**ANCOVA versus Change Score for the Analysis of Nonexperimental Two-Wave Data:
A Structural Modeling Perspective**

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Abstract

There is an ongoing debate on whether the analysis of covariance (ANCOVA) or the change score approach is more appropriate when analyzing nonexperimental pre-post designs. In this article, we use a structural modeling perspective to clarify the different assumptions that are made by the ANCOVA and the change score approaches to identify the causal effect of a treatment variable. We show that the change score approach offers the option of controlling for unobserved confounders but relies on strong assumptions about the effects of these unobserved confounders and does not allow for dynamic causal relationships. By contrast, the ANCOVA approach is based on a selection-on-observables approach and assumes that all relevant confounders are measured. Furthermore, we illustrate conditions under which the two approaches give lower and upper bounds of the true treatment effect, and we discuss the role of measurement error. Implications for the analysis of nonexperimental two-wave data are discussed.

Keywords: causal effects, ANCOVA, change scores, longitudinal data, observational data

ANCOVA versus Change Score for the Analysis of Nonexperimental Two-Wave Data: A Structural Modeling Perspective

Assessing the effects of an intervention or construct on change in another construct is a central goal in virtually all areas of psychology. One popular strategy in nonexperimental research is to use a pre-post design in order to estimate how change in a construct is related to a particular treatment or construct (Little, 2013; Reichardt, 2019). For example, differential and developmental researchers are interested in how the occurrence of particular life events (e.g., job loss, marriage) affects changes in well-being or personality. In educational psychology, for example, researchers investigate how teacher characteristics (e.g., competence, motivation) predict changes in students' achievement. Most researchers rely on one of two basic strategies when analyzing the prospective effects of one construct on another construct in the context of two-wave data (Werts & Linn, 1970). In the analysis of covariance (ANCOVA) or conditioning approach, the posttest (Y_2) is conditioned on the pretest (Y_1) by regressing the posttest on the pretest and the treatment variable (T). In the change score approach, the difference between the Time 2 and Time 1 scores ($Y_2 - Y_1$) is regressed on the treatment variable T . As pointed out by many scholars, for a given application, it can be challenging to decide which of the two approaches is more appropriate (e.g., Köhler, Hartig, & Schmid, 2020; Maris, 1998; O'Neill, Kreif, Grieve, Sutton, & Sekhon, 2016; van Breukelen, 2013). However, the decision is often critical as the two approaches can yield results that substantially differ concerning the magnitude, sign, and statistical significance of the estimated treatment effect (Lord, 1967; Wright, 2006).

In the methodological literature there is an ongoing debate about whether the ANCOVA approach or the change score approach should be preferred for analyzing nonexperimental pre-post designs (e.g., Castro-Schilo & Grimm, 2018; Eriksson & Häggström, 2014; Farmus, Arpin-Cribbie, & Cribbie, 2019; Gollwitzer, Christ, & Lemmer, 2014; Stoolmiller & Bank, 1995; Xiao, Higgins, & Kasim, 2019). One frequently made

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argument is that—from a descriptive perspective—the two approaches address different research questions. In the ANCOVA approach, for example, the focus is on whether the occurrence of a life event predicts well-being at Time 2 after controlling for the initial level in well-being at Time 1. By contrast, the change score approach estimates whether interindividual differences in change in well-being are associated with the occurrence of a life event. However, we suspect that psychological theories rarely provide researchers with enough guidance on how to differentiate between these two research approaches in a given application. In the present article, we take an alternative view and adopt a causal inference perspective on this issue (Glymour, Weuve, Berkman, Kawachi, & Robins, 2005; Holland & Rubin, 1983; Kenny, 1975; Kim & Steiner, 2019; Maris, 1998; Steyer, 2005). Our main argument is that from a causal inference perspective, the two approaches address the same question (i.e., estimating the causal effect of the treatment) but rely on different—unfortunately untestable—assumptions about the effects of confounding variables.

More specifically, we use a structural modeling perspective (Pearl, Glymour, & Jewell, 2016; see Rohrer, 2018, for a recent introduction, and Duncan, 1975, for an early treatment) to discuss the causal assumptions of the ANCOVA and change score approaches. We show that the ANCOVA approach is based on a selection-on-observables approach and assumes that all relevant confounder variables are measured and included in the regression model. By contrast, the change score approach offers the option of controlling for unobserved confounders but relies on strong assumptions about the effects of these unobserved confounders and does not allow for dynamic causal relationships (i.e., past outcomes affect current treatment). By clarifying the causal assumptions of these two widely used approaches, we hope to support researchers in utilizing the strength of longitudinal data and, at the same time, we hope to raise the awareness of the strong assumptions that are needed for making causal conclusions. This is also in line with recent prominent calls in psychology that nonexperimental research should begin to talk more openly about causal assumptions and

causal effects because causal questions are central to most psychological theories (e.g., Brick & Bailey, 2020; Foster, 2010; Grosz, Rohrer, & Thoemmes, 2020; Quynh Nguyen, Schmid, & Stuart, 2020).

Before we start, we would like to point out that we are not concerned with the problem of measuring change at the individual level. Many different estimators have been proposed for quantifying change at the individual level (e.g., gain score, residualized gain), and there is a longstanding debate about which of these estimators provide the most accurate estimate of individual change (e.g., Lord, 1956; Cronbach & Furby, 1970). As we hope to clarify, the accurate measurement of change at the individual level is distinct from the estimation of the treatment effect in a pre-post design (Campbell & Kenny, 1999; Nicewander & Price, 1978; for a comprehensive comparison of different estimators of individual change see Castellano & McCaffrey, 2020).

