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Scale Coarseness as a Methodological Artifact

Correcting Correlation Coefficients Attenuated From Using Coarse Scales

Herman Aguinis
University of Colorado Denver
Charles A. Pierce
University of Memphis
Steven A. Culpepper
Metropolitan State College of Denver

Scale coarseness is a pervasive yet ignored methodological artifact that attenuates observed correlation coefficients in relation to population coefficients. The authors describe how to disattenuate correlations that are biased by scale coarseness in primary-level as well as meta-analytic studies and derive the sampling error variance for the corrected correlation. Results of two Monte Carlo simulations reveal that the correction procedure is accurate and show the extent to which coarseness biases the correlation coefficient under various conditions (i.e., value of the population correlation, number of item scale points, and number of scale items). The authors also offer a Web-based computer program that disattenuates correlations at the primary-study level and computes the sampling error variance as well as confidence intervals for the corrected correlation. Using this program, which implements the correction in primary-level studies, and incorporating the suggested correction in meta-analytic reviews will lead to more accurate estimates of construct-level correlation coefficients.

Keywords: *scale coarseness; Likert-type scale; correlation coefficient; meta-analysis; artifact*

Proponents of psychometric approaches to meta-analysis extend arguments from measurement theory to contend that effect sizes such as correlation coefficients are often underestimated in primary-level studies because of the operation of methodological and

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statistical artifacts (e.g., Aguinis & Pierce, 1998; Aguinis, Sturman, & Pierce, 2008; Schmidt, 2008). Hunter and Schmidt (2004) provided a description of the 11 known methodological artifacts (e.g., sampling error, measurement error, direct range restriction) that reduce the magnitude of observed correlations in relation to their population counterparts. Because methodological artifacts have a large impact on the magnitude of estimated correlations, there is ongoing interest in the organizational sciences in the biasing effects of methodological artifacts as well as how to minimize these biases (e.g., Le, Oh, Shaffer, & Schmidt, 2007; Schmidt, Oh, & Le, 2006). Perhaps just as important, the organizational sciences, like any other scientific field, need to continue to improve the accuracy of measurements and estimations of theoretically important parameters (Aguinis, 2001; Schmidt et al., 2006). This need provides the impetus for the ongoing interest in procedures for estimating parameters such as correlation coefficients more accurately (e.g., see Hunter, Schmidt, & Le, 2006).

In this article, we address a methodological artifact that can produce a downward bias in observed correlations but has thus far received virtually no attention in the organizational sciences: *scale coarseness*. This is an important problem for the organizational sciences and related fields because, although this biasing artifact remains largely unacknowledged, in many situations researchers may have underestimated construct-level correlations because of scale coarseness.

Our article is organized as follows. First, we describe scale coarseness and its effects on the correlation coefficient and we discuss implications of scale coarseness for organizational science theory and practice. Second, we describe a method that corrects for the downward biasing effect of scale coarseness. Third, we describe results of two Monte Carlo simulations that examine the accuracy of the correction procedure and the extent to which scale coarseness attenuates the correlation coefficient under various conditions (i.e., value of the population correlation, number of item scale points, and number of scale items). Fourth, we derive the currently unknown sampling error variance and confidence intervals for the corrected correlation. Fifth, we offer a user-friendly, Web-based computer program that enables researchers to implement the correction for individual correlations as well as the computation of sampling error and confidence intervals for the corrected correlation. Finally, we describe a new procedure for incorporating the scale coarseness correction in a meta-analysis. In short, the goals of this article are to: (a) identify scale coarseness as a pervasive and important, yet often ignored, methodological artifact in organizational science research; (b) demonstrate the biasing effects of scale coarseness under various conditions; and (c) provide a solution to mitigate these biasing effects in future primary-level as well as meta-analytic research. As noted by Vandenberg (2008), our goal is to address an issue that has ''immediate applicability to researchers both methodologically and empirically'' (p. 7).

Scale Coarseness

A measurement scale is coarse when a construct that is continuous in nature is measured using items such that different true scores are collapsed into the same category. In these situations, errors are introduced because continuous constructs are collapsed or crippled (Bollen & Barb, 1981). Although this fact is seldom acknowledged, organizational science researchers use coarse scales every time continuous constructs are measured using Likert-type (Bollen & Bard, 1981) or ordinal (O'Brien, 1979) items.[1] We are so accustomed to using these types of items that we seem to have forgotten they are intrinsically coarse. As noted by Blanton and Jaccard (2006), "scales are not strictly continuous in that there is coarseness due to the category widths and the collapsing of individuals with different true scores into the same category. This is common for many psychological measures, and researchers typically assume that the coarseness is not problematic" (p. 28).

As an illustration, consider a typical Likert-type item including 5 scale points or anchors ranging from 1 (*strongly disagree*) to 5 (*strongly agree*). When one or more Likert-type items are used to assess continuous constructs, such as job satisfaction, personality, organizational commitment, and job performance, information is lost because individuals with different true scores are considered to have identical standing regarding the underlying construct. Specifically, all individuals with true scores around 4 are assigned a 4, all those with true scores around 3 are assigned a 3, and so forth. However, differences may exist between these individuals' true scores (e.g., 3.60 vs. 4.40 or 3.40 vs. 2.60, respectively), but these differences are lost due to the use of coarse scales because respondents are forced to provide scores that are systematically biased downwardly or upwardly. This information loss produces a downward bias in the observed correlation coefficient between a predictor X and a criterion Y. In short, scales that include Likert-type and ordinal items are coarse, imprecise, do not allow individuals to provide data that are sufficiently discriminating, and yet they are used pervasively in the organizational sciences to measure constructs that are continuous in nature. This is unfortunate, given that cognitive psychologists and psychometricians have concluded that humans are able to provide data with greater degree of discrimination and precision by using scales with as many as 20 (Garner, 1960) or 25 (Guilford, 1954) points.

Unique Bias Introduced by Scale Coarseness

Scale coarseness creates a form of nonlinear and systematic error (Symonds, 1924) and creates a bias that is distinct from the effects of random measurement error and the dichotomization of predictor and/or criterion variables. Next, we discuss the relationship between scale coarseness and (a) measurement error and (b) dichotomization.

Scale coarseness and measurement error. Scale coarseness and random measurement error are both impossible to correct at the individual level. However, the *random* error created by lack of perfect reliability of measurement is different in nature from the *systematic* error introduced by scale coarseness, so these artifacts are distinct and should be considered separately. Based on classic measurement theory, $X_t = X_o + e$, where X_t is the true score, X_o is the observed score, and e is the error term, which is composed of a random and a systematic (i.e., bias) component (i.e., $e = e_r + e_s$). For example, consider an individual who has a true score of 4.4 on the latent construct "trust in your supervisor" (i.e., $X_t = 4.4$). A measure of this construct is not likely to be perfectly reliable, so if we use a multi-item Likert-type scale, X_o is likely to be greater than 4.4 for some of the items and less than 4.4 for some of the other items, given that e_r can be positive or negative because of its random nature. On average, the greater the number of items in the measure, the more likely it is that positive and negative e_r values will cancel out and X_o will be closer to 4.4. So, the greater the number of items for this scale, the less the detrimental impact of random measurement error on the difference between true and observed scores; this is an important reason why multi-item scales are preferred over single-item scales.

Now, in sharp contrast to the measurement error discussion above, let's consider the scale coarseness artifact. If we use a scale with only one Likert-type item with, for example, 5 scale points, X_o is systematically biased downwardly because this individual respondent will be forced to choose 4 as his response (i.e., the closest to $X_t = 4.4$) given that 1, 2, 3, 4, and 5 are the only options available. If we add another item and the scale now includes 2 items instead of only one, the response on each of the 2 items will be biased systematically by $-.4$ due to scale coarseness. So, in contrast to the effects of measurement error, the error caused by scale coarseness is systematic and the same for each item. Consequently, increasing the number of items does not lead to a canceling out of error. Similarly, an individual for whom $X_t = 3.7$ will also choose the option 4 on each of the items for this multi-item Likert-type scale (i.e., the closest to the true score, given that 1, 2, 3, 4, and 5 are the only options available). So, regardless of whether the scale includes one or multiple items, information is lost due to scale coarseness and these two individuals with true scores of 4.4 and 3.7 will appear to have an identical score of 4.0.

