1 The danger of inferring causal direction from a partialling technique with longitudinal data

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Abstract

A growing number of researchers in organizational science are interested in directionality and have begun using a statistical routine to support inferences of causal direction. The technique involves partialling a prior observation of the outcome variable, and doing so is thought to mitigate concerns of reverse causality. We review the technique and then evaluate its ability to discriminate directional effects using Monte Carlo simulations. Our results demonstrate that directional inferences cannot be established using this statistical approach. We therefore suggest caution when directional inferences are based on statistical modeling only. We recommend that researchers stress their assumptions when inferring causal direction rather than relying on statistics alone.

Keywords: Directionality, causal inference, philosophy of science, longitudinal, temporal ordering, organizational psychology

The danger of inferring causal direction from a partialling technique with longitudinal data

Although researchers can make a variety of inferences about the processes they observe in longitudinal data (e.g., growth), studies across different domains of psychology often examine relationship directionality. Schneider, Hanges, Smith, and Salvaggio (2003) ask whether employee satisfaction or organizational performance comes first, stating that their research is necessary because prior studies were inconclusive regarding the direction of causality; Riketta (2008) begins his paper on attitudes by asking, “Do job attitudes increase performance? Is it the other way around?” (p. 472); Granziera and Perera (2019) assess relationships between self-efficacy, engagement, and satisfaction among teachers, and they state that their longitudinal design allows for a more robust test of the direction of effects; and Ciarrochi et al. (2016) examine the temporal ordering of internet use and mental health. Discriminating these effects is important when the goal of a study is to understand a process where many directions of influence are possible.

A set of studies in organizational psychology have recently argued that a particular statistical model applied to longitudinal data reveals causal direction (Gabriel, Koopman, Rosen, & Johnson, 2017; Johnson, Lanaj, & Barnes, 2014; Koopman, Lanaj, & Scott, 2016; Lanaj, Johnson, & Barnes, 2014; Lanaj et al., 2016b; Scott & Barnes, 2011). Researchers in these studies state that using a multi-level model on longitudinal data and partialling a prior observation of the outcome variable, or including y_{t-1} as a predictor of y_t , increases confidence that other predictors in the model (e.g., x_t) have a directional effect on y_t . Said another way, including a baseline of the dependent variable in a multi-level model is thought to “diminish concerns of reverse causality” (Lanaj et al., 2016b, p. 1102) and “remove some of the ambiguity regarding causal direction” (Scott & Barnes, 2011, p. 130).

Examining the effects mentioned above is clearly valuable for improving our understanding of a process. Although methods researchers have critiqued the ability of statistical techniques to support causal inferences (Bollen, 1989; McElreath, 2016; Pearl,

2009; Shadish, Cook, & Campbell, 2002), is it possible that this modeling strategy reveals relationship directionality? The purpose of the current study is to examine this question. If so, researchers employing the technique can yield greater confidence in their methods and models. If not, there will need to be a consensus change away from the belief that this modeling approach supports directional inferences. Ultimately, we argue that such inferences cannot be established with the given technique.

We begin by outlining the approach itself and the studies employing it. Then, we apply the statistical technique to simulated data where the population level parameters evoke different directional effects and assess its ability to capture them. If the models do reduce causal direction ambiguity, then they should be able to discriminate across situations where the direction of effects differ. Finally, our intent is to help the field avoid inferential errors and improve the overall quality of research. Studies and discussions of this type are necessary when initial enthusiasm surrounding the ability of a new statistical technique to support causal inferences is curtailed and corrected as research develops. For instance, passive measurements of individual differences were once partialled out of multiple regression equations as a substitute for random assignment (Cook, Campbell, & Day, 1979), and we needed intense debate on the ability of models to capture cause following the emergence of structural equation modeling (Brannick, 1995; Williams, 1995) and directed acyclic graph models (Dawid, 2010).

