Obtaining Unbiased Results in Meta-Analysis: The Importance of Correcting for Statistical Artefacts

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Abstract

Most published meta-analyses address only artefactual variance due to sampling error and ignore the role of other statistical and psychometric artefacts, such as measurement error (due to factors including unreliability of measurements, group misclassification, and variable treatment strength) and selection effects (including range restriction/enhancement and collider biases). These artefacts can have severe biasing effects on the results of individual studies and meta-analyses. Failing to account for these artefacts can lead to inaccurate conclusions about the mean effect size and between-studies effect-size heterogeneity, and can influence the results of meta-regression, publication bias, and sensitivity analyses. In this paper, we provide a brief introduction to the biasing effects of measurement error and selection effects and their relevance to a variety of research designs. We describe how to estimate the effects of these artefacts in different research designs and correct for their impacts in primary studies and meta-analyses. We consider meta-analyses of correlations, observational group differences, and experimental effects. We provide *R* code to implement the corrections described.

Keywords

psychometric meta-analysis; measurement error; reliability; range restriction; range variation; selection bias; collider bias

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Meta-analysis is a critical tool for increasing the rigor of research syntheses by increasing confidence that apparent differences in findings across samples are not merely attributable to statistical artefacts (Schmidt, 2010). However, most published meta-analyses are concerned only with artefactual variance due to sampling error and ignore the role of other statistical and psychometric artefacts, such as measurement error (stemming from factors including unreliability of measurements, group misclassification, and variable treatment strength) and selection effects (including range restriction/enhancement and collider bias). In this paper, we provide a brief introduction to the biasing effects of measurement error and selection effects and their relevance to a variety of research designs. We

describe how to estimate the effects of these artefacts in different research designs and how to correct for their impacts in meta-analyses. We consider meta-analyses of correlations, observational group differences, and experimental effects.

As noted above, most published meta-analyses are concerned only with sampling error and do not correct for other statistical artefacts. For example, of the 71 meta-analyses published in *Psychological Bulletin* during 2016–2018, only 6 made corrections for measure-

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ment error and only 1 corrected for selection biases (a similar review by Schmidt, 2010, found similarly low rates of corrections for statistical artefacts). Corrections for measurement error are commonly applied in industrial–organizational (I–O) psychology meta-analyses, but rarely in other psychology subfields or other disciplines (Schmidt & Hunter, 2015; Schmidt, Le, & Oh, 2009). Corrections for selection effects are even rarer. They are typically only performed in meta-analyses of personnel selection and educational admissions research (Dahlke & Wiernik, 2019a; Sackett & Yang, 2000). Measurement error and selection artefacts have severe biasing effects on the results of individual studies and meta-analyses. Failing to account for these artefacts can lead to inaccurate conclusions about the mean effect size and between-studies effect-size heterogeneity, and can influence the results of meta-regression, publication bias, and sensitivity analyses. When measurement error and selection effects are considered in meta-analyses outside of I–O psychology, they are often treated simply as indicators of general “study quality” and used as exclusion criteria or as moderators (Schmidt & Hunter, 2015, p. 485); both approaches are suboptimal, as measurement error and selection effects have predictable effects on study results. The best way to handle measurement error and selection effects is to apply statistical corrections that account for the known impacts of these artefacts. Below, we describe the impacts of measurement error and selection effects on primary studies and meta-analyses, as well as methods to correct for these artefacts.

Measurement Error

Measurement error is an artefact that causes observed (i.e., measured) values to deviate from the “true” values of underlying latent variables (Schmidt & Hunter, 1996).¹ For example, consider a political psychologist assessing political orientation using a 10-item measure with items rated on a 7-point scale. A respondent might obtain a mean score of “5” (somewhat conservative) or “3” (somewhat liberal) across the 10 items, when their true score is in fact “4” (moderate/centrist). Measurement error is also called unreliability (Rogers et al., 2002), observational error (Walter, 1983), and information bias (Alexander et al., 2014a). For continuous variables, measurement error is also called (low) precision (Stallings & Gillmore, 1971); for dichotomous or

grouping variables, measurement error is also called misclassification (Alexander et al., 2014a).

Measurement error can come from a variety of sources, including truly random response errors (e.g., momentary distractions, arbitrary choices between adjacent scale points), transcription errors, transient/temporal effects (e.g., performing poorly on a cognitive assessment due to fatigue on a particular day), environmental effects, content sampling effects (i.e., the specific items or content used in the measure do not function the same way as possible alternative items or content), rater effects (e.g., differential knowledge/motivation or idiosyncratic beliefs across raters), and low sensitivity or specificity of measurement instruments, among others. These diverse sources of error can be grouped into four “classical” categories (Cronbach, 1947; Schmidt et al., 2003); see Table 1 for descriptions. Importantly, measurement error is not only about whether participants’ responses were correctly recorded, but more about the process that generated the responses and whether the responses would be the same if the process were repeated at different times, by different raters, or with a different instrument or item set (Schmidt & Hunter, 1996). For psychological measures, random response error and transient error are typically the largest sources of measurement error (Ones et al., 2016).

The amount of measurement error in a sample of scores is quantified using a reliability coefficient, defined as the proportion of the observed-score variance that is consistent (i.e., believed to be “true”), or one minus the proportion of the observed-score variance attributable to measurement error:

$$r_{xx'} = \frac{\sigma_{true}^2}{\sigma_{obs}^2} = 1 - \frac{\sigma_{error}^2}{\sigma_{obs}^2} \quad 1$$

Conceptually, the reliability coefficient is the correlation between two parallel measures of a construct. The

¹ In classical test theory, an individual’s “true score” is the expected value of the individuals’ response to a measurement process that has been repeated an infinite number of times, such that all errors of measurement average out to zero across observations. That is, the “true score” is the score on the measure without any measurement error. The “true score” does not necessarily correspond to the individuals’ standing on the intended latent construct—that is a question of measure validity, not reliability. If a measure has poor validity as an indicator of the intended construct, even correlations corrected for measurement error will poorly reflect correlations with the latent construct variable (for discussions, see Borsboom, 2006; Schmidt et al., 2013).

Table 1. Four classical sources of measurement error and reliability estimators sensitive to each.

Source of error	Description and examples
Random response error	Truly random error specific to each item/response (e.g., random fluctuations in response time, momentary lapses in attention); unique to each measurement <i>Reliability estimators:</i> – All reliability estimators
Transient error	Error due the specific time or environment in which data are gathered (e.g., participant mood, environmental distractions); shared by measures completed within a meaningfully short time span <i>Reliability estimators:</i> – Test-retest reliability (coefficient of stability) – Delayed parallel forms reliability (coefficient of equivalence and stability) – Delayed internal consistency (e.g., test-retest alpha; Green, 2003)
Content sampling error (item/test specific factor error)	Error due to specific content used on a measure (e.g., interpretations of specific test items); shared by measures with the same or highly similar content <i>Reliability estimators:</i> – Parallel forms reliability (coefficient of equivalence) – Delayed parallel forms reliability (coefficient of equivalence and stability) – Internal consistency (e.g., α , ω , split-half reliability, IRT-based reliability estimates) – Delayed internal consistency (e.g., test-retest alpha; Green, 2003)
Rater sampling error (rater specific factor error)	Error due to the specific raters used to gather data; including both rater main effects (e.g., idiosyncratic beliefs, mood, leniency) and rater $\times$ ratee interactions (e.g., differences in opportunities to observe); shared by measures with the same rater <i>Reliability estimators:</i> – Interrater reliability (see Putka et al., 2008, for a discussion of estimators) – Delayed interrater reliability

square root of the reliability coefficient ($q_x = \sqrt{r_{xx'}}$; also called the measurement quality index; Schmidt & Hunter, 2015) is the correlation between the measured (observed) variable and its underlying latent variable. This relationship is illustrated in Figure 1.²

²Measurement error can include both systematic and random components. Systematic error (also called *bias*) affects each score in the same manner (e.g., consistent underestimates) and generally refers to the mean error across persons. Random error affects each score differently and refers to the variance of the errors across persons. Typically, only the error variance, not the mean error, affects standardized effect sizes such as correlations and Cohen's *d*. However, if a measure is differentially biased across groups or score ranges, these errors can also impact correlations or *d* values. The methods described in this article assume that errors are uncorrelated with true scores on the included variables, and they will not correct for such systematic errors (see the section "When Should You Correct for Measurement Error?" below for more discussion).

At the level of individual scores, random measurement error cannot be corrected, as the effect on a specific score is unknown; the best that can be done is to place a confidence interval around a score by using the reliability coefficient and the standard deviation of observed scores to estimate the standard error of measurement (Revelle, 2009). However, when scores are aggregated in data sets and are used to compute correlations between two variables or to compare scores across groups, measurement error has a known, predictable impact of biasing the effect size toward the null (i.e., toward zero). For example, consider the case shown in Figure 2. A researcher is studying the relationship between intentions to recycle and actual recycling behavior, both measured using self-reports. If the true correlation between these latent constructs is $\rho = .50$, but the reliability of each measure is $r_{xx'} = .80$, then the expected observed correlation is only $r_{xy} = .40$.

Fig. 1. Structural model illustrating the relationship between measures and their underlying latent variable, along with the reliability coefficient ($r_{xx'}$) and measure quality index ($\sqrt{r_{xx'}}$ or q_x).

The expected observed correlation underestimates the true correlation between the latent constructs by 20%.³

Impacts of Measurement Error in Meta-Analysis

Measurement error will impact the results of meta-analyses in three ways: (1) biasing the mean effect size, (2) inflating effect size heterogeneity and confounding moderator effects, and (3) confounding publication bias and sensitivity analyses.

Mean effect size. When constructs are measured with error, the mean effect size in a meta-analysis of relations between measures of these constructs will be biased toward the null (i.e., toward zero for r or d values). The amount of this null-bias in the mean effect size is a function of mean reliability across studies. For example, if the true mean correlation between Neuroticism and life satisfaction across 30 studies is $\bar{\rho} = -.32$,

the average reliability for Neuroticism is $\bar{r}_{xx'} = .80$ ($\sqrt{r_{xx'}} = .89$), and the average reliability for life satisfaction is $\bar{r}_{yy'} = .70$ ($\sqrt{r_{yy'}} = .84$), then the expected mean correlation between measures of Neuroticism and life satisfaction will be only $\bar{r}_{xy} = \bar{\rho} \times \sqrt{r_{xx'}} \times \sqrt{r_{yy'}} = -.32 \times .89 \times .84 = -.24$, 25% smaller in magnitude than the true mean correlation between the latent constructs.

Effect size heterogeneity and moderator effects.

