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REPLY

Correcting for Range Restriction in Meta-Analysis:
A Reply to Oh et al. (2023)Paul R. Sackett¹, Christopher M. Berry², Filip Lievens³, and Charlene Zhang⁴¹ University of Minnesota, Minneapolis² Indiana University³ Singapore Management University⁴ Amazon

Oh et al. (in press) question a number of choices made in our article (Sackett et al., 2022); here we respond. They interpret our article as recommending against correcting for range restriction in general in concurrent validation studies; yet, we emphasize that we endorse correction when one has access to the information needed to do so. Our focus was on making range restriction corrections when conducting meta-analyses, where it is common for primary studies to be silent as to the prior basis for selection of the employees later participating in the concurrent validation study. As such, the applicant pool information needed for correction is typically not available. Sackett et al. highlighted that in many situations, range restriction will be small; so, the inability to correct for it results in only a modest underestimate of validity. Oh et al. mention settings that would result in substantial range restriction; here, we present our rationale as to why we view such settings as uncommon rather than as making up the bulk of the studies contributing to meta-analyses.

Keywords: meta-analysis, range restriction, validation

In a recent article, we (Sackett et al., 2022) identified previously unnoticed issues with the way range restriction corrections have been applied in prior meta-analyses of personnel selection tools. We offered revised estimates of operational validity (i.e., the level of validity expected after correcting for unreliability in the criterion and for range restriction, where appropriate), which are often quite different from the prior estimates. Oh et al. (in press) replied to our article, offering a different perspective on a number of choices we made and conclusions we drew. Here we respond to central themes in their reply.

The Statement That We Assert That One Should Not Correct for Range Restriction in Concurrent Validation Studies

Oh et al. (in press) attribute to us the position that one should not correct for range restriction in concurrent validity studies. This permeates their article and is even found in the title: "Revisiting Sackett et al.'s (2022) rationale behind their recommendation against correcting for range restriction in concurrent validation studies." Our recommendation is more nuanced: We argue against attempting a correction in such studies in the absence of the information needed for a correction. This is a very different message than recommending no corrections.

We note that in concurrent studies, the variable or composite of variables used for the original selection decisions is commonly unknown and unmeasured: Many research reports on concurrent validation studies do not speak to the issue of the original basis for selection. As correction requires the correlation between the predictor (x) being examined in the concurrent study and the original basis for selection (z), correction is not possible without an estimate of this r_{zx} in the applicant population, and without an estimate of the unrestricted and restricted standard deviation (SD) of z . In settings where one has no knowledge of z (e.g., a research report silent as to the basis for initial selection), we do not see a basis for a correction.

Correcting Concurrent Validity Estimates in Meta-Analysis Versus in Primary Studies

The focus of Sackett et al. (2022) was on correcting meta-analytic estimates of validity. Oh et al. (in press) appear to often be considering corrections in individual local (primary) validation studies and interpret our statements about meta-analytic corrections as applying equally to individual study corrections. If an individual study is in a setting where one has knowledge of the original basis for selection, we fully support using this information and making a correction for range restriction.

Sackett et al. (2022) challenged the typical practice in meta-analyses of computing the mean ratio of unrestricted to restricted predictor SD s from the subset of predictive validity studies where unrestricted predictor SD s were available and using this as the basis for applying a correction to all studies, both predictive and concurrent. We suggested applying the artifact distribution derived from predictive studies only to the set of predictive studies.

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This leads to the question of what to do with concurrent studies in terms of correction. There is often little to no information about the basis for initial selection in reports of studies. Absent such information, we argued that no correction is the conservative course of action.

Does choosing not to correct for range restriction in concurrent studies lead to substantial underestimates of validity? We suggested this will generally not be the case. Indirect range restriction (i.e., selection on a third variable z , the form of range restriction present in concurrent studies) will substantially restrict the range of the predictor of interest in the concurrent study only in very specific conditions. First, indirect range restriction can have large effects in concurrent studies with very high r_{zx} (e.g., .80–.90): in this case, u (an index of the amount of range restriction, which is the ratio of restricted to unrestricted SDs) drops below .90 at almost any selection ratio. As r_{zx} gets smaller, u falls below .90 only at increasingly smaller selection ratios. We suggested that the correlation between z and x will generally not reach this .80–.90 threshold, and that the meta-analytic mean r_{zx} will generally be less than the .50 value needed for u to drop below .90 even at smaller selection ratios. As a result, the degree of range restriction will typically be small. Thus, we argued that not correcting due to a lack of needed information will result in only a modest under-correction in a great many circumstances.