An Overview of the Approach

The studies that use the statistical approach that we analyze employ ESM as their research design. ESM represents a broad class of methodology where researchers sample participants in their natural environment (Beal, 2015) to capture temporal and dynamic subjective responses to experiences (Alliger & Williams, 1993). These designs are implemented by clinical, developmental, cognitive, and social scholars because they reduce recall bias, improve ecological validity, and allow researchers to capture within-person

processes (Iida, Shrout, Laurenceau, & Bolger, 2012; Shiffman, Stone, & Hufford, 2008). Our focus is on a specific statistical technique employed by a growing number of studies in the organizational literature whose overall design fits within this broader methodology.

A consistent goal within the studies acknowledged here is to mitigate concerns of reverse causality, or, in other words, support directional inferences. To provide an example, researchers may be interested in concluding that an experience, such as providing support to a coworker, influences affect and affect does not have a directional influence on coworker support. The operation that provides confidence is statistical in nature, and we identify it using a few exemplars below.

Scott and Barnes (2011) studied the relationship between emotional labor, state affect, and work withdrawal. During each weekday for two weeks, 68 bus drivers completed a survey measuring state affect when they arrived, and subsequently completed a survey assessing state affect and emotional labor just prior to leaving. Their multi-level statistical model regressed “evening” state affect on “evening” emotional labor while partialling the “morning” observation of state affect. They acknowledge that this analysis “does not by itself establish causality,” however, they also report that “controlling for affect at the beginning of the work day removes some of the ambiguity regarding causal direction” (p. 130).

Lanaj, Johnson, and Wang (2016a) examined the relationship between helping behaviors and depletion. A sample of 68 executive MBAs completed surveys for 15 days. One survey administered in the morning assessed depletion, and a second survey in the afternoon measured depletion and self-reported helping behaviors. Their multi-level model regressed afternoon depletion on afternoon helping behaviors while controlling for morning depletion. The authors report that “we controlled for baseline depletion because doing so is more empirically rigorous and diminishes concerns of reverse causality” (p. 1102).

Rosen, Koopman, Gabriel, and Johnson (2016) investigated the association between

incivility and self control among a sample of 107 employees. For ten days, these employees completed multiple surveys, the first assessing self control in the morning and the second measuring self control and instigated incivility in the afternoon. Their statistical model regressed afternoon self control on afternoon incivility while partialling morning self control. They report that “controlling for previous levels of self control mitigates concerns of reverse causality” (p. 11).

Similar multi-level statistical models and justifications are found in Gabriel et al. (2017), Johnson et al. (2014), Koopman et al. (2016), Lanaj et al. (2016b), and Lanaj et al. (2014). The approach, therefore, is one in which a lagged dependent variable is included as a predictor. Formally, this can be expressed as:

$$y_{it} = b_0 + b_1x_{it} + b_2y_{i(t-1)} + e_{it} \quad (1)$$

where y_{it} is the value of the outcome for the i^{th} person at time t , x_{it} is the value of the predictor variable for the i^{th} person at time t , $y_{i(t-1)}$ is the value of the outcome for the i^{th} person at time $t - 1$, e_{it} is a normally distributed error term for the i^{th} person at time t , and b_0 , b_1 , and b_2 represent the regression intercept and partial regression coefficients relating the predictors to the outcome.

The natural nesting of observations within units in ESM requires accounting for clustering when estimating the model parameters. Hierarchical linear modeling (HLM), also known as multi-level or random intercepts modeling, is the estimation technique used in all studies reviewed here. Many excellent overviews of this modelling approach exist already (e.g., Finch, Bolin, & Kelley, 2014; Schonfeld & Rindskopf, 2007), so we only cover what is necessary for the present purposes. HLM accounts for clustering or unit effects by incorporating random effects that may vary across units. HLM is capable of accounting for many forms of clustering but, typically, ESM studies use an approach allowing intercepts to

vary across units and, therefore, assume that the random intercepts follow a normal distribution. The generic model presented above may be generalized to represent this model by incorporating random intercepts such that:

$$y_{it} = b_{0_i} + b_1x_{it} + b_2y_{i(t-1)} + e_{it} \quad (2)$$

where all terms are defined as above except the intercept b_{0_i} may now be unique to the i^{th} person or unit and b_{0_i} is assumed to be distributed $N(\gamma, \sigma_{b_0}^2)$.