If the studies included in a meta-analysis differ in their measure reliabilities, estimates of the between-studies heterogeneity random-effects variance component (i.e., τ^2 , SD_{res} , SD_{ρ} , SD_{δ}) will be artefactually inflated, erroneously suggesting larger potential moderator effects. This is most serious if a moderator variable is correlated with measure reliability across studies. For example, a meta-analysis might compare the efficacy of exposure therapy and mindfulness practice for treating posttraumatic stress disorder (PTSD). If, in reality, these therapies are equally effective (e.g., $\bar{\delta} = .40$), but PTSD symptoms are measured more reliably in studies of exposure therapy ($\bar{r}_{yy'} = .90$) versus studies of mindfulness ($\bar{r}_{yy'} = .60$), this would falsely suggest that exposure therapy is more effective (observed $\bar{d} = .38$) than mindfulness (observed $\bar{d} = .31$).

Publication bias and sensitivity analyses. Measurement error can cause publication bias analyses to suggest the presence of bias when none exists. For example, consider a case where a meta-analyst compares published versus unpublished studies and finds that published studies have larger effect sizes (e.g., $\bar{r}_{xy} = .45$ versus $.30$). They might regard this finding as suggesting publication bias and conclude the published studies substantially overestimate the true relationship. However, if the published studies also have better reliability than the unpublished studies (e.g., mean reliability = $.90$ versus $.60$), an alternative explanation is that unpublished studies were rejected due to the poor quality of their measures, rather than due to bias against null findings. More seriously, accurate results from cumulative meta-analysis, trim-and-fill, PET-PEESE, selection models, and other publication bias procedures require that included studies have equal reliability or be corrected for measurement error (Carter et al., 2017; Schmidt & Hunter, 2015). If measure reliability is

³The expected effect of measurement error is to bias standardized effect sizes toward the null. However, this is only the *expected* (asymptotic) effect. When sample sizes are small, measurement error can sometime have smaller or larger than expected effects on observed effect size, or even make observed effect sizes larger, because sampling error can produce nuisance correlations between construct scores and measurement error scores (Stanley & Spence, 2014; Wacholder et al., 1995). In a meta-analysis with a sufficiently large number of studies, these random deviations from the expected null-bias effect will tend to average out to zero (Stanley & Spence, 2014; Wiernik & Ones, 2017), though low reliability can exacerbate upward-biasing effects of publication bias and questionable research practices in small samples (Loken & Gelman, 2017; Simmons et al., 2011).

Fig. 2. Impact of measurement error on correlation coefficients. Both the predictor (X) and criterion (Y) measures have reliabilities of $r_{xx} = .80$ ($q_x = .89$).

correlated with sample size, small studies may have larger effect sizes for reasons other than publication bias. For example, a meta-analysis might find that smaller- N studies show a larger relationship between personality traits and mental health outcomes. However, if these smaller studies used longer, more reliable personality scales, but the larger studies used less reliable, ultra-short scales (cf. Credé et al., 2012), then this small-sample effect is the result of differences in measurement quality, not publication bias. This effect is illustrated in Figure 3. Here, the funnel plot for observed correlations is asymmetric. Fitting PET and PEESE regression models (Carter et al., 2017) showed substantial standard error–effect size relations (PET: $b_{SEi} = 1.19$ [.53, 1.86], PEESE: $b_{vi} = 9.35$ [4.04, 14.66]). These results would typically be interpreted as indicating at least some effect size inflation due to publication bias or questionable research practices. Conversely, the funnel plot for corrected correlations is more symmetric, and the PET/PEESE models showed negligible standard error–effect size relations (PET: $b_{SEi} = .06$ [−.60, .73], PEESE: $b_{vi} = .62$ [−3.78, 5.01]).

Correcting for Measurement Error

As noted above, measurement error poses a serious threat to the accuracy of conclusions from primary

studies and meta-analyses. Researchers are well-advised to reduce these biases by using more reliable scales. However, measurement error can also be statistically corrected post-hoc to estimate the true relation between the latent constructs without measurement error. In the sections below, we discuss impacts of measurement error on correlations and group comparisons, and present methods for correcting for measurement error for different research designs.

Applying measurement error corrections will generally yield less biased estimates of construct relationships, particularly in meta-analyses, but corrections for measurement error do not come without a cost. Applying statistical corrections increases the sampling error in the corrected effect sizes, leading to increased standard errors and wider confidence intervals. It is always better to reduce measurement error by using more reliable measurement procedures during data collection, rather than relying on statistical corrections after the fact.

Correcting Measurement Error in Correlations

When a meta-analysis cumulates correlation coefficients, the correlation will be attenuated by measure-

Fig. 3. Funnel plots illustrating biasing effects of differential reliability on publication bias analyses. Vertical lines indicate mean effect sizes, dashed (95%) and dotted (99%) lines indicate confidence regions. Blue long-dashed lines are PET regression lines. Green dash-dotted lines are PEESE regression lines. True correlations between constructs have $\bar{\rho} = .50$ ($SD_{\rho} = .05$). Large studies at the top of each funnel plot (mean $N = 1000$, $SD_N = 50$) have low reliability ($\bar{r}_{xx'} = .50$, $SD = .10$). Smaller studies (mean $N = 100$, $SD_N = 10$) have high reliability ($\bar{r}_{xx'} = .80$, $SD = .10$). The construct true correlations plot shows a high degree of symmetry. The observed correlations (with measurement error) plot is asymmetric due to differential reliability, erroneously suggesting publication bias.

ment error in both variables. This is the case for both psychological scales (e.g., personality measures) and other variables (e.g., course grades, self-reported eating behavior, objectively-measured exercise behavior, clinician-rated symptoms, and even demographic variables, such as rounding errors in age, mis-marking a response, or adjusting one's response depending on whether or not one is allowed to endorse multiple races/ethnicities or a non-binary gender). The amount of attenuation is a function of the measure quality index:

$$r_{obs} = r_{true} \sqrt{r_{xx'}} \sqrt{r_{yy'}} \quad 2$$

To correct for this attenuation due to measurement error, divide the observed correlation by the product of the measure quality indices (Spearman, 1904):⁴

$$r_c = \frac{r_{obs}}{\sqrt{r_{xx'}} \sqrt{r_{yy'}}} \quad 3$$

This formula is analogous to estimating the correlation between latent variables in a structural equations model when the specified model has simple structure (Westfall & Yarkoni, 2016).

Correcting Measurement Error in Group Comparison Research

Measurement error in the dependent variable.

Measurement error is also an issue in group-comparison research (e.g., studies of gender differences or studies comparing people with and without a psychological diagnosis). In these studies, measurement error in the dependent variable (DV) has a similar effect on the standardized mean difference (Cohen's d) as on correlations. Cohen's d is calculated as:

$$d_{obs} = (Mean_1 - Mean_2) / \sqrt{\frac{(N_1-1)Var_1 + (N_2-1)Var_2}{N_1 + N_2 - 2}} \quad 4$$

$$d_{obs} = (Mean_1 - Mean_2) / \sqrt{Var_{pooled}} \quad 4a$$

where Var_1 and Var_2 are the variances for groups 1 and 2, respectively, and Var_{pooled} is the weighted average within-group variance.

⁴ R code implementing all the methods described in this paper is given in Appendix A.

Random measurement error in the DV does not impact the difference between group means, but it does increase the magnitude of the within-group variances:

$$Var_1 = Var_{true1} / r_{yy'1} \quad 5$$

$$Var_2 = Var_{true2} / r_{yy'2}$$

Note that dividing by $r_{yy'}$, a number less than 1, will increase the value of Var_{true} . These increased group variances cause the denominator of Equation 4 to be too large:

$$d_{obs} = \frac{(Mean_1 - Mean_2)}{\sqrt{\frac{(N_1-1)(Var_{true1}/r_{yy'1}) + (N_2-1)(Var_{true2}/r_{yy'2})}{N_1 + N_2 - 2}}} \quad 6$$

$$d_{obs} = \frac{(Mean_1 - Mean_2)}{\sqrt{Var_{truepooled} / r_{yy'pooled}}} \quad 6a$$

where $r_{yy'pooled}$ is the sample-size-weighted average within-group reliability coefficient for the DV. By increasing the size of the denominator used to standardize the d value, measurement error in the DV thus systematically biases d_{obs} toward zero as a function of DV measure quality:

$$d_{obs} = d_{true} \sqrt{r_{yy'pooled}} \quad 7$$

Measurement error in the DV can be corrected in a similar manner as correlations:⁵

$$d_c = d_{obs} / \sqrt{r_{yy'pooled}} \quad 8$$

If estimates of $r_{yy'pooled}$ or the within-group reliability coefficients are unavailable, measurement error in the DV can also be corrected using $r_{yy'}$, the reliability coefficient for the DV computed in the combined total sample. The total-sample reliability coefficient includes between-group variance, so it will generally be larger than the pooled within-group reliability coefficient, $r_{yy'} > r_{yy'pooled}$ (Waller, 2008). A three-step procedure is used to correct d using the total-sample $r_{yy'}$ (cf. Schmidt & Hunter, 2015). First, convert d to a point-biserial correlation coefficient:

$$r_{obs} = d_{obs} / \sqrt{\frac{1}{p(1-p)} + d_{obs}^2} \quad 9$$

where p is the proportion of the sample in one group. Second, correct r_{obs} using Equation 3: $r_c = r_{obs} / \sqrt{r_{yy'}}$.

Finally, convert r_c back to a d value:

$$d_c = r_c / \sqrt{p(1-p)(1-r_c^2)} \quad 10$$

This three-step approach is also used to correct d values for other statistical artefacts (see below).

Group misclassification. Measurement error can also be present in the independent variable in group comparisons. This occurs when individuals are misclassified as being a member of one group when they are actually members of another group (Wacholder et al., 1995). For example, in a study comparing coping behaviors for people with and without a specific disorder, some participants in the “no disorder” group may actually have the disorder but be undiagnosed. Similarly, in a study comparing substance users to non-users, some participants may be unwilling to respond honestly when asked about their substance use. The impact of misclassification on observed d values is illustrated in the simulated data in Figure 4. Here a misclassification rate of 20% reduced the magnitude of the group difference from a true value of $d = 1.01$ to an observed value of only $d = 0.72$, a 29.7% reduction. Expected magnitudes of attenuation in d values caused by differing degrees of misclassification and varying levels of DV reliability are shown in Table 2.

The impact of group misclassification on d values is a function of r_{gG} , the correlation between observed group membership (g) and actual group membership (G):⁶

$$d_{obs} = r_{gG} r_{obs} / \sqrt{p(1-p)(1-r_{gG}^2 r_{obs}^2)} \quad 11$$

where r_{obs} is the point-biserial correlation calculated using Equation 9.