A Structural Model for Two-Wave Data

In the following, we introduce a structural model for two-wave data that represents the causal relationships between the variables and allows us to clearly state the causal assumptions that are made by the two different approaches (Allison, 1990; Kenny, 1975; Kim & Steiner, 2019). We assume that an outcome (e.g., well-being) is measured at two measurement waves and is denoted as Y_1 and Y_2 . Our main goal is to estimate the effect of a treatment T (e.g., the occurrence of a life event) on the outcome Y_2 . Furthermore, we assume that a confounding variable U (e.g., physical health) that affects both the outcomes (Y_1 and Y_2) and the treatment is present (see also Figure 1):

$$Y_1 = \alpha_1 U + \varepsilon_1$$

$$T = \alpha_T U + \delta Y_1 + \varepsilon_2$$

$$Y_2 = \alpha_2 U + \beta Y_1 + \gamma T + \varepsilon_3. \tag{1}$$

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In the interests of simplicity and transparency, we assume that all effects are linear and that the variables are standardized (i.e., mean-centered with unit variance). We also assume that the variables are measured without error but, in a later section, we address how measurement error affects our conclusions.

Figure 1

Structural Model for Two-Wave Data: Effect of Treatment T on Outcome Y_2 With Effects of the Past Outcome Y_1 and a Confounder U . The Error Terms are not Depicted (see Equation 1)

Four remarks about the data-generating model in Equation 1 seem to be in order. First, we assume that the treatment T temporally precedes the outcome Y_2 . In real applications, assumptions about the temporal ordering of treatment and outcome can be challenging with two-wave data. If T is measured at the same time as Y_2 , it is often difficult to rule out reverse causation as Y_2 could also have affected T . If a researcher is, for example, interested in the effect of motivation on achievement in a school subject (e.g., mathematics), and both motivation and achievement are assessed at Time 2, it needs to be ruled out that achievement has affected motivation (Marsh, Trautwein, Lüdtke, Köller, & Baumert, 2005). If, instead, T and the past outcome Y_1 are assessed at the same time, it can be difficult to determine whether Y_1 is affected by T . In this case, Y_1 would not be a confounder but a mediator on the causal

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pathway from T to Y_2 , and controlling for Y_1 would block some of the effect of T on Y_2 . Thus, in some settings, three waves of data would need to be available to distinguish between confounding and mediation effects. With three waves, it would be possible to establish temporal ordering by measuring the pretest at Time 1, the treatment at Time 2, and the outcome/posttest at Time 3 (for a further discussion see VanderWeele, Jackson, & Li, 2016; VanderWeele, Mathur, & Chen, 2018). Second, we are interested in the effect of the treatment at a single point in time. In many settings, the treatment also varies across time. For example, when assessing the effect of teachers' burnout on student achievement, teachers' burnout changes across time. With several waves of data, it would be possible to assess the effects of a treatment trajectory on final outcomes. However, the assumptions made when assessing the joint effect of a treatment over a series of time points are more complex (Hernán & Robins, 2020; Robins, Hernán, & Brumback, 2000; see also Daniel et al., 2013). Third, other data-generating models could be proposed for two-wave data; there are many ways to address the issue of the estimation of treatment effects with longitudinal data. Later (see "Alternative Structural Model Using Transient and Stable Components" section), we show that an alternative data-generating model that has been suggested in the literature (Kenny, 1975) can be considered to be a special case of the model in Equation 1. Fourth, in order to simplify our discussion of the biases of the ANCOVA and change score approaches, we assume that the effects in the data-generating model are not negative, even though the formulas we provide also include conditions in which some of the effects are negative.

Bias of ANCOVA and Change Score Estimators

We now discuss the conditions under which the ANCOVA and change score approaches provide unbiased estimators of the treatment effect, under the assumption that the model in Equation 1 is true.

ANCOVA Approach

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For the ANCOVA approach, a regression model is specified that estimates the effect of the treatment T on an outcome Y_2 measured at a later time point while controlling for the initial values of the outcome Y_1 :

$$Y_2 = b_0 + \gamma_{\text{ANCOVA}}T + b_1Y_1 + \varepsilon, \quad (2)$$

where b_0 is the regression intercept and the ANCOVA estimator γ_{ANCOVA} is the regression coefficient of the regression of Y_2 on T and Y_1 . Thus, the ANCOVA approach controls for preexisting differences in the outcome when assessing the effect of the treatment. It can be considered to be a special case of a more general class of conditioning methods (e.g., matching methods) in which the causal effect is obtained by conditioning on the pretest and other observed covariates (see Morgan & Winship, 2015). The basic idea is that the pretest is sufficient to control for the confounding effect of unmeasured covariates U . Notice that in the ANCOVA approach, the pretest has the same status as any other covariate. Under the assumption that the data-generating model in Equation 1 holds, the bias of the ANCOVA estimator is given by (see Appendix A):

$$\gamma_{\text{ANCOVA}} = \gamma + \frac{\alpha_2\alpha_T(1-\alpha_1^2)}{1-2\delta\alpha_1\alpha_T-\delta^2-\alpha_1^2\alpha_T^2}. \quad (3)$$

The ANCOVA approach produces an unbiased estimate of the treatment effect if, conditional on Y_1 , the unobserved covariate U does not affect the treatment (i.e., $\alpha_T = 0$) or the outcome Y_2 (i.e., $\alpha_2 = 0$). Another view of the ANCOVA approach is that it uses the past outcome and other observed covariates as a proxy for the unobserved confounder (Kim & Steiner, 2019). It needs to be emphasized that the ANCOVA estimator is unbiased when the confounder U is measured and included in the regression in Equation 2. Thus, the selection of covariates is crucial in the ANCOVA approach. Many scholars have pointed out that the pretest is the single most important covariate in many settings (e.g., Steiner, Cook, Shadish, & Clark, 2010; VanderWeele, 2019).