Formally representing these models helps reveal the notion of directionality expressed in the studies outlined above. The assumption is that a model that includes both $y_{(t-1)}$ and x_t as predictors of y_t improves our ability to discriminate the direction of influence between x_t and y_t beyond a model where x_t is the lone predictor of y_t . To continue the example, this would mean that a statistical model where prior values of self reported coworker support are partialled more clearly indicates the direction of effects between coworker support and self reported affect than a model where prior values of coworker support are not partialled.

These studies are at the forefront of examining within-person processes relevant to organizational behavior, but our longitudinal modeling literature is still nascent and, similar to the early multi-level literature, likely contains several well-intentioned false starts and inferential errors. Moreover, the literature contains somewhat conflicting thoughts on the ability of longitudinal models to support causal inferences. For example, Bollen (1989) and von Eye and DeShon (2012) demonstrated mathematically that lagging variables and regression techniques, respectively, could not establish causal priority – but these topics were not framed in the context of recent longitudinal designs. Conversely, in a review of organizational ESM studies, Beal (2015) noted that “ESM analyses that account for theoretically informed controls, lagged, cross-lagged, and concurrent effects can further improve evidence for causal direction” (p. 396), although he also mentions that this is only

relative to other observational designs. These notions of cause are bogged down by the more prevalent topics surrounding new statistical models, which receive more attention than deeper notions of causal inference in both methods publications (Aguinis & Edwards, 2014; Mathieu & Taylor, 2006) and graduate school training (Aiken, West, & Millsap, 2008; Tett, Walser, Brown, Simonet, & Tonidandel, 2013). Complex statistical procedures are required to handle longitudinal data (Kuljanin, Braun, & DeShon, 2011), therefore “how-to” papers on the latest techniques are constantly emerging, such as Jebb, Tay, Wang, and Huang’s (2015) time series presentation; Pitariu and Ployhart’s (2010) paper on dynamic mediation; Bliese and Ployhart’s (2002) discussion of growth modeling; DeShon’s (2012) chapter on multivariate dynamics; Schonfeld and Rindskopf’s (2007) application of hierarchical linear modeling to longitudinal data; Liu, Rovine, and Molenaar’s (2012) comparison of different longitudinal modeling strategies; or Wang, Zhou, and Zhang’s (2016) overview of dynamic modeling. These papers are unquestionably valuable but their prevalence may lead researchers to overweight the role of statistical techniques in the process of making scientific inferences.

In the following sections, we use Monte Carlo simulations to examine if the modeling technique outlined above in the context of within-person ESM investigations can support an inference of causal direction.

Method

Monte Carlo

The focus of the current investigation is on directional inferences given the research design and modeling strategy described above. We simulate different directions of influence and then evaluate the ability of a statistical model that includes prior levels of the outcome variable to discriminate directional effects above and beyond a model that does not include this term. Lag effects are also inherent to longitudinal investigations and are critical to understand for discerning directional influence (Beal, 2015; Maxwell & Cole, 2007; Zaheer,

170 Albert, & Zaheer, 1999) – we therefore incorporate them here.

171 Data Generating Process (DGP)

172 We use a recursive strategy from the dynamics literature to generate our data. In this
 173 approach, we specify the true data generating process (DGP) and its coefficients and then
 174 apply it to random initial conditions through a burn-in period until only the causal
 175 parameters are left driving the system. The underlying DGP induces observed data, which
 176 we then apply models to. Ultimately the goal is to determine if the models that we apply to
 177 the observed data recover the parameters that we originally set in the DGP. We use R for all
 178 data generation and analyses (R Core Team, 2017).