⁵ All formulas in this paper apply equally to Hedges's g . For Glass's Δ , $r_{yy'}$ and u_y (if correcting for selection effects, see below) should be computed within the control group only.

⁶ In a minority of cases, if group members with the most extreme values are misclassified, misclassification can also have the effect of reversing the direction/sign of the effect size. This effect is illustrated in Appendix B. The frequency with which such sign changes occur increases as the proportion of cases misclassified grows. In individual studies, it is not possible to determine if such a sign reversal has occurred, but in meta-analysis, this can be addressed by cumulating studies using the weighted *median* (versus mean) d value and the weighted *median absolute deviation from the median* (versus standard deviation) of d values (cf. Lin et al. 2017).

Fig. 4. Impact of misclassification on d values. The misclassification rate is 20% (10% in each group). The dependent variable (Y) is measured without error.

Table 2. Magnitudes of attenuation of d values for varying proportions of group misclassification and varying levels of dependent variable reliability for a δ parameter of 1.00

DV reliability	Proportion misclassified ^a										
	.00	.10	.20	.30	.40	.50	.60	.70	.80	.90	1.00
1.00	1.00	.77	.56	.36	.18	.00	-.18	-.36	-.56	-.77	-1.00
.90	.94	.72	.53	.34	.17	.00	-.17	-.34	-.53	-.72	-.94
.80	.87	.68	.49	.32	.16	.00	-.16	-.32	-.49	-.68	-.87
.70	.81	.63	.46	.30	.15	.00	-.15	-.30	-.46	-.63	-.81
.60	.74	.58	.42	.28	.14	.00	-.14	-.28	-.42	-.58	-.74
.50	.67	.52	.39	.26	.13	.00	-.13	-.26	-.39	-.52	-.67
.40	.59	.46	.34	.23	.11	.00	-.11	-.23	-.34	-.46	-.59
.30	.51	.40	.30	.20	.10	.00	-.10	-.20	-.30	-.40	-.51
.20	.41	.32	.24	.16	.08	.00	-.08	-.16	-.24	-.32	-.41

Note. All values are relative to a true construct-level group difference of $\delta = 1.00$. Tabled values are observed d values. Magnitudes of attenuation of d values for varying proportions of group misclassification and varying levels of dependent variable reliability. Group sizes and misclassification rates are assumed equal.

^aproportion misclassified in total sample with equal treatment and control sample sizes and equal rates of misclassification in the treatment and control groups

To correct a d value for group misclassification, use the three-step procedure described earlier (cf. Schmidt & Hunter, p. 262). First, convert d_{obs} to r_{obs} using Equation 9. Second, correct r_{obs} using the same disattenuation formula described in Equation 3:

$$r_c = r_{obs} / r_{gG} \quad 12$$

Note that r_{obs} is divided by r_{gG} itself, not the square root of r_{gG} . This is because r_{gG} is analogous to the measure quality index (correlation between the measured variable and the latent construct). Third, convert r_c back to d_c using a variant of Equation 10:⁷

$$d_c = r_c / \sqrt{p^{true}(1 - p^{true})(1 - r_c^2)} \quad 13$$

where p^{true} is an estimate of the true group proportion (without misclassification). If p^{true} is unknown, the observed group proportion, p , can be used with the assumption that misclassification is equal across groups.

r_{gG} can be estimated by conducting a study to quantify the accuracy of observed group classifications. For example, in the case of diagnosis for a disorder, epidemiological data could be consulted to estimate rates of overdiagnosis and underdiagnosis of the disorder. For the example of substance users, self-report accuracy could be assessed using chemical drug tests for a subset of the sample (or in a similar external example). With this information, r_{gG} is computed as the phi coefficient between observed and actual group membership:

$$r_{gG} = \sqrt{\frac{\chi_{gG}^2}{N}} \quad 14$$

where χ_{gG}^2 is the chi-squared statistic for the 2×2 contingency table for group membership and N is the sample size. r_{gG} can also be computed directly as:

$$r_{gG} = \frac{TP - BR_{obs}BR_{true}}{\sqrt{BR_{obs}(1 - BR_{obs})BR_{true}(1 - BR_{true})}} \quad 15$$

where TP is the proportion of “true positives” (individuals correctly assigned to one of the two groups) in the total sample, BR_{obs} is the base-rate proportion of the sample observed to be part of that group, and BR_{true} is the base-rate proportion of the sample actually part of that group.

If the group sizes and misclassification rates are similar for both groups, this correlation can be approx-

imated using the proportion of misclassified individuals in the full sample:

$$r_{gG} \approx 1 - 2 \times (1 - \text{Accuracy Rate}) = 1 - 2 \times \text{Proportion Misclassified} \quad 16$$

When the group sizes or misclassification rates for treatment control groups are uneven, using Equation 16 will overestimate r_{gG} and thus undercorrect the d value for misclassification. This undercorrection can be severe in cases when the group sizes differ greatly (Chicco, 2017). In these cases, Equations 14 or 15 will be more accurate.

Correcting Measurement Error in Experimental Research

The same principles described above for observational research apply to group differences calculated in experimental and treatment research. Measurement error in both the manipulation/intervention and the dependent variable will attenuate observed effect sizes.

Measurement error in the dependent variable. In experimental research, dependent variable constructs can be measured in a variety of ways, including responses to scales or questionnaires, observers’ ratings of participants’ behaviors, or performance on trials of behavioral tasks. Each of these methods can produce multiple types of measurement errors that can be accounted for by specific types of reliability coefficients.

Scales or questionnaires. If the DV is participants’ responses to a scale or questionnaire, reliability can be estimated using internal consistency (α , ω , IRT-based reliability, etc.), parallel forms reliability, or test-retest reliability for the scale, depending which sources of measurement error are most important (see the section on Choosing an Appropriate Reliability Estimate, below). To correct for this unreliability, calculate the reliability coefficient within each group, then compute the sample-size-weighted average within-group reliability, $r_{yy'pooled}$. Finally, correct the d value for the experimental effect using $r_{yy'pooled}$ in Equation 8. Alternatively, correct using the total-sample reliability, $r_{yy'}$, by converting the d value to r , applying the correction, and converting back to d , as shown in Equations 9–10.

Multiple trials. In experiments where participants complete multiple trials of a task (e.g., several trials in a

⁷ Simulation results showing the accuracy of this procedure are shown in Appendix B.

Stroop task) and group comparisons are made on subjects' average performance across tasks, the key source of measurement error is random response error (Cooper et al., 2017). For example, a participant may randomly have a somewhat longer reaction time on one trial versus the next. For these designs, reliability can be estimated using Coefficient α (treating each trial as an item) or the Spearman-Brown-corrected split-half reliability, r_{SBSH} :

$$r_{SBSH} = (2 \times r_{h1h2}) / (1 + r_{h1h2}) \quad 17$$

where r_{h1h2} is the correlation of a participant's mean on one half of the trials with their mean on the other half of the trials. r_{SBSH} is useful because it can be calculated if participants complete varying numbers of trials (e.g., if only correct trials are retained). r_{SBSH} is also appropriate if participants complete trials for the same stimuli in different orders and order/fatigue effects are a concern. Values of r_{SBSH} can vary depending on how trials are split (e.g., odd-even, first half-last half, etc.), so r_{SBSH} should be calculated by creating a large number of random splits (e.g., 5000), calculating r_{h1h2} for each split, then using the average r_{h1h2} across splits to compute r_{SBSH} . The *splithalf* package in R (Parsons, 2017/2018) provides functions to facilitate calculating r_{SBSH} for multi-trial data.

If the DV is the difference between participants' average performance in two conditions (e.g., congruent versus incongruent trials), then reliability must be estimated for these difference scores, not for the individual condition averages. Generally, difference scores are much less reliable than a single average score (Williams & Zimmerman, 1977). To calculate r_{SBSH} for difference scores, divide the trials for each condition in half and calculate difference scores using these half-sets of trials (i.e., $D_{h1} = A_{h1} - B_{h1}$, where A_{h1} is a participant's mean performance on half the trials in Condition A, B_{h1} is the participant's mean performance on half the trials in Condition B, and D_{h1} is the participant's difference score for half of the trials). Calculate r_{h1h2} as the correlation between the difference scores for the two halves of the trials and enter this value in Equation 17.

To correct for measurement error in the experimental d value, calculate the reliability coefficient (α or r_{SBSH}) within each experimental group, then compute the sample-size-weighted average within-group reliability, $r_{yy'pooled}$. Finally, correct the d value for the experimental effect using $r_{yy'pooled}$ in Equation 8.

Single outcomes. If the dependent variable in an experiment is performance on a single-trial behavioral task (e.g., the length of time participants persist in attempting to solve unsolvable anagrams; whether a participant donates to a charity; the amount of material recycled by an office during a month), a key question is how consistent performance on this task would be even if the treatment condition did not change. If performance is inconsistent (i.e., there is a high degree of random response and transient error), this will underestimate the effect of the experimental manipulation on the construct underlying the task performance. In these cases, researchers can estimate test-retest reliability by having members of the control group complete the measure again at a later time and using this value ($r_{yy'control}$) to correct the experimental d value using Equation 8.

Observer ratings. If the DV is observers' ratings of participants' behaviors (e.g., ratings of participants' aggressive behaviors), the key source of measurement error is rater-specific error. In these cases, interrater reliability is the most appropriate method to correct for measurement error. Interrater reliability should be estimated using an appropriate ICC(1) based on the number of raters and the measurement design (see Putka et al., 2008 for a discussion of considerations for estimating interrater reliability, including the use of reliability estimates based on generalizability theory to account for ill-structured measurement designs). Calculate the reliability coefficient within each group and the sample-size-weighted average within-group reliability, $r_{yy'pooled}$. Then, the experimental d value is corrected using $r_{yy'pooled}$ and Equation 8. Note that interrater reliability will not capture inconsistency in participants' actual responses over time. To correct for this form of measurement error, a similar design as for single outcomes can be employed. Have the control group complete the task a second time, then calculate the ICC(1) for different raters rating at different times. Use this estimate of $r_{yy'control}$ to correct the experimental d value using Equation 8.

Measurement error in experimental manipulations. Measurement error in experimental manipulations can occur if participants differ in their attentiveness or responsiveness to the manipulation. For example, in a study using vignettes to study attributions of responsibility for accidents, some participants may not

read the vignette carefully and thus not be affected by differences in situational features across conditions. Similarly, in a study on responses to aggressive versus non-aggressive behaviors by a confederate, participants may differ in how they evaluate the behaviors. In such cases, members of the experimental condition might effectively function as though they were actually members of the control group or vice versa. In intervention or treatment studies, there may be differences in treatment strength across participants due to treatment noncompliance or logistical challenges. For example, in a study evaluating a mindfulness intervention where treatment group members are supposed to complete a daily mindfulness exercise for 14 days, treatment group members may only complete the exercise on some of the intended days, leading to dosage variability within the treatment group. Treatment measurement error might also occur if control group members are inadvertently exposed to the treatment. For example, in the mindfulness study, some control group members might engage in their own mindfulness practice outside of the experiment.

