If it Were Only That Easy: The Use of Meta-Analytic Research by Organizational Scholars

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Abstract

This paper evaluates how researchers are currently citing meta-analytic results and provides specific recommendations for interpreting the information provided by meta-analysis (MA). The past four decades have seen a proliferation of MA research across the organizational sciences and myriad improvements to how MA is conducted. MAs are cited more frequently than individual primary studies and have a substantial influence on subsequent research and theorizing. Yet, the consumption of meta-analytic results in organizational scholarship remains superficial. We evaluate citation practices for four seminal MAs and find that authors predominantly interpret meta-analytic findings in the simplest way possible: as evidence of the existence of a relationship between variables. In focusing only on this basic finding, citing authors neglect the complexity and rich detail provided by MA. We offer advice for how researchers can more effectively leverage the strengths of meta-analytic findings to inform subsequent research by taking advantage of the benefits that meta-analytic methodology can provide for the explanation of organizational phenomena.

Keywords:

Meta-analysis; interpreting research results

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“It ain't what you don't know that gets you into trouble. It's what you know for sure that just ain't so.” (Often attributed to Mark Twain, but actual source unknown (Platt, 1989).)

Research in the organizational sciences advances knowledge through the cumulation of empirical studies (Schmidt & Hunter, 2015; Light & Smith, 1971). Unfortunately, empirical studies differ with respect to research setting, operationalization of constructs, and reported results. Traditionally, qualitative reviews provided a mechanism to integrate the findings of different yet related studies. However, qualitative reviews are necessarily subjective as they rely on the perspective of the author(s) to determine which studies to integrate and how to best summarize the results (Glass, 1977; Borenstein, Hedges, Higgins, & Rothstein, 2009).

In response, many researchers called for review articles to adopt a more systematic approach (Glass, 1976; Light & Smith, 1971). Combined with pressure to focus on effect size estimates as opposed to null hypothesis significance testing (NHST), these efforts culminated in the development of meta-analysis (MA) as a more objective research integration tool (Glass, 1976; Schmidt, Hunter, & Urry, 1976). Although MA still requires some subjective elements, meta-analytic methodology provides authors with the ability to combine quantitative effect size estimates across multiple studies to produce a meta-analytic effect size (MAES) estimate. Meta-analysts also have the ability to examine a relationship in a variety of settings and provide rich detail regarding the nature and boundary conditions of an effect.

Since its introduction, MA has become a popular tool for organizing and summarizing empirical studies (often called *primary studies*). MAs have influenced both theory development

and the conduct of subsequent empirical research. Aguinis, Dalton, Bosco, Pierce, and Dalton (2011) report that MAs are cited at a substantially higher rate than primary studies. Given the impact MA has on the organizational sciences, it is important for those who cite, use, or read MAs to interpret meta-analytic results appropriately.

The primary purpose of this paper is to provide a critical examination of the ways researchers cite meta-analytic results. Ours is not the first paper to examine this phenomenon. Carlson and Ji (2011) found that researchers citing MAs were more likely to reference the existence of a relationship than the magnitude of an MAES. Citing researchers rarely noted effect size heterogeneity and failed to indicate which of the reported MAESs were relevant to their research. The majority of these citations did not even indicate that they were referencing meta-analyses, citing meta-analytic evidence similarly to how they cited primary studies.

Our current investigation seeks to extend the important work of Carlson and Ji (2011) by taking a closer look at how authors reference four heavily cited MAs with particular focus on which characteristics citing authors are attending to or ignoring. Carlson and Ji's 2011 emphasis on heterogeneity and moderation is important, but we expand our scope to include other common aspects of meta-analytic methodology such as publication bias, psychometric adjustments, and confidence/credibility intervals. Each of these components of MA has important implications for accurate, meaningful interpretation of meta-analytic results. In addition to considering these additional meta-analytic methods, we examine various forms of miscitation, including misquoting or overgeneralizing an MA.

Unfortunately, we suspect (and our results will demonstrate) that Carlson and Ji's (2011) work has not resulted in sweeping changes to citation practices. Carlson and Ji provide a starting point for discussing MA citation practices, and we echo many of their suggestions. We extend

their recommendations by focusing on the consequences of miscitation of MAs and providing explicit guidance for how to accurately interpret and cite meta-analytic results. In doing so, we hope to shed new light on meta-analytic findings, enhance the interpretation of MA, and improve the field by identifying novel opportunities for the interpretation of meta-analytic results.

The Complexity of MA

The conduct of MA has evolved substantially over the past 40 years (Schmidt, 2008). When Schmidt et al. (1976) bemoaned the lack of power in organizational research, their focus on type II error implied a belief that a relationship between two constructs exists and can be identified and quantified. Their logic was that focusing on type I error is fruitless when the null hypothesis is false.¹ By assuming the existence of an effect, MA can serve as a confirmatory method for identifying the nature and magnitude of a relationship. The rationale for conducting MA is that the combined results of a series of studies should be more accurate than the results of any of the component primary studies considered in isolation (Schmidt, 1992). This logic is consistent with the estimation of an omnibus MAES capable of summarizing the literature as accurately as possible (Borenstein et al., 2009; Schmidt & Hunter, 2015). However, there is ongoing debate about the optimal methods of achieving that accuracy.

For example, there is disagreement about which effect sizes to combine and the weights to use when combining them (Matt, 1989). Researchers also debate the merits of considering research quality when selecting or weighting studies (Aguinis, Pierce, Bosco, Dalton, & Dalton, 2011; Ioannidis, 2006; Slavin, 1995). In addition, there are multiple schools of thought concerning the appropriate treatment of statistical artifacts (Schmidt & Hunter, 2015), heterogeneity (Higgins & Thompson, 2002), moderating effects (Cortina, 2003), and publication

¹ The contention that the null hypothesis is always false is a common argument against significance testing (Nickerson, 2000; see also Cortina & Dunlap, 1997).

bias (Rothstein, Sutton, & Borenstein, 2005). Each of these considerations influences the calculation, accuracy, and generalizability of an estimated MAES as well as the conclusions readers draw from the analysis (Aguinis, Dalton, et al., 2011; Wanous, Sullivan, & Malinak, 1989).² Modern meta-analysts are encouraged to consider each of these issues to provide the most accurate MAES possible. Accordingly, meta-analytic reporting standards have become increasingly elaborate (APA, 2008; Moher, Liberati, Tetzlaff, Altman, & PRISMA, 2009). These standards encourage transparency by asking meta-analysts to provide detailed methodological information to facilitate the accurate interpretation of results (Aguinis, Pierce, et al., 2011; Aytug, Rothstein, Zhou, & Kern, 2012).

Meta-analysts routinely supplement the computation of an MAES with descriptions of boundary conditions or differences across settings. However, there exists no corresponding set of standards requiring readers to attend to the full range of information in meta-analytic research. Limiting the interpretation of an MA to the mere existence (or absence) of a relationship tacitly ignores quantitative estimates and qualifying statements present in the MA. By focusing solely on the existence of an overall relationship, citing researchers are interpreting meta-analytic results identically to how they would interpret the results of a primary study, which negates the advantages of MA and is antithetical to MA's pursuit of accuracy. Simplifying meta-analytic results in this way can lead to the misunderstanding, misinterpretation, or misrepresentation of an MA's findings. Leveraging meta-analytic findings fully should result in richer and more accurate theory as well as better informed subsequent research.

We should note that accurate citation practices are desirable in all areas of research. Citation problems are not limited to MA or to the organizational sciences. Individual primary

² Additional assumptions of MA as well as information about the assumptions and methodology associated with many of these considerations are presented in our online supplement.

studies often demonstrate complex relationships between variables (such as mediation and moderation) which should also be cited appropriately. In addition, many other fields have highlighted the detrimental effects of inaccurate citation practices on the generation of robust scientific knowledge (e.g., Haussmann, McIntyre, Bumby, & Loubser, 2013; Todd, Yeo, Li, & Ladle, 2007). Many recommendations we offer in this paper also apply to the citation of primary studies and research in other fields. Nevertheless, given the pronounced influence of MA in organizational research and the particularly detrimental effects of miscitations of MAs (which we demonstrate later), we constrain the scope of our discussion to citation practices in MA.

In the following sections, we describe our efforts to identify the ways in which researchers typically cite meta-analytic results. We interpret patterns suggested by our findings as they pertain to underlying assumptions about the meaning and applicability of meta-analytic results. We then turn our discussion to meta-analytic methodology to explore how better understanding some of the analyses and judgment calls made by meta-analysts can aid readers in interpreting the results of an MA. We conclude by making suggestions for how to improve the reception of and interpretation of MA. These include steps meta-analysts can take to clearly communicate and extend their contribution to the field, ways in which the scientific process can assist both researchers and readers in optimizing the use of MA, and methods of leveraging meta-analytic results to inform and enhance future research endeavors.

Method

To examine how researchers use meta-analytic findings in their research, we identified four highly-cited MAs, identified the most recent 100 citations of each MA, and coded how authors who cited each MA referred to its findings. This approach differs in some ways from the approach Carlson and Ji (2011) took in their examination of citation practices. Carlson and Ji

examined all articles that cited MAs in three journals over four one-year periods to identify general trends in citation practices. However, since no two MAs are alike, the available information that citing researchers reference (or ignore) will differ based on the MA they are citing. As such, Carlson and Ji were only able to offer general statements about citation practices as opposed to statements informed by the specific information available in each of the cited MAs (e.g., publication bias analyses, psychometric adjustments, confidence intervals).

We complement Carlson and Ji's (2011) approach by analyzing trends across 100 recent citing papers for each of four specific MAs to explore which information citing authors reference. We identify patterns related to what citing authors interpret, disregard, or cite inaccurately when drawing conclusions for their own work. We also explore how citation practices may affect theorizing in subsequent work. For example, systematically examining citations (or lack thereof) of the specific moderation effects uncovered in the four MAs allows us to draw conclusions about whether citing authors take boundary conditions into account and incorporate them into their subsequent theorizing. We investigate whether citing authors extrapolate the findings of the given MA appropriately or overgeneralize meta-analytic results to research contexts or samples excluded from the original MA. Our detailed focus on the citation practices for each MA allows us to explicitly address the consequences of inaccurate citation practices for research in our field.