179 We evaluate the models across three independent DGP, which we call cases.

180 **Case 1:** In the first case, data are generated such that x causes y . That is, the causal
 181 structure underlying the data generating process is consistent with what researchers believe it
 182 to be in the ESM studies described above. The data generating equation for y in this case is:

$$y_{it} = b_{0_i} + b_1 x_{i(t-n)} + b_2 y_{i(t-1)} + e_{it} \quad (3)$$

183 where all terms are defined above and n represents the lag structure which systematically
 184 varies between zero and five.

185 **Case 2:** Data are generated in the reverse direction in the second case such that y
 186 causes x :

$$x_{it} = b_{0_i} + b_1 y_{i(t-n)} + b_2 x_{i(t-1)} + e_{it} \quad (4)$$

187 where all terms are defined above.

Case 3: In the final case, a third variable, z , causes both x and y . The generation equations for x and y are, respectively:

$$x_{it} = b_{0_i} + b_1 z_{i(t-n)} + b_2 x_{i(t-1)} + e_{it} \quad (5)$$

$$y_{it} = b_{0_i} + b_1 z_{i(t-n)} + b_2 y_{i(t-1)} + e_{it} \quad (6)$$

where z is the third variable and all terms are defined above.

To summarize, in Case 1 the data generating process is x causes y , while in Case 2 y causes x , and, finally, in Case 3 z causes both x and y . Within each of these cases we independently examine six different causal lag structures, such that the true data generating processes are $x_t \Rightarrow y_t$, $x_{(t-1)} \Rightarrow y_t$, $x_{(t-2)} \Rightarrow y_t$, $x_{(t-3)} \Rightarrow y_t$, $x_{(t-4)} \Rightarrow y_t$, $x_{(t-5)} \Rightarrow y_t$ for Case 1 while the arrows are flipped in Case 2. Each of these lag structures are generated and examined independently, meaning that we evaluate six different simulations for Case 1, and six different simulations for Case 2. Finally, Case 3 consists of both equivalent and non-equivalent lag structures from z to x and z to y . In other words, the causal lags from z to x and y initially vary in the same manner (one-lag, two-lag, three-lag, etc.) and then alter sequentially (concurrent to x and one-lag to y ; concurrent to x and two-lag to y , etc.).

The true causal coefficients are held constant across the cases, and we chose their values based on a review of the aforementioned studies. The average reported effect of a presumed independent variable on a presumed dependent variable is 0.082 ($SD = 0.15$), therefore we hold b_1 across all cases at 0.082. The average reported autoregressive effect is 0.22 ($SD = 0.042$), therefore we hold b_2 across all cases at 0.22. The other terms in our DGPs include the unit effect (unobserved heterogeneity) and time-specific error. Unit effects represent aggregate between unit differences. Between unit effects are distributed $N(0, 1)$

and then remain stable for any one particular unit across time. The error terms are sampled from a distribution with $N(0, 1)$ at each time point for every unit.

Finally, we also vary the sample size in every case. The average number of participants in the reviewed studies was 72, so we generate data within each case with samples of 72, 150, 250, 350, 450, and 550, respectively.

Statistical Models

We then use two HLM models to estimate the parameters of the “true” processes just described. One model, consistent with the reviewed research body, includes a predictor term representing the outcome variable at the prior time point, whereas the second model does not include this term. In both models, and across all simulations, the presumed independent variable (x) is person-mean centered to mirror the analytic technique commonly used in ESM studies. We estimate the HLM parameters using the nlme package in R and save the output to assess performance. For every lag structure simulation within each case we assess five hundred replicates. An outline of the simulation routines is shown in Table 1.