Experimental researchers are often interested in the relationship of the dependent variable with the construct target by a manipulation or intervention (e.g., perceived aggression or mindfulness), rather than merely the impact of assigning participants to a treatment intended to influence this target construct. This is analogous to the distinction in clinical research between treatment “as-assigned” and treatment “as-received” (Ten Have et al., 2008). If there is substantial within-group heterogeneity on attentiveness to, interpretation of, or compliance with a manipulation, this can strongly bias the experiment d value as an index of the relationship between the target construct and the dependent variable. If a valid manipulation check or compliance check is available, this bias can be estimated or corrected using several methods for experimental causal mediation analysis (Imai et al., 2010). For example, in the aggression study, participants can be asked to rate how aggressive they perceived the confederate’s behavior to be. In the mindfulness study, participants can be asked to report how often they engaged in any sort of mindfulness or meditation practice during the study period.

The most straightforward correction method is the instrumental variable estimator (IVE; Angrist et al., 1996). Here, the reliability of treatment is estimated as r_{gM} , the correlation between assigned group member-

ship (g) and the measured manipulation or compliance check (M). To correct a d value between conditions for measurement error due to differential treatment attentiveness/compliance, use the three-step procedure described earlier. First, convert d_{obs} to r_{obs} using Equation 9. Second, correct r_{obs} using the same type of disattenuation formula described in Equation 12:

$$r_c = r_{obs} / r_{gM} \quad 18$$

Again, note that r_{obs} is divided by r_{gM} itself, not its square root. Third, convert r_c back to d_c using Equation 10.

IVE relies on three key assumptions (Imai et al., 2010; cf. McNamee, 2009; Mealli & Rubin, 2002):

1. The manipulation/compliance check fully accounts for the effect of the treatment on the dependent variable (i.e., that treatment effect is fully-mediated through the manipulation check; this is the *exclusion restriction assumption*);
2. The treatment impacts the manipulation/compliance check in the same way for all participants (e.g., no one misinterprets the aggressive confederate as behaving *less aggressively*; no one assigned to mindfulness practices *less mindfulness* than they would have otherwise; this is the *monotonicity assumption*);
3. There is no interaction between the treatment and the manipulation check (e.g., the manipulation check procedure does not enhance the treatment effect by clueing participants in on a concealed purpose of the study; Hauser et al., 2018; this is the *non-interaction assumption*).

If these assumptions are unreasonable, other estimators with different assumptions available (Imai et al., 2010), but these are less readily applied in meta-analysis.⁸

Choosing an Appropriate Reliability Estimate for Corrections

What are the major sources of measurement error? As shown in Table 1, there are a variety of different types of reliability coefficients, each sensitive to

⁸ Schmidt and Hunter (2015, p. 265) described an alternative method for estimating treatment reliability by assuming that a treatment is equally effective for all participants (full homogeneity of experimental effects). This assumption is very strong and rarely justifiable, so we do not recommend this method.

different sources of measurement error. When choosing a reliability coefficient to correct for measurement error, it is important to carefully consider which sources of error are likely to have major impacts on the measures and to select a method that is sensitive to these sources (for overviews, see Revelle, 2009; Schmidt & Hunter, 1996; Schmidt et al., 2003). For example, in a study using a personality scale to predict supervisor-rated job performance, a major source of error will be the supervisors' idiosyncratic beliefs and ability to observe; interrater reliability is an appropriate method to capture this source of error (Connelly & Ones, 2010; Viswesvaran et al., 1996). In a study predicting life satisfaction, transient effects, such as participant mood or temporary circumstances, will be a major source of error; test-retest reliability is an appropriate method to capture this source of error (Green, 2003; Le et al., 2009; Schmidt et al., 2003). Although internal consistency methods such as Coefficient α are the most commonly reported reliability estimates, in many cases these will not capture critical sources of measurement error; correcting for internal consistency alone can substantially mis-estimate true correlations among constructs.

Internal consistency methods can also be inappropriate even if content sampling error is a major concern. Internal consistency assumes *item homogeneity*—that items are all indicators of the same underlying construct and that the covariance among items reflects how reliably they capture this construct. Some measures are heterogeneous composites of items with non-redundant content. For example, a biodata inventory designed to predict borderline personality disorder (BPD) might assess a diverse set of life experiences that are each related to BPD but not highly correlated with each other. Such a measure would have low internal consistency but could still be reliable as a measure of overall BPD risk due to life experiences. For these types of measures, parallel forms reliability or test-retest reliability would be more appropriate reliability estimators, provided the lag between measurements is long enough to account for transient effects yet short enough that participants do not systematically change (e.g., BPD symptoms could be mitigated by therapy between temporally distant testing occasions, which would undermine a reliability estimate). A related example is short-form measures of personality traits and other constructs. Many short-form measures of constructs include items chosen to broadly tap a construct with limited redundancy (Yarkoni, 2010;

Rammstedt & John, 2007). By removing redundancy, these measures will show low internal consistency, but they can still show high convergent correlations with longer measures. For short-form measures, parallel forms or test-retest are also more appropriate methods to estimate reliability.⁹

What if reliability estimates are not available for a sample? When conducting a meta-analysis, many studies do not report reliability estimates for their measures/experimental manipulations or do not report the most appropriate form of reliability (e.g., they report Coefficient α rather than test-retest or interrater reliability; Flake et al., 2017; Fried & Flake, 2018). Even in primary studies, it is often not possible to estimate a relevant form of reliability with a specific sample. To correct for measurement error in these cases, it is necessary to draw an estimate from some other source. One approach is to draw reliability estimates from external sources, such as test manuals, previous meta-analyses, or previous studies using the measure/manipulation. This approach is reasonable if it can be assumed that the sample(s) in the present study and the external source(s) are drawn from the same underlying population (or at least comparable populations). Another approach is to conduct one or more new studies specifically for the purpose of estimating relevant reliability coefficients. For example, Beatty et al. (2015) conducted a large-scale study to estimate the reliability of university grade point averages based on varying numbers of courses taken. In meta-analyses, if reliability information is available for some studies but not others, artefact distribution and reliability generalization methods can also be used to address missing reliability data in individual studies (see the section on Correcting for Artefacts in Meta-Analysis, below).

When Should You Correct for Measurement Error?

Whether and how to correct for measurement error is the subject of ongoing debate in many areas of psychology and other fields (Muchinsky, 1996; Schmidt &

⁹Conversely, some short measures contain highly redundant items that essentially repeat the same content multiple times (e.g., the 3-item job satisfaction measure used by Judge et al., 1994). For these measures, internal consistency will *overestimate* the reliability of the scale as a measure of a broader construct. Parallel forms reliability or test-retest reliability would also be more appropriate.

Fig. 5. Structural models illustrating the assumptions of Spearman's disattenuation formula for two types of reliability coefficients—internal consistency (e.g., coefficient α , coefficient ω) and the coefficient of stability (CS; e.g., test-retest reliability). Dashed lines indicate key assumed-zero paths.

Hunter, 1996; cf. Schennach, 2016 for more favorable attitudes in econometrics). The appropriateness of correcting for measurement error depends on the nature of the research question. Are you interested in understanding measures or their underlying constructs? For example, if the goal of a study is to evaluate the diagnostic accuracy of a tool for practical use, correcting for measurement error in the criterion variable would be appropriate, but correcting for measurement error in the diagnostic tool would overestimate its practical utility because practitioners only have access to patients' observed scores, not their standing on the underlying construct (Viswesvaran et al., 2014). However, if the scientific goal of a study is to identify the nature of underlying constructs, relationships, and principles, then *not correcting* for measurement error can lead to highly inaccurate theoretical conclusions (Westfall & Yarkoni, 2016). Advancing scientific knowledge requires estimating and correcting for the impacts of measurement error on research findings, especially in meta-analysis. For meta-analyses, even if the research question concerns observed measures, controlling for *differences* in reliability across studies is important to ensure accuracy of moderator and sensitivity analyses and avoid erroneously interpreting differences across studies as substantive in origin (see Figure 3).

The accuracy of measurement error corrections depends on meeting the assumptions of the measurement error model used in the corrections. One important assumption of the correction procedures described above is that error scores for the IV are

unrelated to the true scores for the DV (and vice versa). A structural model illustrating this assumption for internal consistency corrections is shown in Figure 5A. Correcting for internal consistency assumes that the dependent variable is only predicted by variance the items have in common. If individual items have unique predictive power for the DV, the corrected correlation will either overestimate or underestimate the true correlation (Rhemtulla et al, 2019; cf. Putka et al., 2014 for similar issues regarding interrater reliability). In these cases, correcting for test-retest reliability instead may yield a more accurate estimate of the true construct correlation, as the transient errors from a measure are less likely to have unique predictive power.

This assumption can also be a particular concern for corrections for group misclassification. For example, in a study comparing impulsivity among substance use patients who report resuming use versus patients reporting remaining sober, it may be that impulsive patients are more likely to lie about resuming use, resulting in an underestimate of the correlation between relapse and impulsivity. In this case, the correction in Equation 12 will undercorrect, and the corrected correlation will still be a negatively biased estimate of the true relationship between these constructs.

A second important assumption of the above procedures is that error scores for the two variables are unrelated. If errors for the two variables are correlated, the above corrections will overestimate the true construct relationship. This assumption is illustrated for test-retest reliability corrections in Figure 5B. If the

predictor and criterion variables are measured at different times (e.g., a personality trait predicting subsequent life satisfaction), then the two measures will not share transient errors, and correcting for test-retest reliability is appropriate. If the two variables are measured at the same time, however, then the measures will share transient errors. In this case, the observed correlation between the two measures will reflect not only covariance between the latent constructs, but also between the shared transient errors. If test-retest reliability is used to correct the correlation, this will overestimate the construct relationship. In this case, parallel forms reliability or internal consistency would be a more appropriate reliability method.¹⁰

When deciding whether to correct for measurement error and which reliability estimate to use, meta-analysts and primary researchers must carefully consider whether these assumptions are reasonable for the study design and the chosen reliability method. Alternative methods for estimating true-score correlations without these assumptions are also possible depending on the specific research design (see, e.g., Charles, 2005; Schennach, 2016; Zimmerman, 2007).