Selection of the Four Original Meta-analyses

We selected four MAs spanning the past three decades. Each is described briefly below.

Barrick and Mount (1991) conducted an MA on the relationship between Five Factor Model (FFM) personality characteristics and organizational outcomes. Although they did not report heterogeneity statistics, the authors included a number of moderators such as occupational

group and criterion type. For each relationship, Barrick and Mount reported an unadjusted MAES, adjusted MAES (attenuation and range restriction), and lower bound of the 90% credibility interval. No publication bias analysis was conducted.

Judge, Thoresen, Bono, and Patton (2001) conducted an MA on the relationship between job performance and job satisfaction. They reported an omnibus MAES as well as the presence of heterogeneity (estimated using the Q statistic) and moderator analyses including publication source, measure used, research design, level of job complexity, and occupational group. For each relationship, the authors reported an unadjusted MAES and adjusted MAES (attenuation). They also reported 80% credibility and 95% confidence intervals for the omnibus MAES and indicated whether these intervals included zero in each moderator-specific MAES. They addressed publication bias by including unpublished studies and analyzed publication status as a moderator.

Kish-Gephart, Harrison, and Treviño (2010) conducted an MA on the correlates of unethical intentions and behaviors in work settings. These correlates included individual differences, moral issue influences, and organizational environment factors. For each effect, the authors documented heterogeneity using both Q and τ^2 . The authors reported the results by predictor and criterion type, but they also included methodological moderators such as publication status, sample type, and study setting. Kish-Gephart and her colleagues reported the unadjusted and adjusted MAES (attenuation), and 95% confidence intervals around the fixed-effect and random-effects estimates. Publication bias was addressed using a file drawer analysis and by including publication status as a moderator.

Dalton, Daily, Ellstrand, and Johnson (1998) conducted an MA on the relationship between board of director composition and financial performance and between board leadership structure and financial performance. The authors assessed the effects of three categories of

moderators: firm size, different firm performance indicators (i.e., market performance and accounting performance indicators), and different operationalizations of board composition (i.e., inside/outside director proportion, affiliated director proportion, independent/interdependent proportion). The authors report the observed and adjusted MAES (attenuation) between the variables of interest. They also report observed and adjusted variance, as well as 90% credibility and 95% confidence intervals around each MAES. Neither heterogeneity of effect sizes nor an analysis of publication bias were included. This study offers a particularly interesting case because Dalton and his colleagues conclude that the effect sizes for all meta-analyzed relationships are small enough to be considered inconsequential. They are very direct in reporting this null finding, and there is no ambiguity about their null conclusion in either the omnibus or moderator-specific analyses.

We selected these four papers for several reasons. First, they were all cited frequently enough (229 to 2,370 times) that we could easily identify 100 recent citations. Aguinis, Dalton, et al. (2011) estimate that MAs are cited, on average, 8.47 times per year. Each of the four MAs we selected has been cited at more than four times this rate. Second, these MAs address different research topics spanning both micro and macro literature.³ Third, we chose MAs published in different decades to ensure our results were not dependent on publication year. Naturally, we could have picked a number of other MAs based on these criteria. However, we do not expect that our results are unique to the specific MAs we selected. As such, we chose four MAs that are often included in graduate education and are likely to be known to the academic audience of this

³ We thank an anonymous reviewer for encouraging us to include an MA on a macro topic (i.e., Dalton et al., 1998). It provides an even starker contrast than the OB/HR papers between the actual findings of the MA and the ways in which these findings are cited.

paper. We hope that selecting these MAs will make our data collection more personally relevant to readers who may have cited these MAs in the past.

Inclusion Criteria and Citation Search

We decided to analyze the most recent 100 citations of each of the four MAs as this provided an adequate and convenient sample size to estimate percentages of citation practices. The primary inclusion criterion for a citing study in our dataset was whether the study referred to the meta-analytic *findings* of the original study as opposed to citing it for other reasons (e.g., definitions, methodology). We used three other inclusion criteria. First, we only examined citations that appeared in published work (i.e., journal articles, book chapters, and publicly available conference proceedings). Second, the citing paper had to be accessible to us (i.e., available either through our respective university libraries, Google Scholar, or Internet search). Third, the paper had to be in a language that we could understand to code it (English, German, or Spanish). All Web of Science citations were in one of these three languages, so no foreign language papers were excluded from our analysis on the basis of this criterion.

To find these citations, we conducted a Web of Science citation search for the most recent citations of each MA in order to examine citation practices in the current cohort of researchers. To obtain 100 citing studies that referred to the meta-analytic findings, we had to exclude 86 studies citing Barrick and Mount (1991), 55 studies citing Judge et al. (2001), 43 studies citing Kish-Gephart et al. (2010) and 100 studies citing Dalton et al. (1998). For the Barrick and Mount (1991) and Judge et al. (2001) MAs, accessing 100 citations that met our inclusion criteria required searching back through 2015. For the Kish-Gephart et al. (2010) and Dalton et al. (1998) papers, we had to search back through 2013.⁴

⁴ References for the papers we coded can be found in an online supplement.

Coding

Two of the three authors coded the citing studies. After reading the original MA in detail to ascertain how the meta-analysts reported their findings, we generated a coding document to capture the core findings and other relevant information. We did not expect every citing article to describe every result in explicit detail when referencing an MA. However, we wanted to get a sense of which aspects of each MA researchers attend to when interpreting its findings. Based on articles about judgment calls (e.g., Aguinis, Dalton, et al., 2011; Wanous et al., 1989), experience reading MAs, and the information and analyses reported in each of the four MAs, we identified 37 core pieces of information about the findings that a citing study might reference (see Table 1).

We organized our research questions into five categories. The first category pertains to general citation practices. Like Carlson and Ji (2011), we documented how many times researchers cited a general relationship as opposed to providing quantitative detail (e.g., an MAES). Going beyond Carlson and Ji, we further assessed whether researchers cited the meta-analytic results relevant to their own study. Specifically, we examined the frequency with which researchers cited meta-analytic findings even in cases where the citing study did not match the inclusion criteria of the original MA with regard to the independent variable, criterion variable, sample, or research setting. If the context or sample to which the citing authors want to generalize the MA's findings would have been excluded from the original MA, then the citing authors are extrapolating the findings beyond the boundaries of what the original meta-analytic results could address. We also coded how often authors overgeneralized meta-analytic results or misquoted the original MA.

Our other four categories involved considerations related to the specific meta-analytic methodology used in each of the original MAs. Specifically, we coded information about

publication bias, adjusted and unadjusted MAESs, confidence and credibility intervals, and heterogeneity and moderation analyses. All four MAs reported both adjusted and unadjusted MAESs. However, only two included information about publication bias, three reported confidence intervals around MAESs, and three reported credibility intervals around MAESs. Additionally, each of the four MAs included a different set of moderators.

The authors met several times to discuss the coding procedure. The second author developed an initial coding scheme while coding 50 citations of Barrick and Mount (1991). She then shared the coding scheme with both co-authors for review and feedback before finishing the coding for this MA. Two coders then applied the revised coding scheme to citing studies of the remaining three MAs. After coding was complete, the two coders randomly selected five studies from each set of coded studies and double-checked each other's coding. Initial coder agreement was 97.4% across all four meta-analyses, and disagreements were resolved via discussion, leading to recoding in less than 1% of coding decisions. We recorded the frequency of the 37 referencing practices independently for each of the four original MAs to facilitate comparisons when appropriate. The reported frequencies reflect how researchers reference the findings of these MAs in their published research.

Findings and Discussion

Table 1, which summarizes our findings across the four MAs, is organized into five sections: General Citation Practices, Confidence and Credibility Intervals, Publication Bias, Psychometric Adjustments, and Heterogeneity/Moderation. We provide additional details about these methods in our online supplement, including relevant citations to guide interested readers toward essential literature on each topic as well as discussions of related techniques such as fixed-effect and random-effects MA and meta-analytic structural equation modeling.

[Insert Table 1 about here]

General Citation Practices

The number of studies that reference only a relationship between variables without citing a specific MAES ranges from 90 to 100. Carlson and Ji (2011) found that only 17.6% of citing studies reference an MAES, but all four MAs we analyzed had an even lower MAES citation rate. The practice of citing quantitative estimates provided by an MA has not improved since the publication of Carlson and Ji's paper. This type of citation disregards much of the rich detail available in an MA. In neglecting to reference the MAES, researchers (a) cite meta-analytic results similarly to how they would cite the results of a primary study, (b) ignore the efforts of meta-analysts to estimate an effect size as accurately as possible, and (c) do not consider any detailed analyses (e.g., moderation, publication bias) reported by the MA. Thus, consistent with Carlson and Ji's findings, the most common practice for citing meta-analytic results is to reference only the existence or nonexistence of an overall relationship, ignoring every benefit that MA offers above and beyond primary studies.

Coding categories 2-6 (see Table 1) identified studies that cite an MA even though their own study would not have been included in that MA. This occurred in at least 20% of citing studies for each MA. In these cases, it is inappropriate to cite the MA because the results are not relevant to the citing study. For example, Judge et al. (2001) excluded studies exploring the satisfaction-turnover relationship. Therefore, Judge et al.'s results are not informative for research describing or examining the relationship of satisfaction with turnover. Each of the four MAs was miscited on the basis of inclusion criteria at least ten times. The citing studies we coded miscited MAs on the basis of the criterion variable more often than the primary

independent variable. The Barrick and Mount (1991) MA was miscited on the basis of inclusion criteria more often than the other three MAs.