Central to the above argument is that the discussion is in the context of corrections in meta-analysis. The key issue is the average degree of range restriction across the concurrent studies compiled for the meta-analysis. It is indeed true that there are specific instances in which there are large correlations between x and z ; Sackett et al. (2022) offered the example of validating an alternate form of a predictor that had been used in selecting the sample of employees. We opined that it is hard to imagine that the studies compiled for meta-analysis come predominantly or exclusively from such settings.

Differing Positions on the Prevalence of Various Conditions in Validation Research

Sackett et al.'s (2022) Table 1 showed the u ratios resulting from various combinations of operational r_{zx} and selection ratio. In many ways, differences between Sackett et al. (2022) and Oh et al. (in press) rest on judgments each research team makes about the prevalence of each of these conditions. Oh et al. note that we do not offer citations to support our inferences about the frequency of these various combinations of r_{zx} and selection ratios but later acknowledge that this information is lacking in the field.

Oh et al.'s (in press) case requires that the bulk of the concurrent validation research contributing to meta-analyses comes either from (a) settings in which one does a concurrent study on a predictor with an extremely high correlation with the original basis for selection (r_{zx}) or from (b) settings with strong (i.e., greater than .50) correlations between z and x paired with a small selection ratio (i.e., .10 or smaller). Their case hinges on this because Sackett et al. (2022) showed that those are the only scenarios in which indirect range restriction can have a sizable effect on criterion-related validity. Thus, here we expand on our rationale for our belief that the bulk of concurrent validation research does not fall into categories (a) and/or (b) above.

First, there are a wide variety of settings in which one undertakes a concurrent validation study. As a rough taxonomy we offer the following. Category 1: A first foray into bringing systematic selection into a setting where formal testing/measurement had not been used. We view this as common and judge that r_{zx} will generally be quite small in such situations. Category 2: An attempt to broaden a selection system. Here a formal system is in place but it focuses on a small set of predictors. The goal is to broaden the set of KSAOs considered. An issue that especially calls attention to this strategy is the long-standing organizational concern for diversity. Many possible strategies for modifying selection systems to increase diversity have been proposed. Ployhart and Holtz (2008) reviewed evidence for 16 strategies and concluded that the only strategy that improves diversity without harm to validity is increasing the range of KSAOs included in the battery. We view adding a broader range of KSAOs as a common basis for a concurrent validity study. But this strategy involves searching for additional predictors with smaller subgroup differences in addition to having the potential to increase validity. Thus, new predictors are sought that have small correlations with predictors already in use, and thus we judge that r_{zx} will generally be quite small in such situations. Category 3: An attempt to replace an existing predictor with a more efficient/lower cost predictor. For example, could a situational judgment test provide insight into the KSAOs currently tapped via a structured interview? Here the intent is to capture similar KSAOs. We expect higher values of r_{zx} than in the first two categories, and the data Sackett et al. reviewed about intercorrelations among predictors are relevant. We found roughly .50 as the upper end of the range of correlations among various predictors, which according to Sackett et al.'s Table 1 would require the selection ratio to be as low as .01 to even result in a u ratio of .88. We view this category as less frequently used than the first two. Finally, Category 4 involves attempting to replace a predictor with a closely related measure of the same construct. For example, an organization may decide to change test vendors and switch to a different test targeting the same construct. Or a firm may replace a written situational judgment test with one with similar content but presented via video. We note that the correlations among measures targeting the same construct vary widely. In the cognitive ability domain, correlations are high, as Oh et al. (in press) discuss. Here one may indeed see r_{zx} values in the .80–.90 range. But in other domains, far smaller relationships are found. Ones (1993) reported mean corrected correlations of .45 between overt integrity tests, .70 between personality-oriented integrity tests, and .39 between overt and personality-oriented integrity tests. Hogan and Ones (1997) reported a mean correlation of .47 between different measures of conscientiousness. So, Category 4 is where one can find at least some values of r_{zx} large enough to result in substantial range restriction. Thus, for there to be substantial range restriction affecting the concurrent validity, studies included in selection meta-analyses require the assumption that the average validity study not only falls into Category 4 but further is a Category 4 validity study using something along the lines of a new cognitive ability test to replace an old cognitive ability test. In sum, across categories, we continue to believe that the mean r_{zx} across studies contributing to a meta-analysis will be modest in magnitude, and failing to correct for range restriction due to lack of knowledge of z will result in only a modest underestimate.