Finally, as noted above, although measurement error corrections can yield less biased estimates of construct relationships, they come with the cost of increased sampling error in the corrected effect sizes. The increased uncertainty in corrected effect sizes can be indexed by adjusting standard errors as shown in Table 3 or by applying the same correction to the endpoints of the effect size confidence interval as is applied to the point estimate, for example:

$$\frac{r_{LowerBound}}{\sqrt{r_{xx'}} \sqrt{r_{yy'}}} \leq \frac{r_{xy}}{\sqrt{r_{xx'}} \sqrt{r_{yy'}}} \leq \frac{r_{UpperBound}}{\sqrt{r_{xx'}} \sqrt{r_{yy'}}} \quad 19$$

When correcting for measurement error, corresponding corrections must always be applied to the standard error/confidence interval as are applied to the effect size itself.

¹⁰ Typically, in psychological measurement, we also assume that measurement errors are normally distributed. However, the correction methods described in this article are robust to non-normal measurement error distributions. See also Schennach (2016) for correction methods without normality assumptions.

¹¹ Anecdotally, in the first author's experience discussing statistical and methodological issues with other researchers on social media, range variation and selection effects are the topic that is most frequently misunderstood.

Selection Effects

Most psychologists are familiar with the concept of measurement error, even if explicit references to or corrections for measurement error in primary studies and meta-analyses are uncommon. Awareness of the diverse types of selection effects, their impacts on study results, and available correction methods is much less widespread.¹¹ Selection effects refer to cases where the relationship observed between two variables in a sample is not representative of the relationship in the target population due to selection or conditioning on one or more variables (Sackett & Yang, 2000). In psychology, selection effects are most frequently discussed in terms of *range restriction*, wherein effect sizes are biased toward null/zero because variables' sample variances are reduced relative to the population. This is only one of many possible effects of selection. Depending on the exact selection mechanism, selection effects can attenuate sample effect sizes, inflate them, or even reverse their direction (Alexander, 1990; Ree et al., 1994; Sackett et al., 2007). These diverse types of selection effects are variously called range restriction, range enhancement, range variation (Schmidt & Hunter, 2015), selection bias (Alexander et al., 2014b), and collider bias (Greenland, 2003; Rohrer, 2018).

The impacts of selection effects on correlations is illustrated in Figure 6. Selection effects can be direct (i.e., one of the two variables in a relationship is directly selected on). For example, only severe patients might be referred to an in-patient treatment center. In such a setting, any relations between potential antecedents and symptoms will necessarily be reduced due to limited symptom variability. In Figure 6A, the correlation between *Z* and *X* is $r = .50$ in the population, but is reduced to only $r = .33$ if we directly restrict *Z* to only the top 50% of scores. Direct selection can also increase observed correlations through *range enhancement*. For example, if a researcher used an extreme-groups design and compared outcomes for the top 25% and bottom 25% of scorers on an attitude measure, the variance on attitudes is artificially inflated and attitude–outcome relationships will be overestimated relative to the full population (Preacher et al., 2005; Wherry, 2014, p. 50).

Selection effects can also be indirect, where selection occurs on a third variable related to one or both of the two focal variables. For example, the relationship between overall health and life satisfaction may be reduced in a sample of professionals, as socioeconomic

In the population, X , Y , and Z are three distinct variables, each intercorrelated $r = .50$. Correlations and points in blue indicate correlations after Z has been selected on to retain only the top 50% of scorers on Z . Selected r_{xz} and r_{yz} are directly range-restricted; selected r_{xy} is indirectly range-restricted.

In the population, X and Y are uncorrelated. Z is a composite of X and Y plus a small amount of error. Correlations and points in blue indicate correlations after Z has been selected on to retain only the top 50% of scorers on Z . Selected r_{xz} and r_{yz} are directly range-restricted; selected r_{xy} is indirectly range-restricted.

Fig. 6. Impact of direct range and indirect range restriction on correlations.

status is related to both health and life satisfaction (Roberts et al., 2007). In Figure 6A, indirect selection on Z reduces the correlation between X and Y from $r = .50$ to only $r = .41$. Indirect selection can even reverse the sign of a correlation if the two focal variables are strongly related to the selection mechanism. For example, imagine that success in a class is equally determined by ability and effort, and that these two predictors are uncorrelated (see Figure 6B). If we select a sample of highly successful students, we will find a strong artefactually negative relationship between ability and effort due to indirect range restriction, or what is sometimes referred to as “conditioning on a collider” (Rohrer, 2018). This effect occurs because high effort can compensate for low ability (or vice versa) as a path to success.

Impacts of Selection Effects in Meta-Analysis

Selection effects impact meta-analysis results in the same three ways as measurement error. The mean effect size in the observed samples will be biased

relative to the desired reference population based on the average selection process across studies. Similarly, if selection processes or the size of selection effects differ across studies (e.g., if some studies were sampled from a general population, but others were restricted to university students; cf. Murray et al., 2014), this will inflate the random-effects variance component and bias the results of moderator and publication bias analyses.

Quantifying and Correcting Selection Effects

The impact of selection effects is quantified using a u ratio—a ratio of the standard deviation of a variable in the sample to the standard deviation in the reference population:

$$u_x = SD_{x_{sample}} / SD_{x_{reference}} \quad 20$$

If a reference standard deviation is unavailable, selection effect u ratios for continuous variables can be estimated from the sample and reference reliability coefficients:

$$u_x = \sqrt{(1 - r_{xx'_{reference}})/(1 - r_{xx'_{sample}})} \quad 21$$

If the exact selection mechanism is known¹², very accurate estimates of the population relationship can be calculated using a multivariate selection correction (Aitken, 1935; Lawley, 1944; based on Pearson, 1903). Such complete information is rarely available in psychology research. However, several accurate approximations are available that rely on information more commonly available to primary researchers and meta-analysts; these are listed in Table 3. An appropriate correction model can be selected based on whether (1) direct selection occurs on one variable (univariate direct range restriction; UVDRR), (2) direct selection occurs on both variables (bivariate direct range restriction; BVDRR), or (3) indirect selection occurs and u ratios are available for one (univariate indirect range restriction; UVIRR) or both (bivariate indirect range restriction; BVIRR) variables. For example, for the indirect selection in Figure 6B, $u = .82$ for X and Y . Using these values and the observed correlation $r_{obs} = -.48$ with the BVIRR correction, the estimated population correlation is $r_c = .005$.

An important nuance to consider is that selection never affects only one variable. When one variable is restricted, other variables it is correlated with are also restricted. For example, grade point average will be less variable in a sample of university students selected on high school grade point average than would be observed in an unselected group. The correction formulas in Table 3 implicitly correct for these indirect selection effects based on assumptions about the selection process (see Dahlke & Wiernik, 2019a; Hunter et al., 2006, for discussions).

¹² We focus on the Pearson family of selection effect corrections because these require only information about variances and covariances among variables, making them readily usable in meta-analysis. If individual-level data are available, depending on what is known about the nature of the selection mechanism, a variety of regression- and missing-data-based selection models can also be applied to estimate relationships in the unselected population (e.g., Fife et al., 2016; Gross & McGanney, 1987; Heckman, 1976, 1976; Olson & Becker, 1983; Pfaffel et al., 2016; Puhani, 2000; Yang et al., 2004). Sackett and Yang (2000) provide a detailed overview of many of these selection procedures, their data requirements, and their relative advantages and disadvantages.

Selection Effects in Correlations

Selection effects are most commonly discussed in terms of correlations, and methods for correcting for selection effects have been developed directly for this metric. To correct for selection effects in correlations, identify an appropriate reference sample and the most likely selection mechanism. Then, apply the appropriate correction formula as listed in Table 3.

Selection Effects in Group Comparison and Experimental Research

Selection effects impact group comparisons in the same way as correlations (Bobko et al., 2001; Li, 2015). For example, a study of gender differences in political orientation may be attenuated in a sample of urban university students due to reduced political orientation variability compared to the general population. In experimental research, d values for a manipulation or treatment may differ across studies due simply to differences in response variability across the studies.

To correct a d value for these selection and range variation effects, use the three-step procedure described above. First, convert d_{obs} to r_{obs} using Equation 9:

$$r_{obs} = d_{obs} / \sqrt{\frac{1}{p(1-p)} + d_{obs}^2}$$

Second, correct r_{obs} using the appropriate formula from Table 3. For these corrections, u_y and $\sqrt{r_{yy'}}$ should be the combined total-sample (not pooled within-sample) values. Third, convert r_c back to d_c using a variant of Equation 10:

$$d_c = r_c / \sqrt{p^*(1 - p^*)(1 - r_c^2)} \quad 22$$

where p^* is an adjusted group proportion. As discussed above, direct selection on one variable also indirectly induces selection on variables it is correlated with. When applied to correlations, the equations in Table 3 implicitly correct for this indirect selection. However, this adjustment must be manually applied when converting back from r_c to the d_c metric. If the observed group proportions were used to convert back from r_c to d_c , this would imply no change in the grouping variable variance after the correction, which is only possible if the d value equals 0 (i.e., there is no correlation between group membership and scores on the dependent variable).

Table 3. Formulas for corrected effect sizes and sampling error variances for different artefact correction models

Artefact model	Effect size	Corrected effect size /	Corrected sampling error variance
(1) Measurement error alone	r	$r_c = r_{obs} / (\sqrt{r_{xx'}} \sqrt{r_{yy'}})$	$SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$
	d	<i>No misclassification</i> $d_c = d_{obs} / \sqrt{r_{yy'} \text{ pooled}}$	$SE_{d_c}^2 = SE_{d_{obs}}^2 \times (d_c / d_{obs})^2$
		<i>With misclassification</i> A. Convert d to r using $r = d / \sqrt{\frac{1}{p(1-p)} + d^2}$.	A. Convert $SE_{d_{obs}}^2$ to $SE_{r_{obs}}^2$ using: $SE_{r_{obs}}^2 = \frac{SE_{d_{obs}}^2}{(1 + d_{obs}^2 p[1 - p])^2 \left(d_{obs}^2 + \frac{1}{p(1 - p)} \right)}$
		B. Apply the correction for r , using the total sample $\sqrt{r_{yy'}}$ and with $\sqrt{r_{xx'}} = r_{gG}$ or $\sqrt{r_{xx'}} = \sqrt{r_{tt'}}$ C. Convert r_c to d_c using: $d_c = r_c / \sqrt{p^{true}(1 - p^{true})(1 - r_c^2)}.$ If possible, use an estimate of true group proportion p^{true} for step C. If p^{true} is unknown, use the observed p with the assumption of equal misclassification across groups.	B. Calculate $SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$ C. Convert $SE_{r_c}^2$ to $SE_{d_c}^2$ using: $SE_{d_c}^2 = SE_{r_c}^2 / [p^{true}(1 - p^{true})(1 - r_c^2)^3]$
(2) Univariate direct selection (UVDRR)	r	$r_c = \frac{r_{obs}}{u_x \sqrt{1 - u_x^2(1 - r_{xx'})} \sqrt{\left(\frac{1}{u_x^2} - 1\right) r_{obs}^2 + r_{yy'}}$	$SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$
	d	A. Convert d to r using $r = d / \sqrt{\frac{1}{p(1-p)} + d^2}$. B. Apply the correction for r , using the total sample $\sqrt{r_{yy'}}$ and with $\sqrt{r_{xx'}} = r_{gG}$ or $\sqrt{r_{xx'}} = \sqrt{r_{tt'}}$ C. Convert r_c to d_c using: $d_c = r_c / \sqrt{p^*(1 - p^*)(1 - r_c^2)}.$ p^* is the true group proportion in the reference population.	A. Convert $SE_{d_{obs}}^2$ to $SE_{r_{obs}}^2$ using: $SE_{r_{obs}}^2 = \frac{SE_{d_{obs}}^2}{(1 + d_{obs}^2 p[1 - p])^2 \left(d_{obs}^2 + \frac{1}{p(1 - p)} \right)}$ B. Calculate $SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$ C. Convert $SE_{r_c}^2$ to $SE_{d_c}^2$ using: $SE_{d_c}^2 = SE_{r_c}^2 / [p^*(1 - p^*)(1 - r_c^2)^3]$

