Industrial and Organizational Psychology, *10*(3), pp 457–501 September 2017.
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Commentaries

Treating Uncertainty in Meta-Analytic Results

Michael T. Brannick
University of South Florida

Tett, Hundley, and Christiansen (2017) describe two main sources of uncertainty that are usually reported in a meta-analysis: uncertainty about the value of the underlying mean correlation (which they describe as SE(r_{xy})) and uncertainty about the individual values of rho that arise from the random-effects variance (the square root of which they describe as SD(rho)). They proceed to recommend descriptions of small, medium, and large values of each uncertainty that meta-analysts should report and consider for interpretation. However, there exists a simpler solution to expressing and interpreting such uncertainty.

The prediction interval (Borenstein, Hedges, Higgins, & Rothstein, 2009) is analogous to the credibility interval but incorporates uncertainty about both the mean and the variance of the underlying distribution of rho. Borenstein et al.'s (2009, p. 133) formula is:

$$PI = M^* \pm t\sqrt{T^2 + V_M}$$

where M* is the summary estimate (overall mean), *t* refers to the *t* distribution (with *df* = number of effect sizes less two) rather than the unit normal because the underlying variance is estimated, the term under the radical on the left refers to the random-effects variance (estimated Var(rho)), and the final term refers to the variance of the mean. Reporting the prediction interval communicates a plausible range in which population correlations are expected to be found based on the current evidence. Such a presentation is easier to grasp than are cutoffs for standard deviations or standard errors. This is not to say that information about the confidence interval for the mean and the size of the random effects variance is unimportant. The prediction interval just expresses such quantities in a single summary that is more easily interpreted.

Michael T. Brannick, Psychology Department, University of South Florida.
Correspondence concerning this article should be addressed to Michael T. Brannick, Psychology Department, PCD 4118G, University of South Florida, Tampa, FL 33620. E-mail: mbrannic@usf.edu

https://doi.org/10.1017/iop.2017.40 Published online by Cambridge University Press

However, in my opinion, this is not the main issue with the typical presentation of meta-analysis results. Tett et al. (2017) nicely described typical meta-analyses in I-O psychology. Meta-analysts commonly compute and report credibility intervals, and then ignore them. It has been my experience that people find the credibility interval difficult to understand; they interpret it the same way as they do a confidence interval (Whitener, 1990). This is analogous to a tailor measuring the waist and inseam for 1,000 people, getting a precise average size, stitching one pair of pants based on the average, and declaring that their new pants are properly one-size-fits-all through the science of Trouser Generalization. If the population random-effects variance is greater than zero, then the data are consistent with the notion that the population correlations really are different in different places, just as some people are taller than others (and therefore need longer trousers).

Paterson, Harms, Steel, and Crede (2016) reviewed over 250 meta-analyses in the organizational sciences. They report an approximate average rho of .28 and an approximate average SD(rho) of .14 (cf. Oswald & McCloy's [2003] "large" SD(rho) value of .125). If a distribution is normal, then going up and down about 2 standard deviations will cover about 95% of the cases. Based on the Paterson et al.'s (2016) data, on average, after a meta-analysis, we can be confident that the true correlation in any local setting is between 0 and .56. Did we learn much from the meta-analysis? That interval is over one-quarter of the possible range of the correlation coefficient; I suggest that there is a great deal of uncertainty remaining after the typical meta-analysis is completed.

The main difficulty is that the random-effects variance is problematic, and it is largely ignored in our reports; at a minimum, we should interpret it. The prediction interval is a start. A large SD(rho) warns that our understanding of the phenomenon is shallow. A large SD(rho) should serve as a trigger for developing models that explain the impact of different measures, demographics of participants, work content, organization type, cultural context, and so forth. By explaining the variation in outcomes, SD(rho) can be reduced to the point where it is trivial. Such modeling of effect sizes is difficult because of the large number of potential moderators compared to the small numbers of studies (Schmidt & Hunter, 2015). However, such modeling is possible, and it is essential if we are to claim an understanding rather than a simple description of organizational phenomena.

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"Life Doesn't Happen at the Between-Person Level," or a Cautionary Note on Generating Scientific Inferences Through Meta-Analyses

Hannah L. Samuelson, Jessica R. Fernandez, and James A. Grand
University of Maryland

The implicit philosophy for how research and practice in industrial and organizational (I-O) psychology has pursued inferences about our field's core phenomena has largely been based on a nomothetic, variable-based, and aggregate/"large-sample" ideal. As Tett, Hundley, and Christiansen (2017) expertly highlight, there are more insightful means for drawing inferences about the nature of such aggregate relationships based on meta-analytic techniques than the current practice in the organizational sciences. However, the motivating force behind our commentary has less to do with the issues raised by Tett et al. (2017) concerning the practice of using meta-analysis for purposes of *validity generalization* and more to do with the practice of using meta-analysis for purposes of *scientific inference*. Between-person philosophies in which the end-goal is to identify general conclusions that apply to the aggregate (cf. Hanges & Wang, 2012) have historically guided our scientific inferences and have supported the proliferation of meta-analytic techniques (including what Tett et al. describe as tertiary analyses based on such findings). These philosophies have led to a dearth of understanding at the *within-person* and *social system* levels—the levels

Hannah L. Samuelson, Department of Psychology, University of Maryland; Jessica R. Fernandez, Department of Psychology, University of Maryland; James A. Grand, Department of Psychology, University of Maryland.

Correspondence concerning this article should be addressed to Hannah L. Samuelson, Department of Psychology, 4094 Campus Dr., 3143 Biology-Psychology Building, University of Maryland, College Park, MD 20742. E-mail: hsamuels@umd.edu

https://doi.org/10.1017/iop.2017.40 Published online by Cambridge University Press