Change Score Approach

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In the change score approach, the difference between the Time 2 and Time 1 scores is regressed on the treatment variable:

$$Y_2 - Y_1 = b_0 + \gamma_{\text{change}}T + \varepsilon, \quad (4)$$

where γ_{change} indicates the difference in change in the outcome between the treatment and control groups. Even though the two approaches use the same variables in the regression model, three differences between the change score and ANCOVA approaches need to be emphasized. First, it is a necessary precondition for calculating the difference score that the measures at the two waves are operationally identical (i.e., Y_1 and Y_2 need to be in the same metric; Allison, 1990; Plewis, 1985). This is not the case in the ANCOVA approach because the pretest is treated like any other covariate. Second, when comparing the two approaches, it can be instructive to rewrite the regression of the change score approach as $Y_2 = b_0 + 1 \cdot Y_1 + \gamma_{\text{change}}T + \varepsilon$ (by moving Y_1 from the left to the right side of the equation and fixing the regression coefficient to one). This expression emphasizes that the pretest receives a special status in the change score approach by restricting the effect of the pretest to one. It is vital to understand that this does not imply that the change score model is a restricted version of the ANCOVA model because our goal is not to maximize the explained variation in the regression but to control for confounding variables in the estimation of the treatment effect (Angrist & Pischke, 2009).¹ Third, it has been pointed out that both approaches imply different concepts of change and stability (e.g., Castro-Schilo & Grimm, 2018; Maxwell & Delaney, 2004; Newsom, 2015). In the ANCOVA approach, the way in which differences in Y_2 are associated with T is assessed after individuals are equated on the initial values Y_1 . Therefore, this approach has sometimes also been referred to as the conditional change model or residualized change score model. By contrast, the change score approach takes the raw

¹ One may think of including Y_1 as an additional predictor in the change score approach. However, the treatment effect in the regression of $Y_2 - Y_1$ on Y_1 and T is equivalent to γ_{ANCOVA} (Werts & Linn, 1970; see also Kessler & Greenberg, 1981).

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individual change as the outcome variable and assesses how interindividual differences in change are associated with the treatment (unconditional change model). From this perspective, one could argue that the two approaches address different specific research questions and that determining which of the two approaches is more suitable depends on the research context.

However, from a causal inference perspective, we can derive the bias for the change score estimator under the assumption that the structural model in Equation 1 holds:

$$\gamma_{\text{change}} = \gamma + \alpha_T(\alpha_2 + \beta\alpha_1 - \alpha_1) + \delta(\beta + \alpha_1\alpha_2 - 1). \quad (5)$$

Thus, in the presence of an unobserved confounder U , two assumptions need to be met to ensure that the change score estimator is unbiased. First, the effect of the unobserved confounder U needs to fulfill a very specific constraint, that is, $\alpha_2 + \beta\alpha_1 = \alpha_1$, so that the second term in Equation 5 is zero. This constraint essentially means that the effects of the confounder variable U are stable across time. In the literature, this assumption is also known as the common trend assumption because it implies that, with a dichotomous treatment variable (treatment vs. control group), the unobservable trend of the outcome for the treatment group under the control condition is parallel to the observed trend for the control group (Lechner, 2010). Second, it needs to be assumed that the treatment is not affected by the past outcome Y_1 (i.e., $\delta = 0$). This assumption has also been referred to as the assumption of “no dynamic causal relationships between the outcome and treatment variables” (Imai & Kim, 2019, p. 477).

The main advantage of the change score approach is that it has the potential to control for the effect of unobserved confounder variables. This is attractive when researchers work with administrative data, which often only include standard demographic variables (e.g., age, sex, race, marital status) that have a limited potential to model the selection process for the treatment. However, the common trend assumption is very restrictive and often difficult to

defend (Angrist & Pischke, 2009; Reichardt, 2019), even though the question of whether including covariates in the change score approach can increase the plausibility of the common trend assumption has been discussed in the literature (e.g., Lindner & McConnell, 2019). We believe that the assumption of no dynamic causal relationships is even more crucial in many applications in psychological research as the occurrence or level of a current treatment often depends on previous levels of the outcome (e.g., the previous achievement affects the level of motivation, previous well-being affects the occurrence of a particular life event). In this case, it can be shown that even if the confounder U is observed and included in Equation 2, the change score approach will, in general, produce biased estimates of the causal effect:

$$\gamma_{\text{change},U} = \gamma + \frac{\delta(1-\beta)(1-\alpha_1^2)}{1-2\delta\alpha_1\alpha_T-\alpha_T^2-\delta^2\alpha_1^2} . \quad (6)$$

As can be seen, when the previous outcome has an effect on the treatment ($\delta \neq 0$; causal dynamics), the change approach provides distorted estimates. Thus, in the change score approach there has to be a tradeoff between the possibility of controlling for the effects of unobserved confounders (with stable effects across time) and the possibility of taking causal dynamics into account (Imai & Kim, 2019; Kim & Steiner, 2019).

Illustration: Bias of ANCOVA Versus Change Score Estimators

Table 1 further illustrates the differences between the ANCOVA and change score estimators for different scenarios. We assumed that the true treatment effect would be modest in size (i.e., $\gamma = .20$) and that the outcome Y would be moderately stable across time (i.e., $\beta = .50$), but we manipulated the effect of U on Y_2 ($\alpha_2 = 0, .1$, and $.2$) and on the treatment T ($\alpha_T = 0$ and $.3$). We also varied whether the past outcome Y_1 had an effect on the treatment ($\delta = 0$ and $.3$). As expected, the ANCOVA estimator tended to overestimate the true treatment effect and was unbiased under conditions in which U did not have an effect on either Y_2 ($\alpha_2 = 0$) or the treatment ($\alpha_T = 0$). By contrast, the change score estimator was only unbiased if causal

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dynamics were absent ($\delta = 0$) and either U did not affect the treatment ($\alpha_T = 0$) or the common trend assumption was met ($\alpha_2 = .2$ so that $\alpha_2 + \beta\alpha_1 = .2 + .50 \times .40 = .4 = \alpha_1$).