(3) Univariate indirect selection (UVIRR)	r	$r_c = r_{obs} / \sqrt{r_{obs}^2 + \frac{u_x^2 r_{xx'}(r_{xx'} r_{yy'} - r_{obs}^2)}{1 - u_x^2(1 - r_{xx'})}}$	$SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$
	d	<p>A. Convert d to r using $r = d / \sqrt{\frac{1}{p(1-p)} + d^2}$.</p> <p>B. Apply the correction for r, using the total sample $\sqrt{r_{yy'}}$ and with $\sqrt{r_{xx'}} = r_{gG}$ or $\sqrt{r_{xx'}} = \sqrt{r_{tt'}}$</p> <p>D. Convert r_c to d_c using: $d_c = r_c / \sqrt{p^*(1 - p^*)(1 - r_c^2)}$ p^* is the true group proportion in the reference population.</p>	<p>A. Convert $SE_{d_{obs}}^2$ to $SE_{r_{obs}}^2$ using: $SE_{r_{obs}}^2 = \frac{SE_{d_{obs}}^2}{(1 + d_{obs}^2 p[1 - p])^2 (d_{obs}^2 + \frac{1}{p(1-p)})}$</p> <p>B. Calculate $SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$</p> <p>C. Convert $SE_{r_c}^2$ to $SE_{d_c}^2$ using: $SE_{d_c}^2 = SE_{r_c}^2 / [p^*(1 - p^*)(1 - r_c^2)^3]$</p>
(4) Bivariate direct selection (BVDRR)	r	$r_c = \frac{\frac{r_{obs}^2 - 1}{2r_{obs}} u_x u_y + \text{sign}(r_{obs}) \sqrt{\frac{(1 - r_{obs}^2)^2}{4r_{obs}^2} u_x^2 u_y^2 + 1}}{\sqrt{1 - u_x^2(1 - r_{xx'})} \sqrt{1 - u_y^2(1 - r_{yy'})}}$	$SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c / r_{obs})^2$
(5) Bivariate indirect selection (BVIRR)	r	$r_c = \frac{r_{obs} u_x u_y + \lambda \sqrt{ 1 - u_x^2 1 - u_y^2 }}{\sqrt{1 - u_x^2(1 - r_{xx'})} \sqrt{1 - u_y^2(1 - r_{yy'})}}$	See Dahlke and Wiernik (2019) for details.

Note. All reliability values in the above formulas are assumed to be estimated in the restricted sample (subject to selection effects). If the reliability values are estimated in samples from the unrestricted reference population, adjust them using the following formula: $r_{xx'}^* = 1 - (1 - r_{xx'}) / u_x^2$;

for bivariate indirect selection, $\lambda = \text{sign}(\rho_{ZX} \rho_{ZY} [1 - u_x][1 - u_y]) \frac{\text{sign}(1 - u_x) \min(u_x, \frac{1}{u_x}) + \text{sign}(1 - u_y) \min(u_y, \frac{1}{u_y})}{\min(u_x, \frac{1}{u_x}) + \min(u_y, \frac{1}{u_y})}$, where ρ_{ZX} and ρ_{ZY} are the true

correlations of the selection construct, Z , with X and Y , corrected for measurement error and selection effects. Note that only the -1 or $+1$ signs of ρ_{ZX} and ρ_{ZY} are needed to compute λ . Performance of bivariate selection correction methods for d values in meta-analysis have not yet been evaluated.

The effect of a correction for selection effects on the variance of the dichotomous grouping variable can be estimated as:

$$var_{g_c} = p(1 - p) \left(1 + r^2 \left[\frac{1}{u_y^2} - 1 \right] \right) \quad 23$$

which can be converted back to a proportion as:¹³

$$p^* = .5 \left(1 - \sqrt{1 - 4var_{g_c}} \right) \quad 24$$

If var_{g_c} is greater than .25 (i.e., the maximum variance of a binary variable), instead convert using:

$$p^* = .5 \left(1 - \sqrt{1 - 4[.5 - var_{g_c}]} \right). \quad 25$$

Choosing a Reference Sample

Appropriate reference samples can be chosen in several ways. If there is a clear population about which meta-analysts wish to make inferences (e.g., to estimate the relationship between attitudes and grades among the whole university student body, rather than only among scholarship recipients; DesJardins et al., 2010) and local *SD* or reliability estimates for this population are available, these can be used directly. Reference group population values can also be drawn from external sources (e.g., *SDs* from test manual norms or national statistical databases). Researchers must ensure that these external reference samples actually represent the population of interest (e.g., an *SD* from test norms for the general population of American adults may not accurately estimate the *SD* for a target population of South African job applicants).

In meta-analyses, even if there is not a clear reference population to which the meta-analyst wishes to generalize results, differences in IV or DV variability across samples will produce artefactual between-studies heterogeneity. In these cases, this range variation should be accounted for by adjusting each effect size to reflect a common pooled or total-sample standard deviation across the samples using the same measure

that are included in the meta-analysis (see Dahlke & Wiernik, 2019a, for details). Corrections based on this reference standard deviation would remove artefactual effect size heterogeneity without changing the mean effect size.

When Should You Correct for Selection Effects?

Selection effects are pervasive in psychological research (Rohrer, 2018). Studies across subfields can be affected by nonrepresentative sampling, attrition, nonresponse, and other selection effects. Failure to consider and correct for selection effects can lead to highly inaccurate substantive conclusions. For example, many studies have observed a negative correlation between cognitive ability and Conscientiousness, suggesting support for an intelligence compensation hypothesis (i.e., that Conscientiousness develops in part as a mechanism to overcome limitations of lower ability). Murray et al. (2014) showed that these findings were the result of selection effects in studies conducted among university students; students are admitted to universities primarily based on high school grade point average (GPA). A high GPA can be earned through high ability, hard work (Conscientiousness), or both. By selecting on an outcome of these two variables, the university admissions process induces an artefactual negative correlation between ability and Conscientiousness (this is the classic “conditioning on a collider” effect; Rohrer, 2018). Similarly, studies relating admissions tests to success among graduate students underestimate the predictive validity of these tests because applicants with very low test scores are rarely admitted to graduate programs (Kuncel et al., 2010). Considering potential selection effects and applying appropriate corrections is critical for drawing accurate theoretical inferences and making sound data-based decisions.

The formulas used to correct for selection effects assume that (1) variables are linearly related and (2) residual variances are equal in the selected sample and in the target population (Sackett & Yang, 2000). Assumption 1 regarding linearity is absolutely essential, as correlations quantify linear relations and corrections for selection effects therefore cannot extrapolate information about non-linear associations. Assumption 2 is satisfied when one’s chosen correction formula

¹³ For observational research, p^* can instead be estimated using the group proportion in the reference population (e.g., the population gender distribution, disorder base rates from epidemiological studies, the proportion of majority and minority group members in the job applicant pool; Bobko et al., 2001; Li, 2015). For observational group comparisons, the difference between the observed group proportion, p , and the population proportion, p^* , reflects an adverse impact effect—restriction on the selection variable causes the group proportions in the selected sample to be unrepresentative of the reference population.

reflects the direct or indirect nature of selection effects and includes all of the selection variables that gave rise to the selection effects (or, in the case of the UVIRR correction, includes a correction variable that fully mediates the effect of the actual selection variable; see Hunter et al., 2006). Incomplete satisfaction of Assumption 2 (e.g., only using a subset of the selection variables to make a correction) is suboptimal but, as long as the right correction formula is used, the corrected statistic will still provide a better estimate of the effect size in the target population than will the observed effect size (e.g., Beatty et al., 2014; cf. Rohrer, 2018).

In both primary studies and meta-analyses, it is valuable to consider potential selection processes that may be at work in the samples and to apply a correction model to account for most likely or most reasonable processes. Doing so can yield less-biased estimates of the true relationships between constructs and can also be regarded as a sensitivity analysis to gauge the potential impact of selection effects on study results. It is even possible to consider multiple potential selection processes and compare results to consider potential impacts of different selection mechanisms.

As with corrections for measurement error, statistical adjustments for selection effects come with the cost of increased sampling error relative to the observed effect size. Confidence intervals and standard errors must be adjusted to account for this increased uncertainty. As with improving reliability, it is always better to improve sampling designs before conducting a study rather than relying on statistical adjustments during data analysis.

Correcting for Artefacts in Meta-Analysis

Both measurement error and selection effects should be considered and corrected in meta-analyses. Artefact corrections can be approached in two ways. First, each effect size can be individually corrected for artefacts before conducting the meta-analysis. Second, uncorrected effect sizes can be meta-analyzed, and the results of this “barebones” meta-analysis (Schmidt & Hunter, 2015) can be corrected post-hoc using the means and variances of the distributions for each

artefact. We describe each approach below, as well as their advantages and disadvantages.

Individual Corrections Meta-Analysis

In the individual corrections approach, each effect size is corrected using artefact values based on the same sample that provided the effect size. For each effect size, determine which artefacts are of concern and apply the relevant formulas in Table 3. For example, if correcting an experimental study d value for dependent variable measurement error, treatment measurement error, and indirect range restriction for the dependent variable, apply the d value procedure for UVIRR in Table 3 using the observed d_{obs} value and the sample values for r_{gG} (or $\sqrt{r_{tt'}}$), $\sqrt{r_{yy'}}$, and u_y . In individual corrections meta-analysis, each effect size can potentially be corrected for a different set of artefacts (e.g., if range restriction is a concern in some studies but not others).¹⁴

Each effect size's sampling error is also adjusted to account for increased uncertainty stemming from the corrections. For correlations, the sampling error adjustment for most corrections takes the form:

$$SE_{r_c}^2 = SE_{r_{obs}}^2 \times (r_c/r_{obs})^2 \quad 26$$

That is, the sampling error is adjusted by the square of the adjustment applied to the effect size. For bivariate indirect selection, the adjustment to sampling error is more complex to account for the unique additive term in this correction (see Dahlke & Wiernik, 2019a, for details).