As noted eloquently by Russell and Bobko (1992) within the specific context of detecting interaction effects, but equally applicable to any other situation in which scales are coarse,

> summing responses to multiple Likert-type items on a dependent scale (as is often done in between-subjects survey designs) is not the same as providing subjects with a continuous response scale. Although the resultant "scale score" obtained by summing item responses could be considered nearly continuous, subjects are not responding with a scale score. Rather, they are responding to each item individually. Thus, if an individual responds to coarse Likert scales in a similar manner across items, the problem of reduced power to detect interaction remains. . . information loss that causes systematic error to occur at the item level would have the same effect on moderated regression effect size regardless of whether the dependent-response items were analyzed separately (as was done here in a within-subject design) or cumulated into a scale score. (p. 339)

Evidence accumulated over the past half a century supports the argument that scale coarseness and measurement error have independent effects in most situations. For example, Bendig (1953) provided evidence that the number of scale points is independent of the scale's reliability. Specifically, study participants rated their knowledge of 12 different countries using items with either 3, 5, 7, 9, or 11 scale points. Results showed that reliability estimates were relatively constant with a slight drop in reliability for the 11 scale-point condition. In a follow-up study, Bendig (1954) also provided evidence regarding the lack of relationship between scale coarseness and reliability. In that study, 236 participants rated a list of 20 foods (i.e., a 20-item scale) using items with 2, 3, 5, 7, or 9 scale points. Thus, in contrast to Bendig (1953), Bendig (1954) used a multi-item scale. He concluded that "The results regarding test reliability are fairly unequivocal. No consistent trend was found in the relation of test reliability and number of scale categories" (p. 39). Moreover, "test reliability was constant over the entire range of categories and was very similar to reliabilities found in another study. . .. It was concluded that test reliability is independent of the number of scale categories" (p. 40).

About 10 years later, Komorita and Graham (1965) collected data from 260 students using two different scales (i.e., Semantic Differential adjectives and California Psychological Inventory), which vary regarding item homogeneity. Also, they manipulated the number of item scale points by using either 2 or 6. The conclusion was:

> With a relatively homogeneous set of items, the reliability of a scale is independent of the number of item scale-points. If the items are relatively heterogeneous, however, the results suggest that the reliability of the scale can be increased not only by increasing the number of items but also by increasing the number of item scale-points. (p. 993)

Also, he argued that "based on the evidence adduced thus far, it would seem that reliability should not be a factor considered in determining Likert-scale rating format, as it is independent of the number of scale steps employed" (p. 670).

In a more detailed examination of the effects of scale coarseness, Mattell and Jacoby (1971) used 18 different groups of 20 students each, who were assigned to experimental conditions varying in terms of number of scale points (i.e., from 2 to 19). In contrast to previous investigations, Mattell and Jacoby computed not only internal consistency but also stability (i.e., test-retest) reliability estimates. The conclusion was that "both reliability measures, test-retest and internal consistency, were found to be independent of the number of scale-points" (p. 666).

Finally, Aiken (1983) administered a teaching evaluation instrument consisting of a multi-item scale with 10 items to 624 undergraduate students in freshman-level courses. He varied scale coarseness by using the same items but varying the number of scale points from 2 to 7. He concluded that "although increased variance is usually associated with increased reliability, such was not the case in this investigation" (p. 400). The internal consistency reliability estimates (alphas) showed no systematic change associated with various levels of scale coarseness. Accordingly, Aiken (1983) concluded that "the fact that the internal consistency reliability coefficients remained relatively constant despite the increase in response variance indicates that efforts to increase the spread of responses by employing a greater number of response categories will not necessarily improve scale reliability" (p. 401).

In sum, the effect of measurement error is random and increasing the number of items decreases the impact of the measurement error artifact on the difference between population and observed correlations. In contrast, the effect of scale coarseness is systematic and increasing the number of items does not necessarily decrease its impact on loss of information due to the collapsing of different true scores within the same category. In addition, evidence accumulated thus far supports the conclusion that, in most situations, scale coarseness and measurement error have independent effects.

Scale coarseness and dichotomization. The scale coarseness artifact is also distinct from the dichotomization of predictor and/or criterion variables artifact. Scale coarseness is an artifact that is introduced when data are collected and is due to the nature of the measurement instrument used and, therefore, it is a research design artifact. In contrast, the dichotomization artifact is introduced after data are collected and then scores are split (e.g., at the median) creating "high" and "low" categories (Cohen, 1983), so dichotomization is a data-analysis artifact. In other words, the bias created by scale coarseness is not related to the bias created by dichotomization, and the presence or absence of scale coarseness is not related to the presence or absence of dichotomization. For example, assume a situation in which there is severe scale coarseness and both the predictor and criterion variables are measured using 3 scale points each (e.g., *below average*, *average*, and *above average* anchors). In this situation, a researcher would apply the scale coarseness correction to the observed correlation coefficient. In addition, prior to computing the correlation coefficient, a researcher may have chosen to split the data into the following two groups based on the criterion scores: (a) individuals scoring above the median and (b) individuals scoring below the median. In this situation, one would apply the dichotomization correction to the observed correlation because the criterion scores were artificially dichotomized. If, on the other hand, the sample is not split into these high and low subgroups, then one would not apply the dichotomization correction but would still apply the scale coarseness correction. In sum, scale coarseness is a research design-based artifact, whereas dichotomization is a data-analysis-based artifact and, hence, they are independent of each other and should be considered separately (cf. Hunter & Schmidt, 2004, pp. 112-115).[2]

Effects of Scale Coarseness on Correlation Coefficients: Implications for Organizational Science Research

The lack of precision introduced by coarse scales has a downward biasing effect on the correlation coefficient computed using data collected from such scales for the predictor, the criterion, or both variables. For example, consider the case of a correlation computed based on measures that use items anchored with 5 scale points. In this case, a population correlation of .50 is attenuated to a value of .44 (we describe the correction procedure later in the article).[3] A difference between correlations of .06 indicates that the correlation is attenuated by about 14%.

The above is a realistic yet hypothetical example. Are the implications of scale coarseness actually meaningful? To answer this question, we first note that scale coarseness is a pervasive phenomenon across a variety of organizational science domains, given that the use of coarse measures including as few as 5, 4, or even 3 scale points is quite common. In fact, this type of operationalization of continuous constructs is ordinary practice in most organizational science domains because of the popularity of Likert-type scales. We reached this conclusion after reviewing recent issues of *Academy of Management Journal* (*AMJ*), *Journal of Management* (*JoM*), *Journal of Applied Psychology* (*JAP*), and *Personnel Psychology* (*PPsych*), in which we had no difficulty finding numerous articles in each issue that used coarse scales to assess continuous constructs (see Table 1 for examples).

To provide a more precise answer to the question of whether the implications of scale coarseness are meaningful, we assessed the underestimation in correlation coefficients, and coefficients of determination (r^2), in the recently published literature (we describe the correction procedure in detail later in the article). Specifically, we reviewed articles published

Table 1
Selective Literature Review Assessing the Impact of Scale Coarseness on the Underestimation of the Coefficient of Determination (r^2)

Study	Predictor	Criterion	Reported r (r^2)	Corrected r (r^2)	Underestimation in r^2 (%)
Allen et al. (2006)	Mentor commitment	Program effectiveness	.38 (.14)	.43 (.18)	28
Barrick et al. (2007)	Team interdependence	Team performance	.17 (.03)	.19 (.04)	25
Brown et al. (2006)	Self-efficacy	Job search behavior	.13 (.02)	.15 (.02)	33
Cramton et al. (2007)	Task cohesion	Satisfaction with team	.18 (.03)	.20 (.04)	24
Dierdorff & Surface (2007)	Social interaction	Tactical proficiency	.71 (.50)	.80 (.64)	27
Fedor et al. (2006)	Job level change	Organizational commitment	−.13 (.02)	−.15 (.02)	33
Ferrin et al. (2006)	Coworker citizenship	Employee trust	.47 (.22)	.53 (.28)	27
Fong & Tosi (2007)	Conscientiousness	Agreeableness	.13 (.02)	.15 (.02)	33
George & Zhou (2007)	Positive mood	Creativity	.25 (.06)	.28 (.08)	25
Griffin et al. (2007)	Team proactivity	Organization proactivity	.79 (.62)	.89 (.79)	27
Klein et al. (2007)	Goal orientation	Motivation to learn	.22 (.05)	.25 (.06)	29
Lam et al. (2007)	Impression motives	Leader–member exchange	−.04 (.00)	−.05 (.00)	56
Marcus et al. (2007)	Agreeableness	U.S. overt integrity	.25 (.06)	.28 (.08)	25
Marler et al. (2006)	Ease of use	Intent to use technology	.27 (.07)	.29 (.08)	15
McKay et al. (2007)	Diversity climate	Turnover intentions	−.26 (.07)	−.29 (.08)	24
Meglino & Korsgaard (2007)	Previous job attributes	Recent job attributes	.15 (.02)	.16 (.03)	14
Rogelberg, Leach et al. (2006)	Task interdependence	Intentions to quit	−.08 (.01)	−.10 (.01)	56
Vecchio & Brazil (2007)	Performance	Satisfaction with leader	.10 (.01)	.11 (.01)	21
Wright & Bonett (2007)	Job satisfaction	Psychological well-being	.37 (.14)	.40 (.16)	17

Note: For underestimation in r^2, Mean (SD) = 29% (11%). For underestimation in r, Mean (SD) = 13% (5%).

