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Errors-in-Variables Regression as a Viable Approach to Mediation Analysis with Random Error-Tainted Measurements: Estimation, Effectiveness, and an Easy-to-Use Implementation

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Abstract

Mediation analysis, popular in many disciplines that rely on behavioral science data analysis techniques, is often conducted using ordinary least squares (OLS) regression analysis methods. Given that one of OLS regression's weaknesses is its susceptibility to estimation bias resulting from unaccounted-for random measurement error in variables on the right-hand sides of the equation, many published mediation analyses certainly contain some and perhaps substantial bias in the direct, indirect, and total effects. In this manuscript, we offer errors-in-variables (EIV) regression as an easy-to-use alternative when a researcher has reasonable estimates of the reliability of the variables in the analysis. In three real-data examples, we show that EIV regression-based mediation analysis produces estimates that are equivalent to those obtained using an alternative, more analytically complex approach that accounts for measurement error—single-indicator latent variable structural equation modeling—yet quite different from the results generated by standard OLS regression that ignores random measurement error. In a small-scale simulation, we also establish that EIV regression successfully recovers the parameters of a mediation model involving variables adulterated by random measurement error while OLS regression generates biased estimates. To facilitate the adoption of EIV regression, we describe an implementation in the PROCESS macro for SPSS, SAS, and R that we believe eliminates most any excuse one can conjure for not accounting for random measurement error when conducting a mediation analysis.

Errors-in-Variables Regression as a Viable Approach to Mediation Analysis with Random Error-Prone Measurements: Estimation, Effectiveness, and an Easy-to-Use Implementation

Researchers throughout the world, be they in the behavioral or natural sciences, business and management, public health, medicine, and many other fields, are interested in establishing through the scientific method those things that produce causal effects of relevance to their theories and their discipline and its application. But the goals of research often go beyond establishing that such effects exist. Researchers also seek to understand the underlying mechanisms by which those effects operate. *Mediation* is the term often employed when discussing the mechanisms that transmit causal effects, and researchers regularly theorize about and test mediation models using *mediation analysis*.

An example of a mediation analysis in action can be found in Grisbook, Dewey, and Cuthbert et al. (2024), who estimated a model examining posttraumatic stress and post-partum depression as mediators of the effect of emergency Caesarean section relative to spontaneous vaginal delivery and planned C-section on internalizing and externalizing behaviors of the child two years after birth. They found that women who delivered through emergency C-section, relative to planned C-section or vaginal birth, reported greater posttraumatic stress and post-partum depression three months later, which in turn was related to greater internalizing and externalizing behaviors in the child two to three years later. These results suggest that the form of delivery a woman experiences during childbirth can affect the child's behavior years later as a result, at least in part, of the psychological experiences and state of the mother that result from different delivery methods.

Additional examples of mediation analysis are abundant in the research literature (e.g., Gaboury, Belleville, & Lebel et al., 2023; Neufeld & Malin, 2022; Rokeach & Wiener, 2022; Smith, Andruski, Deng, & Burnham, 2022; Yasuda & Goegen, 2023). Indeed, when browsing any issue of journals that publish empirical research, it is common to find at least one mediation analysis in its pages (e.g., Chan, Hu, & Mak, 2022; Hayes & Scharkow, 2013; Pieters, 2017). And methodology articles about mediation analysis are among some of the most highly cited papers in the journals they are published in and in behavioral sciences methodology as a whole. This reflects the widespread popularity and transdisciplinary relevance of mediation analysis.

Mediation analysis is typically conducted using a linear model-based path analysis, such as a set of ordinary least squares (OLS) regression analyses or a simultaneous estimation system such as observed variable structural equation modeling (SEM). Well-known among methodologists but not everyone who applies their work is the deleterious effects of random measurement error in the variables being analyzed on the estimation of the effects of those variables in linear models (Bollen, 1989; Buonaccorsi, 2010; Cohen, Cohen, West, & Aiken, 2003; Darlington & Hayes, 2017; Shear & Zumbo, 2013). Random measurement error is ubiquitous in research that behavioral scientists conduct and is difficult to avoid when measuring constructs that researchers study. But in practice, and as we document later, random measurement error is often ignored by researchers at the analysis phase, and this can produce bias in the estimation of effects and can invalidate inferential tests of the effects that come out of an analysis, including a mediation analysis (e.g., Cole & Preacher, 2014). Advice offered by methodologists to counteract the biasing effects of random measurement error include minimizing it at the design phase by using good measurement instruments, using SEM with a measurement model that captures random measurement error, or utilizing various other (and typically complex) methods for correcting bias that otherwise results when measurement error is ignored (Buonaccorsi, 2010; Culpepper & Aguinis, 2011; Ledgerwood & Shrout, 2011; Pieters, 2017).

There are many plausible explanations for neglecting random measurement error in analysis, including a lack of awareness of its effects on estimation and inference or the programming skill or familiarity with needed software to account for it. Regardless, it seems likely that the practice of ignoring measurement error in mediation analysis will continue without some further intervention. In this manuscript, we offer such an intervention by discussing and describing the implementation of a simple approach to managing the deleterious effects of measurement error in mediation analysis: errors-in-variables (EIV) regression. After a brief review of the mechanics of mediation analysis and the effects of random measurement error in estimation and inference, we describe EIV regression as an old, largely unused, but promising approach to overcoming the biasing of estimates of effects in a linear model that results from ignoring random measurement error. By way of three real-data examples and a small-scale simulation, we demonstrate the effectiveness of EIV regression relative to ignoring random measurement error or accounting for it using a more complicated single-indicator latent variable modeling approach using SEM. We end with a discussion of implementation of EIV regression in the freely-available PROCESS macro for SPSS, SAS, and R that now eliminates most any remaining excuses for not accounting for random measurement error when conducting a mediation analysis.

Note that although our treatment is very applied and directed at the user of mediation analysis, it is at the same time narrowly focused on the topic of random measurement error and how it can be better and easily acknowledged during an analysis. It is not our intention for this manuscript to serve as a tutorial on how to conduct a mediation analysis, its strengths and weaknesses, assumptions and limitations, dealing with missing data, sensitivity analysis, and various other data- and design-related issues and problems that researchers may encounter when trying to make unequivocally-supported causal statements about the mechanisms by which effects operate. Others have written about these topics (e.g., Bullock, Green, & Ha, 2010; Enders, Fairchild, & MacKinnon, 2013; Hayes, 2022; Imai, Keele, Tingley, and Yamamoto, 2011; MacKinnon, 2008; Shrout & Bolger, 2002; Spencer, Zanna, & Fong, 2005; Stone-Romero & Rosopa, 2010; VandeWeele, 2015; Wu & Jia, 2013) and we encourage researchers to familiarize themselves with this literature eventually, preferably prior to undertaking a mediation analysis.

Mediation Analysis and Random Measurement Error

Although mediation analysis can take many forms depending on the nature of the variables and measurement systems used, the most common is the use of OLS regression analysis or SEM, with mediator M^O and outcome Y^O being continuous *observed* measurements (and hence the "O" superscript, as opposed to the theoretical *true scores* discussed later) of assumed-to-be continuous underlying constructs *M* and *Y*. In this scenario, and relying on the standard assumptions of regression for meaningful interpretation and valid inference (linearity in relationships and errors in estimation that are independently, identically, and normally distributed), the simplest mediation analysis is typically parameterized with a set of two equations, one for M^O and one for Y^O:

$$M_i^O = d_{M^o} + aX_i^O + e_{M_i^o} \tag{1}$$

$$Y_i^O = d_{Y^o} + c'X_i^O + bM_i^O + e_{Y_i^o} \tag{2}$$

where *a*, *b*, and c' are unstandardized regression weights, e_{M^o} are e_{Y^o} are errors in estimation of M^O and Y^O, respectively, d_{M^o} and d_{Y^o} are regression constants, and the *i* subscript denotes case, participant or observation $i = 1$ to *n* where *n* is the sample size. X^O can be a continuous observed measurement of construct *X*, just as M^O and Y^O, or it can be dichotomous numerical codes representing two groups, in the case of two-group experimental manipulation of *X* for example.[1] Modifications for multicategorical nominal or ordinal X^O variables are discussed elsewhere (e.g., Hayes & Preacher, 2014; Hayes, 2022, pp. 201-230) and beyond the scope of this paper, as is a discussion of models of dichotomous, count, or other forms of M^O or Y^O. Covariates can be added to the right-hand sides of equations 1 and 2 to deal with confounding of the effects in a mediation

[1] Our words are carefully chosen here. X^O, M^O, and Y^O are a measures or manipulations of *something* but not necessarily what the researcher claims is being measured or manipulated. This paper is about the effects of and accounting for *random* measurement error, not about validity (i.e., whether the researcher is measuring and therefore studying what the researcher intends or claims). Whenever we used the term "measurement error," we are talking about random measurement error.

analysis by shared causal influences on X^O, M^O, and/or Y^O. A visual representation of this model can be found in Figure 1, panel B.

A mediation analysis provides a quantification of three effects of *X*: the indirect effect, the direct effect, and the total effect of X. Of most relevance to mediation is the *indirect effect*, quantified as the product of *a* and *b* from equations 1 and 2 and Figure 1 panel B. This, product, *ab*, estimates the difference in Y between two cases that differ by one unit on X resulting from the joint effect of X on M (estimated with *a*) which in turn affects Y (estimated with *b*). An indirect effect that is different from zero by some kind of inferential standard provides evidence consistent with mediation of the effect on X on Y by M.[2] Of course, mediation, as a causal process, cannot be established merely through a statistical analysis or examining the output of a statistical routine. Whether an effect can definitively be deemed causal requires strong theoretical argument and relevant design as much or even more than it does evidence than an effect, however quantified, is different from zero.

The *direct effect* of X is estimated with c' in equation 2 and Figure 1, panel B. It quantifies the difference in Y between two cases that differ by one unit on X but that are equal on M and is the component of the relationship between X on Y that that does not operate through M. As such, it tells us nothing about mediation. The sum of the direct and indirect effect of X is the *total effect* of X, often denoted *c*. That is, $c = c' + ab$. The total effect estimates the average difference in Y attributable to a one-unit difference in X. There was a time when a mediation analysis would be undertaken only with affirmative evidence of an association between X and Y captured by *c* or a related statistic. But it is now understood that only *ab* (which is equivalent to $c - c'$ in single-mediator models estimated using regression analysis) is pertinent to mediation and this indirect effect can be different from zero even if *c* is not (see e.g., Hayes, 2009; Kenny & Judd, 2014; Shrout &

[2] A bootstrap confidence interval that does not contain zero is a popular inferential approach, given the typical asymmetry of the sampling distribution of the product of two regression coefficients. For discussion of various methods available, see Hayes (2022, p. 97-109) or Hayes and Sharkow (2013).

Bolger, 2002; O'Rourke & MacKinnon, 2018). Thus, *c* and inference about it tells us nothing about mediation or whether X may be indirectly influencing Y through M.

But often, if not usually, researchers are more interested in the direct, indirect, and total effects of X estimated not from equations 1 and 2 but instead from

$$M_i^* = d_{M^*} + a^* X_i^* + e_{M_i^*} \tag{3}$$

$$Y_i^* = d_{Y^*} + c'^* X_i^* + b^* M_i^* + e_{Y_i^*} \tag{4}$$

as diagrammed in Figure 1 panel A, where X^*, M^* , and Y^* , are the are the underlying *true scores* of constructs *X*, *M*, and *Y* that the researcher is measuring. In this model, the direct, indirect, and total effects of *X* on *Y* are estimated as c'^*, a^*b^*, and $c^* = c'^* + a^*b^*$, respectively.

The distinction between true and observed scores is abstract but important and one with which most researchers are at least vaguely familiar. Recalling the example mediation analysis described at the beginning of this paper, Grisbook et al. (2024) used the Edinburgh Postnatal Depression Scale (EPDS) and the Psychiatric Diagnostic Screening Questionnaire (PDSQ) to measure postnatal depression and post traumatic stress. These self-report measurement instruments generate observed scores that can be used in estimation of equations 1 and 2. But Grisbook et al. (2024) were interested in the effects of emergency C-section on child behavior through *depression* and *posttraumatic stress*, not through scores on the EPDS and the PDSQ. That is, they were interested in the effects estimated with equations 3 and 4, not equations 1 and 2.