Table 1

Illustration of Bias in the ANCOVA and Change Score Estimators (see Figure 1): Size of the Estimated Treatment Effect as a Function of α_2 , δ , and α_T

α_2	δ	α_T	γ_{ANCOVA}	γ_{change}
0	0	0	.20	.20
0	0	.3	.20	.14
0	.3	0	.20	.05
0	.3	.3	.20	-.01
.1	0	0	.20	.20
.1	0	.3	.23	.17
.1	.3	0	.20	.06
.1	.3	.3	.23	.03
.2	0	0	.20	.20
.2	0	.3	.25	.20
.2	.3	0	.20	.07
.2	.3	.3	.26	.07

Note. It is assumed that the true treatment effect is $\gamma = .20$, the stability of Y is moderate ($\beta = .50$), and the effect of the confounder U on Y_1 is $\alpha_1 = .40$. Unbiased estimates are printed in bold.

Interestingly, the illustration reveals that the ANCOVA and change score estimators fulfill a useful bracketing relationship (Angrist & Pischke, 2009; see also Ding & Li, 2019). Namely, if the assumptions of the ANCOVA approach are met, then mistakenly applying the change score approach will underestimate the treatment effect. By contrast, if the assumptions of the change score approach hold, the ANCOVA approach will overestimate the treatment effect. It can be shown that under mild assumptions about the data-generating model in Equation 1, the ANCOVA estimator provides an upper bound and the change score estimator provides a lower bound for the true treatment effect:

$$\gamma_{\text{change}} \leq \tau \leq \gamma_{\text{ANCOVA}} \quad . \quad (7)$$

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More specifically, it needs to be assumed that the (cumulative) effect of U on the outcome is smaller for the posttest than for the pretest, that is, $(1 - \beta)\alpha_1 - \alpha_2 > 0$, and that $\beta < 1$ (see Appendix A; Angrist & Pischke, 2009). The bracketing property is helpful in practice where one does not know if either the assumptions of the ANCOVA or the change score approach are true. Both approaches can be used to check the robustness of the results against different identifying assumptions for the treatment effect. The extent to which the two approaches converge often increases the credibility of the causal conclusions (Kenny, 1975; Reichardt, 2019).

Alternative Structural Model Using Transient and Stable Components

Our discussion of the conditions under which the ANCOVA and change score approaches give unbiased estimates of the treatment effect was based on a data-generating model that assumed a specific unobserved confounder variable U . However, other data-generating processes for two-wave data are possible and have been proposed in the literature. Kenny (1975) introduced a model that decomposes potential confounding influences in terms of stable and transient components of the outcome variable. As can be seen in Figure 2, this model assumes that interindividual differences in the outcome and treatment are caused by a stable factor U and transient factors Z_1 and Z_2 . An interesting feature of this model is that it does not specify a direct effect of Y_1 on Y_2 but assumes that the stability in the outcome across time is due to the components U , Z_1 , and Z_2 . A very similar model was also used by Allison (1990; see Model 2) and Köhler et al. (2020) in their discussions of the ANCOVA and change score approaches. It should be emphasized that U , Z_1 , and Z_2 are unmeasured, theoretical components that summarize the effects of stable and transient factors that influence Y_1 , T , and Y_2 .

Using this structural model, Kenny (1975; see also Allison, 1990) showed that the change score estimator is unbiased if the treatment assignment is caused by the stable component and not by the transient component (i.e., $\delta^* = 0$), and if the effects of the stable

components are invariant across time ($\alpha_1^* = \alpha_2^*$). By contrast, the estimation of the ANCOVA approach is unbiased when the treatment assignment is due to the transient component Z_1 but is not influenced by the stable component ($\alpha_T^* = 0$). However, Kenny's model (1975) can be seen as a special case of the structural model with an unobserved confounder U in Equation 1 (see Appendix B). For example, in the case of the change score estimator, the assumption that treatment assignment is only caused by the stable component is equivalent to the assumption of no causal dynamics and common trends in the structural model with an unobserved confounder. We believe that in most applications, it is more convenient and more straightforward for researchers to conceptualize issues of confounding in terms of variables that are—at least in principle—measurable.

Figure 2

Alternative Structural Model That Decomposes the Effects of Unobserved Confounders Into Transient Components Z_1 and Z_2 , and a Stable Component U (Kenny, 1975; see also Allison, 1990). The Error Terms are not Depicted (see Equation B1)

The Role of Measurement Error

In our discussion, we made the simplifying assumption that the variables are measured without error. In practical applications, the assessment of covariates, including the pretest, is often affected by measurement error. It has been pointed out that the presence of measurement

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error can bias the estimates of the ANCOVA approach (Culpepper & Aguinis, 2011; Sengewald, Pohl, & Steiner, 2019). Moreover, one advantage of the change score approach is that it is not sensitive to measurement error in the pretest (e.g., Kim & Steiner, 2019). In the following, we extend the structural model in Equation 1, and we assume that we only have error-prone measures $\tilde{Y}_1$, $\tilde{T}$, and $\tilde{Y}_2$ that are given by $\tilde{Y}_1 = Y_1 + E_1$, $\tilde{T} = T + E_2$, and $\tilde{Y}_2 = Y_2 + E_3$, where E_1 , E_2 , and E_3 are measurement errors with zero mean and variances $\sigma_{E_1}^2$, $\sigma_{E_2}^2$, and $\sigma_{E_3}^2$. The structural model with fallible measures is then represented by the following equations (see Figure 3)²:

$$\begin{aligned}\tilde{Y}_1 &= Y_1 + E_1 = \alpha_1 U + \varepsilon_1 + E_1 \\ \tilde{T} &= T + E_2 = \alpha_T U + \delta Y_1 + \varepsilon_2 + E_2 \\ \tilde{Y}_2 &= Y_2 + E_3 = \alpha_2 U + \beta Y_1 + \gamma T + \varepsilon_3 + E_3 .\end{aligned}\tag{8}$$