in 2006 and 2007 in *AMJ*, *JoM*, *JAP*, and *PPsych*. We acknowledge that this is not an exhaustive or even a comprehensive review. Such a review would include the vast majority of articles ever published in these journals given the pervasiveness of Likert-type scales in organizational science research. Instead, in our review we selected a few articles a priori without looking at the correlations reported or the number of scale points used in the measures. Articles we found include coarse measures of team cohesion, team communication, and team interdependence (Barrick, Bradley, Kristof-Brown, & Colbert, 2007); mood states at work (George & Zhou, 2007); team member and organization member proactivity (Griffin, Neal, & Parker, 2007); supervisor-attributed impression management motives and quality of leader–member exchange (Lam, Huang, & Snape, 2007); job satisfaction and psychological well-being (Wright & Bonett, 2007); conscientiousness (Fong & Tosi, 2007); accuracy of recent and previous job attributes (Meglino & Korsgaard, 2007); intention to use technology (Marler, Liang, & Dulebohn, 2006); interpersonal organizational citizenship behaviors, employee trust in coworkers, and interpersonal communication (Ferrin, Dirks, & Shah, 2006); task interdependence (Rogelberg, Leach, Warr, & Burnfield, 2006); job search self-efficacy (Brown, Cober, Kane, Levy, & Shalhoop, 2006); personality and integrity (Marcus, Lee, & Ashton, 2007); organizational commitment, diversity climate perceptions, and turnover intentions (McKay et al., 2007); peer ratings of performance (Dierdorff & Surface, 2007); learning goal orientation and motivation to learn (Klein, Noe, & Wang, 2007); job level change and change commitment (Fedor, Caldwell, & Herold, 2006); and mentor commitment and perceived program effectiveness (Allen, Eby, & Lentz, 2006), among others. Thus, related to our earlier assertion regarding the pervasiveness of the scale coarseness artifact, it seems it would be difficult to name an area within the organizational sciences in which the scale coarseness artifact has not produced downwardly biased estimates of how well a predictor relates to a criterion.

Table 1 summarizes results of our review. Scale coarseness caused an average underestimation in r of about 13% and an average underestimation in r^2 of about 30%. In other words, on average, scale coarseness led researchers to conclude that their predictor explains about 30% less of the variance in their criterion than is actually the case. So, the underestimation is meaningful. But, is it nontrivial and can it lead to incorrect inferences? We provide four different types of arguments and evidence to answer this question in the affirmative.

First, an attenuation in the correlation coefficient between 10% to 20% is comparable to, and in some cases larger than, the percent of attenuation typically caused by measurement error (e.g., Bowling & Beehr, 2006; Halbesleben, 2006) and range restriction (Hunter et al., 2006) in some organizational science domains. Besides sampling error, measurement error and range restriction are the two methodological artifacts most frequently studied in the organizational sciences and are also the two methodological artifacts for which meta-analysts correct most frequently (Hunter & Schmidt, 2004).

We reviewed recent issues of several organizational science journals to compare the relative impact of the measurement error and scale coarseness artifacts on estimates of the correlation coefficient. Consider the case of the relationship between subordinates' degree of coaching guidance and degree of social support from supervisor (Heslin, Vandewalle, & Latham, 2006). Coaching guidance, which is a continuous construct, was measured using a 5-point agreement scale with anchors ranging from 1 (*not at all*) to 5 (*to a very great* *extent*). The internal consistency reliability estimate for this scale was .92. Social support was also measured using a 5-point scale with anchors ranging from 0 (*don't have any such person*) to 4 (*very much*) and this scale's reliability was .79. The observed correlation between coaching guidance and support was .46. Applying the measurement error correction to both variables to obtain the disattenuated correlation (i.e., $r_{xy}/[\sqrt{r_{xx}}\sqrt{r_{yy}}]$) leads to a value of .54. Alternatively, using the scale coarseness correction (which we describe in more detail later in this article) leads to a comparable value of .52. As a second recent illustration, consider the case of the relationship between data usage/good use of surveys, measured on a 5-point scale ranging from 1 (*strongly disagree*) to 5 (*strongly agree*; reliability = .87), and attitudes toward (i.e., like) filling out surveys, also measured using a 5-point scale ranging from 1 (*strongly disagree*) to 5 (*strongly agree*; reliability = .92; Rogelberg, Spitzmüeller, Little, & Reeve, 2006). The corrected correlation based on measurement error on both the predictor and the criterion leads to a corrected value of .34, and applying the scale coarseness correction also leads to a value of .34. Given the accumulated knowledge regarding the detrimental effects of artifacts such as measurement error on estimates of correlation coefficients (Schmidt & Hunter, 1996), it would be incorrect to argue that the measurement error correction should not be implemented because the biasing effect of measurement error is "too small." Accordingly, if scale coarseness causes a degree of attenuation comparable to the attenuation caused by measurement error, then scale coarseness should also be considered a nontrivial methodological artifact.

Second, attenuations in the 10% to 20% range are larger than the percentage improvement in the estimation of the sampling error variance of the correlation coefficient caused by taking into account indirect range restriction (Aguinis & Whitehead, 1997) and an improved formula (Aguinis, 2001). Thus, if improvements in the estimation of sampling error variance in the correlation coefficient in the 5% to 10% range are considered sufficiently important for substantive conclusions in organizational science research (Aguinis, 2001; Aguinis & Whitehead, 1997), then improvements in the estimation of the correlation coefficient in the 10% to 20% range should also be taken seriously.

Third, we argue that, in some research domains, an underestimation in r in the order of 10% to 20% can lead to imprecise inferences. An underestimation in r of about 13% may be seen as a small effect. However, it is generally not appropriate to equate Cohen's (1988) definitions of small and large effects with *unimportant effect* and *important effect*, respectively, without taking into account the research context and domain in question (Aguinis, Beaty, Boik, & Pierce, 2005; Aguinis & Harden, in press). Cohen (1988) noted that "the terms 'small,' 'medium,' and 'large' are relative, not only to each other, but to the area of behavioral science or even more particularly to the specific content and research method being employed in any given investigation" (p. 25). Specifically, in some contexts, what may be labeled as a "small" effect using Cohen's definitions can actually have important consequences for organizational science theory and practice. For example, consider the result reported in Table 1 that the underestimation in r^2 is, on average, about 30%, in the context of research on diversity and performance management. Martell, Lane, and Emrich (1996) found that an effect size of 1% regarding male–female differences in performance appraisal scores led to only 35% of the highest-level positions being filled by women. Accordingly, Martell et al. (1996) concluded that "relatively small sex bias effects in performance ratings led to substantially lower promotion rates for women, resulting in proportionately fewer women than men at the top levels of the organization" (p. 158). Another relevant example in the organizational sciences is the area of staffing decision making. For example, the algorithms and program developed by Aguinis and Smith (2007) show that differences in correlations that are as small as .03 across ethnicity-based subgroups are sufficiently large to cause important changes in the resulting adverse impact and false positive and false negative ratios. These are important outcomes that change based on whether the scales are coarse. In sum, the difference between the attenuated and disattenuated (by scale coarseness) correlation has important implications for organizational science theory and practice in several domains (for an additional illustration of the practical impact of what may seem "small" differences between correlations, see Schmidt, Hunter, McKenzie, & Muldrow, 1979). The underestimation caused by scale coarseness can lead researchers to make incorrect inferences from their findings, such as concluding that male–female differences in performance appraisal scores do not affect the career progression of women, and that a selection test is not likely to lead to adverse impact. Perhaps just as noteworthy is that an important hallmark of any scientific field is continuous improvement in accuracy of measurements and estimations of theoretically important parameters (Aguinis, 2001; Schmidt et al., 2006). Taking into account scale coarseness will contribute to improved accuracy in the estimation of population correlation coefficients.

Finally, an additional important issue to consider in terms of making incorrect inferences is the biasing effect of scale coarseness on meta-analytically derived correlation coefficients. Results of validity generalization studies involving continuous constructs measured using coarse scales are likely to have underestimated the resulting mean validity coefficient. For example, Judge, Heller, and Mount's (2002) meta-analysis of the relationship between overall job satisfaction and the five-factor model of personality likely underestimated population correlations because most of the studies included in this meta-analysis used coarse scales to measure job satisfaction.

In sum, we have provided evidence in support of the conclusion that the implications of scale coarseness are meaningful, nontrivial, and can result in making imprecise inferences. Next, we discuss a procedure for correcting the downward bias introduced by scale coarseness in primary-level studies. Then, we describe results of two Monte Carlo simulations that examine the accuracy of the correction procedure and conditions under which the effects of scale coarseness are most pronounced. After reporting results of the simulations, we offer a new procedure for computing sampling error variance and confidence intervals for the resulting corrected correlation. Also, we describe a user-friendly, Web-based computer program that enables the implementation of this correction procedure at the primary level, including the computation of sampling error variance and confidence intervals for the corrected correlation. Finally, we describe a new procedure for incorporating a scale coarseness correction procedure in conducting a meta-analysis.

Correcting for the Downward Bias Introduced by Scale Coarseness

As is the case with other statistical and methodological artifacts that produce a downward bias in the correlation coefficient, elimination of the methodological artifact via research design before data are collected "is vastly superior to elimination by statistical formula after the fact" (Hunter & Schmidt, 2004, p. 98). Thus, as implemented by Arnold (1981) and Russell and Bobko (1992; Russell, Pinto, & Bobko, 1991), and suggested by others (Krieg, 1999), one possibility regarding the measurement of continuous constructs is to use a continuous graphic rating scale (i.e., a line segment without scale points) instead of Likert-type scales. Then, a researcher can measure the distance (e.g., in millimeters) from the left side of the line segment to the individual's response point, thereby resulting in a nearly-continuous measure (Arnold, 1981; Russell & Bobko, 1992; Russell et al., 1991). However, this type of data collection procedure is not practically feasible in most situations unless data are collected electronically (Aguinis, Bommer, & Pierce, 1996).