The true scores are unobserved or "latent" and not directly quantified or available in the data, so equations 3 and 4 can't be directly estimated. Under classical test theory, a measurement theory motivating many measurement procedures in the behavioral sciences (see e.g., Nunnally, 1978), the observed scores are conceptualized as caused by the true scores (represented with the dashed arrows in Figure 1) but are not equivalent to the true scores because the observed scores are a function of both the true scores and, typically, some random measurement error:

$$X_i^O = X_i^* + \varepsilon_{X_i}$$

$$M_i^O = M_i^* + \varepsilon_{M_i}$$

$$Y_i^O = Y_i^* + \varepsilon_{Y_i}$$

where ε_{X_i}, ε_{M_i}, and ε_{Y_i} are the random measurement errors for case *i*. These random errors in measurement can come from various sources that depend on the specifics of the measurement procedure or instruments being used, will vary between people, and can even vary in direction or magnitude over time and so can depend on when a person is measured. The point is that on any measurement occasion, the set of observed scores available in the data rarely will be the same as the true scores.

The amount of random measurement error that exists in a set of observed scores is the *reliability* of the observed scores, defined theoretically as the ratio of true score to observed score variance, the latter being the sum of the true score and random error variance under the assumption of classical test theory that errors are uncorrelated with true scores. For example, in the case of M^O:

$$\rho(M^O) = V(M^*) / V(M^O) = V(M^*) / [V(M^*) + V(\varepsilon_M)] \tag{5}$$

where V denotes variance and $\rho(M^O)$ is the reliability of M^O (reliability of X^O and Y^O are defined similarly). Reliability is the proportion of the variance in observed scores that is the result of variance in the true scores and so is between 0 and 1, with 0 reflecting observed measurements that are all random measurement error and 1 meaning an absence of random measurement error. Even though the true scores and therefore the variance of the true scores and random measurement error cannot be known, psychometric theory produces a variety of means of estimating the reliability of the observed scores, including Cronbach's α, McDonald's Ω, the correlation between observed measurements over time (test-retest reliability), and others.

When there is no random measurement error $X_i^O = X_i^*$, $M_i^O = M_i^*$, $Y_i^O = Y_i^*$, and equations 1 and 3 and equations 2 and 4 are functionally equivalent. So in the case of equivalence between the observed and true scores, $c' = c'^*$, $ab = a^*b^*$, and $c = c'^* + a^*b^*$ and all is well when equations 1 and 2 are used to estimate the effects of interest in a mediation analysis.

But theory and research has shown that, typically, all is not well in a mediation analysis when observed X and/or M contain random measurement error. Such fallible measurement is quite common in research, but ignoring it when conducting a mediation analysis using equations 1 and 2 means that one or more of the effects, c', ab and c are likely to diverge from c'^*, a^*b^*, and c^*, with the extent of the divergence dependent on how much random measurement error exists in X^O and/or M^O, i.e., how far $\rho(X^O)$ and/or $\rho(M^O)$ deviate from 1. That is, the result of using such fallible measurements and estimating equations 1 and 2 will be inaccurate estimates of one or more of the effects of interest, c'^*, a^*b^*, and/or c^*, and inferential tests for those effects from computations based on equations 1 and 2 that are likely to be invalid. For a discussion and evidence, see Cheung and Lau (2008); Cole and Preacher (2014); Fritz, Kenny, and MacKinnon (2016); Gonzales and MacKinnon (2021); Valeri, Lin, and VanderWeele (2014); VanderWeele, Valeri, and Ogburn (2012).

Note that if Y^O contains random measurement error but X^O and M^O are perfectly reliable, the *unstandardized* effects (the effects we are focusing on in this paper) estimated by equations 1 and 2 will be estimating the same thing that c'^*, a^*b^*, and c^* in equations 3 and 4 do.[3] However, sampling variance of these effects will, in theory, be larger, meaning power to detect the direct, indirect, and total effects will be reduced and confidence intervals will be wider than they otherwise would be if Y^O were free of random measurement error.

Approaches to Managing the Effects of Measurement Error

We believe anyone familiar with empirical research in their area would agree that the typical practice for mitigating the effects of random measurement error in a mediation analysis is to

[3] This is true for unstandardized effects. Accuracy in estimation of standardized effects will be influenced by random measurement error in Y^O as well. A discussion of the effects of measurement error on standardized paths and effects in a mediation analysis is beyond the scope of this paper.

do nothing. To informally assess the scope of this problem, we reviewed empirical articles published between 2020 and 2025 in *Psychological Science*, a journal with broad interdisciplinary content and audience. We identified 84 articles that contained a mediation analysis. Each of these did include the reporting of a reliability coefficient for variables likely to contain random measurement error, but none of them discussed random measurement error as a concern in their analyses. Only two articles used an SEM framework (discussed below) with accompanying measurement models in their mediation analysis. The remaining papers made no statistical adjustments or otherwise accounted for random measurement error. While 28 articles had putative causal variables (X in our discussion above) that probably contained no random measurement error (e.g., randomly assigned experimental conditions), there were no articles with analyses that included a mediator that could be said to be free of random measurement error. In short, consistent with our and probably the impressions of others as well, 97.6% of empirical studies contained no correction or otherwise accounted for random measurement error in the mediation analysis.

Given that random measurement error is often reported in the form of a reliability estimate, it appears that researchers are aware of what random measurement error is and the need to report its existence, but either lack the knowledge of its effects or the ability or motivation to correct for potential bias it produces in models of observed variables. Furthermore, given the use of popular tools that simplify a mediation analysis such as the PROCESS macro for SPSS, SAS, and R (Hayes, 2022) and its precursors (Preacher & Hayes, 2004, 2008), the mediation package in R (Tingley, Yamamoto, Hiros, Imai, & Keele, 2014), PROC CAUSALMED in SAS, and other computational aids that have (heretofore) no means of incorporating measurement error into the estimation, most published mediation analyses likely contain some and perhaps substantial bias as a result, along with the corresponding effects of such bias on the accuracy and validity of inference (c.f., Cole & Preacher, 2014).

That said, this strategy of ignoring random measurement error in an analysis may not always be quite as problematic as it might seem. As mentioned earlier, random measurement error in Y^O does not by itself bias the estimation of effects when using equations 1 and 2, and when X is experimentally manipulated or codes groups into which people are accurately classified, the reliability of X^O can be assumed to be one.[4] That leaves measurement error in M^O (as well as any covariates) as the remaining source of measurement error-induced bias in estimation. If the investigator is careful to minimize random measurement error in M^O (and covariates), the bias is likely to be smaller, though still not zero, than it otherwise would be when X^O is not the same as X^*.

Latent Variable SEM

If an investigator wants to acknowledge random measurement error when conducting a mediation analysis, SEM would be a natural choice, though using an SEM program for estimation does not in of itself do anything to address the problem if the model being estimated is just a path analysis linking observed variables that contain random measurement error together in a structural model. Rather, the model must include a measurement model component in addition to the structural component that keeps the random measurement error out of the mathematics that generates the structural path coefficients between latent variables (the SEM-equivalent of the "true scores."). This can be done using either a multiple indicator latent variable (MILV) approach or a single-indicator latent variable (SILV) approach.

The MILV approach can be used when one or more of the variables X, M, and/or Y in a mediation model is measured with two but preferably more indicators of the underlying latent variable. Indicators might be, for example, the individual questions on a self-report measure that a person is asked to respond to during the measurement procedure. Measurements of these indicators are observed and therefore available in the data and specified as causally influenced by

[4] An exception would be when an investigator creates a categorical variable by artificially categorizing a continuous variable. In this case, the reliability of the categorical variable will be less than one (MacCallum, Zhang, Preacher, and Rucker, 2002)

the latent variable, which is unobserved and so not in the data. The quantification of the effect of the latent variable on an indicator is the indicator's factor loading. The model of each indicator also includes an error in estimation whose variance is estimated and that captures both random measurement error and error in the estimation of the indicator variable from the latent variable (as rarely would an indicator be perfectly predictable from the latent variable causally influencing it). These linkages between latent variables and their indicators constitute the measurement component of the model. The structural component of the model is the paths, assumed to be causal in a mediation model, that connect the latent variables together in a causal system, just as in an observed variable model. This procedure keeps the random measurement error on the measurement side of the model and will produce more accurate estimates of the effects of the latent variables in the mediation analysis. Examples of the MILV approach in action in a mediation analysis can be found in Fye, Kim, and Rainey (2022) and Wang, Sang, Li, and Zhao (2016). Mediation analysis using the MILV approach is discussed in more detail by Cheung and Lau (2008), Falk and Biesanz (2015), Lau and Cheung (2012), and MacKinnon (2008). Bollen (1989) and Kline (2023) discuss the theory and practice of SEM and latent variable modeling.

When using the MILV approach, no information is required about the reliability of an observed variable. By contrast, the SILV approach involves the estimation of the effects in a structural model just as in the MILV approach, but the measurement component of the model is simpler, with the observed variables X^O, M^O, and/or Y^O being the sole indicators of their corresponding latent variables, as in Figure 2. As the measurement model for each variable won't be identified when there is only one indicator, constraints must be imposed that both identify the measurement side of the model and specify the reliability of the single-item indicators of the latent variables. This is accomplished by fixing the effect of the latent variable on the observed variable to 1 and the variance of the random error in the observed variable to the variance of the observed variable multiplied by 1 minus an estimate of the reliability of the observed variable. Thus, unlike the MILV approach, the SILV approach requires an external estimate of the reliability of the observed variables. The important point is that whether using the SILV or MILV approaches, the variance-covariance matrix for the latent variables used in the estimation of the structural paths in the mediation model is less contaminated by measurement error than it otherwise would have been if the reliability of the observed variables were assumed to be one and the model estimated as an observed variable SEM or using separate OLS regression equations.

Errors-in-Variables (EIV) Regression

An alternative and arguably simpler approach to accounting for the effects of random measurement error, EIV regression is often attributed to Fuller (e.g., Fuller, 1987) and has origins in the economics literature. This approach is both elegant and not particularly difficult to implement (the computations are described in Appendix A for the EIV implementation we use throughout this manuscript). The premise is that regression coefficients are derived from the variance-covariance matrix of the variables in the model, but the variance-covariance matrix of observed scores is contaminated by measurement error. Under the assumptions of classical test theory that the random measurement errors and true scores are uncorrelated and the random errors are uncorrelated with each other, the covariance between observed scores is equal to the covariance between the true scores. Thus, the covariances of the observed scores are accurate even with random measurement error in the observed scores. But the variances of the observed scores contain a component attributable to random measurement error.

EIV regression involves a modification of the variances of variables on the right-hand sides of the equations defining a model that removes the part of the variance of the observed scores attributable to random measurement error. Under the laws of classical test theory, the variance of the random measurement errors for the mediator is, from equation (5),

$$V(\varepsilon_M) = [1 - \rho(M^O)]\, V(M^O) \tag{6a}$$

Using a similar logic for X,

$$V(\varepsilon_X) = [1 - \rho(X^O)]\, V(X^O) \tag{6b}$$

and so the variance of the mediator and X true scores are, respectively,

$$V(M^*) = V(M^O) - [1 - \rho(M^O)]\, V(M^O) \tag{7a}$$

$$V(X^*) = V(X^O) - [1 - \rho(X^O)]\, V(X^O) \tag{7b}$$

With information about the reliability and variance of the observed scores for X, M, and covariates if used, the variance of the true scores can be estimated. Reliability estimates are not difficult to generate using methods discussed in the psychometrics literature, taught in many methodology classes researchers take in graduate school and elsewhere, and are programmed into most good statistics packages. Of course, the variance of the observed scores is known. Since the input to the regression routine is the variance-covariance matrix of the variables in the model, EIV regression works with a version of the variance-covariance matrix whose diagonal elements for the variances of the variables on the right-hand side of the equation are produced by subtracting from the observed variances the part of the variance in the observed scores due to random measurement error, as in equations 7a and 7b (and likewise for covariates if used).