In addition, we allow for the possibility that the measurement errors of the pretest and posttest are correlated, that is, $\rho_E = \text{Cov}(E_1, E_3)/(\sigma_{E_1} \sigma_{E_3})$. This is often a reasonable assumption when the same instrument is used as pretest and posttest and when the time lag between the two tests is not too long (Kim & Steiner, 2019; Palmquist & Green, 1992). As we assume that the true variables are standardized, the reliabilities of $\tilde{Y}_1$, $\tilde{T}$, and $\tilde{Y}_2$ are given by $r_1 = 1/(1 + \sigma_{E_1}^2)$, $r_2 = 1/(1 + \sigma_{E_2}^2)$, and $r_3 = 1/(1 + \sigma_{E_3}^2)$.

To obtain consistent estimates of the treatment effect, an errors-in-variables approach (Carroll, Ruppert, Stefanski, & Crainiceanu, 2006; Fuller, 1987; see also Culpepper & Aguinis, 2011; Mayer, Dietzfelbinger, Rosseel, & Steyer, 2016) can be used to adjust for measurement error in the observed variables. In this approach, the variance-covariance matrix of the observed predictor variables is corrected for the reliabilities. Note that the dependent

² Note that in the data-generating model, we assume that selection into the treatment depends on the true pretest Y_1 . In certain designs, individuals are assigned to the treatment according to their observed values on the pretest (Reichardt, 2019). In this case, the observed pretest values should be included in the ANCOVA approach, and correcting for measurement error in $\tilde{Y}_1$ would result in biased estimates of the treatment effect (Sengewald et al., 2019).

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variable can be measured with error because unreliability in the dependent variable does not result in biased estimates of unstandardized regression coefficients (Carroll et al., 2006). More specifically, the variances of the error-prone predictor variables are adjusted by their reliabilities, that is, $r_1 \cdot \text{Var}(\tilde{Y}_1)$ and $r_2 \cdot \text{Var}(\tilde{T})$. Furthermore, under the assumption that the errors are uncorrelated, the covariances between the observed variables provide consistent estimates of the covariances between the true variables (Carroll et al., 2006). Thus, the ANCOVA estimator adjusted for measurement error can be written as:

$$\gamma_{\text{ANCOVA}}^* = \frac{r_1 \text{Var}(\tilde{Y}_1) \text{Cov}(\tilde{Y}_2, \tilde{T}) - \text{Cov}(\tilde{Y}_2, \tilde{Y}_1) \text{Cov}(\tilde{T}, \tilde{Y}_1)}{r_1 \text{Var}(\tilde{Y}_1) r_2 \text{Var}(\tilde{T}) - \text{Cov}(\tilde{T}, \tilde{Y}_1)^2}. \quad (9)$$

Note that it would also be possible to correct the covariances of the observed variables for correlated measurement error. However, as these correlations are difficult to determine, we assume that the presence of correlated measurement errors is ignored in the estimation of the treatment effect.

Figure 3

Structural Model for Two-Wave Data With Measurement Error: $\tilde{Y}_1$, $\tilde{T}$, and $\tilde{Y}_2$ are the Observed Measures That Assess Y_1 , T , and Y_2 With Errors E_1 , E_2 , and E_3 (see Equation 8)

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The main challenge for the application of the errors-in-variables approach is that the reliabilities of the observed variables are required. Usually, the true reliabilities are not known, and researchers need to obtain estimates of the true reliabilities. Let $\lambda_1 = \hat{r}_1/r_1$ and $\lambda_2 = \hat{r}_2/r_2$ denote the ratios of the estimated to the true reliabilities, indicating whether a researcher overestimates (i.e., $\lambda_1 > 1$) or underestimates (i.e., $\lambda_1 < 1$) the true reliability. Then the bias of the error-adjusted ANCOVA estimator can be derived as follows (see Appendix C):

$$\gamma_{\text{ANCOVA}}^* = \gamma + \frac{\alpha_2 \alpha_T (\lambda_1 - \alpha_1^2) + (\lambda_1 - 1)(\beta \delta + \delta \alpha_1 \alpha_2 + \beta \alpha_1 \alpha_T) - (\lambda_2 - 1)\lambda_1 \gamma - \rho_E (\alpha_1 \alpha_T + \delta)}{\lambda_1 \lambda_2 - 2\delta \alpha_1 \alpha_T - \delta^2 - \alpha_1^2 \alpha_T^2}. \quad (10)$$

The following observations can be made. First, if the reliabilities of the pretest and the treatment variables are correctly estimated ($\lambda_1 = \lambda_2 = 1$), and the errors are uncorrelated ($\rho_E = 0$), the bias expression for the unadjusted ANCOVA estimator is obtained (see Equation 3). Second, mistakenly overestimating the reliability of the pretest ($\lambda_1 > 1$), including the extreme case of ignoring measurement error in the pretest, will result, in general, in positively biased estimates of the treatment effect. In this case, the true pretest Y_1 acts as a confounder for the effect of T on Y_2 (see the second term in the nominator of the fraction in Equation 10). Third, overestimating or ignoring the reliability of the treatment variable ($\lambda_2 > 1$) will result in a downward bias on the absolute size of the treatment effect estimates. Interestingly, the size of this bias depends on the reliability of the pretest and is larger or smaller when the reliability of the pretest is overestimated or underestimated, respectively. Fourth, positively correlated errors of the pretest and posttest ($\rho_E > 0$) will introduce additional bias into the estimates of the treatment effect when the unobserved confounder U affects the pretest and the treatment (i.e., $\alpha_1 \neq 0$ and $\alpha_T \neq 0$) or the pretest has an effect on the treatment ($\delta \neq 0$).