Because using continuous graphic rating scales is often difficult to implement in practice and because there are so many established operationalizations of truly continuous constructs using Likert-type scales, the next best solution to address the downward bias produced by scale coarseness is to implement a statistical correction after data are collected. This correction was derived by Peters and van Voorhis (1940, pp. 396-397) by solving for the disattenuated relationship between X and Y in terms of the attenuated/observed correlation using partial correlation formulas. They benefited from recognizing that the relationship between a categorical X and continuous Y controlling for the continuous X equals zero and employed Sheppard's correction to the coarse variance (Sheppard, 1897, 1907). Peters and van Voorhis's (1940) correction factors assume a linear relationship between X and Y, equally spaced intervals for the categorical X and Y (e.g., coding a 3-point scale as 1, 2, and 3, or 0, 1, and 2), and a finite space on the latent continuum (i.e., they calculated their correction factors by creating intervals of standardized scores between -3 and 3).

The correction procedure for scores assumed to be normally distributed in the population is accurate even when the distributions for the predictor and criterion scores are severely skewed unless one or both variables are measured using a binary scale (Wylie, 1976). For example, Martin (1978) tested the Peters and van Voorhis correction procedure and found that corrected correlations of .81 and .49 were almost identical to their population counterparts of .80 and .50, respectively. Based on the results of his Monte Carlo simulations, Martin (1978) concluded that "the corrected *r*s are for pragmatic purposes the same as the 'true' *r*s" and he also concluded that "the correction factors are valuable, but they are rarely used" (p. 307).

Unfortunately, as noted by Martin (1978), researchers do not seem to be aware of this correction procedure. This is particularly true for researchers in the organizational sciences. We reached this conclusion after conducting an extensive literature search of all articles that cited Peters and van Voorhis (1940). We conducted our search using Web of Science, including the Science Citation Index Expanded and the Social Sciences Citation Index, from January 1965 to July 2007. This search resulted in 195 citations of Peters and van Voorhis (1940). None of these citations were from articles published in premier management journals such as *Academy of Management Journal*, *Journal of Management*, *Strategic Management Journal*, and *Administrative Science Quarterly*. Three of these citations were for articles published in *PPsych* (Buel, 1969; Carlson, 1969; Taylor & Griess, 1976); 6 were for articles published in *JAP* (Ash, 1971; Buel, 1965; Carlson, 1967; Kemery, Dunlap, & Griffeth, 1988; Russell & Bobko, 1992; Williams, 1990); and 1 each for *Organizational Research Methods* (*ORM*; Edwards, 2001); *Journal of Organizational Behavior* (*JOB*; Corrigan et al., 1994); *Sloan Management Review* (*SMR*; Gross, 1972); and *Organizational*

Behavior and Human Performance (*OBHP*; Andrews & Farris, 1972). Each of the *PPsych*, *ORM*, *JOB*, *SMR*, and *OBHP* and three of the six *JAP* articles (Buel, 1965; Carlson, 1967; and Kemery et al., 1988) cited sections of the Peters and van Voorhis book that *do not* address the topic of scale coarseness. Two other *JAP* articles (Russell & Bobko, 1992, and Williams, 1990) cited Peters and van Voorhis to support the general statement that scale coarseness produces a downward bias on the correlation coefficient, but neither article mentioned the existence of a correction procedure that would address this bias. Finally, Ash (1971) noted in one sentence in the article's Results section that "since the recommended-not recommended judgment may be considered a continuous variable, a Pearson product-moment correlation, corrected for broad categories (Peters & van Voorhis, 1940, p. 395-399), was computed at $r = .43$" (p. 163). However, Ash (1971) did not cite Peters and van Voorhis or refer to the bias produced by scale coarseness anywhere else in his article, did not mention the value of the observed correlation (in fact, the abstract states that $r = .43$ without clarifying this is a corrected and not an observed correlation), did not describe the steps taken to obtain the corrected correlation, and also did not offer a procedure for computing the sampling error and confidence intervals for the corrected correlation.

To summarize, we were unable to find any articles in organizational science journals that described the procedure so that a reader would be able to implement the correction. This is one likely reason for the fact that, of the approximately 8,000 articles published in *JAP* and *PPsych* since 1940, 2,700 articles published in *AMJ* since 1963, 1,600 articles published in *SMJ* since 1980, and thousands of articles published in other organizational science journals, only one article applied the scale coarseness correction in spite of the fact that many of these articles, possibly in the thousands, suffer from the biasing effects of scale coarseness. Hence, it is obvious that organizational science researchers are not aware of the correction procedure. If organizational science researchers are not aware of the problem and the correction procedure, they are not likely to address the issue in their future work.

The correction assumes that the scaling intervals are centered around the midpoint of the interval and is based on the following equation:

$$r_{x'y'} = \frac{r_{xy}}{r_{xx'}r_{yy'}} \tag{1}$$

where $r_{x'y'}$ is the disattenuated correlation, r_{xy} is the observed correlation, and $r_{xx'}$ and $r_{yy'}$ are the correction factors for the number of scaling intervals on the X and Y scale, respectively. Peters and van Voorhis (1940) also made assumptions about the underlying distributions of X and Y and derived correction factors for normal and uniform distributions. Table 2 includes the correction factors for a normal distribution.

As an example, if X includes 4 scale points (i.e., correction factor of .916; see Table 2) and Y includes 5 scale points (i.e., correction factor of .943; see Table 2), disattenuating an observed correlation of .35 using Equation 1 leads to:

$$r_{x'y'} = \frac{.35}{(.916)(.943)} = .41 \tag{2}$$

which represents an increase of about 17% in the magnitude of the correlation.

Table 2
Correction Factors for Disattenuating Correlation Coefficients From the Effects of Scale Coarseness

Number of Scale Points	Correction Factor
2	.816
3	.859
4	.916
5	.943
6	.960
7	.970
8	.977
9	.982
10	.985
11	.988
12	.990
13	.991
14	.992
15	.994

Source: The values in this table are from Peters & van Voorhis (1940, p. 398).

Monte Carlo Simulations to Assess the Accuracy of the Scale Coarseness Correction and the Impact of Scale Coarseness Under Various Conditions

We conducted two Monte Carlo simulations. The overall purpose of these simulations was to asses the accuracy of the scale coarseness correction factors derived by Peters and van Voorhis (1940) and the impact of scale coarseness on the difference between population and observed correlations under various conditions. In Study 1, we investigated single-item scale situations, whereas in Study 2, we investigated multi-item scale situations. Table 3 includes a summary of the values for the following parameters we manipulated in each of the simulations: true correlation ρ_{XY} (i.e., computed using continuous scores), number of items for the X scale i_X, number of items for the Y scale i_Y, number of scale points for the X scale a_X, and number of scale points for the Y scale a_Y.

Study 1: Single-Item Scales Simulation

This simulation improves on the Martin (1973, 1978) studies on the accuracy of the scale coarseness correction because results are less likely to be affected by sampling error. Specifically, Martin used a single replication with sample sizes of 1,000. In contrast, our simulation used 30 replications with samples of $N = 100{,}000$. We used the same parameter values used by Martin (1978, Table 1) so as to be able to make a direct comparison between his results and ours. The combination of parameter values resulted in 270 unique permutations.

Table 3
Parameter Values Used in the Monte Carlo Simulations

	Simulation 1 (Single-Item Scales)	Simulation 2 (Multi-Item Scales)
Parameter	Parameter Values	Parameter Values
ρ_{XY}	.1, .2, .5, .7, .8, .9	.1, .2, .5, .7, .8, .9
Number of scale items		
X (i_X)	1	2, 7, 15
Y (i_Y)	1	2, 7, 15
Number of scale points		
X (a_X)	2, 4, 6, 8, 10, 12, 14, 16, 20	2, 4, 6, 8, 10, 12, 14, 16, 20
Y (a_Y)	2, 4, 6, 8, 10, 12, 14, 16, 20	2, 4, 6, 8, 10, 12, 14, 16, 20

Note: ρ_{XY} = population (i.e., true) correlation computed using continuous scores for X and Y.