So long as the estimate of reliability of the observed scores is reasonably accurate, this modification produces a data matrix that corresponds more closely to the data one would have if the true scores rather than the observed scores had been quantified. The new data matrix is then used with standard OLS regression algebra (and with the same assumptions) to produce estimates of the effects of variables in the model that are likely to be closer to their "true" or correct values had the true scores been observed instead of the error-contaminated observed scores. Because of the modification to the data, standard errors for the resulting regression coefficients must be adjusted to account for this modification (see Appendix A for various options available). Note that in EIV regression, random measurement error in the variable on the left-hand side of a model equation is not removed from the data, as random measurement error in that variable when serving as outcome does not bias the estimation of regression coefficients.

Methodologists have examined the utility of EIV regression when analyzing behavioral science data. For example, Culpepper and Aguinis (2011) found that EIV regression produced more accurate estimates of mean differences while maintaining Type I error control compared to OLS regression and a few alternative approaches to random measurement error correction in the two-group analysis of covariance when the covariate contains random measurement error. And Culpepper (2012) provides evidence of the superior performance of EIV regression compared to OLS regression and the SILV approach for testing interaction between two variables when one is dichotomous. Counsell and Cribbie (2017) also found good performance of EIV regression relative to change scores or analysis of covariance in two-group comparisons of change over time, but they were less optimistic about is use in some circumstances such as when the sample size was small or the assumed reliability of the variable over time was different than the actual test-retest reliability.

So EIV regression appears to have promise as a straightforward solution to the problems in estimation of effects in linear models that result when measurement error is ignored. But the impact of this work on the practice of data analysis, mediation analysis in particular, has thus far been limited as evidenced by the lack of use of EIV regression in the behavioral science literature. Two explanations for this seem probable. First, EIV regression likely remains largely unknown to most researchers, as is not implemented in most data analysis software behavioral scientists prefer to use (the exception being Stata) nor is it discussed in many of the books often used in regression and linear modeling classes. Second, the work to date on EIV regression has not been undertaken with mediation analysis in mind and so perhaps has not attracted the attention of those who otherwise have taken great interest in elucidating mechanisms that underly causal effects through mediation analysis.

In the rest of this manuscript, we explore and discuss the potential contribution of EIV regression as an approach to managing the effects of random measurement error in mediation analysis. We first do a set of example mediation analyses, analyzing real data sets using OLS regression analyses of observed variables as well as using EIV regression. We do the same analyses using the SILV approach to compare the results it yields relative to OLS and EIV regression. We then describe a simulation that addresses better than our three examples whether EIV reduces or eliminates the bias observed when random measurement error is not accounted for using OLS regression. We end with a discussion of an implementation of EIV regression in the PROCESS macro for SPSS, SAS, and R, acknowledging some potential limitations and caveats of using EIV regression.

Example Mediation Analyses Using Real Data

In this section, we provide three example mediation analyses using real data sets from published studies, each of which contained a mediation analysis. Two of these are models with a single mediator, one with both X and M observed with random measurement error, and one with X manipulated and containing no random measurement error but M imperfectly measured. The third included an observed X, and two observed mediators configured in parallel form, all three of which were imperfectly measured. In all examples, the measurement of Y also contained random measurement error. We analyzed the data the investigators made publicly available using three approaches, thereby allowing a comparison of point estimates and inferences they yield: OLS regression which does not account for random measurement error, EIV regression accounting for random measurement error in variables on the right side of equations, and the SILV modeling approach. Code used to conduct each analysis can be found in Appendix B and data files used are available at https://osf.io/x8we5/. Note that our use of data from these three studies should not be construed as an endorsement of the analyses the investigators report or the convincingness of the causal arguments made justifying their analysis and interpretation. We chose them because the papers were published, the data were publicly-available, and the analyses the investigators reported represent typical practice in the use of mediation analysis in the behavioral sciences.

Example 1: Compassion Fatigue and Mindset

The first example is based on data taken from Gainsburg and Cunningham (2023). The data include responses from 308 adults who completed a task designed to elicit compassion fatigue by showing them photos of people experiencing distressing situations. Participants were asked about their beliefs about compassion as a limited resource, expected compassion fatigue from the task, and resulting compassion fatigue. The mediation model specifies that beliefs about whether compassion is a limited resource (compassion mindset: X) as the cause of compassion fatigue elicited from the task (experienced compassion fatigue: Y), operating indirectly through their anticipated compassion fatigue from the task (expected compassion fatigue: M). Participants' expectations about how fatiguing the task will be were theoretically caused by their beliefs about compassion as a limited resource, which in turn would increase their experience of compassion fatigue. Measures of all three variables were constructed as unweighted averages of Likert-scaled responses to multiple indicators. The reliabilities of observed scores reported by the investigators and using Cronbach's α were 0.73 for compassion mindset (four indicators), 0.86 for expected compassion fatigue (eight indicators), and 0.84 for experienced compassion fatigue (eight indicators).

A mediation analysis was conducted first using OLS regression models of expected compassion fatigue (M: Equation 1) and experienced compassion fatigue (Y: Equation 2), which ignores random measurement error in all three variables. The analysis was conducted using the OLS regression implementation in PROCESS (Hayes, 2022), with inference for the indirect effect conducted using a percentile bootstrap confidence interval based on 5,000 bootstrap samples. Using maximum likelihood estimation with structural equation modeling of observed variables would generate the same point estimates of effects (Hayes, Montoya, & Rockwood, 2017; Lang, 1973), though standard errors would differ slightly from OLS standard errors.

We next used EIV regression as implemented in the PROCESS macro as of Version 5 that is described in more detail later. The code to estimate the model can be found in Appendix B and the mathematical details of the implementation can be found in Appendix A. EIV regression accounts for random measurement error on the right-hand sides of each equation but not the left-hand sides. In the model predicting expected compassion fatigue (M), random measurement error was accounted for in compassion mindset (X) during model estimation through modification of the data matrix as described earlier and in Appendix A. In the model predicting experienced compassion fatigue (Y), the model was estimated after this same data modification procedure for both compassion mindset (X) and expected compassion fatigue (M). Inference for the indirect effect was conducted using percentile bootstrap confidence intervals based on 5,000 bootstrap samples.

The model was then estimated using the SILV modeling approach as diagrammed in Figure 2 and using the lavaan package version 0.6-18 in R (code is provided in Appendix B, with accompanying code for the same analysis in Mplus and Stata available in supplementary materials online). The unweighted average of indicators for each variable served as the sole indicator of their respective latent variables, with the factor loading constrained to one and the error in the estimation of the indicator variables constrained to the observed variance multiplied by one minus the reliability estimate. The mediation model was then estimated replacing the observed variables with corresponding latent variables using maximum likelihood estimation of model parameters, and inference about the indirect effect estimated using a percentile bootstrap confidence interval based on 5,000 bootstrap samples.

The results of these three analyses are shown in Table 1. Notice first that the OLS estimates of each path and the indirect effect differ from those generated by the EIV and SILV approaches, with the OLS estimates attenuated (i.e., closer to zero) relative to those generated with the EIV and SILV approaches. This attenuation of effects is consistent with previous literature on the effects of measurement error on the estimates of effects in linear models. But as discussed in Cole and

Preacher (2014), the consequences of unaccounted-for unreliability can be either the under or overestimation of effects depending on the complexity of the model, which variables are measured imperfectly, and how unreliable those measurements are (as our second example will illustrate).

Second, notice in Table 1 that the point estimates of the effects using EIV regression and the SILV approach are the same. It makes little difference which of the two methods is used. The point estimates are not affected by the choice. The standard errors do differ somewhat though this is expected. The SILV approach but not EIV regression accounts for random measurement error in variables on the left-hand sides of the equations. All other things being equal, this would tend to produce smaller standard errors, though we don't see consistent evidence of that in this example. In addition, the estimation theory and math of standard error estimation is different for maximum likelihood estimation compared to EIV regression and can produce some differences in estimates, though less so as sample size increases.

Third, notice that the squared multiple correlations are different. This is also expected. When the variables in the OLS regression contain random measurement error, this would reduce the multiple correlation relative to when random measurement error is accounted for. The multiple correlation for EIV regression is larger than when using OLS but smaller relative to SILV. This is because EIV regression accounts for random measurement error in variables on the right-hand sides of equations but not (at least as currently implemented in PROCESS) variables on the left-hand sides of equations, whereas SILV accounts for random measurement error in variables on both the left- and right-hand sides.

Finally, observe that substantively, and thinking only dichotomously in terms of whether an effect can be said to be zero or not, the results are very similar between the three approaches. All three yield 95% confidence intervals for the indirect effect that are positive and exclude zero, and both result in total and direct effects that are not statistically different from zero by a null hypothesis test or confidence interval.

Example 2: Nature and Self-Actualization

The second example is based on data from Yang et al. (2024, Study 4), who investigated the effect of exposure to nature on authenticity, defined as a sense of humanistic self-actualization with behaviors congruent with the self. One hundred seventy one participants were randomly assigned to view either pictures of nature ($X = 1$) or pictures of urban environments ($X = 0$), then answered questions about their resulting mood (positive affect: M) and sense of authenticity (authenticity = Y). It was hypothesized that participants in the nature condition relative to those in the urban condition would have a greater sense of authenticity (12 indicators averaged to produce an observed score; Cronbach's $\alpha = 0.82$) indirectly through an enhancement of positive mood (18 indicators averaged; Cronbach's $\alpha = 0.91$), which would in turn prompt greater authenticity.

The mediation analyses were conducted just as described in the first example (accompanying code is shown in Appendix B), with results from the three analytical approaches shown in Table 2. Models predicting M are largely identical across approaches, since the experimental manipulation contains no random measurement error and measurement error in the variable on the left-hand side of the equation (M) does not bias the estimate of X's effect. The differences between approaches become apparent when looking at the estimates in the equation for Y. Like the previous example, the OLS estimate of the effect of the mediator on Y is attenuated relative to the EIV and SILV estimates, but the direct effect of X is closer to zero in the EIV and SILV models compared to the OLS model. Given that the effect of X on M and the total effect of X on Y is same across approaches (a requirement of an X without measurement error), the attenuation of the effect of M on Y in OLS means that the direct effect of X would have to increase to offset the resulting decrease in the indirect effect relative to the EIV and SILV approaches. While all three approaches produce statistically significant indirect effects and non-significant direct effects, the magnitude of the indirect effect is larger and the direct effect is subsequently smaller in EIV and SILV models. And like in the first example, the EIV and SILV approaches produce the same estimates of the effects and similar though not identical standard errors (for the reasons described in the first example), but the estimates are different from those produced by OLS regression. The multiple correlations also show the same pattern as the first example, with the multiple correlation smallest for OLS regression and largest for the SILV approach.

Example 3: Photo-Editing and Self-Perceived Attractiveness

The final example is based on data taken from Ozimek et al. (2023). Unlike the prior two examples, this example represents a multiple mediator model with two mediators operating in parallel, with neither mediator specified as effecting the other (see e.g., Preacher & Hayes, 2008). Such a mediation analysis includes an additional equation for the second mediator, as in equations 1 and 3, while also including the second mediator in the model of Y (equations 2 and 4). The result is two specific indirect effects of X on Y, one through mediator M_1 and another through M_2, as well as a total indirect effect that is the sum of the two specific indirect effects. This model also allows for a comparison of the two indirect effects, estimated as their raw difference (i.e., indirect effect 1 minus indirect effect 2) in the analysis we reported below (see e.g., Preacher & Hayes, 2008; Coutts & Hayes, 2023).