For the change score estimator, it is essential that the estimates of the treatment effect are not sensitive to measurement error in the pretest and posttest because measurement errors

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in $\tilde{Y}_1$ and $\tilde{Y}_2$ do not affect estimates of $\text{Cov}(\tilde{Y}_2 - \tilde{Y}_1, \tilde{T})$. However, the measurement error in $\tilde{T}$ needs to be taken into account and the corrected change score estimator γ_{change}^* is calculated as $\text{Cov}(\tilde{Y}_2 - \tilde{Y}_1, \tilde{T})/[r_2 \text{Var}(\tilde{T})]$. Under the assumption of the data-generating model in Equation 8, the bias of γ_{change}^* is given by:

$$\gamma_{\text{change}}^* = \gamma + \frac{\lambda_2 - 1}{\lambda_2} \gamma + \frac{1}{\lambda_2} [\alpha_T(\alpha_2 + \beta\alpha_2 - \alpha_1) + \delta(\beta + \alpha_1\alpha_2 - 1)] . \quad (11)$$

Again, it can be seen that if the reliability of the treatment variable is correctly estimated ($\lambda_2 = 1$), the bias expression for the unadjusted change score estimator is obtained (see Equation 5). Importantly, Equation 11 reveals that the adjusted change score estimator is not attenuated by correlated measurement errors. However, the change score estimator will still be biased if the common trend assumption is not met ($\alpha_2 + \beta\alpha_2 \neq \alpha_1$) or if the pretest affects the treatment ($\delta \neq 0$).

As pointed out, adjusting for error in the predictor variables requires estimates of the reliability to be available (Lockwood & McCaffrey, 2014; Reichardt, 2019). In many applications, researchers use internal consistency measures of reliability (e.g., Cronbach's alpha), or they apply structural equation modeling (SEM; Bollen, 1989) with items as multiple indicators to correct for measurement error in the covariates. However, internal consistency measures of reliability only focus on measurement error that is caused by the finite number of items on the test. It has been argued that other sources of error (e.g., short-term fluctuations of the measure) should also be taken into account when controlling for the effects of error-prone variables (Kane, 2017; see also DeShon, 1998). A full discussion of different estimates of reliability is beyond the scope of the present article (see Brennan, 2001; Revelle & Condon, 2019).

Conclusion

In the present article, we argue that from a causal perspective, the ANCOVA and change score approaches rely on different assumptions when identifying causal effects. We

emphasized that in a given application, both approaches can be wrong and that there is no statistical test that can tell researchers which of the two approaches is more plausible.

Furthermore, we showed that the change score approach offers the option of controlling for the effects of unobserved confounders. However, this comes at the price of a very restrictive assumption about the effects of the unobserved confounder (i.e., common trend assumption). This assumption often does not seem plausible in practice. Also, we showed that even if all relevant confounders are included in the change score approach, it does not allow for dynamic causal relationships and will produce biased estimates of the causal effect as long as the past outcome affects the current treatment. Given these limitations, we tend to prefer the ANCOVA approach for estimating treatment effects in pre-post designs because it provides a clear rationale for including observed covariates in the analysis (VanderWeele, 2019)³.

In practical applications of the ANCOVA approach, the selection of covariates and the correct specification of their effects (e.g., interactions and quadratic effects) is of central importance. Propensity score methods have been recommended as a flexible tool for adjusting for the effects of covariates when estimating treatment effects with nonexperimental data (e.g., Morgan & Winship, 2015; Schafer & Kang, 2008). In these methods, the propensity score (i.e., the conditional probability of treatment given the covariates) is used to balance the distribution of observed covariates between the treatment and control groups, in order to ensure that an estimated treatment effect is not due to differences in observed characteristics between the groups (Austin, 2011; Rosenbaum & Rubin, 1983). A balance of the covariate distributions is achieved by matching, stratifying, or reweighting the observations according to the propensity score (Lunceford & Davidian, 2004). However, it has been pointed out that

³ In their discussion of the unconfoundedness-approach (equivalent to the ANCOVA approach in the present article) and the differences-in-differences approach (DID; equivalent to the change score approach), Imbens and Wooldridge (2009, see p. 70) conclude: “As a practical matter, the DID approach appears less attractive than the unconfoundedness-based approach in the context of panel data. It is difficult to see how making treated and control units comparable on lagged outcomes will make the causal interpretation of their difference less credible, as suggested by the DID assumptions.”

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the choice of a particular method of adjustment is not nearly as important as the set of measured covariates that is available for the adjustment (Reichardt, 2019; Steiner et al., 2010). Including a rich set of reliably measured covariates increases the credibility of the treatment effect estimates that are obtained from the ANCOVA approach. General recommendations for the selection of covariates are discussed by VanderWeele (2019).

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Appendix A: Derivations of Bias for the ANCOVA and Change Score Approaches

In this appendix, we provide bias derivations for the ANCOVA and change score approaches. All variables are assumed to be standardized. The covariances between the observed variables Y_1 , Y_2 , and T are derived as follows: $\text{Cov}(T, Y_1) = \delta + \alpha_1\alpha_T$, $\text{Cov}(Y_2, Y_1) = \beta + \delta\gamma + \alpha_1\alpha_2 + \alpha_1\alpha_T\gamma$; $\text{Cov}(Y_2, T) = \gamma + \beta\delta + \alpha_2\alpha_T + \beta\alpha_1\alpha_T + \delta\alpha_1\alpha_2$. The ANCOVA estimator (without controlling for the unobserved confounder U ; see Equation 2) is given as follows:

$$\begin{aligned} \gamma_{\text{ANCOVA}} &= \frac{\text{Cov}(Y_2, T) - \text{Cov}(Y_2, Y_1)\text{Cov}(T, Y_1)}{1 - \text{Cov}(T, Y_1)^2} = \frac{\gamma + \alpha_2\alpha_T - 2\delta\alpha_1\alpha_T\gamma - \gamma\delta^2 - \alpha_2\alpha_T\alpha_1^2}{1 - 2\delta\alpha_1\alpha_T - \delta^2 - \alpha_1^2\alpha_T^2} \\ &= \gamma + \frac{\alpha_2\alpha_T(1 - \alpha_1^2)}{1 - 2\delta\alpha_1\alpha_T - \delta^2 - \alpha_1^2\alpha_T^2} . \end{aligned} \quad (\text{A1})$$