We conducted our simulation using classical test theory (i.e., parallel forms model) as described by Bandalos and Enders (1996) and Bernstein and Teng (1989). First, the correlation was specified between the latent values of X and Y using the following equation:

$$F_{Yj} = rF_{Xj} + \left(1 - r^2\right)^{1/2} e_{ij} \tag{3}$$

where F_{Yj} and F_{Xj} represent the latent constructs for X and Y, r is the correlation, and e_{ij} is random error. The following unidimensional models were used to derive continuous item scores for the X and Y scales:

$$X_{ij} = a_X F_{Xj} + \left(1 - a_X^2\right)^{1/2} e_{Xij} \tag{4}$$

$$Y_{ij} = a_Y F_{Yj} + \left(1 - a_Y^2\right)^{1/2} e_{Yij} \tag{5}$$

where X_{ij} and Y_{ij} are the observed score on item i for subject j, F_{Xj} and F_{Yj} are the latent constructs, and e_{Xij} and e_{Yij} are random error components for item i and person j for X and Y respectively. Equations 4 and 5 also include item statistics a_X and a_Y, which are factor loadings (also the square root of the test-retest reliability coefficient) that specify the relationship between an observed item and its latent construct for X and Y, respectively. This study did not examine the impact of measurement error, so the factor loadings (a_X and a_Y) equaled one. The generated observed scores for X and Y were then transformed into coarse standardized scores by segmenting values on the latent continuum between -3.2 and 3.2 into equal intervals as described by Martin (1973, 1978). Note that the values of -3.2 and 3.2 were chosen to replicate Martin's (1973, 1978) simulations. For example, 2 scale points were created by assigning X_{ij} scores less than zero to equal 1 and X_{ij} greater than zero to equal 2. Additionally, for three categories, the latent continuum between -3.2 and 3.2 was divided into three equal segments at two threshold values: -1.07 and 1.07, and, for four categories, the three threshold values were -1.6, 0, and 1.6, and so forth.

Results summarized in Table 4 show observed correlations as a function of population correlations based on continuous scores for X and Y, scale coarseness, and number of scale points for X and Y. Recall that, similar to Martin (1973, 1978), this simulation included one item for X and one item for Y and there was no measurement error. In Table 4, the column labeled *M* refers to observed correlations reported by Martin (1978, Table 1) and the column labeled *NS* refers to observed correlations obtained in our new simulation. The column labeled *PvV* shows attenuated correlations (by scale coarseness) that we computed using Peters and van Voorhis (1940) correction factors. Specifically, we used Equation 1 to solve for the observed correlation r_{xy} given that the population correlation is a known parameter in our simulation.

Results shown in Table 4 indicate that, across all cells in the simulation design, differences between our generated correlations and those produced by the Peters and van Voorhis (1940) correction procedure are negligible. Specifically, across all cells in our simulation, the mean absolute-value difference between the correlations is .008, which represents a coefficient of determination $r^2 = .000064$. These results confirm Martin's conclusion regarding the accuracy of the Peters and van Voorhis (1940) correction factors. Table 4 also shows that differences between our results and those obtained by Martin (1978) are also negligible, given that the mean absolute-value difference between the correlations is .01 (i.e., $r^2 = .001$). As noted earlier, this negligible difference is due to the use of more stable estimates in our simulation compared to Martin's. Finally, Table 4 also shows that differences between our correlations and those produced by the Peters and van Voorhis correction factors, although still negligible, increase for correlation values of .7 or greater. This is an expected result because the correction factors are less precise for "extremely high correlations" (Peters & van Voorhis, 1940, p. 396). In any case, although discrepancies are negligible and not practically significant in most situations, such extremely large correlations are rare in the organizational sciences.

Table 4 also provides information regarding the conditions under which scale coarseness produces the largest amount of bias. Specifically, as the number of scale points decreases and the value of the population correlation increases, the effect of scale coarseness becomes more pronounced. For example, an examination of the NS columns in Table 4 shows that a population correlation of .1 yields an observed correlation of .09 when X and Y include 6 scale points each. However, the same population correlation is decreased to a smaller value of .081 when X and Y include 4 scale points each. Considering the case of a population correlation of .5, for 6 scale points for both X and Y the observed correlation is .45 and this same population correlation is decreased substantially to .406 when 4 scale points are used.

Study 2: Multi-Item Scales Simulation

The second Monte Carlo simulation involved the use of multi-item scales for both X and Y. There are several areas in the organizational sciences in which the use of single-item scales is common (e.g., performance measurement and performance management, Aguinis, 2009; job and life satisfaction, Judge et al., 2002). However, using multi-item scales is typically advantageous because, compared to single-item scales, they are less likely to suffer from deficiency and contamination and, therefore, they are superior in terms of construct validity. In addition, multi-item scales are beneficial because they allow for the computation of a greater variety of reliability estimates (e.g., internal consistency).

Table 4
Observed Correlation Coefficients as a Function of Scale Coarseness Reported by Martin (1978), Computed in the Present Study, and Computed Based on the Correction Factors Derived by Peters and van Voorhis (1940)

Number of Scale Points	Correlation Based on Continuous Scales for *X* and Y																	
	.1			.2			.5			.7			.8			.9		
(*X-Y*)	M	NS	PvV	M	NS	PvV	M	NS	PvV	M	NS	PvV	M	NS	PvV	M	NS	PvV
20-20	.094	.096	.099	.208	.195	.198	.505	.485	.496	.701	.681	.694	.803	.778	.793	.897	.875	.892
20-16	.090	.098	.099	.209	.194	.198	.494	.485	.495	.701	.680	.692	.800	.777	.791	.891	.874	.890
20-14	.090	.097	.099	.211	.194	.197	.499	.485	.494	.706	.679	.691	.800	.775	.790	.892	.873	.888
20-12	.090	.097	.098	.201	.193	.197	.502	.484	.492	.702	.677	.689	.795	.774	.787	.886	.871	.886
20-10	.088	.097	.098	.205	.193	.196	.494	.481	.490	.691	.674	.685	.781	.771	.783	.884	.867	.881
20-8	.103	.096	.097	.191	.191	.194	.491	.478	.485	.683	.668	.679	.785	.764	.776	.879	.860	.873
20-6	.103	.094	.095	.199	.188	.190	.478	.469	.476	.663	.657	.666	.767	.750	.761	.858	.844	.856
20-4	.095	.089	.090	.178	.179	.181	.462	.446	.451	.629	.624	.632	.735	.714	.722	.819	.803	.813
20-2	.078	.079	.081	.159	.160	.163	.401	.397	.407	.549	.557	.569	.638	.636	.650	.719	.716	.732
16-16	.095	.096	.099	.207	.194	.197	.486	.485	.493	.689	.678	.691	.785	.775	.789	.888	.872	.888
16-14	.095	.096	.098	.210	.194	.197	.484	.483	.492	.691	.677	.689	.792	.774	.788	.887	.871	.886
16-12	.099	.097	.098	.201	.193	.196	.496	.482	.491	.684	.675	.687	.791	.772	.785	.885	.869	.884
16-10	.097	.096	.098	.204	.192	.195	.484	.480	.488	.684	.672	.684	.786	.768	.781	.880	.865	.879
16-8	.103	.096	.097	.208	.190	.194	.487	.476	.484	.686	.667	.677	.775	.763	.774	.887	.858	.871
16-6	.108	.093	.095	.185	.188	.190	.485	.467	.474	.671	.655	.664	.762	.749	.759	.856	.842	.854
16-4	.094	.089	.090	.183	.178	.180	.450	.445	.450	.633	.623	.631	.727	.711	.721	.817	.801	.811
16-2	.072	.079	.081	.162	.159	.162	.390	.396	.406	.561	.555	.568	.638	.635	.649	.716	.714	.730
14-14	.096	.096	.098	.213	.193	.197	.488	.483	.491	.694	.676	.688	.795	.772	.786	.887	.869	.885
14-12	.099	.096	.098	.201	.192	.196	.505	.482	.490	.687	.674	.686	.788	.770	.784	.886	.867	.882
14-10	.098	.096	.097	.210	.192	.195	.495	.479	.487	.681	.672	.682	.786	.767	.780	.880	.863	.877
14-8	.100	.095	.097	.205	.190	.193	.487	.475	.483	.682	.666	.676	.777	.760	.773	.877	.856	.869
14-6	.108	.093	.095	.187	.186	.189	.485	.467	.474	.670	.653	.663	.763	.747	.758	.856	.841	.852
14-4	.095	.088	.090	.182	.177	.180	.450	.445	.450	.633	.622	.629	.730	.711	.719	.819	.800	.809
14-2	.073	.080	.081	.162	.158	.162	.392	.395	.405	.563	.554	.567	.639	.634	.648	.717	.713	.728
12-12	.110	.096	.098	.201	.192	.195	.492	.480	.488	.688	.672	.684	.784	.768	.781	.880	.864	.879

(Continued)

Table 4
(Continued)