The data include responses from 617 adults who actively use social media about their photo editing behavior and self-perceptions. Photo editing (X) was measured using the unweighted average of five indicators measuring the participant's tendency to edit and use filters on pictures of themselves for social media, with higher scores representing more photo editing behavior (Cronbach's $\alpha = 0.75$). Participants were also asked about their self-objectification (M_1), a 14-item measure quantifying the degree to which participants view themselves and their own self-worth through their attractiveness as perceived through the eyes of others (Cronbach's $\alpha = 0.89$). They were also asked about their tendency to engage in physical appearance comparisons between their own appearance and the appearance of others (M_2; 5 items, Cronbach's $\alpha = 0.73$). Finally, participants were asked to rate their self-perceived attractiveness (Y) using six indicators assessing their general satisfaction with their appearance (Cronbach's $\alpha = 0.91$). They proposed that higher photo editing behaviors contribute to lower self-perceived attractiveness indirectly through increases in self-objectifying and physical appearance comparisons with others, which would in turn lower self-perceived attractiveness.

The mediation analyses were conducted as in the previous two examples, with the modifications described earlier to accommodate the second mediator, and the results are shown in Table 3. In line with the previous two examples, EIV and SILV approaches produce the same point estimates of regression coefficients, total, direct and indirect effects, as well as the difference between the indirect effects. But all of these estimates are different than those from OLS regression. Standard errors are somewhat different for the same reasons in prior examples, as are multiple correlations. Although most of the null hypothesis tests and confidence intervals resulted in the same inference about whether the effect was zero, in this example there was one noteworthy exception in the difference between indirect effects. Notice than using OLS estimation, the indirect effect through M_1 (self-objectification) is 2.7 times as large as the indirect effect through M_2 (physical attractiveness comparisons). And a bootstrap confidence interval for the difference between these two indirect effects does not include zero, leading to the conclusion that the indirect effect through self-objectification is stronger. In contrast, when using the EIV and SILV approaches, the two indirect effects are much less different from each other (by a ratio of only about 1.76), and a bootstrap confidence interval for the difference does include zero, resulting in the conclusion that these two indirect effects do not differ in strength. Thus, in this more complex example, the choice to ignore random measurement error by using OLS regression leads to some substantively different conclusions and inferences than when accounting for random measurement error.

The Effectiveness of EIV Regression: Simulation Evidence

Our example analyses presented in the prior section show that EIV regression produces estimates of the effects in a mediation model that are the same as the estimates obtained from the

SEM-based SILV approach, both of which diverge from estimates generated by OLS regression. But two people can agree the earth is flat while both being wrong. Who is to say that the EIV and SILV approaches generate more accurate estimates and inferences? Without some objective truth against which these results can be evaluated, the fact that EIV results correspond to the SILV model results doesn't mean the EIV results are more trustworthy. Perhaps they are both wrong in the same manner and the OLS results better estimates of reality. To answer this question, we conducted a small-scale simulation in which we defined truth to determine whether the EIV approach that accounts for random measurement generates more accurate estimates of the effects in a mediation model than does OLS regression that ignores that random measurement error.

We assume that researchers who ignore random measurement error by conducting a mediation analysis using OLS regression are comfortable treating their point estimates of effects and the fit of their models (using R^2) as estimates of corresponding parameters in the population from which they have sampled or the true data generating mechanism. Although we have reason to doubt that the OLS estimates are good ones, we designed the simulation giving the benefit of the doubt to the OLS regression results we report in Tables 1-3. We treated the OLS point estimates of all the paths in those models and the resulting direct, indirect, and total effects as population values or "parameters" of the population or data generating mechanism. Furthermore, we treated the squared multiple correlations as population values of proportion of variance explained in M and Y by each equation. We used these parameters in our simulations, creating data sets of sizes $n = 50$, 100, 200, 300, 500, and 1000 that represent random samples from population mediation models defined by the OLS regression estimates in Tables 1-3. For the simulation corresponding to examples 1 and 3 with continuous X, the data generation started with a set of random standard normal deviates for X, whereas for example 2, X was a dichotomous variable coded 0 and 1 for the two groups with the sample split equally between the two groups. The errors in estimation in the models of M and Y were random normal deviates with variance set to the value required to produce the corresponding squared multiple correlation (within expected sampling error) for each model equation.

This first stage of data generation just described produced a multivariate sample from the population with X, M (M_1 and M_2 in the case of example 3) and Y containing no random measurement error, i.e., representing true scores on the latent variables, with relationships between them defined by the population mediation model. Next, we added random normal measurement error in the form of independent standard normal deviates onto X (except in the simulation corresponding to example 2 with a dichotomous X from a random assignment procedure), M (M_1 and M_2 in the third example) and Y such that the resulting observed scores had reliabilities equal to (within expected random sampling error) the reliabilities reported in our example analyses. Using the resulting sample, now adulterated by random measurement error, we estimated the direct, indirect, and total effects of X using OLS regression and EIV regression, specifying the same reliabilities in the EIV regression routine that were used to generate the observed data. For the simulation corresponding to example 3, we also estimated the difference between the two specific indirect effects as the indirect effect through mediator 1 minus the indirect through mediator 2. This procedure was repeated for a total of 5,000 times for each sample size, recording each of the estimated effects in each repetition as well as whether a 95% confidence interval for the effect included the known population value. As the sampling distribution of an indirect effect is not normal in form, confidence intervals for the indirect effect (as well as the difference in the third simulation) were generated using the percentile bootstrap method based on 5,000 bootstrap samples, whereas confidence intervals for the direct and total effects were generated in the usual way, assuming the sampling distribution is roughly normal in form, as the point estimate plus or minus approximately 2 standard errors (the appropriate critical value from the t distribution was used rather than 2, with degrees of freedom equal to the residual degrees of freedom for the regression equation). We repeated this entire simulation for a total of 18 times, each time using one of the sets of population parameters defined by the models reported in Tables 1, 2, and 3 and for 6 sample sizes. All simulations were programmed in R and using the OLS and EIV regression routines built into the PROCESS macro described later as the computational engine for estimation of the model coefficients and calculating inferential statistics. The PROCESS implementation of EIV regression is documented in Appendix A.

The results are found in Tables 4 and 5 for each population and sample size combination. Table 4 provides the mean estimate of each effect over the 5,000 replications, as well as the mean bias percentage, defined as Bias% = 100 (Mean Estimate – Parameter) / Parameter for the first two examples, and corresponding results for the third example are in Table 5. The value of the parameter of each effect is found in each of the subheadings or parentheses. A negative value for Bias% reflects attenuation of the effect toward zero, whereas a positive value reflects an overestimate (i.e., bias away from zero). Tables 4 and 5 also provide 95% confidence interval coverage, meaning the percentage of times over the 5,000 replications that the confidence interval for that effect included the known value of parameter being estimated. Good performance is reflected in a mean estimate of the effect close to the corresponding parameter (i.e., Bias% near zero) and confidence interval coverage near 95%.

Given that we have simulated only three populations varying unsystematically in the sizes of the effects being estimated and the reliabilities of the observed variables, we are cautious to not overanalyze and overinterpret these results. But there are a few patterns that jump right off the page and are noteworthy. First, notice in Tables 4 and 5 that OLS (and by extension, observed variable SEM) generally gets the effects wrong, and sometimes substantially so, as reflected by the difference between the population effects and the mean estimates of those effects over the 5,000 replications. And observe that increasing the sample size has no effect on the bias in OLS estimates. You can't diminish or make the problem produced by unaccounted-for random measurement error go away by just collecting more data. But EIV regression generally gets it right regardless of sample size, with very little discrepancy between mean estimates and the population values of the effects.

Second notice that confidence interval coverage using EIV regression is generally right on the money, with about 95% of 95% confidence intervals containing the parameter. Not so for OLS regression-based confidence intervals, but with a caveat discussed later. Notice that in a few of our example populations, OLS confidence interval coverage for the indirect effect is below 95% even in small samples, and the larger the sample, the worse things get. This is the result of the estimation bias of OLS regression. As the sample size increases, the confidence interval becomes increasingly narrow, converging around the *wrong* estimate and increasingly excluding the parameter. In smaller samples, the bias is likely offset by the larger confidence interval width such that the interval is more likely to capture the parameter even though the interval is centered around a biased estimate.

Earlier we mentioned that random measurement error is not always a major problem for estimation and inference. The second simulation (example 2 in Table 4) based on the nature and self-actualization study makes this point. In this simulation, X is dichotomous and contains no random measurement error and M, though not free of measurement error, is measured with fairly high reliability (0.91). Measurement error in M but not X, regardless of the extent of measurement error in Y, will tend to bias the estimation of the effect of M on Y and therefore the indirect effect of X while also influencing to some extent the accuracy in the estimation of the direct effect of X. This is what we see in the second simulation. But it would not bias the estimation of the total effect, as seen in Table 2. For this to happen, the biases in the estimation of the direct and indirect effects would have to be similar in magnitude but opposite in direction given that the total effect is the sum of the direct and indirect effects. This is also what we see in Table 4. But given the high reliability of M in this example, the bias in estimation of the direct and indirect effects is small, and confidence coverage is still respectable even in moderate to large samples.

In summary, the evidence from this limited simulation is consistent with the conclusion that OLS estimation is the flat-earther here and that EIV regression (and, by logical extension given the example analyses presented in the prior section, the SILV approach) correctly sees the world as round. This conclusion agrees with research and analytical derivations in other modeling contexts that random measurement error in variables on the right-hand side of linear models can wreak havoc on accurate estimation of the effects the researcher is trying to quantify and test.

An Easy-to-Use Implementation of Errors-in-Variables Regression

Fortunately, EIV regression is not some obscure computational technique that one must have a degree in statistics or computer science to employ. It is already available in Stata (StataCorp, 2023) as well as in R (Culpepper & Aguinis, 2011; with code later updated at *www.hermanaguinis.com/eiv.html*), including in the "eivtools" package available through CRAN. These implementations differ and will produce slightly different results and have different output options even though they are based on roughly the same statistical theory. But these implementations were not created with mediation analysis in mind and so require the user to estimate all the equations for a mediation model with separate commands, and obtaining inferential tests for indirect effects requires additional programming that likely goes beyond the skills of many researchers. To facilitate adoption of EIV regression in mediation analysis (and linear modeling more generally) by easing the programming and computational burden, we instead recommend the easy-to-use PROCESS macro for SPSS, SAS, and R (Hayes, 2022) used in our examples and simulations. PROCESS is freely-available at *www.processmacro.org* and already enjoys wide use in mediation analysis throughout the behavioral sciences. EIV regression is implemented as of version 5 and can be used for any mediation model PROCESS can estimate, including simple (single mediator), multiple (parallel or serial), blended (combining parallel and serial) and custom mediation models programmed as described in Appendix B of Hayes (2022).[5] Covariates with corresponding reliabilities can also be included in any mediation model. Only a single line of code is required rather than lengthier syntax required when using an SEM framework. SPSS users have the option of setting up the model with a user-friendly graphical user interface if desired. PROCESS takes care of all the computational work, including inference for indirect effects using bootstrapping. PROCESS implements the computations described in Appendix A. How to set up PROCESS and execute a PROCESS command is documented extensively in Hayes (2022), though the documentation there does not include any discussion of the EIV routine as it was implemented after the printing of the book. Below we provide a brief discussion of the EIV regression options available in PROCESS.

For a mediation analysis, PROCESS will expect the user to specify a single (and only one) independent variable X following **x=**, a single outcome or dependent variable Y following **y=** , and at least 1 mediator M following **m=**. Covariates can be included if desired using the optional **cov=** option. Reliabilities for X and M are entered using the **relx=** and **relm**= options. A model number is typically also required. Using **model=4** in the PROCESS command specifies a simple (single mediator) or parallel multiple mediator model, whereas **model=6** specifies a serial multiple mediator model. Models 80, 81, 82 are models that blend parallel and serial mediation (see the documentation).

In Appendix B, we provide the PROCESS command for the SPSS, SAS, and R versions of the example analyses presented earlier using EIV regression. As can be seen there, the PROCESS command for the compassionate fatigue study specifies the variables in the data named “compass” as X, “efatigue” as M, and “fatigue” as Y, with the estimated reliabilities of compass and efatigue being 0.73 and 0.86, respectively. The model=4 option tells PROCESS to set this up as a mediation

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[5] The EIV regression routine in PROCESS is not yet available for moderation models or models that combine moderation and mediation (conditional process models).

model. The resulting PROCESS output can be found in Appendix C. Notice that PROCESS provides model summary information, the regression coefficients, standard errors, *t*- and *p*- values, confidence intervals, and estimates of the direct, indirect, and total effects of *X* with corresponding inferential information for those effects.

