your paper.

1. What is (are) the existing theoretical framework(s) most prevalent in this research space? If there is more than one, how do they fit with each other? Are there places where they would make different predictions? What do the data say about the support for these theories? If there is no dominant theoretical model, why not?
2. How can you organize/make sense of existing research? If you could draw a model depicting the most salient issues in this research space, what would it look like? If you could present a 2x2 (or similar) of the issues relevant to this research space, can you fit all of the research into those cells? Are any cells left unfilled?
3. What are the next steps for this research space? If you can pose questions, can you pose potential solutions?

Where have we been? To the first question, you need to put your manuscript into perspective. You are unlikely to be reviewing a literature that has a sum total of three empirical papers, as there is no reason for a meta-analysis at that point. Given that there is a large amount of research on your area of interest, you need to consider what have been the motivating factors behind this research. Reputable journals in psychology, management, and sociology require a theoretical model for nearly all published research—what model(s) have been used for your research area?

This is the first opportunity to be creative and have an impact. If you see a literature with only one theoretical perspective, you can tell us what that perspective says and test whether it is supported. If it is supported, are we done? Is there anywhere else to go with that literature? My own example of this is in the job design literature. In job design, the big theory has been the job characteristics model (Hackman & Oldham, 1980). It was popular, it was frequently tested, and it was supported (Fried & Ferris, 1987). Yet, the very fact that it was empirically supported through a well-crafted meta-analysis may actually have driven the literature “underground” for several decades. My colleagues and I found that there had been a large amount of research on job design published in the two decades following Fried and Ferris’s meta-analysis. Yet the vast majority of it was published outside of the core industrial/organizational or organizational behavior research

journals, perhaps because it did not focus on the factors raised in the job characteristics model (Humphrey, Nahrgang, & Morgeson, 2007). Understanding that there was another way to conceptualize job design helped us make sense of this literature, as well as helped us frame the discussion about where we expect the literature to move in the future.

How do I make sense of this literature?

The second set of questions encourages you to create a framework for understanding the literature. Consider the recent publication by Rupp (2011) where she tackled the organizational-justice literature (a literature with a volume of publications that rivals nearly all other organizational behavior topics). One major contribution from her study was that she organized the organizational-justice literature into three categories: looking in, looking around, and looking out. What is particularly striking about this framework (beyond its intuitive appeal) is that it highlights that the preponderance of research in organizational justice has focused on the “looking in” category (i.e., answering the question: “how am I treated?”), with some research on the question of how we are treated (looking around) and almost no research on how *others* are treated (looking out). From the perspective of a literature review, this research shows where we have been, but also highlights where we need to go—clearly, we need more research that is not so self-centered in orientation.

Where are we going?

The third set of questions focuses on the future of the literature. Too often we are focused on our own research and not enough on where we situate our research in context. A great review acknowledges the limitations of existing research and provides a significant amount of space to where the literature should proceed. The subtlety here is that great papers do not

objectively speculate on anything that could happen in the future. Great research poses questions and considers potential answers based on an intimate knowledge of the research space. In many ways, this is the strength of the Cohen and Wills’ (1985) article, and is the hallmark of future-leaning, cutting-edge review papers.

What should I be avoiding?

With all of this focus on what you should do, I want to devote a small amount of space to what you should avoid. First, we are not looking for meta-analyses that just provide effect sizes without providing perspective. You need to situate your paper in the research context, and interpret findings in a meaningful way. All too often I read a manuscript that utilizes meta-analytic techniques but fails to interpret what its findings actually mean for the literature (seemingly suggesting that the effect sizes themselves were the primary reason for the review). As an author reviewing a literature, you likely know that literature as well as anyone else. Tell us something we did not know. Tell us what your results mean. Give us guidance.

Second, we are not looking for meta-analyses that are simply empirical papers with larger datasets. To paraphrase (and twist) a statement from van Knippenberg’s (2011) editorial, if your meta-analysis is a single empirical test, there is more value in publishing it in an empirical journal. At *OPR*, we want to publish big questions and big ideas that will affect numerous subsequent publications.

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