Number of Scale Points	Correlation Based on Continuous Scales for *X* and Y																	
	.1			.2			.5			.7			.8			.9		
(*X*-*Y*)	M	NS	PvV	M	NS	PvV	M	NS	PvV	M	NS	PvV	M	NS	PvV	M	NS	PvV
12-10	.105	.095	.097	.181	.191	.194	.495	.478	.486	.681	.668	.680	.779	.765	.777	.875	.861	.875
12-8	.095	.094	.096	.205	.189	.193	.485	.475	.481	.677	.663	.674	.771	.758	.770	.871	.853	.866
12-6	.093	.093	.094	.198	.186	.189	.474	.465	.472	.663	.651	.661	.755	.745	.755	.853	.838	.850
12-4	.098	.089	.090	.189	.177	.179	.477	.443	.448	.635	.619	.627	.717	.708	.717	.815	.798	.807
12-2	.078	.079	.081	.155	.158	.161	.397	.395	.403	.554	.553	.565	.630	.631	.646	.712	.711	.726
10-10	.092	.095	.097	.213	.190	.193	.485	.475	.483	.670	.666	.677	.777	.760	.773	.873	.856	.870
10-8	.090	.094	.096	.176	.189	.192	.479	.470	.479	.668	.660	.671	.768	.755	.766	.868	.849	.862
10-6	.086	.092	.094	.180	.185	.188	.471	.463	.470	.659	.649	.658	.758	.741	.751	.848	.834	.845
10-4	.098	.088	.089	.171	.176	.178	.455	.442	.446	.632	.617	.624	.720	.705	.713	.811	.793	.803
10-2	.072	.078	.080	.160	.157	.161	.397	.394	.401	.549	.550	.562	.633	.629	.642	.711	.708	.723
8-8	.095	.093	.095	.201	.188	.190	.479	.468	.475	.675	.654	.664	.768	.748	.759	.862	.841	.854
8-6	.091	.091	.093	.182	.183	.186	.474	.459	.465	.653	.642	.651	.750	.734	.745	.843	.826	.838
8-4	.088	.086	.088	.171	.175	.177	.446	.437	.442	.625	.611	.618	.710	.699	.707	.808	.786	.795
8-2	.078	.078	.080	.160	.155	.159	.391	.389	.398	.550	.545	.557	.626	.623	.636	.710	.701	.716
6-6	.095	.090	.091	.183	.180	.183	.455	.450	.456	.640	.630	.639	.738	.720	.730	.828	.812	.821
6-4	.079	.085	.087	.178	.172	.173	.432	.429	.433	.614	.600	.606	.695	.685	.693	.790	.772	.780
6-2	.076	.076	.078	.151	.152	.156	.383	.381	.390	.534	.535	.546	.612	.611	.624	.692	.690	.702
4-4	.081	.081	.082	.164	.163	.164	.413	.406	.411	.587	.570	.576	.664	.655	.658	.770	.755	.740
4-2	.071	.072	.074	.144	.144	.148	.364	.363	.370	.517	.511	.518	.588	.589	.592	.680	.680	.666
2-2	.064	.064	.067	.126	.128	.133	.331	.333	.333	.491	.494	.467	.590	.591	.533	.711	.713	.600

Note: M = reported by Martin (1978); NS = results obtained in the new simulation conducted as part of the present study; PvV = computed using correction factors derived by Peters and van Voorhis (1940).

Thus, because multi-item scales are usually beneficial regarding both conceptual and statistical issues, the majority of research domains in the organizational sciences favor their use over single-item scales.

Accordingly, in Study 2 we generated multiple items via a unidimensional parallel forms classical test theory model (i.e., equal item locations and factor loadings across items) using the approach outlined by Bandalos and Enders (1996) described earlier. That is, a normally distributed latent variable was generated for each subject. A key difference from Study 1 is that we generated continuous items i by specifying the relationship between each item and the latent variable (the factor loadings were held constant across items to one, so there was no measurement error). Coarse items were then created using equal intervals as described earlier for single-item scales and total scores were computed. The coarse items were them summed to create total scores for X and Y and the observed correlation was computed between the two coarse scales. The parameter values used are summarized in Table 3 and combining these values resulted in a total of 2,268 unique permutations.

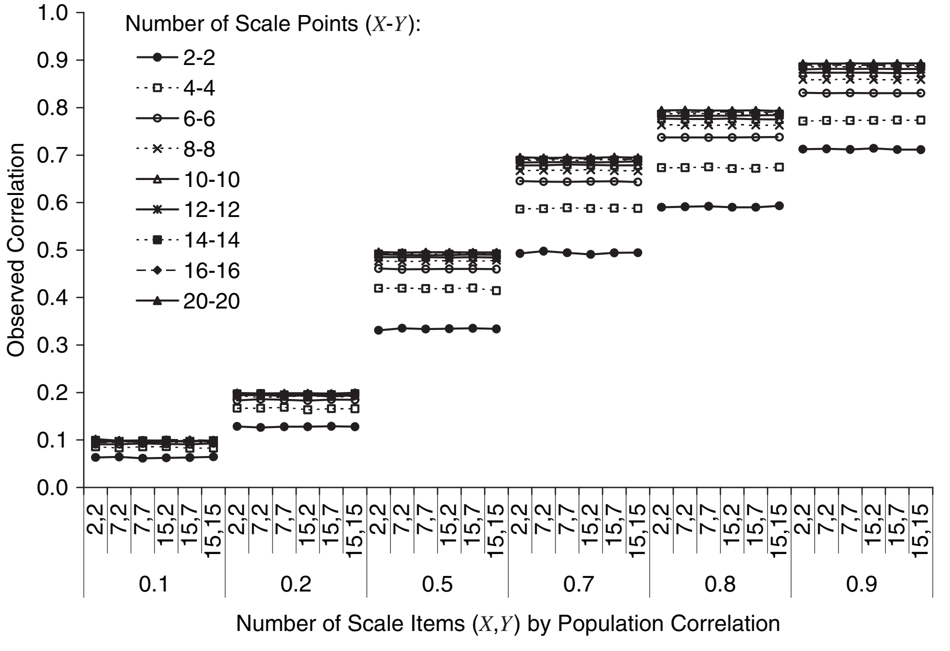
Results of Study 2 are displayed in Figure 1. As shown in this figure, increasing the number of items for the X and Y scales did not affect the impact of scale coarseness on the observed correlations. We computed standard deviations of the observed correlations within each number-of-items condition. Of the 2,268 unique permutations in the simulation, there were 486 unique combinations of scale points. We computed the observed correlations for each of these 486 cells and the standard deviation for the correlations to assess any variability due to number of scale items. The average of these 486 standard deviations was 0.0009, which indicates negligible fluctuations in the observed correlations for different numbers of items within each combination of scale points.

Summary and Conclusions

Results from Study 1 and Study 2, together with results reported by Martin (1973, 1978) about the accuracy of the correction factors and the conditions under which scale coarseness affects observed correlations, lead to the following conclusions. First, in terms of accuracy, the Peters and van Voorhis (1940) correction factors are accurate across the entire range of possible values for the correlation coefficient. The correction factors are not as accurate for correlations of about .7 or larger, but the amount of discrepancy is negligible in most situations (i.e., around .01 on average). Also, these correction factors are accurate for item scale points ranging from 2 to 20. This conclusion supports findings by Martin (1973, 1978), but our conclusion is based on more stable and precise estimates of the degree of attenuation produced by scale coarseness, given our sample size of 100,000 and the use of 30 replications per design condition. In contrast, Martin simulated sample sizes of 1,000 and did not use replications.

Second, the effect of scale coarseness is more pronounced for situations involving fewer scale points and larger correlations. In terms of scale points, using scales with 8 or fewer scale points is likely to produce noticeable decreases in the observed correlation. In addition, this decrease is more noticeable for correlations that are about .30 or larger. We emphasize that these are general rules of thumb that may not be meaningful without a

Figure 1
Observed Correlations Attenuated by Scale Coarseness as a Function of Population (i.e., Computed Using Continuous Scores for *X* and *Y*) Correlation, Number of Scale Points for *X* and *Y*, and Number of Scale Items for *X* and *Y*



consideration of context and outcomes because a small decrease in the correlation in one research domain may be considered large in another (Aguinis et al., 2005; Aguinis & Harden, in press).

Third, our simulation suggests that increasing the number of scale items does not affect the extent to which scale coarseness attenuates the correlation coefficient. As shown in Figure 1, the degree of attenuation was nearly identical regardless of the number of items included in the *X* and *Y* scales. This is because, as noted by Russell and Bobko (1992):

> Summing responses to multiple Likert-type items on a dependent scale (as is often done in between-subjects survey designs) is not the same as providing subjects with a continuous response scale. . . subjects are not responding with a scale score. Rather, they are responding to each item individually. . . if an individual responds to coarse Likert scales in a similar manner across items. . . information loss that causes systematic error to occur at the item level would have the same effect. (p. 339)

Thus, the effects of scale coarseness are the same for single-item and multi-item scales when constructs are unidimensional and items are similarly valid (i.e., represent the underlying construct to the same degree).

Sampling Error and Confidence Interval Computations for the Corrected Correlation

As is the case for the effects of other methodological artifacts, correcting for scale coarseness results in a sampling error that is larger than the sampling error of the uncorrected correlation (cf. Bobko, 1983; Bobko & Rieck, 1980). Consequently, confidence intervals around the corrected correlation are also wider than those around the observed correlations. Peters and van Voorhis (1940) did not provide equations for computing sampling error variance or confidence intervals for the corrected correlation and, to the best of our knowledge, these equations are not available elsewhere. Bobko and Rieck (1980) and Hunter and Schmidt (2004, pp. 118-120) provided an equation for the sampling error variance for the correlation coefficient corrected for simple artifacts such as measurement error and artificial dichotomization of continuous variables. This equation is the following:

$$\sigma^2_{e_c} = \frac{\sigma^2_e}{A^2} \tag{6}$$

where A is the correction factor and σ^2_e, the sampling error for the observed (i.e., uncorrected) correlation, is

$$\sigma^2_e = \frac{(1 - r^2_{xy})^2}{N - 1} \tag{7}$$

Next, we replace A in Equation 6 with the correction factor for scale coarseness to obtain the sampling error variance for the correlation coefficient in an individual study corrected for scale coarseness:

$$\sigma^2_{e_c} = \frac{\sigma^2_e}{(r_{xx'} r_{yy'})^2} \tag{8}$$

where $r_{xx'}$ and $r_{yy'}$ are the correction factors for scale coarseness on the predictor and the criterion as defined in Equation 1. To compute a confidence interval for the corrected correlation, we apply the correction shown in Equation 1 to the lower and upper limits of the interval.