When correcting d values only for dependent variable measurement error using $\sqrt{r_{yy'}^{pooled}}$, the sampling error adjustment follows the same process as Equation 26:

$$SE_{d_c}^2 = SE_{d_{obs}}^2 \times (d_c/d_{obs})^2 \quad 27$$

For corrections involving converting d to r and back, the sampling error is adjusted by converting $SE_{d_{obs}}^2$ to $SE_{r_{obs}}^2$, applying Equation 26, then converting $SE_{r_c}^2$ back to $SE_{d_c}^2$:

$$SE_{d_c}^2 = \frac{SE_{d_{obs}}^2 (r_c/r_{obs})^2}{(1+d_{obs}^2 p[1-p])^2 (d_{obs}^2 + \frac{1}{p(1-p)}) p^* (1-p^*) (1-r_c^2)^3} \quad 28$$

where p^* is the true group proportion in the reference population.¹⁵

¹⁴To correct for measurement error in one variable but not the other, set the reliability value for the uncorrected variable to 1.0. To correct for selection effects but not measurement error, set the reliability values for both variables to 1.0.

If meta-analyses are conducted using inverse-sampling-error weights (or weights based on these, such as Hedges-Vevea random-effects weights; Hedges & Vevea, 1998), the adjusted sampling error variances for the corrected effect sizes are used in the weights in place of the original sampling error variances. The meta-analytic computations then proceed normally to calculate the mean effect size, random-effects variance component (in Hedges-Vevea notation, τ_c^2 ; in Hunter-Schmidt notation, SD_ρ^2 or SD_δ^2)¹⁶, and other results.

If sample-size or effective-sample-size weights are used to avoid bias due to dependency between study weights and study effect sizes, as recommended by numerous simulation studies (Hedges & Olkin, 1985, p. 110; Kulinskaya & Bakbergenuly, 2018; Schmidt & Hunter, 2015; Shuster, 2010), then adjusted weights are computed as:¹⁷

$$\begin{aligned} w_i^* &= N_i \times (r_{obs_i}/r_{c_i})^2 \\ w_i^* &= N_i \times (d_{obs_i}/d_{c_i})^2 \end{aligned} \quad 29$$

In the Hunter-Schmidt meta-analytic procedure, these adjusted weights are then used to compute the mean effect size, observed and expected effect-size variances, and the random-effects variance component (in the Hunter-Schmidt notation, SD_ρ^2 or SD_δ^2 ; in the Hedges-Vevea notation, τ_c^2):

$$\begin{aligned} \bar{\rho} &= \sum w_i^* r_{c_i} / \sum w_i^* \\ \bar{\delta} &= \sum w_i^* d_{c_i} / \sum w_i^* \end{aligned} \quad 30$$

$$\begin{aligned} SD_{r_c}^2 &= \sum w_i^* (r_{c_i} - \bar{\rho})^2 / \sum w_i^* \\ SD_{d_c}^2 &= \sum w_i^* (d_{c_i} - \bar{\delta})^2 / \sum w_i^* \end{aligned} \quad 31$$

¹⁵ If correcting only for group misclassification (and potentially DV measurement error), in place of p^* , use p^{true} (an estimate of the true group proportion) if known or the observed group proportion p if p^{true} is unknown.

¹⁶ We use the notation τ_c^2 , SD_ρ^2 , or SD_δ^2 to indicate random-effects variance component estimates for corrected effect sizes and τ^2 or SD_{res}^2 to indicate random-effects variance component estimates for observed/uncorrected effect sizes.

¹⁷ When correcting for bivariate indirect selection (BVIRR), a different set of weights is used. See Dahlke and Wiernik (2019a) for details.

¹⁸ Another popular approach is to impute missing artefact values using the mean artefact value from included studies. While this approach will not bias the mean effect size, if the amount of missing artefact data is large, it can substantially reduce artefact variability and upwardly bias the random-effects variance component.

$$\begin{aligned} \overline{SE_{r_c}^2} &= \sum w_i^* SE_{r_{c_i}}^2 / \sum w_i^* \\ \overline{SE_{d_c}^2} &= \sum w_i^* SE_{d_{c_i}}^2 / \sum w_i^* \end{aligned} \quad 32$$

$$\begin{aligned} \tau_c^2 &= SD_\rho^2 = SD_{r_c}^2 - \overline{SE_{r_c}^2} \\ \tau_c^2 &= SD_\delta^2 = SD_{d_c}^2 - \overline{SE_{d_c}^2} \end{aligned} \quad 33$$

Meta-analyses of corrected d values can also be computed using the r_c values for each sample, with the final meta-analytic results converted back to the δ metric using:

$$\bar{\delta} = \bar{\rho} / \sqrt{\bar{p}^*(1 - \bar{p}^*)(1 - \bar{\rho}^2)} \quad 34$$

$$\tau_c^2 = SD_\delta^2 = SD_\rho^2 / [\bar{p}^*(1 - \bar{p}^*)(1 - \bar{\rho}^2)^3] \quad 35$$

Corrected effect sizes, sampling error variances, and weights are also used in other meta-analytic procedures, such as meta-regression and subgroup moderator analyses, publication bias analyses, and sensitivity analyses. Using corrected effect sizes for these analyses is critical to prevent heterogeneity in artefacts across studies from biasing results.

It is often the case that not all studies in a meta-analysis report the necessary statistics to correct for measurement error or selection effects (i.e., reliability coefficients, standard deviations, reference population standard deviations). Several options are available to address missing artefact data. A simple option is to impute missing artefacts by bootstrapping (i.e., sampling with replacement) artefact values from the studies that do report artefact information. This approach works well and generally yields unbiased estimates of the mean effect size, its standard error, and the random-effects variance component (Schmidt & Hunter, 2015). A more robust approach is to apply reliability/selection generalization (Vacha-Haase, 1998), wherein meta-regression is used to predict missing reliability values or selection u ratios based on sample and study design characteristics, measure used, scale properties, and other moderators. The reliability/selection generalization approach can yield somewhat more accurate results if these study and measure design factors have strong impacts on observed artefact values. A third approach is to sidestep the issue of missing data by not correcting effect sizes individually, but instead to correct overall results of the meta-analysis based on the artefact means and standard deviations. This approach is called the artefact distribution method (Schmidt & Hunter, 2015) and is described next.¹⁸

Artefact Distribution Meta-Analysis

In the artefact distribution method, studies are initially meta-analyzed using observed effect sizes, without correcting for measurement error or selection effects. Then, the initial meta-analysis results are corrected using the means and variances of the distributions of artefacts that are observed. The mean effect size is corrected using the mean measure quality indices ($\sqrt{r_{xx'}}$ and $\sqrt{r_{yy'}}$) and selection SD ratios ($\bar{u}_x$ and $\bar{u}_y$) and the appropriate equation given in Table 3. The random-effects variance component is corrected using a weighted sum of the variances of each artefact; the weights are determined by a Taylor Series Approximation (TSA)—essentially, a function that reflects how each artefact individually impacts the value of the corrected effect size. The TSA variance estimator for each artefact model in Table 3 takes the same general form:

$$\tau_c^2 = [\tau^2 - Var_{art}]/b_{metric}^2 \quad 36$$

where τ_c^2 (also labeled SD_ρ^2 or SD_δ^2) is the random-effects variance component for corrected effect sizes, τ^2 (also labeled SD_{res}^2) is the random-effects variance component for uncorrected effect sizes, Var_{art} is the variance in effect sizes attributable to artefacts, and b_{metric} is a scaling factor that converts τ_c^2 from the metric of uncorrected effect sizes to the corrected (true-score) effect-size metric. The exact form of the TSA function varies for each artefact model. For example, the TSA estimator when correcting correlations for measurement error alone is:

$$\tau_c^2 = \frac{[\tau^2 - (\bar{\rho}^2 \cdot \bar{q}_y^2 \cdot \text{var}(q_x) + \bar{\rho}^2 \cdot \bar{q}_x^2 \cdot \text{var}(q_y))]/(\bar{q}_x^2 \bar{q}_y^2)}{37}$$

where $q_x = \sqrt{r_{xx'}}$, $q_y = \sqrt{r_{yy'}}$, and $\bar{\rho}$ is the mean corrected correlation. TSA estimators for other artefact models are described by Dahlke and Wiernik (2019a), Hunter et al. (2006), and Raju et al. (1991); see also the *psychmeta* package for *R* (Dahlke & Wiernik, 2018).

Taylor Series artefact distribution methods are derived from the principle of maximum likelihood (Raju & Drasgow, 2003), and they yield comparably accurate results as individual correction meta-analyses (Hunter et al., 2006; Raju et al., 1991; Schmidt & Hunter, 2015). It should be noted that these artefact distribution methods assume that the population artefact values (u ratios and unselected measure quality

indices) are uncorrelated; this assumption is reasonable in most cases (Raju & Drasgow, 2003).

Artefact distribution methods have several potential advantages over individual-correction meta-analyses. First, artefact distribution methods are easier to apply because meta-analysis results can be corrected after computation, rather than needing to individually correct each effect size beforehand. This is particularly the case when there is missing artefact data across studies, as no imputation is required. Second, artefact distribution methods allow meta-analyses to be corrected using artefact distributions reported in previously published meta-analyses. For example, many studies of job performance do not report appropriate interrater reliability estimates to correct for measurement error; the artefact distributions reported by Viswesvaran et al. (1996) have been widely used in subsequent meta-analyses to correct for unreliability in this variable. Third, in some cases, artefact distribution methods can be more accurate than individual-correction methods (Dahlke & Wiernik, 2019a).

However, artefact distribution methods also have several disadvantages. Unlike the individual corrections approach, the same artefact correction model must be applied to all effect sizes (e.g., it is not possible to correct for direct range restriction in one sample and indirect range restriction in another).¹⁹ Moreover, because the artefact distribution method does not correct effect sizes individually, it cannot correct for the impact of differential reliability/selection across studies on results of meta-regression or publication bias analyses. Instead, these analyses are conducted with the uncorrected effect sizes. For example, if smaller-sample studies with null results were more likely to be unpublished due to low reliability or restricted sampling, rather than publication bias, analyses such as trim-and-fill or cumulative meta-analysis would not detect this.