Consider a modification to this example analysis that includes an additional mediator in the model named "rational" in the data and measured with reliability 0.82. In addition, suppose we wanted to include three covariates: "age" in years, whether or not a person self-identified as "male" (coded 1 in the data, 0 otherwise), and self-esteem ("selfest") measured with reliability 0.95. Assuming that age and whether a person identifies as male were measured without random measurement error (i.e., with reliabilities equal to 1), the PROCESS command below estimates a mediation model with "affect" and "rationale" operating as parallel mediators, with the **relcov=** option listing the reliabilities of the covariates:

<u>SPSS</u>

```
process y=fatigue/x=compass/m=efatigue rational/cov=age male
  selfest/model=4/relx=0.73/relm=0.86,0.82/relcov=1,1,0.95.
```

<u>SAS</u>

```
process (data=compfat,y=fatigue,x=compass,m=efatigue rational,cov=age
  male selfest,model=4,relx=0.72,relm=0.86 0.82,relcov=1 1 0.95)
```

<u>R</u>

```
process(data=compfat,y="fatigue",x="compass",m=c("efatigue",
  "rational"),cov=c("age","male","selfest"),model=4,relx=0.72,
  relm=c(0.86,0.82),relcov=c(1,1,0.95))
```

When more than one mediator or covariate is listed following **m=** or **cov=**, reliabilities, if any, must be provided for all variables in the same order the variables are listed in the PROCESS command. Unknown reliabilities could be set to 1 or the user could provide a reasonable guess for the unknown reliability if assuming 1 is not defensible. If the reliability of measured variables is not provided by using the **relx**, **relm**, or **relcov** options (if covariates are included in the model), the reliability for those variables without a reliability provided is assumed to be 1 by PROCESS. In other words, reliability of 1 is the default for the **relx, relm,** and **relcov** options. When neither **relx**, **relm**, nor **relcov** options are used in the command, PROCESS does an OLS regression analysis rather than EIV regression.

In a mediation analysis, PROCESS defaults to the production of a 95% bootstrap confidence interval for the indirect effect(s) using the percentile method based on 5,000 bootstrap samples. The number of bootstrap samples can be changed with the **boot** option (e.g., **boot=10000** for 10,000 bootstrap samples) and the confidence level changed with the **conf** option (e.g., **conf=90** for 90% confidence intervals). Bootstrap inference for each of the regression coefficients in the model is also available rather than for just the indirect effect(s) by using the **modelbt** option. Output can also be saved for use later if desired using the **save** option, which we relied on to conduct the simulation reported earlier. See the PROCESS documentation in Hayes (2022) for a discussion of these options.

EIV regression relies on a modification of the data based on the reliabilities of the observed variables provided by the user. In some circumstances, the resulting modified variance-covariance matrix may not be possible to observe in nature. This can occur in small samples and/or when one or more of the entered reliabilities is too small. In that case, PROCESS will produce an error saying the model could not be estimated as a result of one or more small reliabilities entered. That can also occur during the bootstrapping phase. When it does, PROCESS will replace the offending bootstrap sample with another. A note at the bottom of the output will tell the user how many times a bootstrap sample had to be replaced. Although there is no guidance available for how many such replacements is acceptable without affecting the validity of bootstrap inference, common sense would suggest fewer replacements would be better. If the user is uncomfortable with the number of replacements required to complete the bootstrapping, a Monte Carlo confidence interval could be used as an alternative for inference about the indirect effect, as it does not require resampling from the data (Preacher & Selig, 2012). PROCESS can conduct a Monte Carlo confidence interval for indirect effects using the **mc** option, as described in the documentation.

Note that a mediator is not required to use PROCESS's EIV regression option. It will also conduct an ordinary EIV regression analysis. For example, using the same variables and reliabilities from the prior hypothetical example, the command below would estimate an EIV regression model of fatigue from compass, efatigue, rational, age, male, and selfest:

SPSS

```
process y=fatigue/x=compass efatigue rational age male selfest/
  relx=0.72,0.86,0.82,1,1,0.95.
```

SAS

```
process (data=compfat,y=fatigue,x=compass efatigue rational age male
  selfest,relx=0.72 0.86 0.82 1 1 0.95)
```

R

```
process(data=compfat,y="fatigue",x=c("compass","efatigue","rational",
  "age","male","selfest"),relx=c(0.72,0.86,0.82,1,1,0.95))
```

Because analysis of covariance (ANCOVA) is just a special case of multiple regression, it follows that the EIV routine in PROCESS can be used to conduct analysis of covariance when the covariate or covariates contain random measurement error (see Culpepper & Aguinis, 2011, for a discussion of EIV regression in ANCOVA). Using the **mcx** option described in the documentation, the user can specify that there are more than two groups being compared in the ANCOVA. This option also works in mediation models with a multicategorical X.

Discussion

In this manuscript, we have made the case for EIV regression as a viable approach to mediation analysis when the variables in the model contain random measurement error. Our example analyses and simulations show that EIV regression produces results that are identical to the single indicator latent variable approach using SEM while eliminating the bias in the estimation of effects that occurs when using OLS regression and ignoring random measurement error.

Furthermore, at least in our examples, EIV-based confidence intervals for effects preserved their meaning as coverage was generally consistent with confidence. But OLS confidence intervals lose their meaning with increasing sample size, as coverage of the true effect decreases (gets worse) with more data as a result of the estimation bias. We recommend that researchers properly address the random measurement error in their mediation analyses, with EIV-regression being a painless and easy-to-apply approach, even for more complicated models than we have focused on here, and certainly better than the standard practice of ignoring it altogether.

It is not our intention to suggest that EIV regression is statistically better than latent variable SEM. On the contrary, we have shown that EIV regression seems to perform as well as the SILV approach, and so which to use is a matter of personal choice. SEM does offer much more flexibility in model estimation, can handle random measurement error in Y, provides various accepted means of dealing with missing data, generates various measures of model fit, and provides other advantages beyond the scope of this paper. Table 6 provides a relative comparison of the properties of OLS, EIV, and SILV using SEM. This table illustrates how OLS regression can be viewed as a special case of both EIV regression and the SILV approach in which random measurement error is simply ignored entirely. EIV regression (at least as implemented in PROCESS) is a special case of the SILV approach in that it accounts for random measurement error in variables when on the right-hand side of equations but not when on the left-hand side. SILV is the most flexible in that it accounts for random measurement error in all model variables if so programmed. But we do feel that EIV regression is substantially less analytically complex to implement for the user and requires (as implemented in PROCESS) less code even for more complex structural models that can be intimidating to the average researcher to program in SEM. EIV regression as implemented in the PROCESS macro requires only a few additional keystrokes in the syntax compared to doing nothing when using OLS and can be easily adopted by even novice researchers. Furthermore, the EIV estimation implemented in PROCESS is quite fast. PROCESS took no more than 10 seconds for each of the analyses reported in Tables 1-3, including the 5,000 bootstrap samples for inference about the indirect effect, whereas the SILV approach in lavaan in R using the code in Appendix B took between 60 and 90 seconds.[6]

Although we encourage researchers to consider EIV regression when conducting a mediation analysis using variables adulterated by random measurement error, it could also be used as supplementary method rather than the main analysis reported. For example, PROCESS makes it easy to expand the kind of sensitivity analysis often recommended in mediation analysis (e.g., Imai et al., 2010; Kinbu-Sakarya, MacKinnon, Valente, & Çetinkaya, 2020; Qin & Yang, 2022) to examine how susceptible one's conclusions are to unaccounted-for random measurement error or when the extent of random measurement error is different than assumed. Perhaps the investigator prefers to use OLS regression but wants to know how vulnerable the conclusions are to unaccounted-for measurement error. The investigator could repeat the mediation analysis that ignored measurement error but using known estimates of reliability in an EIV routine such as programmed in PROCESS. Consistency in the results, at least substantively, can then be used as a defense against criticism that the results reported when unreliability is ignored are just artifacts of unaccounted-for measurement error. Another possibility is to follow up an EIV-based mediation analysis with alternative reliability estimates that are smaller than the reliabilities used. This would assess the vulnerability of the original EIV regression analysis to random measurement error that is larger than assumed. This could be particularly useful in smaller samples, where reliability estimates would likely be less accurate, by using the lower bound of interval estimates of reliability rather than point estimates. Or if there is no estimate of reliability available for one or more variables in

[6] When executed on a Dell Latitude 5310 64bit Intel i5 CPU @ 1.7GHz with 8GB RAM and Windows 10 operating system. The Mplus code for the SILV approach provided in the supplementary materials ran in about 4 seconds. The corresponding Stata code required between 6 and 8 minutes to execute.

the model, the investigator could try different values of reliability to see how much the results are affected by different assumptions about the unknown reliability.

In the realm of pedagogy, we hope our findings about the viability of EIV regression-based mediation analysis and its implementation in PROCESS will encourage statistics and methodology instructors to address the shortcomings of unaccounted-for measurement error in data analysis and how it can be easily addressed in some circumstances. Contrary to the difficulties many students experience learning to use an SEM program, we have found that it takes only 10-15 minutes of classroom time to get students familiar with PROCESS syntax, and its implementation in the graphical user interface in SPSS makes it still easier to employ, even in undergraduate classrooms. The speed at which PROCESS generates output is an additional advantage of its use in the classroom relative to lavaan.

But an analysis is only as good as the data it is given. By the standard of reliability, the less well a variable in a model is measured, the less well that variable will capture the effect of what the researcher claims to be studying. The same can be said for any strategy that attempts to account for the effects of random measurement error. EIV regression, like the SILV approach, requires information about the reliability of the observed variables in the model. In the example analyses we reported here, we treated the reliability estimates reported by the original investigators as truth. And in the simulations, we defined that truth. But in practice, investigators don't know the reliability of their measurements. They can only estimate their reliability, and there are different approaches to estimation that can give different estimates. To satisfy its mission of accounting for the effects of random measurement error, EIV regression must be given good data to make the adjustment, and that means providing the algorithm with reasonably accurate reliability information. Had we used different reliability estimates in our examples or used incorrect reliability information in our simulation, EIV regression would have gotten things wrong too. As Culpepper (2012) and Counsell and Cribbie (2017) note in a different analytical context, the debiasing effects of EIV-regression are dependent on providing the routine with accurate estimates of reliability of the observed variables. So investigators should take care in generating those estimates.

Lockwood and McCaffrey (2020) report that EIV standard errors may be too small in some circumstances, and Counsel and Cribbie (2017) reported elevated Type I error rates in smaller samples for inferences about group differences in a two-group pretest-posttest design. Inaccurate standard errors would not affect the validity of inferences for the indirect effect using a percentile bootstrap confidence interval, as no standard error is used in the derivation of a bootstrap confidence interval calculated using the percentile method. But it could affect the validity of inferences in small samples about individual paths, including the total and direct effects. If you squint at Tables 4 and 5 you might notice EIV confidence interval coverage that is slightly below 95% for the total and direct effects in smaller samples, consistent with small-sample concerns expressed by Lockwood and McCaffrey (2020) and Counsel and Cribbie (2017). We suggest (as did Lockwood and McCaffrey, 2020) double-checking inferences for individual paths using either bootstrap confidence intervals or a bootstrap estimate of sampling variance. This can be done easily in PROCESS using the **modelbt** option. See the documentation.

Our enthusiasm for the results we describe here is somewhat tempered by our awareness that this manuscript is more of a demonstration by example rather than a more rigorous analysis and simulation that varies widely and orthogonally various factors that may influence these results as well as other approaches, including the sizes of the effects being estimated, the reliability of the observed variables, more approaches to accounting for random measurement error, sample size, and so forth. We encourage future research on the performance of EIV-regression relative to alternatives to further explore the boundary conditions and generality of our claims and wisdom of our recommendations.