Model Bias and Performance

We evaluate model performance by saving, for each model, the estimate of the coefficient relating x_t to y_t ($\hat{b}_1$, see table 1) and its associated standard error and significance test. Bias in $\hat{b}_1$ refers to the extent to which the estimate of that coefficient is different from its true value, and perfect recovery would result in zero bias. In case one, the true coefficient is 0.082, whereas in the other cases there is no causal effect from x_t to y_t and therefore any $\hat{b}_1$ estimate above or below 0 is biased.

We will also assess the standard errors on $\hat{b}_1$ and the proportion of significant $\hat{b}_1$ estimates across the 500 replicates. Type I errors occur when $\hat{b}_1$ is significant above the nominal rate ($\alpha = 0.05$) despite the true data generating process stemming from the second

or third case. That is, a Type I error occurs when parameter estimates relating scores on x to y are significant above the nominal rate despite there being no influence of x on y .

We also draw attention to null results across the various lag structures. A lag assumption is always implied in longitudinal research, if not explicitly then implicitly by the model. Even if the process works in the proposed direction the model might fail to capture processes with longer or shorter lags. These Type II errors will occur in the form of alpha levels at the nominal level in the first case where x causes y .

Results

The code used to generate the simulations can be found at the following anonymous repository (<https://anonymous.4open.science/r/95b9b914-1119-4833-8249-76520c98222f/>). The Figures described below can be found by navigating to the following anonymous repository (<https://anonymous4articles.github.io/anonymous4articles.github.io/directionalityfigs/>).

Case 1 (X causes Y)

In the first case, we generated data such that x causes y across lag structures ranging from 0 (i.e., concurrent causation) through 5 (i.e., delayed causation). In other words, the true data generating mechanisms were $x_t \Rightarrow y_t$, $x_{(t-1)} \Rightarrow y_t$, $x_{(t-2)} \Rightarrow y_t$, $x_{(t-3)} \Rightarrow y_t$, $x_{(t-4)} \Rightarrow y_t$, $x_{(t-5)} \Rightarrow y_t$. We then fit two models to the data generated in each of these simulated lag structures. The first statistical model included x_t and the first lag of the outcome variable, $y_{(t-1)}$ as predictors of y_t . The second model used x_t as the lone predictor of y_t .

Figures 1, 2, and 3 demonstrate bias and performance for the two models in case one (i.e., rows 1 and 2 of Table 1). Recall that in case one the true b_1 coefficient is 0.082. Figure 1 presents the bias in the $\hat{b}_1$ estimate for each model across the varying sample sizes and true time lags. For the model including the $y_{(t-1)}$ covariate, bias increases in the negative direction when the true time lag varies from one to three; it then plateaus at -0.08 across

even greater time lags. The model that does not partial prior y also demonstrates a bias increase in the negative direction as the true time lag varies from one to three, and bias remains stable across greater lags. Both models demonstrate little to no bias under the concurrent causal structure. Figure 2 shows the standard errors on the $\hat{b}_1$ estimate for each model across varying sample sizes and true time lags. Standard errors do not change across the time lags for the model that partials $y_{(t-1)}$, but across sample sizes the standard error on $\hat{b}_1$ decreases from 0.027 to 0.01. Model performance is identical for the model that does not include the covariate. Figure 3 presents the proportion of significant results for the test of $H_0 : b_1 = 0$ across the varying true time lags for each model. For the model including $y_{(t-1)}$ as a covariate, the proportion of coefficient estimates that are significant reaches 100% when x is a true concurrent cause of y and falls to the nominal alpha level for all other lag structures. A similar pattern occurs for the model that does not use $y_{(t-1)}$ as a covariate: the proportion of coefficient estimates that are significant again reach 100% when x is a concurrent cause of y and fall to near nominal across the other lag structures.