For example, consider a study in which the observed correlation is .20, X is overall job satisfaction measured using a 5-point scale (e.g., Fuller et al., 2003) and Y is job performance measured using a 3-point scale (e.g., Lyness & Heilman, 2006). Using Equation 1 yields:

$$r_{x'y'} = \frac{.20}{(.943)(.859)} = .25 \tag{9}$$

Thus, in this realistic example based on articles published recently, scale coarseness reduced the correlation by 20%. To compute a confidence interval around the corrected correlation, we must first obtain the sampling error for the observed (i.e., uncorrected) correlation using Equation 7. Assuming that sample size in this study is 100,

$$\sigma_e^2 = \frac{(1 - .20^2)^2}{100 - 1} = .0093 \tag{10}$$

And, using Equation 8, the sampling error in the corrected correlation is

$$\sigma_{e_c}^2 = \frac{\sigma_e^2}{(r_{xx'} r_{yy'})^2} = \frac{.0093}{([.943][.859])^2} = .0142 \tag{11}$$

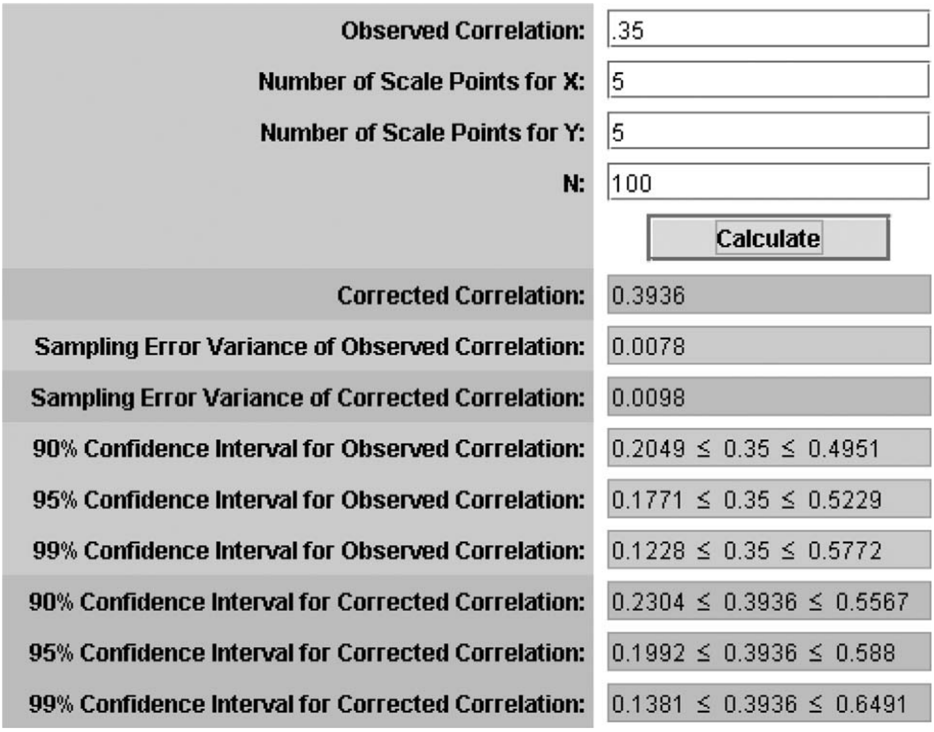
The 95% confidence interval for the observed correlation is $r_{xy} \pm 1.96\sigma_e$, which is $.01 \leq r_{xy} \leq .39$. Correcting the lower limit yields $\frac{.01}{(.943)(.859)} = .01$ and correcting the upper limit yields $\frac{.39}{(.943)(.859)} = .48$. This results in a 95% confidence interval for the corrected correlation of $.01 \leq r_{x\prime y\prime} \leq .48$. Thus, the center of the confidence interval shifts from the observed correlation of .20 to the corrected correlation of .25. Also, the width of the confidence interval around the corrected value is about 24% larger than the width of the confidence interval around the observed value, demonstrating that sampling error is larger in the corrected correlation.

Online Computational Tool for Correcting Individual Correlations

Blanton and Jaccard (2006) noted that the use of coarse scales is typical yet problematic in psychology research. In addition, as confirmed by our Web of Science literature search noted previously, researchers in the organizational sciences are not aware of the procedure to correct the correlation coefficient for the bias caused by scale coarseness. Even if a researcher is aware that scale coarseness is an important methodological artifact, he or she may not implement a correction because of a lack of a computational tool that allows for its implementation. This may be another reason why the correction has been implemented in only one of the more than 15,000 articles published in organizational science journals since the publication of Peters and van Voorhis's book in 1940. Finally, neither Peters and van Voorhis's book nor any other published source of which we are aware provides equations to compute the sampling error or confidence intervals for the corrected correlation. Accordingly, we developed a user-friendly computer program that allows researchers to correct correlation coefficients for scale coarseness. The program is available free of charge and can be executed online by visiting the following Web site: http://carbon.cudenver.edu/~haguinis/ (please click on the "Scale Coarseness Program" link).

As an illustration, Figure 2 includes a screen shot of the program. Users input the observed correlation and number of scale points for each of the two variables. In the hypothetical yet realistic example shown in Figure 2, the observed correlation is .35 and each of the two variables were measured using 5-point scales. Figure 2 shows a disattenuated correlation of about .39, which represents an increase of about 13% in the correlation's magnitude. In addition to the observed correlation and number of scale points for the predictor and criterion, the program includes an input box for sample size (N). This information is needed so the program can compute the sampling error as well as confidence intervals for the corrected correlation using the equations described previously. In this particular example

Figure 2
Screen Shot of Computer Program That Implements All Computations for (a) Correcting Correlations Attenuated From Using Coarse Scales, and (b) Calculating Sampling Error and Confidence Intervals for the Observed as well as Corrected Correlations



Note: The program is available at http://carbon.cudenver.edu/~haguinis (please click on the "Scale Coarseness Program" link).

shown in Figure 2, the sampling error variance for the observed correlation is .0078 and the sampling error variance for the corrected correlation is .0098 (i.e., an increase of approximately 25%). In addition, the screen shot displayed in Figure 2 shows that the 95% confidence interval for the observed correlation of .35 ranges from .18 to .52, whereas the 95% confidence interval for the corrected correlation of .39 ranges from .20 to .59. For the sake of completeness, the program's output also includes 90% and 99% confidence intervals for both the observed and corrected correlation. Next, we describe a new procedure that allows researchers to incorporate the scale coarseness correction in a meta-analysis.

Incorporating the Scale Coarseness Correction in a Meta-Analysis

Thus far, our discussion has focused on the scale coarseness correction of a correlation coefficient in an individual study (i.e., primary-level studies). However, a meta-analyst may wish to correct the observed correlations as reported in primary-level studies and then meta-analyze the resulting corrected correlations. Moreover, a meta-analyst may wish to correct for the effects of scale coarseness and also for the bias introduced by other methodological artifacts (e.g., measurement error in X and/or Y, dichotomization of a truly continuous variable, direct range restriction). Despite this need, the scale coarseness artifact is not discussed in any of the most widely cited and used sources on meta-analytic methods (e.g., Hedges & Olkin, 1985; Hunter & Schmidt, 2004) or in any of the articles published in the *ORM* feature topic on meta-analysis in January 2008 (Aguinis et al., 2008; Bobko & Roth, 2008; Dalton & Dalton, 2008; Kisamore & Brannick, 2008; Schmidt, 2008; Steel & Kammeyer-Mueller, 2008; Wood, 2008). Also, we are not aware of any published source describing an extension of the scale coarseness correction from the primary to the meta-analytic level.

Correcting correlations for scale coarseness at the meta-analytic level is feasible in organizational science research because scale coarseness information is usually available for each of the studies included in a meta-analysis. In other words, the Method section of each article included in a meta-analytic review is likely to include information about the measures and number of items and scale points used. The availability of this information represents an advantage in comparison to other artifacts for which a correction factor may not be available at the individual study level or at all (e.g., range restriction, Halbesleben, 2006).

Hunter and Schmidt (2004, chapter 3) provide a description of how to apply corrections based on several methodological artifacts simultaneously. Hunter and Schmidt's equations provide a framework for incorporating a scale coarseness correction factor in a meta-analysis. Essentially, one computes a compound multiplicative attenuation factor including all of the correctable artifacts for each study included in the meta-analysis. For example, if a_1 is the attenuation factor for reliability of the predictor X (i.e., $\sqrt{r_{xx}}$) and a_2 is the attenuation factor for reliability of the criterion Y (i.e., $\sqrt{r_{yy}}$), the compound attenuation factor for these two artifacts is $A = a_1 a_2$. The value for each of these attenuation factors may be known for each individual study. However, most likely this information is not available for each study. Thus, the attenuation factor is an average of the study-level attenuation factor information for those studies for which this information is available (Hunter & Schmidt, 2004, chapter 4).