An additional feature influencing our thinking on the issue of the expected value of mean r_{zx} is the fact that in many settings z includes not only one or more specific formal selection tools but also discretionary decision-maker judgment. Tests can be used in at least two fundamentally different ways: (a) central control by a testing authority, as in requiring top-down selection or requiring that no one below a specified cutoff can be hired and (b) test information provided to the hiring manager, who has discretion as to whether to accept or reject the information. We view the latter as common, and see many systems in which a vendor offers a recommendation (some variant on “green,” “yellow,” “red” or “thumbs up,” “caution,” “thumbs down”). We have long admired a article by Brown (1979) examining a carefully validated biodata system used in the life insurance industry. Hiring managers received a pass–fail evaluation but had discretion as to who to hire. High success rates for those hired despite a “fail” were viewed by Brown as reflecting managers working harder to train and support candidates for whom they had gone out on a limb and hired despite the formal recommendation not to. The key idea here is one may know that a given predictor is part of z but it would be incorrect to equate z with scores on that predictor. We expect r_{zx} to, on average, be smaller than the correlation between x and the predictor known to be a contributor to z .

In sum, we are thus not convinced by Oh et al.’s (in press) counterargument that high r_{zx} values are widespread. One example they offer is our Category 4 above. We agree this can produce high r_{zx} values but have a hard time with the notion that such scenarios dominate the set of studies going into a meta-analysis. As another example, they offer the scenario in which the predictor of interest x is part of the composite z that was used for hiring decisions. If x is part of z , they argue, then r_{zx} is a part-whole correlation and as such will commonly be quite large. We agree that the “ x is part of z ” scenario is an issue in applications of indirect range restriction. However, we suggest that this will be uncommon in concurrent validation studies. If x was measured in applicants, one would simply collect criterion data and conduct a predictive validity study. One can conjure up settings where one might readminister a predictor to incumbents, such as loss of the data files containing the original predictor scores, but we see this as the exception, not the norm.

The Definition of “ r_{zx} ”: A Key Concept in the Article

Oh et al. (in press) stated:

The key to evaluating Sackett et al.’s (2022) statement regarding the value of “ r_{zx} ” is understanding exactly what this correlation means. Unfortunately, they do not make it clear. Ostensibly, the “ r_{zx} ” can be easily construed as an observed/restricted sample correlation (r_{zx_i}) between the third variable Z where actual selection occurred ... and the observed score X .

Here is our text from p. 2042: “we show the effect on SD_x of indirect range restriction due to selection on a third variable z as two things vary ... The first is the unrestricted correlation between z and x , with no measurement error in either variable.” Thus, we did define “ r_{zx} ” as an unrestricted correlation in Sackett et al. (2022).

Although it is not related to Oh et al.’s suggestion that we did not define r_{zx} as an unrestricted correlation, in a friendly review, a

colleague notes one minor area in which we were inconsistent in our use of “ r_{zx} .” In the first two panels of Table 1 in Sackett et al. (2022), we presented findings with no measurement error. But in the subsequent panels, we reported findings with varying degree of measurement error, still labeling the resulting correlations as “ r_{zx} .” If “ T ” is the true score of x , it might have been better to label those correlations as r_{zT} in Panels 1 and 2 and then as r_{zx} in the subsequent panels. We apologize if this caused any confusion.

The Statement That We Reported Uncorrected r_{zx} Values From Incumbents and That Correcting These for Statistical Artifacts Would Result in r_{zx} Values of .90