Coding categories 7-11 identified citing studies that directly misinterpreted meta-analytic findings. While only 2 to 13 citing studies directly overgeneralized the meta-analytic results when citing the MA, a substantially higher number (4 to 56) directly misquoted the MA. These citation practices are clearly unacceptable as they reflect a misunderstanding about the content or generalizability of the MA. The only MA to directly recommend citation practices was Dalton et al. (1998, p. 282), who report that their results “lead to the very strong conclusion that the true population relationship across the studies included in these meta-analyses is near zero.” Despite this unequivocal wording, more than half of the studies citing Dalton et al.’s work misinterpreted the meta-analytic findings as either mixed results (46 studies) or supporting the existence of a relationship (11 studies). In these cases, citing authors disregarded the MA authors’ conclusions, interpreting the opposite of what the meta-analytic results suggested and explicitly ignoring or actively misappropriating meta-analytic evidence. We find these practices especially concerning as they are potentially harmful to the proliferation of evidence-based knowledge in our field.

Confidence and Credibility Intervals

Coding categories 12 and 13 addressed the citation of confidence and credibility intervals. It is customary to report a 95% confidence interval and/or an 80% credibility interval (Schmidt and Hunter, 2015). We provide additional information pertaining to the methodology and differences between types of intervals in our online supplement. Aguinis, Dalton, et al. (2011) report that 54.3% of MAs report confidence intervals and 34.1% report credibility intervals. Only one of the 400 studies we coded cited the confidence and credibility interval. Specifically, Bowling, Khazon, Meyer, and Burrus (2015, p. 89) write:

The 80% credibility interval reported by Judge et al. (2001) ranged from .03 to .57, which they characterized as “relatively wide” (p. 387). Furthermore, they reported that statistical artifacts could account for only about 25% of the variance in effect sizes across studies and that the Q statistic was statistically significant. Together, these findings suggest the existence of substantive moderators of the satisfaction–performance relationship.

Bowling and colleagues rely on the reported Q statistic and information about the credibility interval reported in Judge et al.’s MA to justify their own exploration of situational moderators of the relationship between job satisfaction and job performance. Furthermore, they compare the confidence interval reported in Judge et al. (2001) to the confidence interval around their own MAES to conclude that both studies found a significant positive relationship between job satisfaction and job performance and that both findings are “virtually identical” (Bowling et al., 2015, p. 93). Overall, this paper serves as a positive example of how citing authors can fully use the information provided in an MA to justify their own theorizing and hypothesis development.

Readers should always consider the width of a confidence or credibility interval when interpreting meta-analytic results. Interval width communicates a range of plausible MAES values and can be more informative than relying on a single point estimate of the MAES. Wide intervals suggest uncertainty and may signal heterogeneity and/or the presence of moderation. Very wide intervals may not provide much more information than the direction of an effect. Effect size estimates that vary by as little as .05 may indicate a lack of transportability and suggest the presence of moderation (Carlson & Ji, 2011). Judge et al. (2001) report a wide 80% credibility interval around their omnibus MAES of .03 to .57. Although this range of values does not include zero, this interval does not denote certainty in the MAES estimate. This interval

covers a large range of positive correlations, and the variance explained may be as small as 0.09% and as large as 32.49%. On the other hand, Kish-Gephart et al.'s (2010) MA reports a 95% confidence interval of .200 to .204 for published studies linking relativistic moral philosophy to unethical behaviors. Again, all values contained in the interval are positive and the interval does not include zero. However, in this case, the narrow interval indicates a relationship explaining between 4.00% and 4.16% of the variance. As such, readers can be relatively confident in their interpretation of the direction and effect of this relationship within the constraints of this MAES's generalizability.

In addition to the width of an interval, readers should consider the confidence level at which the interval is reported. Higher confidence levels are associated with wider intervals than lower confidence intervals. For example, Judge et al.'s (2001) aforementioned 80% credibility interval of .03 to .57 would be even wider at the 90% credibility level (-.05 to .65) or the 95% credibility level (-.11 to .71). In this case, higher confidence levels would expand the interval to include zero and may change how readers interpret the result. When intervals are wide or confidence levels are low, it may be misleading to rely on a single point estimate (i.e., an omnibus MAES) when interpreting meta-analytic results. In these cases, we also discourage readers from relying solely on a determination of whether or not an interval includes zero. A simple interpretation such as "job satisfaction is related to job performance" does not capture the complexity underlying this relationship. Instead, we recommend that researchers incorporate the interval type, width, and credibility level into their interpretation. In some cases, it may be worthwhile to directly incorporate this information into the citation as Bowling et al. (2015) did. Researchers could use this information to inform study design, perhaps by comparing their effect

sizes to the MAES and corresponding interval or noting that the width suggests the presence of moderating effects that should be explored in subsequent research.

Publication Bias

Researchers have long suspected that journals disproportionately publish studies with statistically significant effects (Rothstein et al., 2005). If non-significant results are underrepresented in the published literature, these studies will be more difficult to find and include in MAs. If small or non-significant effect sizes are less likely to be published, an MAES may misestimate the true population effect size (Dickersin, 2005; Kepes, Banks, McDaniel, & Whetzel, 2012). The potential inflation of an MAES due to publication bias runs athwart MA's goal of estimating effect sizes as accurately as possible.

MA reporting standards recommend assessing publication bias (APA, 2008; Moher et al., 2009). Despite this recommendation, 60% to 90% of meta-analyses do not consider publication bias at all, one-quarter to one-half of MAs and systematic reviews do not include unpublished results, and only 8% of results included in MAs come from unpublished studies (Borenstein et al., 2009; Moher, Tetzlaff, Tricco, Sampson, & Altman, 2007; Sutton, 2005). The presence of publication bias mitigates the representativeness and generalizability of meta-analytic results. Reviews of MA have concluded that publication bias has a modest to severe impact on meta-analytic results in approximately 50% of meta-analyses (Rothstein et al., 2005).

Although Barrick and Mount (1991) and Dalton et al. (1998) did not assess publication bias, the other two MAs we analyzed did. Publication bias is mentioned in one citation of Judge et al. (2001) despite the MA only reporting small or negligible effects of publication bias. On the other hand, some of Kish-Gephart et al.'s (2010, Tables 5, 6, and 7) results indicate that MAES estimates and corresponding confidence intervals differ based on publication status. Kish-

Gephart et al. report, but do not interpret file drawer analyses for their main effects. However, they examine publication status as a moderator, reporting 95% confidence intervals around each MAES. Differences ranged from small (e.g., the ethical culture intervals were $-.570$ to $-.150$ in published studies and $-.506$ to $-.135$ in unpublished studies) to large (e.g., job satisfaction intervals were $-.471$ to $-.162$ published and $-.212$ to $.073$ unpublished). Seven of these analyses lead to different interpretations for published and unpublished studies. Despite this, none of the studies we analyzed referenced publication bias in this MA.

Readers should always examine the Method section of an MA to determine whether publication status was considered in the inclusion criteria or analyses. If an MA neglects to account for publication status, readers should interpret the results with caution. When publication bias exists, readers should incorporate this information into their interpretation. For example, when citing Kish-Gephart et al. (2010), readers should avoid drawing conclusions such as “job satisfaction is negatively related to unethical choices” as this relationship is supported in published studies, but unsupported in unpublished studies. Readers should instead acknowledge the possibility that a subsequent study will produce an effect size similar to that of an unpublished study. On the other hand, when publication bias is assessed, but found to be negligible (e.g., Judge et al., 2001), no qualifying statement or interpretation is necessary.

We also recommend that readers consider primary study quality even when meta-analysts do not. Like publication status, study quality can be associated with effect size magnitude in primary studies (see online supplement). We encourage readers to become familiar with at least a subset of the primary studies used in the MA. By reading some of these studies (particularly those relevant to the reader’s sample, measurement, or context of interest), consumers of MA will have a better sense of the quality of primary research that influences the relationships they

are interpreting. In most cases, we hope that readers will conclude that the primary studies are high-quality, instilling greater confidence in meta-analytic results. However, in the rare cases where primary studies are deemed untrustworthy, the meta-analytic results should be as well (Bobko & Stone-Romero, 1998; Ioannidis, 2006). We describe some common publication bias analyses and quality considerations in more detail in our online supplement.

Psychometric Adjustments

In the organizational sciences, meta-analysts often adjust MAES estimates for statistical artifacts such as attenuation (unreliability of measures) or range restriction. This practice known as psychometric MA, is thought to provide a more accurate estimate of the magnitude of the relationship at the construct level (Schmidt & Hunter, 2015). Cortina (2003) reports that 78.6% of the 1,647 MAs published in the *Journal of Applied Psychology* up until 1997 adjusted for at least one artifact. Aguinis, Dalton, et al. (2011) found that 51.2% of effect sizes in MA were adjusted for criterion unreliability, 48.3% for predictor unreliability, 9.5% for range restriction on the dependent variable, and 11.6 % for range restriction on the independent variable. All four of the MAs we analyzed employed psychometric adjustments.

Our results indicate that the vast majority of citing articles did not address the use of psychometric adjustments in the MA. This is consistent with the fact that very few referenced a quantitative estimate of any type. Of the ten or fewer citing studies that referenced an MAES, adjusted values were cited more often for Judge et al. (2001), while adjusted and unadjusted values were cited at similar rates for Barrick and Mount (1991) and Kish-Gephart et al. (2010). Neither the adjusted nor the unadjusted estimate was ever referenced for Dalton et al. (1998).

Understanding psychometric adjustments is critical to correctly interpreting meta-analytic results. Unadjusted estimates reflect variable-level relationships intended to quantify effects in

applied settings (DeSimone, 2014). Adjusted estimates, on the other hand, are more indicative of construct-level relationships intended to quantify effects under ideal conditions such as unrestricted range and/or perfect measurement (Schmidt & Hunter, 1996). Accordingly, adjusted estimates are typically larger than their unadjusted counterparts since they are estimates of the magnitude of effect sizes that are not attenuated by these statistical artifacts.