Furthermore, the change score estimator is given by:

$$\begin{aligned} \gamma_{\text{change}} &= \text{Cov}(Y_2, T) - \text{Cov}(Y_1, T) = \gamma - \delta - \beta\delta + \alpha_1\alpha_2 - \alpha_1\alpha_T + \beta\alpha_1\alpha_T + \delta\alpha_1\alpha_2 \\ &= \gamma + \alpha_T(\alpha_2 + \beta\alpha_1 - \alpha_1) + \delta(\beta + \alpha_1\alpha_2 - 1) . \end{aligned} \quad (\text{A2})$$

Thus, the change score estimator is unbiased if $\delta = 0$ and if $\alpha_2 + \beta\alpha_1 = \alpha_1$ (i.e., the effect of U is stable with respect to Y_1 and Y_2).

Inclusion of an Observed Confounder U

It is evident that the ANCOVA estimator is unbiased if the confounder U is included in the regression in Equation 2. However, it can be shown that the change score estimator is biased even if the confounder U is included in the regression in Equation 3:

$$\begin{aligned} \gamma_{\text{change}, U} &= \frac{\text{Cov}(Y_2 - Y_1, T) - \text{Cov}(Y_2 - Y_1, U)\text{Cov}(T, U)}{1 - \text{Cov}(T, U)^2} = \frac{\gamma - \delta + \beta\delta - 2\delta\alpha_1\alpha_T\gamma + \delta\alpha_1^2 - \gamma\alpha_T^2 - \beta\delta\alpha_1^2 - \gamma\delta^2\alpha_1^2}{1 - 2\delta\alpha_1\alpha_T - \alpha_T^2 - \delta^2\alpha_1^2} \\ &= \gamma + \frac{\delta(1 - \beta)(1 - \alpha_1^2)}{1 - 2\delta\alpha_1\alpha_T - \alpha_T^2 - \delta^2\alpha_1^2} . \end{aligned} \quad (\text{A3})$$

In this case, the change score estimator would be unbiased if $\delta = 0$ (i.e., past outcome Y_1 does not affect the treatment).

Bracketing Property

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We will show that under mild assumptions about the data-generating model (see Figure 1), the ANCOVA and change score estimators fulfill a bracketing property. This means that the two estimators provide upper and lower bounds for the true treatment effect γ (Angrist & Pischke, 2009; Ding & Li, 2019). First, we assume that the ANCOVA estimator is positively biased (see Equation A1) because it is often reasonable to assume that $\alpha_1\alpha_T$ is larger than zero. Thus, γ_{ANCOVA} can be considered to be an upper bound of γ . Next, we consider the change score estimator and rewrite the bias of γ_{change} as:

$$\gamma_{\text{change}} = \gamma - \alpha_T((1 - \beta)\alpha_1 - \alpha_2) - \delta(1 - \beta - \alpha_1\alpha_2). \quad (\text{A4})$$

We now make the plausible assumption that the cumulative influence of the unobserved confounder U is smaller for the posttest than for the pretest:

$$(1 - \beta)\alpha_1 - \alpha_2 > 0. \quad (\text{A5})$$

Note that the common trend assumption (which is a necessary assumption for the unbiasedness of γ_{change}) is $(1 - \beta)\alpha_1 - \alpha_2 = 0$. Furthermore, given the assumption in Equation A5, it is also plausible to assume that:

$$1 - \beta - \alpha_1\alpha_2 > 0. \quad (\text{A6})$$

Note that for $\alpha_1 > 0$ this assumption is equivalent to $(1 - \beta)\alpha_1 - \alpha_1^2\alpha_2 > 0$, which is automatically fulfilled with $\alpha_1 < 1$ and the assumption in Equation A5. Inserting the inequalities of Equations A5 and A6 into Equation A4 shows that it is plausible to expect that the change score estimator is negatively biased and that the following bracketing property holds (in large samples):

$$\gamma_{\text{change}} \leq \gamma \leq \gamma_{\text{ANCOVA}}. \quad (\text{A7})$$

Appendix B: Equivalence to Model with Stable and Transient Components

The data-generating model that conceptualizes potential confounding influences in terms of stable and transient components of the pretest, posttest, and treatment is given by (see Figure 2; Kenny, 1975; Allison, 1990):

$$\begin{aligned}
 Y_1 &= \alpha^* U + b_1 Z_1 \\
 T &= \alpha_T^* U + \delta^* Z_1 + \varepsilon_2 \\
 Y_2 &= \alpha^* U + b_2 Z_2 + \gamma T .
 \end{aligned} \tag{B1}$$