On article page 9, Oh et al. (in press) wrote: “Our issue is that although the “ r_{zx} ” in their Table 1 is (and should be) in fact an r_{zL_a} value, the “ r_{zx} ” they used in other places appears to be an r_{zx_i} value.” Then, on page 11 Oh et al. wrote: “Those correlations (r_{zx_i}), if they were properly corrected for [range restriction] and measurement error, are likely to correspond to the r_{zL_a} values of around .90 when the selection ratios are realistic.” Oh et al. are referring to intercorrelations between selection predictors (r_{zx} ’s) that Sackett et al. (2022) reported on p. 2042. Many of those r_{zx} ’s were, in fact, corrected values, so they would obviously not be as high as .90 when corrected for statistical artifacts. Four r_{zx} ’s we reported were not fully and appropriately corrected or we could not determine if they were; all came from Schmidt and Hunter (1998). The r_{zx} ’s that cognitive ability had with work samples (.38) and job knowledge (.48) were only corrected for reliability but not for range restriction. We cannot tell whether the r_{zx} ’s that cognitive ability had with biodata (.50) or assessment centers (.50) in Schmidt and Hunter (1998) were corrected. Schmidt and Hunter’s biodata-ability correlation is from Rothstein et al. (1990) and they did not report whether that specific correlation was corrected; they did note that there was little to no range restriction on biodata in their data; though, so range restriction should not be a significant concern. Schmidt and Hunter’s ability-assessment center correlation is from an unpublished article by Collins (1998) that we cannot obtain. However, Sackett et al. noted there were more recent meta-analyses of this relationship, reporting smaller intercorrelations that were corrected: .45 and .27 in Meriac et al. (2008) and Hoffman et al. (2015), respectively. Thus, the possible lack of corrections for Schmidt and Hunter’s ability-biodata and ability-assessment center correlations are not masking fully corrected correlations in the .90 range. We agree with Oh et al. that we should have only reported appropriately corrected r_{zx} ’s and acknowledge that the reliability-corrected correlations that cognitive ability had with work samples (.38) and job knowledge (.48) in Schmidt and Hunter were not corrected for range restriction. It is thus worth asking whether these correlations would reach .90 if they were appropriately corrected for range restriction. We think they would not in most circumstances. Per Oh et al.’s Table 1, a selection ratio of around .10 is required for a range-restricted correlation of .48 to correct to .90; a selection ratio of around .01 is required for a range-restricted correlation of .38 to correct to .90. We do not view it as likely that the average selection ratios that were used to hire job incumbents in the samples contributing to Schmidt and Hunter’s ability-work sample and ability-job knowledge meta-analyses were in the .01–.10 range. Even if this was the case, it would only be the case for these two

selection predictor pairs and in Sackett et al., we reviewed many other selection predictor pairs that had corrected intercorrelations much lower than .90. Thus, we believe that r_{zx} values as high as .90 are not the rule, especially when r_{zx} refers to a meta-analytic average correlation between selection predictors. Further, we believe it is worth considering what it would mean if r_{zx} was truly commonly as high as .90, as Oh et al. suggest. This would mean that in applicant pools, selection predictors are commonly intercorrelated as highly as .90. This raises the question of why firms would include such highly correlated, and thus redundant, predictors in selection batteries. We believe they do not.

The Statement That We View A Selection Ratio of .30 as “Extreme”

On article page 12, Oh et al. (in press) wrote: “Sackett et al. (2022) state that selection ratios of .10–.30 are extremely low.” We reviewed Sackett et al. and found 12 instances of the use of the word “extreme.” The only selection ratios used in conjunction with “extreme” are .10 and .05. To be clear, we do not view selection ratios of .30 as “extreme.” It is the case that in individual selection settings, one can find selection ratios of .10 or smaller. We view selection ratios of .10 or smaller as “extreme” in the case of *meta-analytic averages*.

The Statement That We Underestimate Validity by Using Case IV Rather Than Case V Corrections

Sackett et al.’s (2022) Table 5 presented simulated data showing operational validity estimates for one predictor (general cognitive ability) at various combinations of selection ratio and r_{zx} . We made use of Case IV range restriction corrections (Le et al., 2006) in the table. Oh et al. (in press) argue that a recent development, namely, Case V, is superior, as it does not require assumptions about the mediating mechanism between the predictor and criterion (Le et al., 2016). Oh et al., state that Case IV commonly produces a serious underestimate of validity.

It is useful to elaborate on the Case V method. It applies to settings in which one has restricted and unrestricted SD s for both x and y . Le et al. (2016), who applied the Case V label to the approach, noted that it will generally not be applicable in personnel selection settings, as unrestricted SD_y is not known. That is, Le et al. posited:

One important obstacle for applying the Case V method is that it may be difficult or impossible to obtain information about the SD of measure Y (SD_Y) in the unrestricted population. For example, this is likely to be true when measure Y is job performance, and thus the Case V method cannot generally be used to correct relationships involving performance for range restriction (p.1004).

Oh et al. (in press) are only able to make use of Case V in their work because their study is a simulation, allowing them to specify the unrestricted SD_y that would not be known in applied settings.