We conclude by warning researchers who might be excited by how easy it now is to account for random measurement error in their mediation analyses that EIV regression should not be used as a means of sweeping deeper measurement problems under the rug. As Flake and Fried (2020) discuss, many researchers are remarkably cavalier in their approach to measurement, often using questionable measurement practices that lower the quality and meaning of their data and make their results hard to interpret. Poor measurement, construct invalidity, and highly unreliable or otherwise low-quality data cannot be made righteous by statistical sleights-of-hand. The EIV implementation we have described here is no exception to this general rule. Its use is best reserved for situations in which investigators have given careful thought to how they are measuring their constructs, are convinced they are measuring what they intend to be measuring and are doing so reasonably well but find they need a little extra estimation help given their measurements still contain some inevitable random measurement error.

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Table 1.

Example 1: A mediation analysis of the effect of compassion mindset on compassion fatigue via expected fatigue from an experimental task using ordinary least squares regression assuming perfect reliability (OLS), a single-indicator latent variable model (SILV), and errors-in-variables regression (EIV) as implemented in PROCESS. Standard errors in parentheses. Standard errors for the indirect effect are bootstrap estimates. $n = 308$.

	Expected Fatigue (*M*) (reliability = 0.86)			Fatigue (*Y*) (reliability = 0.84)		
	OLS	EIV	SILV	OLS	EIV	SILV
Constant	4.266 (0.210)	4.631 (0.280)	4.632 (0.287)	3.004 (0.302)	2.916 (0.413)	2.916 (0.424)
Compassion Mindset (*X*) (reliability = 0.73)	–0.250 (0.052)	–0.342 (0.070)	–0.342 (0.071)	–0.146 (0.050)	–0.178 (0.071)	–0.178 (0.072)
Expected Fatigue (*M*)				0.464 (0.054)	0.529 (0.064)	0.529 (0.066)
R^2	.071	.097	.113	.254	.293	.349

	OLS	EIV	SILV
Total effect of Compassion Mindset	–0.262 (0.054)	–0.359 (0.073)	–0.359 (0.075)
Direct effect of Compassion Mindset	–0.146 (0.050)	–0.178 (0.071)	–0.178 (0.072)
Indirect effect of Compassion Mindset	–0.116 (0.032)	–0.181 (0.051)	–0.181 (0.050)
[95% percentile bootstrap CI]	[–0.185, –0.059]	[–0.292, –0.093]	[–0.292, –0.093]

Table 2.

Example 2: A mediation analysis of the effect of experimental nature exposure on self-authenticity via positive affect using ordinary least squares regression assuming perfect reliability (OLS), a single-indicator latent variable model (SILV), and errors-in-variables regression (EIV) as implemented in PROCESS. Standard errors in parentheses. Standard errors for the indirect effect are bootstrap estimates. $n = 171$

	Positive Affect (*M*) (reliability = 0.91)			Authenticity (*Y*) (reliability = 0.82)		
	OLS	EIV	SILV	OLS	EIV	SILV
Constant	3.443 (0.063)	3.443 (0.063)	3.443 (0.063)	2.823 (0.392)	2.651 (0.429)	2.650 (0.428)
Nature Condition (*X*) (reliability = 1)	0.320 (0.089)	0.320 (0.089)	0.320 (0.089)	0.143 (0.133)	0.127 (0.134)	0.127 (0.133)
Positive Affect (*M*)				0.468 (0.111)	0.519 (0.122)	0.519 (0.122)
R^2	.070	.070	.077	.121	.131	.160

	OLS	EIV	SILV
Total effect of Nature Condition	0.292 (0.135)	0.292 (0.135)	0.292 (0.134)
Direct effect of Nature Condition	0.143 (0.133)	0.127 (0.134)	0.127 (0.133)
Indirect effect of Nature Condition	0.150 (0.053)	0.166 (0.059)	0.166 (0.058)
[95% percentile bootstrap CI]	[0.058, 0.263]	[0.064, 0.293]	[0.063, 0.289]

Table 3.
Example 3: A mediation analysis of the effect of photo-editing on self-perceived attractiveness via self-objectifying and physical attractiveness comparisons using OLS regression assuming perfect reliability, a single-indicator latent variable model (SILV), and errors-in-variables regression (EIV) as implemented in PROCESS. Standard errors in parentheses. Standard errors for the indirect effects are bootstrap estimates. $n = 671$

	Self-Objectification (M_1) (reliability = 0.89)			Physical Attract. Comp (M_2) (reliability = 0.73)			Self-perceived Attract. (Y) (reliability =0 .91)		
	OLS	EIV	SILV	OLS	EIV	SILV	OLS	EIV	SILV
Constant	1.545 (0.073)	1.283 (0.091)	1.283 (0.096)	2.262 (0.078)	2.034 (0.099)	2.033 (0.103)	1.429 (0.048)	1.304 (0.071)	1.304 (0.075)
Photo Editing (X) (reliability = 0.75).	0.405 (0.035)	0.540 (0.045)	0.540 (0.048)	0.355 (0.038)	0.473 (0.049)	0.473 (0.051)	0.075 (0.017)	0.085 (0.024)	0.085 (0.025)
Self Objectifying (M_1)							0.243 (0.023)	0.231 (0.055)	0.231 (0.058)
Physical Attractiveness Comparisons (M_2)							0.105 (0.021)	0.150 (0.060)	0.150 (0.063)
R^2	.166	.222	.249	.117	.156	.213	.454	.496	.545

	OLS	EIV	SILV
Total effect of Photo Editing	0.210 (0.019)	0.280 (0.025)	0.280 (0.026)
Direct effect of Photo Editing	0.075 (0.017)	0.085 (0.024)	0.085 (0.025)
Indirect effect via Self-Objectification (M_1)	0.099 (0.014) [0.072, 0.127]	0.125 (0.046) [0.021, 0.200]	0.125 (0.045) [0.020, 0.198]
Indirect effect via Physical Attract. Comparisons (M_2)	0.037 (0.013) [0.015, 0.063]	0.071 (0.050) [0.002, 0.195]	0.071 (0.050) [0.003, 0.195]
Difference between indirect effects	0.061 (0.022) [0.018, 0.104]	0.054 (0.093) [-0.169, 0.191]	0.054 (0.092) [-0.168, 0.190]

Table 4.
Mean Estimated Effect ("Mean"), Mean Bias Percentage ("Bias%"), and 95% Confidence Interval (CI) Coverage in the Simulation for Examples 1 and 2 (True Effect Being Estimated in Parentheses).

		OLS			EIV		
Example 1	n	Mean	% Bias	95% CI Coverage	Mean	%Bias	95% CI Coverage
Total effect (-0.262)	50	−0.191	−27.2	91.4	−0.261	−0.3	93.9
	100	−0.192	−26.5	88.1	−0264	0.6	94.5
	200	−0.191	−27.1	80.0	−0.261	−0.2	94.8
	300	−0.191	−27.0	72.4	−0.262	0.0	94.8
	500	−0.192	−26.6	58.5	−0.264	0.6	94.8
	1000	−0.192	−26.7	31.0	−0.263	0.4	95.0
Direct effect (-0.146)	50	−0.117	−19.8	93.8	−0.146	0.1	92.9
	100	−0.118	−19.3	93.7	−0.147	0.5	94.4
	200	−0.118	−19.5	92.0	−0.147	0.4	94.0
	300	−0.117	−19.7	90.8	−0.146	0.1	94.6
	500	−0.118	−19.2	89.2	−0.147	0.8	94.6
	1000	−0.118	−19.3	82.0	−0.147	0.6	94.2
Indirect effect (-0.116)	50	−0.074	−36.6	84.3	−0.115	−0.8	94.3
	100	−0.075	−35.6	79.7	−0.117	0.9	94.7
	200	−0.073	−36.7	66.9	−0.115	−1.0	94.5
	300	−0.074	−36.2	55.6	−0.116	−0.1	95.2
	500	−0.074	−35.9	38.0	−0.116	0.3	94.7
	1000	−0.074	−35.9	11.7	−0.116	0.2	94.7
Example 2							
Total effect (0.293)	50	0.287	−1.9	95.0	0.287	−1.9	95.0
	100	0.294	0.5	94.8	0.294	0.5	94.8
	200	0.292	−0.1	94.9	0.292	−0.1	94.9
	300	0.296	1.0	94.5	0.296	1.0	94.5
	500	0.293	0.1	94.7	0.293	0.1	94.7
	1000	0.293	0.0	95.8	0.293	0.0	95.8
Direct effect (0.143)	50	0.153	6.7	95.5	0.137	−4.1	95.3
	100	0.157	10.0	95.3	0.142	−0.6	95.1
	200	0.158	10.3	95.3	0.143	0.1	95.1
	300	0.160	12.1	94.5	0.146	1.9	94.3
	500	0.159	10.9	94.3	0.144	0.8	94.6
	1000	0.157	9.9	94.6	0.143	−0.3	95.6
Indirect effect (0.150)	50	0.135	−10.1	92.1	0.150	0.2	93.5
	100	0.137	−8.5	93.7	0.152	1.5	94.8
	200	0.135	−10.1	92.6	0.149	−0.4	94.7
	300	0.135	−9.6	92.6	0.150	0.1	94.7
	500	0.135	−10.2	91.9	0.149	−0.6	95.1
	1000	0.136	−9.5	89.4	0.150	0.2	95.5

Note: Reliabilities for X, M, and Y are, respectively, 0.73, 0.86, and 0.84 in example 1 and 1.00, 0.91, and 0.82 in example 2.

Table 5.

Mean Estimated Effect (“Mean”), Mean Bias Percentage (“Bias%”), and 95% Confidence Interval (CI) Coverage in the Simulation for Example 3 (True Effect Being Estimated in Parentheses).

		OLS			EIV		
	n	Mean	% Bias	95% CI Coverage	Mean	%Bias	95% CI Coverage
Total Effect of X (0.210)	50	0.159	−24.7	85.7	0.211	0.3	93.9
	100	0.159	−24.7	75.3	0.212	0.4	94.1
	200	0.158	−25.0	55.1	0.211	0.0	94.1
	300	0.158	−25.0	38.8	0.211	0.0	94.6
	500	0.158	−25.1	16.7	0.210	−0.1	94.2
	1000	0.158	−25.0	1.6	0.211	0.0	94.1
Direct Effect of X (0.075)	50	0.070	−6.5	95.7	0.075	−0.3	93.6
	100	0.070	−6.4	95.0	0.076	0.9	93.9
	200	0.070	−7.3	94.6	0.075	−0.1	93.5
	300	0.069	−7.6	93.7	0.075	−0.4	93.3
	500	0.069	−7.7	93.3	0.075	−0.4	93.9
	1000	0.069	−7.6	92.1	0.075	−0.4	93.9
Total Indirect Effect of X (0.136)	50	0.089	−35.8	76.0	0.137	0.7	95.8
	100	0.088	−34.9	59.7	0.136	0.1	95.2
	200	0.089	−34.7	33.9	0.136	0.1	94.7
	300	0.089	−34.7	17.1	0.136	0.1	95.3
	500	0.089	−34.7	4.1	0.136	0.0	95.4
	1000	0.089	−34.6	0.1	0.136	0.1	95.1
Indirect via M_1 (0.099)	50	0.067	−31.5	81.1	0.099	0.9	94.2
	100	0.067	−31.6	72.6	0.099	0.2	94.1
	200	0.067	−31.6	53.4	0.099	0.1	94.6
	300	0.067	−31.7	38.4	0.098	0.0	94.8
	500	0.067	−31.5	17.9	0.099	0.2	94.6
	1000	0.067	−31.6	1.8	0.099	0.1	95.0
Indirect via M_2 (0.037)	50	0.021	−43.4	81.4	0.037	0.0	94.4
	100	0.021	−43.4	73.3	0.037	−0.5	94.1
	200	0.021	−42.9	58.8	0.037	0.0	95.1
	300	0.021	−42.6	46.9	0.038	0.5	95.2
	500	0.021	−43.2	25.4	0.037	−0.3	95.0
	1000	0.021	−42.6	5.0	0.037	0.3	95.3
Difference Between Indirect Effects (0.061)	50	0.046	−24.1	92.1	0.062	1.5	96.8
	100	0.046	−24.4	89.8	0.062	0.7	95.5
	200	0.046	−24.7	86.2	0.061	0.2	95.2
	300	0.046	−25.0	82.1	0.061	−0.3	95.6
	500	0.046	−24.5	73.9	0.061	0.5	95.4
	1000	0.046	−24.9	54.3	0.061	0.0	95.3

Note: Reliabilities for X, M_1, M_2, and Y are 0.75, 0.89, 0.73, and 0.91, respectively.