Insert Figures 1, 2, and 3 about here

Case 2 (Y causes X)

In the second case, we generated data in the reverse direction, such that y causes x across the six lag conditions (i.e., $y_t \Rightarrow x_t$, $y_{(t-1)} \Rightarrow x_t$, $y_{(t-2)} \Rightarrow x_t$, $y_{(t-3)} \Rightarrow x_t$, $y_{(t-4)} \Rightarrow x_t$, $y_{(t-5)} \Rightarrow x_t$). We then fit the same two models used above to the resulting data (i.e., rows 3 and 4 of Table 1). In this case there is no true effect from x to y , so when the models estimate a coefficient relating x_t to y_t ($\hat{b}_1$) it should be zero if there is no bias. Figures 4, 5, and 6 demonstrate model performance for case two.

Figure 4 presents the bias in the $\hat{b}_1$ estimate for each model across the varying sample sizes and true time lags. The model that partials prior y shows immense bias under the concurrent causal structure, as does the model that does not partial prior y . Again, there is no effect from x to y in this case (i.e., the true coefficient is zero), but both models estimate a significant effect of 0.082. Bias decreases substantially for both models when the true causal structure relating y to x is greater than zero (not concurrent). Figure 5 shows the standard errors on the $\hat{b}_1$ estimate for each model across varying sample sizes and true time lags. Model performance is identical to the first case: both models show the same standard error patterns across true time lags and sample sizes. Figure 6 depicts the proportion of significant $\hat{b}_1$ estimates across the simulation replicates for each of the two fitted models. For the first model that includes $y_{(t-1)}$ as a covariate, the significance tests on the $\hat{b}_1$ coefficient substantially exceed the nominal level (100%) under the concurrent-lag structure (i.e., zero-lag). Since the relationship direction is misspecified in this model, these results represent Type I errors. The rejection rates then fall near the nominal level under longer lag structures. At large sample sizes, however, rejection rates remain above the nominal level. Moving to the second model, which did not include prior y as a covariate, the proportion of significant coefficients also reaches 100% under the concurrent causal structure, falls to roughly the nominal level under other lag structures, but exceeds the nominal level at large sample sizes.

Insert Figures 4, 5, and 6 about here

Case 3 (Z causes X and Y)

In our final case, a third variable (z) causes both x and y , meaning that x and y are not causally linked and no meaningful inference of directionality can be supported by the observed relationship between x_t and y_t ($\hat{b}_1$). We examine the performance of the fitted

HLM models under three patterns of true lagged causal structure. In the first, the lag effect from z is symmetric for both x and y . That is, if z_{t-2} causes x_t then z_{t-2} causes y_t . In the second and third the lag effect of z on x and y is asymmetric.

We first consider results when the lag effect from z is symmetric for both x and y . Figure 7 presents the bias in the $\hat{b}_1$ estimate for each model across the varying sample sizes and true time lags. The model that partials prior y shows bias near 0.07 across all sample sizes and time structures, and the model that does not partial prior y demonstrates similar behavior. Figure 8 shows the standard errors on the $\hat{b}_1$ estimate for each model across varying sample sizes and true time lags. Again, performance is identical to cases one and two. Figure 9 shows the proportion of significant $\hat{b}_1$ estimates under both fitted models. With large samples, significance tests exceed the nominal level across all lag structures for both models, whereas the pattern varies at lower sample sizes.

Insert Figures 7, 8, and 9 about here

We now turn to results with varying lag structures across x and y . That is, z_t causes y_t but the lag effect from z to x varies (and then the reverse is evaluated). Figures 10, 11, and 12 depict results when the lag effect of z on x varies. Figures 13, 14, and 15 demonstrate results when the lag effect of z on y varies. As prior, behavior across the fitted HLM models is similar across these simulation environments.

Insert Figures 10, 11, and 12 about here

Summary of Results

We simulated various true directional and lag effects across three different cases and subsequently fit two models to the data. One model included y_{t-1} as a covariate while the other did not, and comparing model behavior allowed us to assess whether including a prior observation of an outcome variable improves directional inferences above and beyond a model that does not include this term. We found similar behavior for both models across all cases and simulated lag structures. These results highlight that the increasingly popular approach that is being applied in the organizational literature does little to garner evidence of directionality.