We offer two recommendations pertaining to psychometric adjustments. First, readers should be cognizant of which adjustments were used in the MA. Two common psychometric adjustments involve attenuation from measure unreliability (Spearman, 1904) and range restriction (Thorndike, 1949). Effect sizes adjusted for either of these statistical artifacts will always be larger than what we would expect to observe naturally, given that our measures contain error and we rarely have access to entirely unrestricted samples. In addition, adjusting for both of these statistical artifacts assumes that predictor reliability, criterion reliability, and range restriction are independent. Since situational factors may simultaneously influence the MAES, range restriction, and measure reliability, this assumption is rarely met (James, Demaree, Mulaik, & Ladd, 1992). Violations of this independence assumption can artificially inflate adjusted MAES estimates (Köhler, Cortina, Kurtessis, & Gölz, 2015). For readers, this means that the adjusted MAES will often be inflated compared to the effect size we would expect to observe in realistic conditions. Readers will be more likely to observe an effect size closer to the unadjusted MAES in their own follow-up study (LeBreton, Schoen, & James, 2017).

Second, readers should generally determine whether the unadjusted or adjusted MAES estimate is most appropriate for interpretation. The unadjusted estimate is more appropriate for understanding the relationship at the practical level, whereas the adjusted estimate is more appropriate for understanding the relationship at the theoretical of construct level. Readers

should also note any differences between the adjusted and unadjusted MAES estimates. Large discrepancies between the adjusted and unadjusted estimates may indicate that primary studies suffer from problems such as extreme range restriction or unreliable measurement.

As an example, Judge et al. (2001) adjust for predictor and criterion reliability, but not range restriction. They report an unadjusted omnibus MAES of .18 and an adjusted omnibus MAES of .30. Some moderator analyses had even larger discrepancies, occasionally doubling the size of the MAES (e.g., unadjusted .26 and adjusted .52 in high complexity jobs, unadjusted .19 and adjusted .45 in scientist-engineer occupations). Judge et al. (2001) note that the average predictor and criterion reliability values were low (.74 and .52, respectively), which contributes to the large differences between adjusted and unadjusted estimates.

Readers who want to apply the findings from Judge et al.'s MA in practical settings should interpret the unadjusted MAES of .18 while readers interested in the theoretical relationship between satisfaction and performance should interpret the adjusted MAES of .30. The large discrepancy between adjusted and unadjusted estimates indicates the presence of measurement error. In these cases, we recommend that readers defer to the unadjusted MAES until the relationship can be reassessed using more reliable measures. In all cases, citing authors should clearly denote which value they are reporting by using the commonly used MA symbols (e.g., $\bar{r}$ for an unadjusted mean correlation and $\hat{\rho}$ for an artifact-adjusted mean correlation).

Heterogeneity and Moderation

Meta-analysts routinely attempt to determine if primary study effect sizes vary as a function of substantive or methodological study characteristics (Cortina, 2003). Multiple authors have cautioned against interpreting main effects in the presence of interactions (Cohen, Cohen, West, & Aiken, 2003; Hunter & Schmidt, 2004). Meta-analytic moderation is no different, as it

is difficult to meaningfully interpret an omnibus MAES when the strength and/or direction of the relationship varies as a function of moderating variables. The vast majority (up to 97%) of MAs rely on primary studies with heterogeneous effect sizes (Aytug et al., 2012; Geyskens, Krishnan, Steenkamp, & Cuhna, 2009), indicating that general statements about the size (or sometimes even the existence) of a meta-analyzed effect are typically insufficient for capturing the complexity of the relationship.

Three of the four MAs we analyzed provided data indicating that MAES estimates vary substantially as a function of the reported moderators. The fourth (Dalton et al., 1998) reported differences in direction of MAES estimates on the basis of moderators, but concluded that the differences in magnitude were trivial. Although moderation was clearly and transparently reported in each MA, moderation was directly referenced 12 or fewer times per MA (out of 100) in the citing studies we explored. Three or fewer citing studies per MA addressed primary study heterogeneity, and none mentioned the standard deviation of reported MAESs. These results are consistent with Carlson and Ji's (2011) corresponding findings that only 0.5% of studies citing an MA reference variability and only 0.4% report an estimate of that variability. Carlson and Ji also report that effect size variability is used to justify moderation analyses in 2.4% of citations, but they do not differentiate between types of moderators or discuss indirect recognition of meta-analytic moderation. Consequently, we also examined two indirect ways in which citing researchers could have interpreted heterogeneity and moderation without directly referencing it.

First, citing researchers could have indirectly acknowledged moderation by referencing variation across levels of a meta-analytic moderator. Less than a third of the studies citing Barrick and Mount (1991) and less than ten percent of the studies citing Judge et al. (2001) or Kish-Gephart et al. (2010) acknowledged the existence of variation across levels of a moderating

variable (e.g., occupational group, measurement, job criteria, job complexity, or research design). Researchers citing Dalton et al. (1998) could acknowledge variation across firm size, performance indicator, or board composition indicator, but they did so six or fewer times for each type of moderator. We were especially surprised to discover instances where authors citing *their own MA* (i.e., the MA they previously co-authored) failed to mention effect size heterogeneity or moderation.

Another method of indirectly acknowledging moderation in the original MA is by citing or interpreting the appropriate relationship for the level of a moderating variable used in the citing study. This type of citation was rare in our analysis (three or fewer times per moderator for studies citing Barrick and Mount (1991) and once or less per moderator for the other three MAs we analyzed). One positive example was King et al.'s (2016) focus on the relationships between performance evaluations and two specific FFM traits (conscientiousness and extraversion). Instead of focusing on a general relationship, King et al. refer to the two most relevant moderator-specific relationships reported in Barrick and Mount's (1991) MA. Unfortunately, we also encountered many negative examples in which authors incorrectly interpreted moderator-specific relationships that were not relevant to their context of interest.

When interpreting or citing meta-analytic results, we recommend focusing on the moderator-specific MAES that most closely reflects the context in which the citing study is set. Consequently, we advise readers to consider each moderation analysis reported in the MA. Our results indicate that the majority of citing researchers focus on omnibus relationships even when more specific results are available in an MA. For example, a common statement made by authors citing Barrick and Mount (1991) is that the MA found a positive relationship between personality and job proficiency. This interpretation is inaccurate as not all FFM dimensions show a positive

or consistent relationship across the different outcome variables or occupational groups included in the MA. It would be preferable for researchers to cite the moderator-specific MAES corresponding to the specific personality variable, type of assessment of job performance, and occupational group relevant to their current study. For example, researchers interested in the effect of conscientiousness on training proficiency should reference either a .13 (unadjusted) or .23 (adjusted) MAES. Similarly, researchers who plan a follow-up study on the effects of agreeableness on job performance should reference the MAES that corresponds most closely to their sample (e.g., a .00 (unadjusted and adjusted) MAES if focusing on sales staff versus a .05 (unadjusted) or .10 (adjusted) MAES for managers).

We encourage all readers to acknowledge exactly which MAES they are interpreting and to make the case that this MAES is the most appropriate for their own study. By attending to moderator-specific MAES estimates, citing authors can draw on more appropriate and informative information for their theoretical statement or situation of interest. Even when a reader is unable to find a combination of moderators that corresponds perfectly to their situation of interest, trends in the reported moderator analyses may indicate whether the overall MAES is larger or smaller than what the reader should expect to find in their planned study setting.

Comparison to Carlson and Ji (2011)

Our findings regarding general citation practices are largely consistent with Carlson and Ji (2011). Specifically, our findings indicate that the vast majority of citing authors refer only to the omnibus relationship analyzed in the MA while ignoring both the magnitude and heterogeneity of that relationship. Carlson and Ji (2011, p. 708) report that “less than one in five studies citing a meta-analysis as evidence about a relationship report its magnitude.” We found that this number was usually less than one in ten for the four MAs we analyzed. Carlson and Ji

also report that only seven of the 1,489 citations (0.5%) they examined contained a direct reference to heterogeneity. Our results indicate that only 2.0% of citing studies mentioned heterogeneity analyses (only Judge et al., 2001 and Kish-Gephart et al., 2010 reported heterogeneity statistics, and only four articles referred to these), while none of the 400 citing studies we coded mentioned the standard deviation (or variance) of the MAES.

In addition to triangulating their results using a different method, we extend Carlson and Ji's findings in a number of ways. First, we found that even indirect acknowledgement of moderated meta-analytic results is notably rare. Second, we found that most citing authors ignore findings related to publication bias, psychometric adjustments, and confidence or credibility intervals even when they are clearly reported in an MA. Most alarmingly, we found that a large number of citing studies misquoted MAs, overgeneralized meta-analytic results, or cited an MA despite the fact that their own study would not have met the MA's inclusion criteria.

Carlson and Ji (2011) argue that ignoring meta-analytic heterogeneity and moderation improperly implies homogeneity to readers. We concur. We also believe that ignoring other methodological issues is tantamount to pretending they do not exist. For example, ignoring MAES magnitude improperly equates effects of different magnitudes on the basis that they are both "not zero." Ignoring publication bias analyses implies the absence of publication bias. Finally, failing to differentiate between adjusted and unadjusted MAESs fail to communicate important differences between the practical and theoretical implications of their statements.

The Impact of MA Citation Practices

The practice of oversimplified reporting of meta-analytic results is alarmingly widespread. However, the impact of underreporting results depends on the statement an author is attempting to make. Researchers often use MA citations to justify model specification, variable

inclusion, hypothesis development, or a specific research design. While it is certainly important for citing researchers to understand meta-analytic results in order to interpret and use them appropriately, there is rarely a need to incorporate every detail of an MA into a citation. We propose that the level of detail required for adequate citation of meta-analytic results depends on the purpose for which the authors employ the citation. In the next few paragraphs, we consider best practices, words of caution, and advice for the appropriate interpretation and citation of meta-analytic results.