Conclusion

Measurement error and selection effects are pervasive in psychological research and other fields, and the detrimental impacts of these artefacts on the validity of research conclusions have been widely documented (Schmidt & Hunter, 2015). The infrequency with which

¹⁹ In artefact distribution meta-analysis, if a specific artefact is a concern in some samples but not others, this can be accommodated by including a value of 1.0 for the artefact in the distribution for each study where it is not a concern.

measurement error and selection effects are considered and corrected in meta-analyses in many literatures risks substantial bias in their results. By applying corrections as described in this article, more accurate meta-analytic inferences and more useful recommendations for future psychological research and practice can be realized.

Appendix A: Correcting Artefacts and Conducting Psychometric Meta-Analyses in R using *psychmeta*

The *psychmeta* package (Dahlke & Wiernik, 2018) in R can be used to correct correlations and *d* values for the artefact models described in this article (including calculating adjusted confidence intervals and standard errors), as well as to conduct individual correction and artefact distribution meta-analyses.

To correct correlations, use the `correct_r` function:

```
correct_r(correction = "bvirr",
  rxyi = .40, n = 150,
  rxx = .80, ryy = .80,
  ux = .90, uy = .80)
```

The `correction` argument is used to specify which artefact model to apply, `rxyi` is the vector of observed correlations, `n` is the sample sizes, `rxx` and `ryy` are the observed reliability values, and `ux` and `uy` are the observed selection effect *u* ratios. The function returns the correlations corrected for measurement error and/or selection effects.

To correct *d* values, use the `correct_d` function:

```
correct_d(correction = "uvirr_y",
  d = .40, n1 = 75, n2 = 75,
  rGg = .80, ryy = .80,
  uy = .80)
```

The `correction` argument specifies which artefact model to apply, `d` is the vector of observed *d* values, `n1` and `n2` are the group sample sizes, `rGg` is the correlations between observed and true group membership, `ryy` is the observed total-sample reliability values, and `uy` is the observed selection effect *u* ratios for the dependent variable. The function converts the *d* values to point-biserial correlations, applies the specified correction, then converts the *r_c* values back to *d_c* values and returns the *d* values corrected for measurement error and/or selection effects.

To conduct individual correction meta-analyses, use the `ma_r` or `ma_d` functions and specify `ma_method = "ic"`:

```
ma_results_r <-
  ma_r(ma_method = "ic",
    rxyi = rxyi, n = n,
    rxx = rxxi, ryy = ryyi,
    ux = ux, uy = uy,
    correct_rxx = TRUE,
    correct_ryy = TRUE,
    correct_rr_x = TRUE,
    correct_rr_y = TRUE,
    indirect_rr_x = TRUE,
    indirect_rr_y = TRUE,
    data = data_r_bvirr)

ma_results_d <-
  ma_d(ma_method = "ic",
    d = d, n1 = n1, n2 = n2,
    ryy = ryyi,
    construct_y = construct,
    data = data_d_meas_multi)
```

The `rxyi` or `d` arguments are the vectors or column names of observed effect sizes, `n`, `n1`, and `n2` are the sample sizes, `rxx` and `ryy` are the observed total-sample reliability values, and `ux` and `uy` are the observed selection effect *u* ratios. The “correct” arguments specify whether each artefact should be corrected. The “indirect_rr” arguments specify whether selection was direct or indirect for each variable.

This function will correct each effect size for measurement error and/or selection effects and conduct Hunter-Schmidt meta-analyses for each set of construct variables. Moderators can also be specified, and a variety of follow-up analyses (sensitivity analyses, meta-regression, etc.) can be conducted. It is also possible to extract a data frame of corrected effect sizes, corrected sampling error variances, and sample and moderator information for use with the meta-analysis models provided in the *metafor* package (Viechtbauer, 2010) using the `get_metafor` function:

```
es_data <-
  get_metafor(
    ma_results_d,
    analyses = list(construct_y = "Y"),
    ma_method = "ic",
    correction_type = "ts")
```

`ma_results` is the results from either `ma_r` or `ma_d`, `analyses` specifies which analyses to extract from `ma_results`, `ma_method` specifies which type of meta-analysis to extract data for (here, individual correction results), and `correction_type` specifies whether to extract data corrected for all artefacts (“ts”,

as here) or uncorrected for measurement in one of the variables. The function returns a data frame of effect sizes, variances, sample information, and moderator variables that can be used with *metafor*.

To conduct artefact distribution meta-analyses, use the `ma_r` or `ma_d` functions and specify `ma_method = "ad"`:

```
ma_results_r <-
  ma_r(ma_method = "ad",
        rxyi = rxyi, n = n,
        rxx = rxxi, ryy = ryyi,
        ux = ux, uy = uy,
        correct_rxx = TRUE,
        correct_ryy = TRUE,
        correct_rr_x = TRUE,
        correct_rr_y = TRUE,
        indirect_rr_x = TRUE,
        indirect_rr_y = TRUE,
        data = data_r_bvrr)
```

The arguments are the same as specified above.

Additional details about these and other functions are provided in the *psychmeta* documentation and vignettes (Dahlke & Wiernik, 2017/2019b).

Appendix B: Accuracy of Corrections for Group Misclassification for d Values

The figures below illustrate the effects of group misclassification on observed d values in group comparison research. We simulated d values, varying (1) the total sample size ($N = 50, 100, 200, 500, 1000$) and (2) the misclassification rate (10%, 20%, or 30%). For each combination of sample size and misclassification rate, we simulated 1,000 samples of scores on a dependent variable Y for two groups with equal sample sizes of $n = N/2$. The population distribution for Group A was specified to have mean = .00 and $SD = 1.0$. The population distribution for Group B was specified to have mean = .40 and $SD = 1.0$. Thus, the population for simulated samples was specified to be homogeneous, with $\delta = .40$. For each simulated sample, we calculated the true-group d value using the standard formula: $d = (Mean_A - Mean_B) / SD_{pooled}$. Distributions of true-group d values for each condition are shown in Figure B1. These distributions reflect only variability introduced by sampling error.

Fig. B1. Distributions of simulated true-group d values.

Fig. B2. Distributions of simulated observed d values.

To illustrate the impact group misclassification, in each simulated sample, we created an “observed group” variable by randomly reassigning a proportion of the sample equal to the misclassification rate to the opposite group, ensuring equal misclassification for both groups. We then calculated the d value for Y between these “observed groups”. Distributions of observed d values between observed groups after misclassification are shown in Figure B2. These distributions reflect both variability introduced by sampling error and the bias and variability introduced by group misclassification.

In Figure B2, it is apparent that the mean observed d value is biased toward 0 relative to the true population value of $\delta = .40$. Moreover, as the total sample size and misclassification rates increase the distributions of observed d values become increasingly negatively skewed and bimodal. This is because in a minority of samples, group members with the most extreme values of Y are misclassified, causing the sign of the observed d value to flip relative to the true-group d value.

We then corrected for group misclassification using Equations 12–13. We calculated r_{gG} using the correlation between the true and observed group membership for each simulated case. Distributions of corrected d_c values are shown in Figure B3.

In Figure B3, the larger mode is centered on the mean true-group d value, indicating the accuracy of the correction. However, corrected d_c values retain the negative skew and bimodal distribution of observed d values because the correction cannot adjust for potential sign-flipping in observed d values.

Summary statistics for the distributions of true-group d values, observed d values, and corrected d_c values are shown in Table B1. Due to the negative skew and bimodal distribution of observed d values, the mean corrected d_c value is negatively-biased as an estimator of the mean true-group d value. Similarly, the standard deviation of the corrected d_c values is positively-biased as an estimator of the true-group d value standard deviation. These biases can be corrected by instead calculating the median corrected d_c value as

Fig. B3. Distributions of simulated corrected d_c values.

an estimate of the mean true-group d value and the median absolute deviation from the median corrected d_c value (MAD) as an estimate of the true-group d value standard deviation. These estimators are much more robust to the presence of outlier observed d values due to sign-flipping (cf. Li et al., 2017). When group misclassification is a potential concern (even if corrections using r_{gG} are not made), we recommend conducting meta-analyses using the median and MAD , rather than mean and SD , of observed or corrected d values.

Author Contributions

B. M. Wiernik outlined the paper, wrote the first draft of the manuscript, and prepared Figures 1, 3, and 5. J. A. Dahlke critically revised and edited the manuscript and prepared Figures 2, 4, and 6.

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Open Practices

Code to reproduce the simulated data and results shown in the tables and figures of this article is available from <https://doi.org/10.17605/osf.io/cp6rt>

R functions for the correction methods described in this article are available in the *psychmeta* package: <https://cran.r-project.org/package=psychmeta>

Table B1. Summary statistics for distributions of simulated true-group d values, observed d values, and corrected d_c values.

% Mis-classified	N	True-group d values				Observed d values				Corrected d_c values			
		Mean	Medn	SD	MAD	Mean	Medn	SD	MAD	Mean	Medn	SD	MAD
10%	50	.39	.38	.29	.29	.28	.29	.34	.32	.33	.35	.41	.39
	100	.40	.40	.28	.26	.25	.29	.34	.32	.31	.36	.63	.54
	200	.40	.40	.29	.29	.26	.31	.34	.33	.33	.38	1.10	.77
	500	.40	.40	.21	.21	.26	.31	.29	.23	.33	.39	.37	.29
	1000	.40	.40	.20	.20	.25	.31	.28	.26	.32	.39	.49	.44
20%	50	.40	.39	.20	.20	.15	.16	.25	.25	.27	.27	.74	.63
	100	.41	.41	.14	.13	.13	.17	.23	.15	.23	.29	.29	.19
	200	.40	.40	.15	.14	.14	.19	.24	.20	.24	.31	.41	.35
	500	.40	.40	.15	.15	.15	.21	.20	.20	.25	.35	.54	.52
	1000	.40	.40	.09	.09	.14	.21	.20	.10	.24	.36	.26	.13
30%	50	.40	.40	.09	.09	.07	.07	.20	.12	.19	.15	.35	.20
	100	.40	.39	.09	.09	.07	.08	.17	.15	.20	.21	.43	.38
	200	.40	.41	.06	.06	.07	.09	.20	.07	.17	.22	.25	.09
	500	.40	.40	.06	.06	.07	.11	.20	.08	.17	.28	.33	.15
	1000	.40	.40	.06	.06	.07	.13	.16	.10	.19	.33	.40	.26

Note. For each condition, distributions are based on 1,000 simulated samples with true population $\delta = .40$; N = total sample size (within-group sample sizes $n = N/2$); Medn = median; SD = standard deviation; MAD = median absolute deviation from the median.

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