However, in order to use Case V in their simulation, they need to also assume a value for the validity of z (i.e., r_{zy}). Crucially, all of their analyses are based on the assumption that z had a validity of .50. When one applies Case V corrections with lower validities of z the Case V corrected values quickly start to get much more similar to our Case IV corrected validities. Oh et al. offer one worked

example with operational r_{zx} and r_{zy} both equal to .50, and a SR of .50. They obtain a Case V-corrected value of .403 in contrast to Sackett et al.’s (2022) Case IV-corrected value of .334. However, this finding hinges on the assumed r_{zy} of .50. When r_{zy} is reduced to .40, Case V-corrected r_{xy} is .38; when r_{zy} is reduced to .30, Case V-corrected r_{xy} is .36. Further, if r_{zy} is low (i.e., about .20), the Case IV (.334) and V (.337) corrected validities are about the same, and if the r_{zy} is lower than about .20, the Case IV correction actually produces larger corrected correlations than the Case V correction. So, the key question is whether the validity of z was as high as .50 in the studies contributing to the cognitive ability meta-analyses Sackett et al. reviewed. That would mean that, on average, the unknown third variable on which selection occurred in these meta-analyses was a highly valid selection method (i.e., validity or r_{zy} of .50). Given the common range of validities observed in our field, our opinion is that this is unlikely.

Rejecting Our “Principle of Conservative Estimation”

Oh et al. (in press) rejected our principle of conservative estimation. They posit that surely range restriction is not zero and as such a correction should be made. Even if the correction is erroneous and results in overcorrection, they view the attempt at correcting as superior to our argument that absent the data needed for correction, it is better to not attempt a correction. They mention that there are “good enough” alternatives for attempting a correction, and that we are demanding “impossibly perfect solutions.”

The “good enough” alternatives they suggest are (a) relying on publisher norms as the basis for an unrestricted SD_x and (b) conducting sensitivity analyses. Sackett et al. (2022) have a section on the use of publisher norms and argue that these make sense in some settings such as job family-specific norms. Regarding sensitivity analyses, we have no objections to these but are unclear how this solves the problem of not having information about z , the basis for initial selection. If a concurrent validation study contributing to a meta-analysis contains no information about the basis for initial selection, and no further information about the study is available from its authors, all one could do is assemble a version of our Table 1: Examine a broad range of possible values of r_{zx} and selection ratios, which would result in a wide range of possible corrected values.

We thus find ourselves unable to see any alternative Oh et al. (in press) have to offer for the prototypical situation Sackett et al. (2022) faced in revisiting prior meta-analyses. Common practice is to include an artifact distribution of u values from predictive validity studies but *not* to code whether studies contributing to the meta-analytic validity are predictive or concurrent. If that information were available, we could apply a correction to the predictive studies based on the artifact distribution but would then face the question of what to do with the concurrent studies. For most older meta-analyses, the response “go back to the original studies and see what information about z can be gleaned from them” is not tenable because including a list of the studies contributing to the meta-analysis has now become the norm but was not in the past. We concluded that we did not have the needed information to attempt a correction.

We understand the instinct to want to correct: if there is an issue one wants to address it. But we are uncomfortable with strategies

such as “let’s assume that r_{zy} was .50 and make a correction.” Given a choice between a correction based on an assumed value, which could overcorrect as easily as it could undercorrect, and not attempting a correction, we repeat our prior commitment to the principle of conservative estimation and endorse making no correction.

The Bottom Line: Do the Central Conclusions of Sackett et al. (2022) Still Hold?

We believe our central conclusions still hold. Importantly, Oh et al. (in press) do not question our key insight, namely, that u ratios derived from predictive studies cannot be applied to concurrent studies. We both agree that there are circumstances under which restriction can indeed be substantial in concurrent studies. We outlined above the basis for our belief that such circumstances are the exception rather than the rule. We stand by our message of “do not correct for range restriction without the information needed for doing so.”

Finally, we stress that we do not question that many of the predictors developed and validated over more than a century in the field of personnel selection have value and impressive validity, even if it is somewhat lower than the field thought prior to Sackett et al. (2022). We still concur with the statement that “The size of validity coefficients is one of the most remarkable achievements in psychology” (Schmitt, 2014; p.58). We also emphasize that our challenge to methods of correcting for range restriction used in meta-analysis should not be interpreted as an argument against the value of meta-analysis. Meta-analysis provides invaluable information about validity findings for various predictors. If the information needed for range restriction correction is unavailable, meta-analysis can still offer useful information about the observed mean and variance of validity across studies, thus informing future selection system development and validation. Corrections can be made for subsets of studies where the needed information is provided (e.g., correcting predictive validity studies using an artifact distribution derived from predictive studies). Findings can be accompanied by acknowledgment that the results are conservative in the face of a lack of needed information for subsets of studies.

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