Discussion

A growing body of research assessing relationships at the day level is advancing to our understanding of workplace behavior. This progress can be seen not only with respect to knowledge about variable relationships, but also in the increasing sophistication of the methods and models used to build confidence in the results. In the midst of these advances, however, well-intentioned exploratory modeling strategies arise that may not adequately capture what researchers would like them to. If left unchecked, these techniques can become norms and stifle research from reaching its full potential. The purpose of this study was to assess the viability of an increasingly popular modeling technique used among a variety of organizational studies.

The technique entails partialling a baseline observation of the outcome variable to diminish “concerns of reverse causality” (Lanaj et al., 2016a, p. 1102). That is, including both y_{t-1} and x_t as predictors of y_t is thought to improve confidence that x_t causes y_t – and that y_t does not cause x_t – to a greater extent than a model that does not include y_{t-1} . We found that both models produced comparable behavior among a variety of directional effects in the population level parameters. Our results demonstrate that observational designs using this technique cannot support relationship directionality inferences.

We recommend that researchers focus less on statistical models for justifying cause and more on assumptions. We want to move away from statistics and toward the rest of science, where assumptions are the foundation for causal inferences, including directionality. The causal inference literature has published many great articles and books in the past two decades (Dawid, 2010; McElreath, 2016; Morgan & Winship, 2015; Pearl, 1998, 2009, 2012; Pearl & Mackenzie, 2018), and there is agreement that assumptions and constraints drive causal inferences, not statistical models.

A clear demonstration of the role of assumptions is in Simon's (1954) presentation of the third variable problem. Suppose that we observe a relationship between marital status (x) and candy consumption (y). If this relationship decreases considerably when we add a third variable, age (z), to the model then two conclusions are possible: a relationship exists between marital status (x) and candy consumption (y) but age (z) is an intervening variable; or the relationship between marital status (x) and candy consumption (y) is spurious because age (z) causes variation in both. We can conclude the second inference (a spurious association) if we are willing to make an assumption regarding the independence of age (z). That is, "apart from any statistical evidence, we are prepared to assert that the age of the person does not depend on either his candy consumption or his marital status" (Simon, 1954, p. 471).

Linger a bit longer on the implication of the example. After applying a statistical routine we were left with two interpretations: either candy consumption causes age or age causes candy consumption and marital status, but the data did not tell us which conclusion to make. All of us agree, we share the same assumption, that candy consumption cannot cause age and therefore age is the common cause. The assumption, not the data, led us to our causal interpretation. Researchers may suggest that certain assumptions or interpretations are more likely than others, but ultimately many of these reduce to untestable statements and therefore require assumptions.

Assumptions often go unnoticed because of our shared intuitions about the research context. For example, Williams and Hazer (1986) found that the relationship between work characteristics (e.g., the amount of perceived repetition) and turnover reduced when attitudes, such as job satisfaction and commitment, were included in the model. Attitudes could therefore be an intervening variable or a common cause. We agree with the authors' conclusion that attitudes should be conceived as an intervening variable, but notice the underappreciated intuition we all share in this case: attitudes cannot cause work characteristics. Said another way, it is challenging to imagine a world in which an individual's attitude produces the amount of repetition in his or her job. Because this intuition is obvious in many investigations the assumption goes unstated. An unintended consequence, however, is that it reduces the perceived need to highlight alternative explanations or foundational assumptions in studies with more ambiguous relationship directionality.

In statistical modeling, assumptions concern the nature of the data generating process. Although knowing whether or not we have the correct cause and all relevant variables are fundamental problems across all stages of scientific inference (Howson & Urbach, 2006), these issues are especially important in modeling because many different data generating processes can result in the same observed data (McElreath, 2016). Statistical models, therefore, cannot identify the causal process without relying on assumptions and the evidence to support them (Pearl, 2009, 2012) because model fit and parameter estimates may be equivalent across different data generating processes (Mathieu, DeShon, & Bergh, 2008). The “answers” models provide are conditional on their own rules for interpreting data (Granger, 2004) and every model has only one way of viewing the problem (McElreath, 2016). Evaluating fit and parameter estimates indicates the discrepancy between the model and data, but it does not reveal the discrepancy between the model and reality (Bollen, 1989). This is not to say that causal inferences should be avoided when using models; we share Pearl's view that they should be emphasized, but only when they are paired with explicit assumptions and their justifications.