Table 6.

Differences and similarities in the properties of OLS, EIV regression, and the SILV approach in SEM

	OLS regression	EIV regression (as implemented in PROCESS v5)	SILV SEM approach
Bias in unstandardized effects due to unaccounted-for random measurement error	Yes, when one or more variables on the right-hand side of an equation contains measurement error	No	No
Accounts for random measurement error in…			
…independent variable (*X*)	No	Yes, when provided by the user	Yes, if so-programmed by the user
…mediator(s) (*M*)	No	Yes, when provided by the user and on the right-hand side of a model equation.	Yes, if so-programmed by the user
…outcome (*Y*)	No	No	Yes, if so-programmed by the user
Relative size of model R^2 (when there is a difference)	Smallest	Between OLS and SILV	Largest
Available in PROCESS	Yes	Yes	No. Requires SEM software.

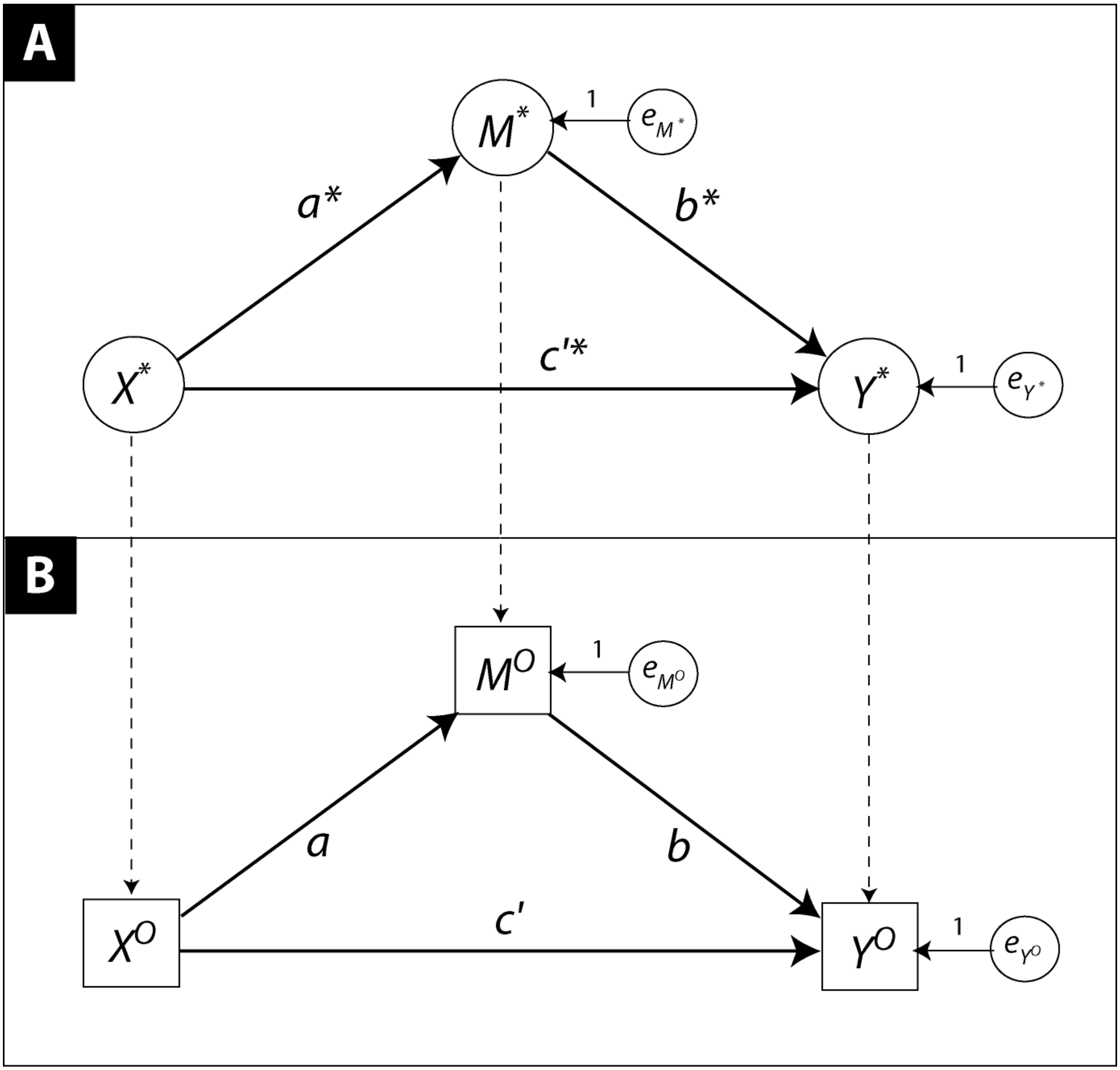

Figure 1. *A mediation model researchers typically want to estimate (panel A) versus what they typically estimate (panel B). See equations 1 through 4. Dashed arrows reflect the measurement assumption that true scores (* superscripts) affect observed scores (O superscripts).*

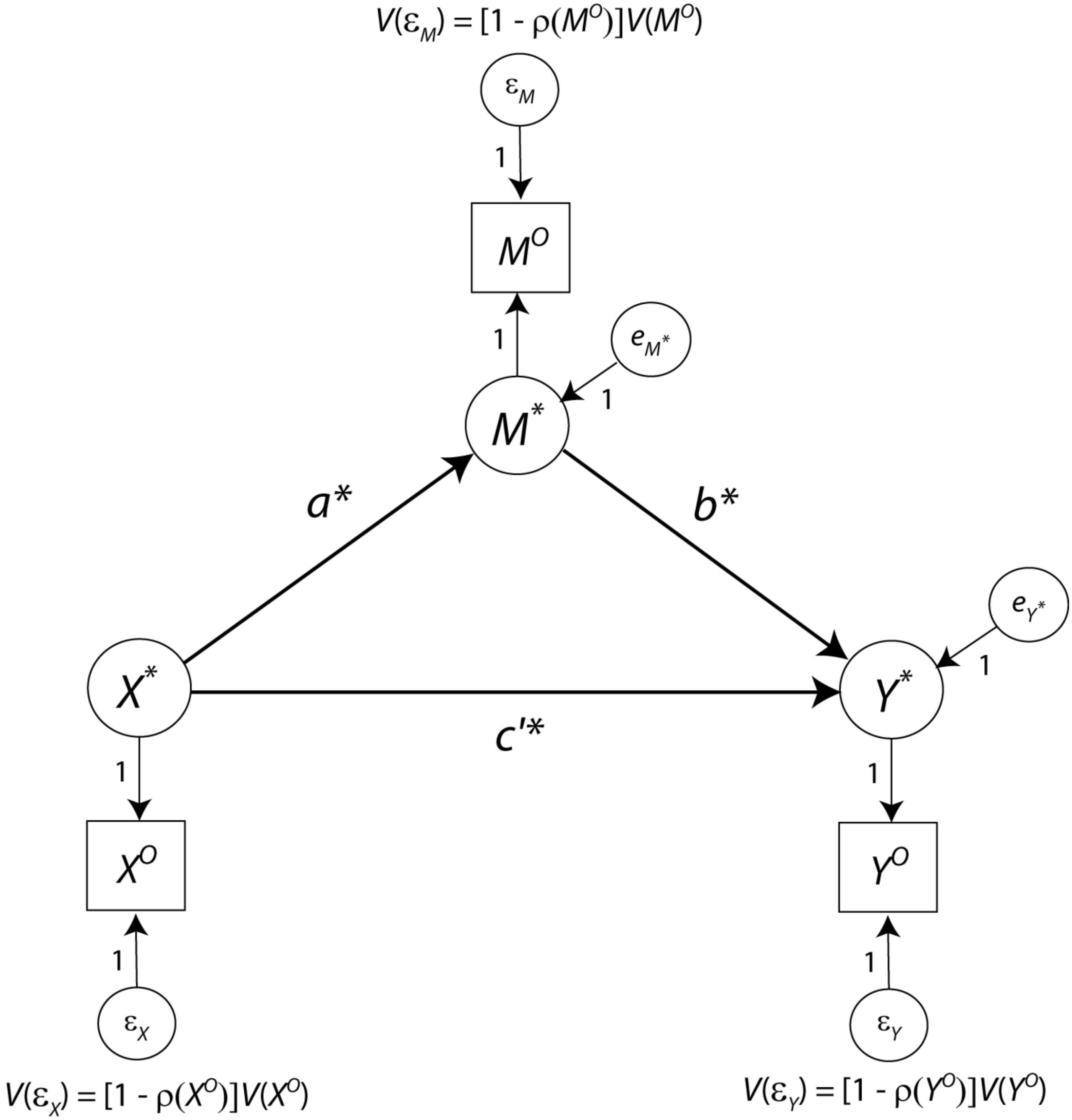

*Figure 2. A single indicator latent variable (SILV) mediation model with one mediator in the notation used throughout this paper. Variables with * superscripts are latent variables. Variables with O superscripts are observed variables. V = variance.*

Appendix A

Errors-in-Variables Regression Computations

The errors-in-variables regression analysis algorithms we use in our examples, the simulation, and that are implemented in PROCESS are motivated by computations done by Stata, which we report below using similar notation found in StataCorp (2023, pp. 594-600).

Let **X** be an $n \times (k+1)$ matrix containing the observed data from n observations for k variables on the right-hand side of a regression model, with the last column containing all ones for the regression constant. Let **y** be an $n \times 1$ vector of observed measurements of Y, the variable on the left side of the regression equation. And let **E** be a $(k+1)$ diagonal matrix with the jth diagonal element set to $n[1-\rho(x_j)]V(x_j)$ where $\rho(x_j)$ and $V(x_j)$ are the reliability and variance, respectively, of the observed data in the jth column of **X**. And, let $\mathbf{D} = \mathbf{X}'\mathbf{X} - \mathbf{E}$. The elements of $\mathbf{b} = \mathbf{D}^{-1}\mathbf{X}'\mathbf{y}$ are the EIV estimates of the regression coefficients for the k variables in **X** in the model of Y, with the last entry being the regression constant. The mean squared error for the EIV model is $s^2 = (\mathbf{y}'\mathbf{y} - \mathbf{b}'\mathbf{D}^{-1}\mathbf{b})/(n-k-1)$ and the squared multiple correlation is $1 - [(n-k-1)s^2/SS_{total})]$ where SS_{total} is the total sum of squares calculated in the usual way in regression analysis.

The variance-covariance matrix of **b** is needed for inference about regression coefficients. The simplest and traditional approach to constructing the variance-covariance matrix of **b** is

$$s^2\mathbf{D}^{-1}\mathbf{X}'\mathbf{X}\mathbf{D}^{-1}. \qquad \text{(A1)}$$

The standard errors for the EIV regression coefficients are the square root of the diagonal elements of the resulting matrix generated by A1 and will be identical to the OLS standard errors when the reliabilities of all the variables on the right-hand side of the equation are equal to 1. This method is implemented in PROCESS and is requested by adding **eiv=5** to the PROCESS command. This method is also available in Stata prior to version 15 and further described in Lockwood and McCaffrey (2000), who show analytically as well as through simulation that this approach can produce standard errors that are too small in some circumstances. Nevertheless, this is the approach to estimating the variance-covariance matrix of **b** we used in our simulations and example analyses, as it shares the homoskedasticity assumption with OLS standard errors as well as the ML standard errors that the SILV approach produces.