Frequently, citing papers refer to meta-analytic findings to justify the inclusion of certain variables in their own model specification and hypothesis development. These justifications are predominantly general statements that only refer to the relationship between constructs without any recognition of the estimated strength (or sometimes even direction) of the relationship or any other pertinent information from the MA, such as the existence of relevant moderators. For example, a common statement when citing Barrick and Mount (1991) involves the assertion that personality is related to job performance. Personality (especially as conceptualized by the FFM, see Block, 1995; Digman, 1990) comprises multiple constructs. Consequently, Barrick and Mount do not present an MAES for the relationship between job performance and some composite of the FFM; in fact, all primary studies using composite measures of the FFM were excluded from their MA. Blanket statements such as “personality is related to job performance” ignore the fact that Barrick and Mount only found conscientiousness to be consistently related to several indicators of job performance across occupations. Ignoring this information and interpreting the MA as if there were, for example, a relationship between emotional stability and job performance in managers could lead to the ill-advised inclusion of emotional stability in models of managerial job performance despite Barrick and Mount only finding a small

relationship in this condition ($\bar{r} = .05$, $\hat{\rho} = .08$, and a 90% credibility interval with a lower bound of $-.04$).

We found several cases in our analysis where citing authors miscited an MA to justify exploring a relationship that the original MA never addressed (e.g., Alabede, 2016; Potosky, 2016). Several papers cited Dalton et al.'s (1998) MA as providing evidence for a significant and meaningful relationship between board of director composition or board leadership structure and firm financial performance despite the fact that Dalton et al. found exactly the opposite (i.e., no relationship) (e.g., Matthew, Ibrahim, & Archbold, 2016; Sanchez, Guerrero-Villegas, & Gonzalez, 2017). This has resulted in a thriving stream of research on this relationship that often results in the citing authors "surprisingly" finding no relationship between these variables in their own data. One may argue that the consequence is at best a waste of effort and money that could be better spent on other research. However, by repeatedly publishing studies that miscite Dalton et al. and argue for a relationship that has been shown not to exist, the field creates a myth of this relationship that may be extremely resistant to eradication by subsequent evidence. Testament to this is the fact that Dalton et al.'s MA was the most frequently miscited MA we examined, and it has been almost 20 years since its publication.

Based on our observations and the potential negative impact of inappropriate citation practices, we urge authors who cite MAs for the purposes of model specification, variable inclusion, or hypothesis development to more carefully attend to the details provided in published MAs. Good hypothesis development involves more specific information than broad theoretical statements (e.g., specific operationalization, expected direction of relationships). Thus, authors should closely examine meta-analytic results prior to designing a new study in an effort to determine influential moderators, boundary conditions, and potential inconsistencies

stemming from different methodological approaches or choices (such as inclusion criteria, construct measurement, or publication status), as well as the most appropriate sample, measures, or research context for their research questions. One of the best examples we found of efficient and detailed reporting of relevant findings from Barrick and Mount (1991) can be found in Rosenthal, Sutton, Austin, and Tsuyuki (2015, p. 210):

Furthermore, a systematic review examining job performance, which was defined as the confluence of job and training proficiency and personnel data, in a number of job groups that included professionals and managers, found a positive correlation with scores on the trait of conscientiousness, as measured across a number of instruments.

Researchers often cite MAs to justify a specific research design or to apply the MA's findings to a specific research setting or sample. When justifying a specific research context or design, it is insufficient to cite the omnibus relationship or MAES. Instead, authors should focus on the moderator-specific relationship(s) or MAES(s) most appropriate for the context and design they are attempting to explore. The MA by Judge et al. (2001) provides different MAESs for different job performance measures (referenced by 2 studies), different levels of job complexity (referenced by 6 studies), and different types of research design (referenced by 2 studies). The previously mentioned Bowling et al. (2015) study acknowledges several of the moderating effects found by Judge et al. (2001) and consequently includes them in their study as control variables. This is a positive example of how authors can incorporate information from an MA into subsequent research. Unfortunately, most other studies that refer to Judge et al.'s moderated results do not include any of these moderators in their own studies.

Researchers sometimes extrapolate findings from an MA to a research context or sample that was not included in the original MA. For example, we found two studies using military

samples that state that Barrick and Mount (1991) found a relationship between job performance and conscientiousness and neuroticism in civilian and military samples (i.e., Barron, Carretta, & Rose, 2016; Colodro, Garces-de-los-Fayos, Lopez-Garcia, & Colodro-Conde, 2016). However, military samples were expressly excluded from Barrick and Mount's MA. It remains unclear to us which MAES these citing authors interpreted for their justification, given that conscientiousness and neuroticism do not universally significantly predict job performance across professions, and not even in perhaps the closest profession to military occupations included in Barrick and Mount (i.e., police personnel).

Many miscitations seem to stem from the misunderstanding or oversimplification of meta-analytic results. As noted above, most MAs published in the organizational sciences conduct detailed analyses (e.g., publication bias, heterogeneity) and report detailed results (e.g., adjusted and unadjusted MAESs, moderator-specific MAESs, confidence and credibility intervals). While it is not always necessary to include all of this information in a citation, it is never appropriate to ignore it. The oversimplification of meta-analytic results may be the result of misunderstandings about meta-analytic methodology. We have included reference information for various aspects of meta-analytic methodology in our online supplement. Another explanation is that readers often elect to ignore the complexity of MA in favor of simple explanations. Citing authors need to know enough about an MA to cite it properly, but also must be motivated to strive for accuracy in their reporting (see Aguinis, Ramani, & Alabduljader, 2018). Graduate training should instill the importance of accurate writing and reporting. Motivation may come in the form of curiosity and interest about the meta-analyzed topic. It can also come from the simple desire to get things right or the aversion to the consequences of inaccuracy. For the reasons stated above (and in our introduction), oversimplification often leads to

misinterpretation. In the following section, we offer some suggestions for interpreting meta-analytic results accurately and appropriately.

Advice for the Field

Throughout this paper, we have focused primarily on consumers of MA. This is because we believe that the onus of responsible interpretation and citation practices ultimately lies with the reader. We certainly do not wish to discourage readers from citing meta-analytic results or imply that only experts are capable of appropriately leveraging MA in theory building and empirical research. We simply hope that our advice in this paper inspires readers to examine meta-analytic results more carefully and critically in the service of more appropriate interpretation and citation of MA. As Murphy (2017, p. 194) notes, “there are probably few research literatures that can be adequately characterized by a single number.” Accordingly, the following sections and Table 2 offer recommendations for how meta-analysts, reviewers/editors, and readers can facilitate the appropriate interpretation of meta-analytic results.

[Insert Table 2 about here]

Advice for meta-analysts. MA plays an important role in organizational research. We make no claims in this paper that meta-analytic methodology or reporting standards are flawed, nor do we accuse meta-analysts of attempting to deceive readers. Rather, we are concerned about the current state of the field in which a large body of organizational researchers seemingly neglect the strengths and potential of MA in their own research⁵. As such, our advice for meta-analysts is limited to reporting practices.

⁵ We also acknowledge that primary research may suffer from similar citation problems (e.g., misunderstanding, oversimplification). While the focus of this paper is on appropriate citation practices for MA, many of the recommendations in this section apply more broadly. However, given the finding that individual MAs are cited at a higher frequency than individual primary studies, we believe it is imperative that researchers are interpreting and referencing meta-analytic results properly. We thank an anonymous reviewer for this point.

First, it is important for meta-analysts to highlight the range of analyses and results reported in the MA. Editors and reviewers routinely suggest analyses such as publication bias and moderation. MAs typically include a plethora of results that are more nuanced than an omnibus MAES to enhance the interpretability of their results (e.g., adjusted and unadjusted correlations, confidence and credibility intervals). We encourage meta-analysts to highlight these analyses in the Method and Discussion sections of their manuscripts in order to attract readers' attention. For example, there is no omnibus MAES reported in Barrick and Mount (1991). Instead, results are reported by personality trait, incorporating moderator analyses into every table. Barrick and Mount also report each trait-specific MAES by moderator (e.g., occupation, criterion) prior to computing the mean across moderator groups. By focusing on moderated relationships as opposed to an omnibus relationship, Barrick and Mount tacitly encourage readers to focus on specific and appropriate relationships. This is reflected in our results by the fact that citations of Barrick and Mount's MA were more likely to explicitly mention variation across moderator levels than citations of the other three MAs we analyzed (see Table 1).

Second, meta-analysts should use their content knowledge to evaluate the state of the field. Conducting an MA requires a comprehensive (if not exhaustive) literature search to obtain all relevant research in an area. Through their immersion in the literature, meta-analysts should be uniquely qualified to identify research gaps and opportunities for subsequent work. Meta-analysts should be able to ascertain which measures, samples, and settings are overrepresented, underrepresented, or absent from the existing literature. We encourage meta-analysts to identify contexts in which a dearth of research has been conducted and suggest future research to fill these gaps. Accordingly, we also urge meta-analysts to identify situations where the exploration of a specific effect is predominantly driven by specific research characteristics (e.g., overreliance

on a particular measure, sample, or research design). In this way, meta-analysts can leverage their content knowledge and literature review to explicitly address the generalizability of the MA's findings and identify opportunities for future research.

Finally, meta-analysts can incorporate theory into their discussion of moderating effects. Specifically, meta-analysts should incorporate theory in an effort to consider *how* and *why* effect sizes might vary as a function of study characteristics. By examining the underlying mechanisms that may influence differences in primary study effect sizes, meta-analysts can better explore the theoretical mechanisms that give rise to the phenomena under investigation.

Advice for editors and reviewers. It is common for reviewers to request that meta-analysts include additional detailed analyses. In doing so, they support the goal of MA to estimate relationships as accurately as possible. In addition to encouraging meta-analysts to conduct additional analyses, reviewers and editors should expect meta-analysts to focus on those analyses in their Discussion sections. By encouraging meta-analysts to focus on specificity, editors and reviewers can assist meta-analysts in communicating the qualifying statements and boundary conditions that accompany their results.

Second, editors and reviewers of empirical research should encourage accurate citation practices by insisting that researchers leverage the strengths of the MAs they cite. NHST-style interpretation of MAs should be discouraged in favor of authors reporting and interpreting effect sizes. Researchers should be encouraged to focus on the correct level of detail for the theoretical or empirical propositions they are trying to support.