What we recommend for researchers moving forward, then, is to (1) avoid basing causal direction statements on only this statistical routine and (2) instead, make clear the presumed direction of effects and how the model aligns with that structure. This actionable recommendation need not require any complex training or development of new statistical architecture, just a reorientation into how researchers discuss evidence with respect to their statistical models. The studies that we cite in this paper are results-oriented: *a given result provides causal direction evidence*. Instead, we recommend that researchers orient themselves with respect to assumptions: *we partialled covariates to align our model with the presumed DGP, but under a different DGP the partialling technique may be inappropriate*. Or, *given an assumed causal direction, we employed a statistical model that identifies the relationship of interest* – perhaps using the front or backdoor criterion (Pearl, 2012). In other words, just as the language of our hypotheses for patterns in longitudinal data required some refinement (Pitariu & Ployhart, 2010), so too does our language with respect to reaching conclusions from those patterns.

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Table 1

Structure of Monte Carlo simulations. Within each case, true causal lags varied from zero to five.

Case	Data Generating Direction	HLM Model Applied
1	$X \Rightarrow Y$	$y_{it} = b_{0i} + b_1 x_{it} + b_2 y_{i(t-1)} + e_{it}$
1	$X \Rightarrow Y$	$y_{it} = b_{0i} + b_1 x_{it} + e_{it}$
2	$Y \Rightarrow X$	$y_{it} = b_{0i} + b_1 x_{it} + b_2 y_{i(t-1)} + e_{it}$
2	$Y \Rightarrow X$	$y_{it} = b_{0i} + b_1 x_{it} + e_{it}$
3	$Z \Rightarrow X, Z \Rightarrow Y$	$y_{it} = b_{0i} + b_1 x_{it} + b_2 y_{i(t-1)} + e_{it}$
3	$Z \Rightarrow X, Z \Rightarrow Y$	$y_{it} = b_{0i} + b_1 x_{it} + e_{it}$

Note. In all conditions b_1 is the focal parameter estimate.

Figure 1. Bias in the b_1 estimate for case 1.

Figure 2. Standard error on the b_1 estimate for case 1.

Figure 3. Proportion of significant b_1 estimates for case 1. The dashed grey line indicates the alpha level, 0.05.

Figure 4. Bias in the b_1 estimate for case 2.

Figure 5. Standard error on the b_1 estimate for case 2.

Figure 6. Proportion of significant b_1 estimates for case 2. The dashed grey line indicates the alpha level, 0.05.

Figure 7. Bias in the b_1 estimate for case 3, with a symmetric lag effect from z to x and y .

Figure 8. Standard error on the b_1 estimate for case 3, with a symmetric lag effect from z to x and y .

Figure 9. Proportion of significant b_1 estimates for case 3, with a symmetric lag effect from z to x and y .

Figure 10. Bias in the b_1 estimate for case 3, with a concurrent effect from z to y and a lag effect from z to x .

Figure 11. Standard error on the b_1 estimate for case 3, with a concurrent effect from z to y and a lag effect from z to x .

Figure 12. Proportion of significant b_1 estimates for case 3, with a concurrent effect from z to y and a lag effect from z to x .

Figure 13. Bias in the b_1 estimate for case 3, with a concurrent effect from z to x and a lag effect from z to y .

Figure 14. Standard error on the b_1 estimate for case 3, with a concurrent effect from z to x and a lag effect from z to y .

Figure 15. Proportion of significant b_1 estimates for case 3, with a concurrent effect from z to x and a lag effect from z to y .