As the transient components Z_1 and Z_2 are allowed to be correlated, we can write $Z_2 = r_{Z_1 Z_2} Z_1 + \varepsilon_3$. If we now set $\beta^* = b_2 r_{Z_1 Z_2}$, the equation for Y_2 can be modified to $Y_2 = \alpha^* U + \beta^* Z_1 + \gamma T + \varepsilon_3$. We now reorder the terms in the Equation for T as follows:

$$\begin{aligned}
 T &= \alpha_T^* U + \delta^* Z_1 + \varepsilon_2 = \alpha_T^* U + \delta^* (Y_1 - \alpha^* U) / b_1 + \varepsilon_2 \\
 &= (\alpha_T^* - \delta^* \alpha^* / b_1) U + (\delta^* / b_1) Y_1 + \varepsilon_2 \\
 &= \alpha_1 U + \delta_1 Y_1 + \varepsilon_2
 \end{aligned} \tag{B2}$$

by using appropriate definitions of $\alpha_1 = \alpha_T^* - \delta^* \alpha^* / b_1$ and $\delta = \delta^* / b_1$. Similarly,

$$\begin{aligned}
 Y_2 &= \alpha^* U + \beta^* Z_1 + \gamma T + \varepsilon_3 = \alpha^* U + \frac{\beta^* (Y_1 - \alpha^* U)}{b_1} + \gamma T + \varepsilon_3 = \\
 &= \alpha^* \left(1 - \frac{\beta^*}{b_1}\right) U + \left(\frac{\beta^*}{b_1}\right) Y_1 + \gamma T + \varepsilon_3 \\
 &= \alpha_2 U + \beta Y_1 + \gamma T + \varepsilon_3
 \end{aligned} \tag{B3}$$

using $\alpha_2 = \alpha^* \left(1 - \frac{\beta^*}{b_1}\right)$ and $\beta = \left(\frac{\beta^*}{b_1}\right)$. Thus, the model with transient and stable components (Equation B1 and Figure 2) can be considered to be a special case of the data-generating model with an unobserved confounder in the main text (see Figure 1). It should be emphasized that in the model of Kenny (1975) and Allison (1990), the existence of a confounder with a stable effect is represented by assuming the same coefficient for the effect of U on Y_1 and Y_2 ($\alpha_1 = \alpha_2$).

Appendix C: Bias for the ANCOVA and Change Score Approaches under Measurement Error

In this appendix, we sketch the bias derivations for the ANCOVA and change score approaches under measurement error. Assuming that the data-generating model in Equation 8 is true (see Figure 3), the covariances between the observed variables $\tilde{Y}_1$, $\tilde{Y}_2$, and $\tilde{T}$ are derived

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as follows: $\text{Cov}(\tilde{T}, \tilde{Y}_1) = \delta + \alpha_1 \alpha_T$; $\text{Cov}(\tilde{Y}_2, \tilde{Y}_1) = \beta + \delta \gamma + \alpha_1 \alpha_2 + \alpha_1 \alpha_T \gamma + \rho_E$; $\text{Cov}(\tilde{Y}_2, \tilde{T}) = \gamma + \beta \delta + \alpha_2 \alpha_T + \beta \alpha_1 \alpha_T + \delta \alpha_1 \alpha_2$. For the ANCOVA estimator (see Equation 2), we specify a measurement error correction for $\tilde{Y}_1$ and $\tilde{T}$, where λ_1 and λ_2 are the ratios of the estimated and the true reliabilities, that is, $\lambda_1 = \hat{r}_1/r_1$ and $\lambda_2 = \hat{r}_2/r_2$. The bias of the corrected ANCOVA estimator is then given as follows:

$$\begin{aligned} \gamma_{\text{ANCOVA}}^* &= \frac{\lambda_1 \text{Cov}(\tilde{Y}_2, \tilde{T}) - \text{Cov}(\tilde{Y}_2, \tilde{Y}_1) \text{Cov}(\tilde{T}, \tilde{Y}_1)}{\lambda_1 \lambda_2 - \text{Cov}(\tilde{T}, \tilde{Y}_1)^2} \\ &= \frac{\lambda_1 \gamma - \beta \delta - \delta \rho_E + \beta \delta \lambda_1 + \alpha_2 \alpha_T \lambda_1 - \beta \alpha_1 \alpha_T - \delta \alpha_1 \alpha_2 - \alpha_1 \alpha_T \rho_E + \beta \alpha_1 \alpha_T \lambda_1 + \delta \alpha_1 \alpha_2 \lambda_1 - 2 \delta \alpha_1 \alpha_2 \gamma - \gamma \delta^2 - \alpha_2 \alpha_T \alpha_1^2 - \gamma \alpha_1^2 \alpha_T^2}{\lambda_1 \lambda_2 - 2 \delta \alpha_1 \alpha_T - \delta^2 - \alpha_1^2 \alpha_T^2} \\ &= \gamma + \frac{\alpha_2 \alpha_T (\lambda_1 - \alpha_1^2) + (\lambda_1 - 1) (\beta \delta + \delta \alpha_1 \alpha_2 + \beta \alpha_1 \alpha_T) - (\lambda_2 - 1) \lambda_1 \gamma - \rho_E (\alpha_1 \alpha_T + \delta)}{\lambda_1 \lambda_2 - 2 \delta \alpha_1 \alpha_T - \delta^2 - \alpha_1^2 \alpha_T^2} . \end{aligned} \quad (\text{C1})$$

The bias of the change score estimator that is corrected for measurement in $\tilde{T}$ is given by:

$$\begin{aligned} \gamma_{\text{change}}^* &= \frac{\text{Cov}(\tilde{Y}_2, \tilde{T}) - \text{Cov}(\tilde{Y}_1, \tilde{T})}{\lambda_2} \\ &= \frac{1}{\lambda_2} [\gamma - \alpha_T ((1 - \beta) \alpha_1 - \alpha_2) - \delta (1 - \beta - \alpha_1 \alpha_2)] \\ &= \gamma + \frac{\lambda_2 - 1}{\lambda_2} \gamma + \frac{1}{\lambda_2} [\alpha_T (\alpha_2 + \beta \alpha_2 - \alpha_1) + \delta (\beta + \alpha_1 \alpha_2 - 1)] . \end{aligned} \quad (\text{C2})$$