Finally, we believe that researchers should not be dissuaded from pursuing additional research in an area just because an MA has been published. Unfortunately, on occasion, meta-analysts have gone so far as to suggest that additional research in an area would be futile (see

Barrick, Mount, & Judge, 2001; Chan & Arvey, 2012). This is a dangerous proposition, as MAs conducted for the purpose of replicating or re-analyzing prior meta-analytic relationships often come to different conclusions. Just as different primary studies often produce different (and sometimes contrasting) results, different MAs also often reach incongruent conclusions. For example, Judge et al. (2001) and Van Iddekinge, Roth, Raymark, and Odle-Dusseau (2012) meta-analyzed relationships that had been previously meta-analyzed by other researchers. In both cases, two independent MAs conducted by two independent teams of researchers came to different conclusions. A single MA (or MAES) should never serve as the “final word” about the nature or magnitude of a relationship. Meta-analytic results can and should influence the conduct of future research, but at no point should researchers, readers, or reviewers consider an area of research concluded on the basis of an existing MA.

Advice for readers. We have already provided detailed advice in the above sections pertaining to meta-analytic methodology and appropriate citation practices. We reiterate our prior suggestions that (a) citation practices will be enhanced by increased understanding of and attention to the specific judgment calls and methodological aspects of an MA, (b) focusing on effect magnitude and moderator-specific meta-analytic results is preferable to interpreting an unquantified and unqualified omnibus relationship, and (c) the level of detail in a citation should reflect the purpose of the citation.

Additionally, we encourage readers to consider MA as a starting point for research in an area. Researchers can use meta-analytic results to enhance their subsequent research efforts, focusing on unanswered questions or gaps in the literature revealed by the MA. As a positive example, Tett and Burnett (2003) introduced Trait Activation Theory after considering between-study variation reported in MAs addressing the effects of different factors of personality on job

performance (e.g., Barrick & Mount, 1991). By describing the situational features most likely to activate personality characteristics, Tett and Burnett predict how various personality factors differently influence job performance across job settings. By using meta-analytic results to inform theory and research design, researchers can leverage the strengths of meta-analytic research to improve the quality of ongoing research.

Limitations

Our investigation has a few potential limitations. First, we limited the number of MAs in our analysis to four. Although we attempted to cover MAs that are well-cited and familiar to many readers, the focus on these specific MAs may limit the generalizability of our results. While the inclusion of additional MAs may yield more robust results, our findings are likely an appropriate representation of the status of current citation practices for two reasons. First, our results seem largely consistent across MAs, research topics, and time. Second, they are consistent with Carlson and Ji's (2011) broader investigation of MA citation practices that did not focus on the citation practices related to any particular MA.

There are many additional meta-analytic methods and judgment calls that we did not examine. For example, we did not consider the role of defining constructs (Wanous et al., 1989), search procedures (Schmidt & Hunter, 2015), or determining which primary study effect sizes to analyze (Matt, 1989). Each of these methodological choices may also influence the inferences that can be made from meta-analytic results. However, these aspects of meta-analytic design may not be as directly relevant to citation practices as the ones included in our analysis.

Conclusions

Our analysis reveals that the *status quo* for citing meta-analytic results is to ignore the detailed analysis provided by the MA in favor of a general statement about a relationship

between two constructs. Most papers do not reference quantitative estimates of effect size. Those that do usually fail to look beyond the omnibus estimate in an effort to identify the most appropriate estimate. An alarming number of studies mistakenly cite an MA when its results are not applicable to the study at hand, and an unacceptable number directly misquote or overgeneralize MAs. In extreme cases, researchers occasionally cite MAs as support for relationships that the MA did not support or did not analyze. In sum, it seems as though the contents of meta-analytic research are largely being disregarded in favor of the simple dichotomous conclusions that MA was designed to avoid. Readers must be aware that the complexities of reality in organizational life cannot be easily summarized by a single statistic from a single method. It is for this reason that research proceeds in an effort to continually discover new phenomena, delineate boundary conditions, and advance knowledge. There are few, if any, universal truths in the organizational sciences.

We encourage readers to take steps to improve citation practices for MAs in the organizational literature. In many cases, meta-analytic citation practices can be improved simply by considering the implications of reported quantitative estimates (e.g., MAES values and associated intervals) alongside any qualifying statements suggested by inclusion criteria, publication bias, or meta-analytic moderation analyses. Examining the range and quality of primary studies and meta-analytic results can reveal the presence of reported results that are more applicable to a given situation than the omnibus relationship. The critical examination of meta-analytic results can also reveal opportunities for future research. When conducted and interpreted properly, MA can serve as a powerful tool capable of simultaneously summarizing and extending research in the organizational sciences.

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Tables

Table 1

Findings from the Analysis of Reporting Practices and Interpretations of Citing Studies

Coding category	Barrick and Mount (1991) Frequency out of 100	Judge et al. (2001) Frequency out of 100	Kish-Gephart et al. (2010) Frequency out of 100	Dalton et al. (1998) Frequency out of 100
General Citing Practices				
1. Referenced only the relationship between variables (no MAES)	93	90	96	100
2. Referenced MA although the study wouldn't have met inclusion criteria with regard to IV measurement	17	9	17	n/a
3. Referenced MA although the study wouldn't have met inclusion criteria with regard to criterion variable	65	75	39	n/a
4. Referenced MA although the study wouldn't have met inclusion criteria with regard to required variables	n/a	n/a	n/a	20
5. Referenced MA although the study wouldn't have met inclusion criteria with regard to the sample	48	6	13	n/a
6. Referenced MA although the study wouldn't have met inclusion criteria with regard to the research setting	68	10	n/a	n/a
7. Overgeneralized the findings of the original MA	13	7	2	6
8. Misquoted the original MA	16	4	17	56
9. Quoted the original MA as evidence of a relationship although the findings indicated no relationship	n/a	n/a	n/a	11

10. Quoted the original MA as evidence of mixed findings although the findings indicated no relationship	n/a	n/a	n/a	46
11. Did not use the original MA's findings in the way recommended by the MA's authors	n/a	n/a	n/a	55
Citing Practices Related to Confidence and Credibility Intervals				
12. Mentioned the confidence interval around the MAES(s)	n/a	1	0	0
13. Mentioned the credibility interval around the MAES(s)	0	1	n/a	0
Citing Practices Related to Publication Bias				
14. Referenced publication bias in original MA	n/a	1	0	n/a
Citing Practices Related to Psychometric Adjustments				
15. Mentioned the adjusted MAES(s)	4	10	2	0
16. Mentioned the unadjusted MAES(s)	3	1	3	0
Citing Practices Related to Heterogeneity and Moderation				
17. Mentioned the standard deviation of the MAES(s)	0	0	0	0
18. Mentioned heterogeneity in effect sizes of the primary studies included in the MA	n/a	3	1	n/a
19. Mentioned the presence of moderator effects	7	5	3	12
20. Mentioned variation across occupational groups	31	2	n/a	n/a
21. Mentioned variation across measures	6	2	n/a	n/a
22. Mentioned variation across job criteria	25	n/a	n/a	n/a
23. Mentioned variation across job complexity	n/a	6	n/a	n/a
24. Mentioned variation across different research designs	n/a	2	n/a	n/a
25. Mentioned variation across different outcomes	n/a	n/a	8	n/a

26. Mentioned variation across firm size	n/a	n/a	n/a	4
27. Mentioned variation across different performance indicators	n/a	n/a	n/a	6
28. Mentioned variation across different board composition indicators	n/a	n/a	n/a	2
29. Cited appropriate effect size for occupation of interest (if occupation was a moderator in the original MA)	2	0	n/a	n/a
30. Cited appropriate effect size for employed measure	0	1	1	n/a
31. Cited appropriate effect size for selected sample	2	0	1	n/a
32. Cited appropriate effect size for chosen criterion	3	1	1	n/a
33. Cited appropriate effect size for job complexity level	n/a	0	n/a	n/a
34. Cited appropriate effect size for research design	n/a	0	n/a	n/a
35. Cited appropriate effect size for firm size	n/a	n/a	n/a	0
36. Cited appropriate effect size for firm performance indicator	n/a	n/a	n/a	0
37. Cited appropriate effect size for board composition indicator	n/a	n/a	n/a	0

Note. MA = meta-analysis; MAES = meta-analytic effect size estimate; In the analysis of studies citing Barrick and Mount (1991), 11 subsequent studies were conceptual papers, meta-analyses, qualitative studies, or simulations. This was also the case for 13 papers citing the Judge et al. (2001) meta-analysis, 8 studies citing the Kish-Gephart et al. (2010) meta-analysis, and 18 studies citing the Dalton et al. (1998) meta-analysis.

Table 2

Recommendations for Interpreting and Reporting Meta-Analytic Findings

Recommendations for Readers	Wording Suggestions
1. Become familiar with the various methodological techniques available to address heterogeneity, moderation, publication bias, interval estimation, and psychometric adjustments.	
2. Instead of treating meta-analytic results similarly to NHST (i.e., limiting the focus to the presence of absence of an overall relationship), reference and interpret the MAES alongside any relevant qualifying information.	“Meta-analytic evidence by [author names of the original MA] shows a relationship between [IV] and [DV] of [$\bar{r} = XX$ or $\rho = YY$] ([boundaries of the confidence or the credibility interval]).”
3. Consider the type, width, and confidence level of credibility and confidence intervals when determining whether or not to consider an MAES a robust estimate of a relationship.	“The width of the confidence interval suggests...” [determine robustness of the relationship by evaluating the degree of potential variance in observed scores included in the confidence interval]”
4. Examine meta-analytic Method and Results sections for inclusion criteria and publication bias analyses when determining whether or not meta-analytic results are accurate.	“A publication bias analysis [name type of analysis employed in the original MA] revealed/did not reveal meaningful effect size differences between published and unpublished studies, indicating that effect sizes in published studies were on average [XX%] larger/smaller than in unpublished studies. This indicates...”
5. Become familiar with the quality of included primary studies in an effort to determine whether or not meta-analytic results are trustworthy.	“An examination of a subsample of the primary studies included in the MA by [author names] reveals that several of the included studies show very low design quality as evidenced by low measurement reliability [or other characteristics].”
6. Interpret unadjusted MAES estimates for practical applications and adjusted MAES estimates when considering theoretical relationships.	“Meta-analytic evidence by [author names of the original MA] shows a relationship between [IV] and [DV] of [$\bar{r} = XX$ or $\rho = YY$]