Note that the order of the corrections only matters in the presence of the range restriction correction. This is because all artifacts, including scale coarseness, are simple artifacts except for range restriction. We could not think of any reason nor did we find a reason in the literature for why scale coarseness should not be considered an additional simple artifact.

Hunter et al. (2006) reached the following conclusion regarding situations when there is no range restriction: "If several simple artifacts are present in a study, their effects combine in a simple multiplicative way. The order in which the artifacts are entered does not play a role. The final result is the same for any order" (p. 595). When range restriction is present and corrected for, then it is important to understand at which point in the research process direct and/or indirect range restriction had an effect because this will determine when the range restriction correction will be applied in a meta-analysis. For instance, in many human resource–management research domains, including staffing and performance management, the criterion typically used is supervisory ratings (Aguinis, 2009). But performance scores of job incumbents suffer from direct range restriction in relationship to performance scores that would be obtained from job applicants. In this case, the direct range restriction correction should be applied before the correction for measurement error, scale coarseness, and other simple artifacts. This is because the measurement errors in the performance scores and the coarseness in the performance scale are not affected by and appear after the distribution is already range restricted (i.e., by virtue of including job incumbents only). Alternatively, in the case of indirect range restriction, the range restriction correction is applied after the measurement error and scale coarseness corrections. We refer readers to Hunter et al.'s (2006) detailed discussion of the issue of the ordering of corrections in meta-analysis.

We can incorporate the scale coarseness correction by adding the scale coarseness attenuation factor $a_{sc} = r_{xx'} r_{yy'}$, as defined in Equation 1, resulting in a compound attenuation factor $A = a_1 a_2 a_{sc}$. Thus, the corrected correlation is $r_{x'y'} = \frac{r_{xy}}{A}$. To compute the sampling error variance for the corrected correlation, we first obtain the sample-size weighted mean observed correlation $\bar{r}_{xy}$ and compute the sampling error variance for the observed correlation as follows (Aguinis, 2001):

$$\sigma^2_{r_{xy}} = \frac{(1 - \bar{r}^2_{xy})^2}{N - 1} \tag{12}$$

where N is the sample size for the study under consideration. Note that using $\bar{r}_{xy}$ instead of r_{xy} yields a more accurate sampling error estimate for the uncorrected correlation (Aguinis, 2001). Similar to Equation 6 and following Hunter and Schmidt (2004, pp. 118-120), the sampling error variance for the corrected correlation is:

$$\sigma^2_{r_{x'y'}} = \frac{\sigma^2_{r_{xy}}}{A^2} \tag{13}$$

Finally, we acknowledge that the possibility that researchers may overcorrect correlations is a long-standing and unresolved issue in the meta-analysis literature (e.g., James, Demaree, & Mulaik, 1986; James, Demaree, Mulaik, & Mumford, 1988). Thus, future research could investigate potential concurrent effects of scale coarseness, various types of measurement error, and other artifacts such as direct and indirect range restriction. There are several questions that this simulation could address, such as the extent to which scale coarseness may interact with some but not other sources of measurement error in biasing the correlation coefficient. Thus, future simulations could manipulate various types of measurement error (e.g., because of items, raters, time, or a combination; Schmidt, Le, & Ilies, 2003) and range restriction (direct, indirect; Hunter et al., 2006), in combination with scale coarseness. This would be a large-scale simulation that goes beyond the scope of the present article.

Conclusion

Because of the pervasive use of Likert-type scales to measure continuous constructs in organizational science research, scale coarseness is a common methodological artifact that causes a downward bias in the correlation coefficient in many situations. As noted by

Russell and Bobko (1992) within the specific context of estimating moderating effects, but equally valid for the estimation of the correlation coefficient, "Investigators should not attempt to discover moderator effects unless the overt measurement scale contains at least as many response options as exist in the theoretical response domain" (p. 339). Although seldom acknowledged and seemingly unbeknownst to most organizational science researchers, the operationalization of continuous constructs using Likert-type scales attenuates correlation coefficients systematically in many situations. In addition to being a pervasive phenomenon in virtually all areas of organizational science research, this downward bias can be meaningful, be nontrivial, and lead to substantive changes in theory and practice (e.g., in staffing decision making), particularly in situations with few scale points and large absolute values for the correlation coefficient. In many cases, the magnitude of the downward bias is comparable to that caused by measurement error and range restriction, which are the two methodological artifacts most frequently studied in the organizational sciences besides sampling error.

Given the availability of the computer program described in this article, future primary-level studies assessing continuous constructs, particularly those relying on the use of measures with few scale points, should report correlations disattenuated from the effects of scale coarseness as well as confidence intervals for the corrected correlations. We also recommend that future meta-analytic reviews incorporate the scale coarseness correction described in this article. Considering that the information needed to correct correlations for scale coarseness is likely to be available in most studies included in a meta-analysis, we suggest that scale coarseness be added to the list of correctable methodological artifacts (cf. Hunter & Schmidt, 2004, Table 3.1, p. 76). The scale coarseness correction could be applied routinely as is done with other commonly corrected artifacts such as measurement error and range restriction. As noted by Schmidt et al. (2006), "Because of its implications for theory development, accuracy of estimation of important parameters is critical in any science" (p. 281). Applying the scale coarseness correction at the primary and meta-analytic levels will lead to more accurate estimates of construct-level correlation coefficients.

Notes

1. A concern with the use of these types of scales is that they do not have interval-level properties (cf. Aguinis, Henle, & Ostroff, 2001). Specifically, the concern is that the resulting data are only ordinal and using a maximum likelihood (ML) factor analysis may result in biased parameter estimates. The polychoric correlation, proposed by Ritchie-Scott (1918) and perfected by Pearson and Pearson (1922), has resurfaced in the literature as a way to conduct a ML factor analysis using data collected with ordinal-level scales (e.g., Flora, Finkel, & Foshee, 2003; Holgado Tello, Chacon Moscoso, Barbero Garcia, & Sanduvete Chaves, 2006). The polychoric correlation is a correlation that would be obtained if the ordinal-level distributions were actually interval-level (the polyserial correlation is used when one of the scales is ordinal and the other is continuous). In these ML factor analyses based on ordinal data, there is a need to first estimate a matrix of polychoric correlations and then compute a weight matrix (i.e., the inverse of the asymptotic covariance matrix of polychoric correlations). Although the computations are quite complex (Bedrick & Breslin, 1996; Lee & Poon, 1986; Olsson, 1979; Poon & Lee, 1987), some commercially available software packages now allow users to compute polychoric and/or polyserial correlations (e.g., SAS, R, PRELIS, MPlus).

In a nutshell, the computation of a polychoric correlation consists of adding tails to the distributions, and it is therefore an estimate relying heavily on the assumption of an underlying continuous bivariate normal distribution. Unfortunately, this assumption is violated frequently and this violation has important consequences in terms of the precision of the resulting estimate (Homer & O'Brien, 1988). A second limitation of using polychoric correlations in conducting a ML factor analysis is that an accurate estimation of the weight matrix requires a large sample size, and inaccurate estimates can be obtained with a sample size as large as 200 (Dolan, 1994). It is encouraging that recent developments are attempting to overcome these and other challenges (e.g., Song & Lee, 2003; Zhang & Browne, 2006).

2. Although they are caused by different mechanisms, in some situations scale coarseness and artificial dichotomization (or polychotomization) can produce the same outcome in terms of the resulting data structure. For example, a researcher may artificially trichotomize a truly continuous variable after data have been collected and assign values to individuals such that 1 = *Group 1*, 2 = *Group 2*, and 3 = *Group 3*. In this situation, the resulting data structure would be identical to the one produced by using a coarse 3-point scale as in the example just discussed.

3. If the confidence interval around the uncorrected correlation includes the correlation corrected for scale coarseness, then one cannot rule out the possibility that a value closer to the corrected estimate would be obtained in a separate sample. This caveat regarding the scale coarseness correction in relation to sample size is equally applicable to all other statistical and methodological corrections (e.g., measurement error, range restriction).

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Herman Aguinis (http://carbon.cudenver.edu/ ~ haguinis/) is the Mehalchin Term Professor of Management in the Business School at the University of Colorado Denver. He has published five books, more than 60 journal articles, and about 20 book chapters and monographs on a variety of human resource management, organizational behavior, and research methods and analysis topics.

Charles A. Pierce (http://profiles.memphis.edu/capierce) is a professor of management in the Department of Management, Fogelman College of Business and Economics, University of Memphis. He received his PhD in 1995 from the University at Albany, State University of New York. His research interests include workplace romance, sexual harassment, and organizational research methods.

Steven A. Culpepper (http://clem.mscd.edu/~sculpepp/) is an assistant professor of psychology at the Metropolitan State College of Denver. He received his PhD in educational psychology and quantitative methods in 2006 from the University of Minnesota, Twin Cities. His research interests include statistics, research methods, and psychometrics.