Two alternatives implemented in PROCESS relax the homoskedasticity assumption. Define

$$\boldsymbol{e_i x_{ij}} + (\boldsymbol{x_{ij}} - \boldsymbol{\overline{x}_j})^{\mathbf{2}}[\mathbf{1} - \boldsymbol{\rho}(\boldsymbol{x_j})]\boldsymbol{b_j} \quad \text{(A2)}$$

as the element in the *i*th row and *j*th column of an $n \times (k+1)$ matrix H where *i* is the observation in the *i*th row in X, *j* is the variable in the *j*th column of X, and $e_i = y_i - \boldsymbol{x}_i\mathbf{b}$. The variance-covariance matrix of **b** is estimated as

$$\mathbf{D^{-1}H'HD^{-1}} \quad \text{(A3)}$$

(Stefanski & Boos, 2002, Buonaccorsi, 2010, Fuller, 1987, as cited in StataCorp, 2023) and the square root of the diagonal elements in this resulting $(k+1) \times (k+1)$ matrix are the standard errors of the regression coefficients in **b**. Note that the standard errors estimated in this manner do *not* converge to the OLS standard errors as the reliability of observed data in columns of **X** converge to 1. This is because this approach to EIV standard error estimation includes a robustification component to offset the effects of heteroskedasticity of unknown form. When reliabilities are all set to 1, expressions A2 and A3 generate heteroskedasticity-consistent standard errors equivalent to HC0 described in Long and Ervin (2000) and Hayes and Cai (2007). Regardless of the setting for reliabilities in the EIV routine, this method, when requested using option **eiv=0** in the PROCESS command, will produce standard errors equivalent to those generated by Stata as of version 15.

But the HC0 correction for heteroskedasticity can produce standard errors that are too small in smaller samples (see Long & Ervin, 2000), resulting in elevated Type I errors and lower confidence interval coverage than the nominal level. PROCESS also implement a different EIV variance-covariance matrix estimator for **b** with an alternative heteroskedasticity correction that yields standard errors equivalent to HC3 (MacKinnon and White, 1985) when all reliabilities are set to 1. Research shows in the perfect reliability case that the HC3 estimator performs better in smaller samples than does HC0 (Long & Ervin, 2000). Defining h_i as case *i*'s leverage, which is the *i*th diagonal element of $\mathbf{X(X'X)^{-1}X}$, the *i*th row and *j*th column of **H** are set to

$$(e_i x_{ij})/(1 - h_i) + (x_{ij} - \bar{x}_j)^2[1 - \rho(x_j)]b_j \quad \text{(A4)}$$

Expression A3 then generates the variance-covariance matrix of **b**, the diagonals of which are the squared standard errors for the regression coefficients in **b**. This is the default variance-covariance estimator of **b** used by PROCESS's EIV regression routine (or can be explicitly specified using **eiv=3** in the PROCESS command). This approach is not available in any other software other than PROCESS as of the writing of this manuscript.

Appendix B

SPSS, SAS, and R Code for the Three Example Mediation Analyses

This appendix provides the code to conduct the analyses reported in Tables 1-3 using the PROCESS macro's regular OLS (listed first) and EIV (listed second) routines as well as, for R, using the single indicator latent variable approach with the lavaan package. The data are available at https://osf.io/x8we5/. The EIV routine in PROCESS is available as of version 5, released in 2025 at https://www.processmacro.org/

SPSS

Example 1: Compassion Fatigue and Mindset

```
process y=fatigue/x=compass/m=efatigue/model=4/total=1/seed=24080.

process y=fatigue/x=compass/m=efatigue/model=4/total=1/seed=24080
   /relx=0.73/relm=0.86/eiv=5.
```

Example 2: Nature and Self-Actualization

```
process y=authcity/x=natcond/m=paffect/model=4/total=1/seed=27654.

process y=authcity/x=natcond/m=paffect/model=4/total=1/seed=27654
   /relm=0.91/eiv=5.
```

Example 3: Photo-Editing and Self-Perceived Attractiveness

```
process y=spa/x=pes/m=sobbs pacs/model=4/total=1/seed=7234/contrast=1.

process y=spa/x=pes/m=sobbs pacs/model=4/total=1/seed=7234/relx=0.75
   /relm=0.89,0.73/contrast=1/eiv=5.
```

SAS

Example 1: Compassion Fatigue and Mindset

```
%process(data=compfat,y=fatigue,x=compass,m=efatigue,model=4,total=1,
   seed=24080)

%process(data=compfat,y=fatigue,x=compass,m=efatigue,model=4,total=1,
   seed=24080,relx=0.73,relm=0.86,eiv=5)
```

Example 2: Nature and Self-Actualization

```
%process (data=nature,y=authcity,x=natcond,m=paffect,model=4,total=1,
   seed=27654)

%process (data=nature,y=authcity,x=natcond,m=paffect,model=4,total=1,
  seed=27654,relm=0.91,eiv=5)
```

Example 3: Photo-Editing and Self-Perceived Attractiveness

```
%process (data=photo,y=spa,x=pes,m=sobbs pacs,model=4,total=1,seed=7234,
   contrast=1)

%process (data=photo,y=spa,x=pes,m=sobbs pacs,model=4,total=1,seed=7234,
   relx=0.75,relm=0.89 0.73,contrast=1,eiv=5)
```

R

Example 1: Compassion Fatigue and Mindset

```
process(data=compfat,y="fatigue",x="compass",m="efatigue",model=4,total=1,
   seed=24080)

process(data=compfat,y="fatigue",x="compass",m="efatigue",model=4,total=1,
   seed=24080,relx=0.73,relm=0.86,eiv=5)

compfat<-read.table("compfat.csv", sep=",",header=TRUE)
library(lavaan)
model.silv<-"Lfatigue=~fatigue
             Lcompass=~compass
             Lefatigue=~efatigue
             Lefatigue~i1*1+a*Lcompass
             Lfatigue~i2*1+b*Lefatigue+cp*Lcompass
             fatigue~0
             compass~0
             efatigue~0
             Lcompass~1
             ab := a*b
             c :=a*b+cp
             #(1-reliability) multiplied by observed variances
             compass~~((1-0.73)*0.912239175726553)*compass
             fatigue~~((1-0.84)*0.879926974491307)*fatigue
             efatigue~~((1-0.86)*0.804416166081476)*efatigue"
modelp<-sem(model.silv,data=compfat)
summary(modelp,rsquare=T)
set.seed(24080)
modelp<-sem(model.silv,data=compfat,se="bootstrap",bootstrap=5000)
parameterestimates(modelp,boot.ci.type="perc")
```

Example 2: Nature and Self-Actualization

```
process(nature,y="authcity",x="natcond",m="paffect",model=4,total=1,
   seed=27654)

process(nature,y="authcity",x="natcond",m="paffect",model=4,total=1,
  seed=27654,relm=0.91,eiv=5)

nature<-read.table("nature.csv", sep=",",header=TRUE)
library(lavaan)
model.silv<-"Lauthcity=~authcity
             Lpaffect=~paffect
             Lpaffect~i1*1+a*natcond
             Lauthcity~i2*1+b*Lpaffect+cp*natcond
             authcity~0
             paffect~0
             ab :=a*b
             c := a*b+cp
             #(1-reliability) multiplied by observed variances
             authcity~~((1-0.82)*0.795094217075379)*authcity
             paffect~~((1-0.91)*0.365603035685468)*paffect"
modelp<-sem(model.silv,data=nature)
summary(modelp,rsquare=T)
set.seed(27654)
modelp<-sem(model.silv,data=nature,se="bootstrap",bootstrap=5000)
parameterestimates(modelp,boot.ci.type="perc")
```

Example 3: Photo-Editing and Self-Perceived Attractiveness

```
process(photo,y="spa",x="pes",m=c("sobbs","pacs"),model=4,total=1,
   seed=7234,contrast=1)

process(photo,y="spa",x="pes",m=c("sobbs","pacs"),model=4,total=1,
   seed=7234,relx=0.75,relm=c(0.89,0.73),contrast=1,eiv=5)


photo<-read.table("photo.csv", sep=",",header=TRUE)
library(lavaan)
model.silv<-"Lspa=~spa
             Lpes=~pes
             Lsobbs=~sobbs
             Lpacs=~pacs
             Lsobbs~i1*1+a1*Lpes
             Lpacs~i2*1+a2*Lpes
             Lspa~i3*1+b1*Lsobbs+b2*Lpacs+cp*Lpes
             spa~0
             pes~0
             sobbs~0
             pacs~0
             Lpes~1
             Lsobbs~~Lpacs
             a1b1 :=a1*b1
             a2b2 :=a2*b2
```

```
            c := a1*b1+a2*b2+cp
            totind := a1*b1+a2*b2
            C1 := a1*b1-a2*b2
            #(1-reliability) multiplied by observed variances
            spa~~((1-0.91)*0.160231020753164)*spa
            pes~~((1-0.75)*0.547490624374402)*pes
            pacs~~((1-0.73)*0.590323019774451)*pacs
            sobbs~~((1-0.89)*0.540627166218156)*sobbs"
modelp<-sem(model.silv,data=nature)
summary(modelp,rsquare=T)
set.seed(7234)
modelp<-sem(model.silv,data=photo,se="bootstrap",bootstrap=5000)
parameterestimates(modelp,boot.ci.type="perc")
```

Appendix C

PROCESS Output from a Mediation Analysis for Example 1 using EIV Regression

The PROCESS output below was generated with the PROCESS command provided in Appendix B.

```
*************** PROCESS Procedure for SPSS Version 5.0 **** *************

          Written by Andrew F. Hayes, Ph.D.       www.afhayes.com
    Documentation available in Hayes (2022). www.guilford.com/p/hayes3

**************************************************************************
  Model: 4
      Y: fatigue
      X: compass
      M: efatigue

Sample
Size:  308

Custom
Seed:     24080

**************************************************************************
                     Errors-in-variables regression

OUTCOME VARIABLE:
 efatigue

Model Summary
       R-sq        MSE          F        df1        df2          p
      .0969      .7289    23.9562     1.0000   306.0000      .0000

Model
              coeff         se          t          p       LLCI       ULCI
constant     4.6305      .2803    16.5179      .0000     4.0789     5.1821
compass      -.3421      .0699    -4.8945      .0000     -.4796     -.2045

**************************************************************************
                     Errors-in-variables regression

OUTCOME VARIABLE:
 fatigue

Model Summary
       R-sq        MSE          F        df1        df2          p
      .2926      .6265    54.7496     2.0000   305.0000      .0000

Model
              coeff         se          t          p       LLCI       ULCI
constant     2.9162      .4131     7.0596      .0000     2.1033     3.7290
compass      -.1779      .0705    -2.5246      .0121     -.3165     -.0392
efatigue      .5290      .0640     8.2708      .0000      .4031      .6548
```

```
************************* TOTAL EFFECT MODEL ***************************
                    Errors-in-variables regression

OUTCOME VARIABLE:
 fatigue

Model Summary
      R-sq       MSE          F        df1        df2          p
     .0974     .7968    24.1138     1.0000   306.0000      .0000

Model
             coeff         se          t          p       LLCI       ULCI
constant    5.3655      .2931    18.3061      .0000     4.7888     5.9423
compass     -.3588      .0731    -4.9106      .0000     -.5026     -.2150


************** TOTAL, DIRECT, AND INDIRECT EFFECTS OF X ON Y **************

Total effect of X on Y
     Effect         se          t          p       LLCI       ULCI
     -.3588      .0731    -4.9106      .0000     -.5026     -.2150

Direct effect of X on Y
     Effect         se          t          p       LLCI       ULCI
     -.1779      .0705    -2.5246      .0121     -.3165     -.0392

Indirect effect(s) of X on Y:
             Effect     BootSE   BootLLCI   BootULCI
efatigue     -.1809      .0514     -.2922     -.0926

********************** ANALYSIS NOTES AND ERRORS ************************

Level of confidence for all confidence intervals in output:
  95.0000

Number of bootstrap samples for percentile bootstrap confidence intervals:
  5000

NOTE: This errors-in-variables analysis assumes the following reliabilities:
  compass efatigue
    .7300    .8600
```