	<p>(<i>boundaries of the confidence or the credibility interval</i>)).”</p>
7. Consider moderation analyses reported in the MA and report finding from the one that is most relevant to your situation, research, or theory.	<p>“Consistent with the focus of our study on [<i>variable, occupation, measurement approach, or something else</i>], the MA by [<i>author names</i>] reports a relationship between [<i>IV</i>] and [<i>DV</i>] of [$\bar{r} = XX$ or $\rho = YY$] (<i>boundaries of the confidence or the credibility interval</i>) for [<i>specific level of moderator</i>].”</p>
8. Consider all of the results reported in an MA to determine whether an omnibus MAES, moderator-specific MAES, or primary study effect size is most relevant and appropriate.	<p>“While [<i>MA citation</i>] found an overall effect of [$\bar{r} = XX$ or $\rho = YY$] (<i>boundaries of the confidence or the credibility interval</i>), their moderation analysis suggested that, in [<i>current study setting or sample</i>], the effect was somewhat lower/higher [$\bar{r} = XX$ or $\rho = YY$] (<i>boundaries of the confidence or the credibility interval</i>). Therefore, we expect...”</p>
9. Consider the characteristics of primary studies included in the MA and whether the primary studies are representative of the literature and situations to which you wish to generalize.	<p>“An examination of a subsample of the primary studies included in the MA by [<i>author names</i>] reveals that the included studies show strong/limited similarity to the study setting of the current paper.”</p>
10. Use meta-analytic results to inform future research by searching for potential boundary conditions or gaps in the existing literature.	<p>“The heterogeneity analyses reported in the MA by [<i>author names</i>] suggests the existence of moderators of the relationship between [<i>IV</i>] and [<i>DV</i>].”</p> <p>“While [<i>author names</i>] found [<i>M1</i>] and [<i>M2</i>] to moderate the relationship between [<i>IV</i>] and [<i>DV</i>], further heterogeneity analysis suggests that additional moderators may affect this relationship.”</p> <p>“The MA by [<i>author names</i>] relies predominantly on studies that have measured [<i>variable name</i>] with</p>

	measures [m]. Measure [m] has been shown to have [<i>the following</i>] weaknesses that are relevant for the given study context. In the current study, we want to examine the relationship between [IV] and [DV], using a different measure [m other].”
Recommendations for Meta-analysts	Wording Suggestions
11. Instead of focusing on the omnibus MAES, focus the Results and Discussion sections around the complexities surrounding the meta-analyzed relationship.	<p>“The overall effect was moderated by [<i>construct, method, or context</i>]. The effect seems to be strongest in [<i>moderator level A</i>] ($\bar{r} = XX$ or $\rho = YY$), [<i>boundaries of the confidence or the credibility interval</i>])) and weakest in [<i>moderator level B</i>] ($\bar{r} = XX$ or $\rho = YY$), [<i>boundaries of the confidence or the credibility interval</i>)).”</p> <p>“Our results suggest that the relationship between [IV] and [DV] is not consistent across settings, but varies from [<i>boundaries of the confidence or the credibility interval</i>] in [<i>setting 1</i>] and [<i>boundaries of the confidence or the credibility interval</i>] in [<i>setting 2</i>].”</p>
12. When relationships are moderated, incorporate theory into the Discussion to provide meaningful explanations for the potential reasons underlying differences in samples, research designs, measures, or other contextual factors.	“Our results suggest that the relationship between [IV] and [DV] is strongest in [<i>setting</i>]. This is consistent with [<i>theory</i>] which suggests that [<i>setting</i>] is associated with ...”
13. Leverage the strengths of an MA's literature search to highlight potential future research areas.	“When conducting our literature search, we noticed that no studies have been conducted in [<i>setting</i>]. [<i>Theory</i>] suggests that [<i>relationship</i>] should apply to [<i>setting</i>], so we encourage future researchers to explore this phenomenon in this population.”

	“The effect of [IV] on [DV] is stronger in [setting 1] than [setting 2], but our analysis in [setting 1] is only based on [K] studies and [N] participants. We encourage future research to continue to investigate [relationship] in [setting 1].”
Recommendations for Editors and Reviewers	Wording Suggestions
14. Continue to encourage meta-analysts to incorporate detailed analyses such as publication bias and moderation.	“[Theory] specifies that [suspected moderator] should influence the relationship between [IV] and [DV]. Please conduct a meta-analytic moderation analysis to investigate this moderation effect.”
15. Encourage the inclusion of multiple statistics (e.g., both adjusted and unadjusted MAESs, both confidence and credibility intervals) to facilitate the dissemination of results that are relevant to multiple audiences.	“We appreciate your inclusion of the lower bound of an 80% credibility interval. We suggest also reporting and interpreting a 95% confidence interval computed around the unadjusted $\bar{r}$ value to provide an alternative perspective for readers.”
16. Encourage researchers who cite meta-analytic results to focus on detailed analyses instead of omnibus relationships when appropriate.	“While the use of [MA citation] was appropriate, we suggest you interpret the [moderated MAES] on [page number] as it is more closely associated with your study characteristics.”
17. Instead of discouraging researchers from pursuing additional research in an area that has been meta-analyzed, encourage researchers to incorporate meta-analytic results into the design of subsequent models, theories, and analyses.	“[Relationship] has already been meta-analyzed by [citation]. The authors of this MA suggested that future research in this area focus on [setting/sample] to further contribute to our understanding of [relationship].”

Online Supplement A

Information about Meta-analysis and Meta-analytic Methodology

Assumptions Underlying Meta-analytic Thinking

The logic underlying meta-analysis (MA) is similar to the logic underlying classical test theory (CTT) in which an observed relationship is modeled as a function of a true relationship as well as error (Borenstein, Hedges, Higgins, & Rothstein, 2009). In a measurement context, this error represents construct-irrelevant variance (Messick, 1995) that can include influences such as response sets, test fatigue, testing conditions, and even fluctuations in luck (Thorndike, 1947). In MA, error can result from unmeasured contextual influences (e.g., moderators) or statistical artifacts such as sampling bias (Hunter, Schmidt, & Jackson, 1982). Both CTT and MA assume that error is randomly distributed. Therefore, averaging over many measurements or observations should reduce or eliminate this error. As a result, psychometricians prefer measures with many items and meta-analysts prefer MAs with many primary studies. The combination of primary studies serves to “average out” sources of error such as unrepresentative samples or context effects that may bias effect size estimates in an individual primary study. By combining the results of many primary studies to account for systematic biases, the meta-analytic effect size (MAES) represents an attempt to quantify a relationship as accurately as possible.

MA and CTT also share the assumption of self-containment, or the absence of unmeasured variables that systematically bias the estimate in a specific direction (James, Mulaik, & Brett, 1982). The existence of such variables yields nonrandom error terms, which then yield inaccurate or misleading effect size estimates. Whether substantive or methodological, violations of self-containment cannot be “averaged out.” By citing or interpreting the omnibus MAES

estimate, readers are tacitly ignoring potentially important between-study differences and treating them as if they were random error as opposed to meaningful systematic variance.

Although meta-analytic results may be more generalizable than the results of a single primary study, the generalizability of an MA is still limited. Recent research has questioned the types of inferences that can be made on the basis of meta-analytic results. Murphy (2017) notes that researchers should take breadth, quality, and effect size homogeneity into consideration when drawing conclusions. Tett, Hundley, and Christiansen (in press) claim that 95% of MAs include effect sizes with sufficient heterogeneity to render generalizability implausible. The inferences that can be drawn from meta-analytic results are a function of the studies included in the MA. Accordingly, reporting standards clearly specify the need for MA authors to describe their process of searching for primary studies and describe and justify their inclusion and exclusion criteria (APA, 2008; Moher, Liberati, Tetzlaff, Altman, & PRISMA, 2009)

The generalization of an MAES relies on the assumption that primary studies randomly sample from the universe of possible study characteristics (such as measures, samples, and settings). The intent of the meta-analyst is irrelevant in this endeavor; just as many primary studies must rely on available or obtainable data (as opposed to true random sampling), meta-analysts are only able to draw from accessible primary studies. If a particular study characteristic is overrepresented in the literature, it will have a disproportionate influence on the calculation of the MAES. For example, if 90% of the analyzed primary studies and the meta-analytic sample size are conducted using student samples, the MAES may not adequately reflect what researchers should expect in applied settings. Our earlier suggestion to focus on moderator-specific MAES values addresses this issue to some extent, but even within moderated MA there may be some study characteristics that are either underrepresented or not represented at all.

When unmeasured variables systematically bias results, aggregating those results only serves to exacerbate the bias. The only solution to violations of self-containment involves modeling additional variables in an effort to reduce systematic bias in error terms and yield more accurate estimates. In MA, this is consistent with the calculation of heterogeneity and search for moderators. By accounting for differences in primary study characteristics, MA can treat these characteristics as meaningful phenomena instead of error.

A Brief Note about Fixed-effect and Random-effects MA

Of the four MAs we analyzed, only Kish-Gephart, Harrison, and Treviño (2010) includes both fixed-effect and random-effects estimates. However, the distinction between fixed-effect and random-effects MA is relevant to the calculation of an MAES as well as many of the meta-analytic methods referenced in this paper. For example, the random-effects model is associated with wider confidence and credibility intervals than the fixed-effect model. Fixed-effect MA assumes homogeneity of the underlying relationship across settings. Specifically, fixed-effect MA assumes that there is one true population effect size and all departures from that effect size represent error (e.g., differences in settings, samples, measures, or influence of statistical artifacts). In fixed-effect MA, the MAES represents an estimate of this one true effect size. Random-effects MA assumes a distribution of underlying true effect sizes that may be influenced by study characteristics (i.e., moderators) or statistical artifacts (Borenstein, Hedges, Higgins, & Rothstein, 2010). In a random-effects MA, the MAES is an estimate of the mean of this distribution. While both models are still used, fixed-effect MA has largely fallen out of favor with organizational researchers (Aguinis, Dalton, Bosco, Pierce, & Dalton, 2011), as it assumes that all primary studies included in an MA are strict replications of one another (Schmidt, Oh, & Hayes, 2009).

Types of Intervals

It is common for meta-analysts to report a confidence interval, credibility interval, or both around an MAES (Whitener, 1990). The width of these intervals communicates uncertainty about the size and/or direction of the MAES. *Confidence intervals* are a function of the MAES and its standard error. *Credibility intervals* are similar, but they adjust the MAES for artifacts (such as measure unreliability and range restriction) prior to calculating the interval.

Confidence and credibility intervals are not interchangeable, though many researchers interpret them as if they were. Confidence intervals are computed using the formula $MAES \pm (Z_{1-\alpha} * se_{MAES})$, where $Z_{1-\alpha}$ refers to the value of the Z-distribution associated with a particular level of confidence (e.g., $Z_{1-\alpha} = 1.96$ for a 95% confidence level) and se_{MAES} is the standard error of the MAES. This mirrors the formula used to compute confidence intervals around effect sizes in a primary study. By estimating the variance in effects attributable to sampling error, confidence intervals serve as an estimate of the accuracy of the MAES.

Credibility intervals are different from confidence intervals in two major ways. First, the MAES is adjusted for statistical artifacts such as measure unreliability and range restriction. Second, the standard deviation of the adjusted MAES is used in place of the standard error. By providing an estimate of the variance in effects after accounting for artifacts, credibility intervals serve as an estimate of the distribution of effect sizes (Schmidt & Hunter, 2015; Whitener, 1990), and allow for a more theoretical interpretation.

Prediction intervals (not used in any of the four MAs we analyzed) are similar to confidence intervals in computation, but replace the Z distribution with a t distribution with $K-1$ degrees of freedom (e.g., $t_{1-\alpha} = 2.05$ for a 95% confidence level when $K = 30$). This change allows for a more practical interpretation. By estimating the variance in effects after accounting

for sampling error, prediction intervals serve as an estimate of the expected upper and lower bounds for effect sizes computed in subsequent primary studies (Borenstein et al., 2009).

Considering the Quality of Primary Studies

Although study quality is routinely considered in MAs conducted in certain fields (e.g., epidemiology, medicine), it is largely ignored in the organizational sciences (Aguinis, Pierce, Bosco, Dalton, & Dalton, 2011). Proponents of quality assessment argue that including low-quality studies detracts from the trustworthiness of meta-analytic results (Eysenck, 1994). However, assessment of research quality is often subjective and rarely transparent (Ioannidis, 2006). Researchers tend to disagree about which aspects of a study should be considered when evaluating study quality (Moher et al., 1995). Research quality assessment may involve considerations of sampling strategy, sample size, research design, and measurement. Many issues remain unresolved, including which aspects to consider, how to meaningfully combine them, and whether standards should differ by research area.

There is much debate about the merits of grey literature and unpublished research (Balk et al., 2002; Chalmers, 1991; Coyne, Hagerdoorn, & Thombs, 2011). Unpublished studies tend to be lower in quality than published studies (Egger, Jüni, Bartlett, Hoenstein, & Sterne, 2003). Further complicating matters, study quality may be negatively related to effect size magnitude (Moher et al., 1998). This renders the effects of publication bias intractable, as an unpublished primary study may have been rejected on the basis of either a small effect size or poor quality (which may be associated with increased effect size). In other words, the inclusion of unpublished studies may attenuate the MAES in some cases and inflate the MAES in others.

Best evidence synthesis (Slavin, 1986, 1995) proposes a compromise between universal inclusion and quality assessment in which meta-analysts specify quality-based inclusion criteria

before searching for primary studies. Meta-analysts then include all published, grey, and unpublished literature that meets these *a priori* quality standards. Hunter and Schmidt (2004) critique best evidence synthesis on the basis that no primary study is without flaws and that the exclusion of studies on the basis of quality lowers the number of effect sizes available for analysis. Hunter and Schmidt also express doubts that study quality is related to study results, endorsing universal inclusion. Given that most meta-analysts rely on the Hunter and Schmidt methodology (Aguinis, Dalton, et al., 2011), it is unsurprising that best evidence synthesis is rarely, if ever, used in organizational research.

Detecting Publication Bias

Publication bias is partially a function of an MA's inclusion criteria. Some authors have suggested that all obtainable studies should be analyzed, including published articles, "grey literature" such as dissertations or conference papers, and unpublished articles (Borenstein et al., 2009; Schmidt & Hunter, 2015). By including every study, regardless of prestige or publication status, publication bias can be assessed and perhaps even mitigated. However, by including every study, meta-analysts risk including questionably relevant or low-quality studies (Eysenck, 1994; Ioannidis, 2006; Pillemer, 1984; Slavin, 1984). In these cases, studies with unrepresentative samples or unvalidated measures may be weighted as heavily as methodologically sound research. A contrarian approach to primary study inclusion encourages meta-analysts to consider primary study quality as either an inclusion criterion or moderating effect.

There are a number of techniques that may be used to detect or correct publication bias. Rosenthal's (1979) *failsafe N* calculates the number of studies with null results that would be required to conclude that a meta-analytic result could be primarily ascribed to publication bias. Second, *drift analysis* (also known as *cumulative MA*) plots primary study effect sizes against

sample sizes (*N drift*) or date of publication (*temporal drift*) to determine whether effect sizes vary with publication year or study power (see Banks, Kepes, & McDaniel, 2012). Third, *funnel plots* and *contour lines* plot effect sizes by either standard error or variance to identify asymmetry in published results, which may indicate publication bias (Peters, Sutton, Jones, Abrams, & Rushton, 2008). Relatedly, when funnel plots suggest the presence of publication bias, *trim-and-fill* analysis (Duval & Tweedie, 2000) may be used to impute studies in an effort to estimate a hypothetical bias-free MAES.

Readers should be aware that although failsafe *N* and drift analysis may be used to detect publication bias, neither technique offers a solution to the problem. Trim-and-fill can be used to “correct” publication bias, but it relies on two very stringent (and untestable) assumptions that (a) only studies with nonsignificant effect sizes are suppressed from the literature and (b) there exist unpublished (and unobtainable) studies with identical study characteristics to the most extreme published effect sizes (e.g., sample size, measures used) and opposing results. It is noteworthy that less popular methods of detecting and modeling publication bias exist (e.g., Egger, Smith, Schneider, & Minder, 1997; Hedges, 1992). Each of these methods is also intended to detect, but not to correct for publication bias. Although publication bias may limit the accuracy of an MAES, any solution to this problem paradoxically requires the acquisition of unobtainable primary studies. As such, it is unlikely that a universally acceptable solution will ever exist.

Methods for Identifying Primary Study Heterogeneity

The MA methodology literature has introduced several methods to quantify heterogeneity. One of the first methods, known as the “75% rule,” suggests that if statistical artifacts (e.g., sampling error, range restriction, or measure unreliability) can account for at least

75% of the variance in primary study effect sizes then there is not a sufficient level of substantive heterogeneity across primary studies to justify meta-analytic moderation (Schmidt, Hunter, & Pearlman, 1982). Although the 75% rule has been largely discredited due to potential overlap between moderators and artifacts (James, Demaree, & Mulaik, 1986), many MAs still employ this method (Cortina, 2003).

Cochran's (1954) X^2 -based Q statistic tests the hypothesis that all primary study effect sizes are equal (Hedges & Olkin, 1985). The Q statistic is also popular despite its tendency to be too conservative when the number of primary studies (K) is small and too liberal when K is large. Three alternative statistics (H^2 , R^2 , and I^2) can be computed using factors such as Q , K , between-study variance, and within-study variance. Specifically, H^2 compares Q to its degrees of freedom, R^2 is a comparison of confidence interval widths for fixed-effect and random-effects models, and I^2 is an estimate of the proportion of effect size variance accounted for by heterogeneity between primary studies (Higgins & Thompson, 2002). I^2 seems to outperform H^2 , R^2 , and Q (Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006) for the detection of heterogeneity. Finally, some meta-analysts directly report between-studies variance (τ^2 ; see Borenstein et al., 2010).

When heterogeneity is detected, it is common for MAs to include moderator analyses. Meta-analytic moderation can be assessed using subgroup analysis (i.e., reporting a separate MAES for each level of each moderator) or meta-regression (Aguinis & Pierce, 1998; Schmidt, 2017). It is common for both contextual (e.g., sample, setting) and methodological factors (e.g., measures, research design) to serve as moderators. Since moderating effects may not be independent, meta-analysts may elect to analyze combinations of moderators, though such analyses are rare. These combinations yield MAES estimates that apply to more specific

situations. However, combining moderator analyses also reduces the number of primary studies on which each MAES is based, which can undermine confidence in these estimates (Hunter & Schmidt, 2004).

Meta-analytic Structural Equation Modeling

Meta-analytic structural equation modeling (MASEM, see Bergh et al., 2016) has increased in prevalence over the past decade. By leveraging the advantages of MA (i.e., combining evidence across a range of primary studies as opposed to relying exclusively on data collected in a single primary study), researchers can test structural models in new and powerful ways. In these cases, however, it is critical that researchers attend to the specific results reported in MAs for a number of reasons. First, as noted by others, the presence of heterogeneity negates the utility of MASEM. As Sheng, Kong, Cortina, and Hou (2016, p. 190) note, ‘If there appears to be heterogeneity in effect sizes or if significant moderators have been identified in the meta-analysis, a single correlation value is no longer appropriate for a given cell.’ Failing to account for heterogeneity may result in the inability to generalize the SEM results (Yu, Downes, Carter, & O’Boyle, 2016). Additionally, it is important to consider whether to use adjusted or unadjusted MAES estimates in the MASEM model as the meta-analytic techniques used to adjust for measure unreliability are similar to those used when moving from manifest to latent variables in SEM (LeBreton, Scherer, & James, 2014).

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