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Optimal Time Lags in Panel Studies

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Cross-lagged regression coefficients are frequently used to test hypotheses in panel designs. However, these coefficients have particular properties making them difficult to interpret. In particular, cross-lagged regression coefficients may vary, depending on the respective time lags between different sets of measurement occasions. This article introduces the concept of an optimal time lag. Further, it is demonstrated that optimal time lags in panel studies are related to the stabilities of the variables investigated, and that in unidirectional systems, they may be unrelated to the size of possible true effects. The results presented also suggest that optimal time lags for panel designs are usually quite short. Implications are (a) that interpreting cross-lagged regression coefficients requires taking the time lag between measurement occasions into account, and (b) that in much research, far shorter time lags than those frequently found in the literature are justifiable, and we call for more "shortitudinal" studies in the future.

Keywords: optimal time lag, longitudinal research, cross-lagged effects, continuous time modeling, shortitudinal study

Panel designs, in which data are gathered from the same individuals on two or more occasions, are very popular. However, designing a panel study can be problematic. As Mitchell and James (2001) noted,

> With impoverished theory about issues such as when events occur, when they change, or how quickly they change, the empirical researcher is in a quandary. Decisions about when to measure and how frequently to measure critical variables are left to intuition, chance, convenience, or tradition. None of these are particularly reliable guides. (p. 533; see also Cole & Maxwell, 2003)

The lack of systematic methods for determining "when to measure" means researchers have little practical guidance for choosing appropriate time lags in longitudinal studies.

In this article, we address the problem of "when to measure" by deriving methods to estimate the optimal time lag between measurement occasions. We focus on the optimal time lag between two variables, X and Y, in a panel design. The regression-based two-wave, two-variable (2w2v) panel design is one of the most common types of repeated measure designs in the applied psychology literature. We derive general principles that are algebraic rather than simulation-based. We also provide practical guidance for estimating optimally lagged effects regardless of the content domain.

Optimal time lags should be considered within the broader question of "when events occur, when they change, and how quickly they change (Mitchell & James, 2001, p. 533)." Collins (2006) argues that an effective longitudinal design depends on capturing the theoretical process that is consistent with the temporal change being investigated. In this study, we assume a process of continuous change through which independent variables exert an ongoing influence on lagged dependent variables. This continuous process can include reciprocal effects between variables, and assumes that there is some level of stability in all variables across time. This theoretical process is very common in panel studies, and some assumptions regarding the underlying process are necessary for general conclusions to be drawn (cf. Voelkle, Oud, Davidov, & Schmidt, 2012). The only alternative approach to finding the optimal time interval would be to conduct a multiwave study with many waves separated by very short intervals, and interpolate from the results.

The present article provides new insights into the temporal design of panel studies. Ultimately, our approach encourages researchers to estimate optimal time lags through "shortitudinal" pilot studies conducted over periods that are shorter than the likely time lag for any planned longitudinal study.

Background

Little research has been devoted to the question of which time lags should be chosen in panel designs. Statements such as "not too short" or "not too long" are common (cf. Boker & Nesselroade, 2002; Hertzog & Nesselroade, 2003) but do not convey specific information about time lags. The reasons that researchers give for choosing specific time lags are many and varied. For example, Dormann and van de Ven (2014) identified six broad types of

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reason for particular lags: reasons related to the constructs under investigation or their operationalizations; the proposed causal mechanisms; the method used; epistemology; or the researcher. First, to assess a construct, such as "annual earnings before interest and taxes," one has to consider an appropriate time frame, which would be 12 months in this instance. The second group of arguments relates to the operationalization of constructs, that is, to the measurement level. For example, Plaisier et al. (2007) used the "2-year incidence of depressive and anxiety disorders" (p. 403) as a dependent variable in their study. The third group of reasons for choosing particular time lags is related to the mechanisms under study. For instance, it was argued that weak effects need a long time to unfold (e.g., Gorgievski-Duijvesteijn, Bakker, Schaufeli, & van der Heijden, 2005; Sacco & Schmitt, 2005). In the fourth group are various methodological reasons. For example, a 12-month lag may be chosen to control for seasonal fluctuations (e.g., de Lange, Taris, Kompier, Houtman, & Bongers, 2003). This issue may be important in studies of work stress because workload (e.g., number of customers) may vary according to the time of the year (e.g., Christmas). Fifth, there are epistemological reasons to choose a particular time lag. For instance, Halbesleben (2010) used a particular time lag that has not been used before in that field to go beyond what was already known. Sixth, time lags may depend on the personal goals of the researcher. For example, researchers might want to demonstrate the sustainability of effects to show they are theoretically or practically important (e.g., Kinnunen, Kaprio, & Pulkkinen, 2005). In such instances, one would probably choose long time lags to address the needs of the target audience.

As outlined thus far, many reasons exist to justify almost any particular time lag. Nevertheless, articles in the applied psychology literature proposing substantive reasons for choosing a particular time lag are scarce. Indeed, little research has been done to provide a sound scientific basis for choosing an adequate time lag.

Extant Research on Optimal Time Lags

Cole and Maxwell (2003) observed that the timing of assessment in the social sciences was often determined by convenience and tradition rather than theory or research. Although there is little systematic research investigating the most appropriate time lags for panel studies, a few studies used simulated data to explore the question of optimal time lags. Dwyer (1983, p. 359) showed that time lags that are too long lead to a slight underestimation of causal effects.

Sims and Wilkerson (1977) found correlations of X and Y that varied in an inverse U-shaped fashion across time. This result was not too surprising, because Sims and Wilkerson explicitly included one element in their simulation that caused effects to increase across time (in an S-curved fashion) and a second factor that caused effects to decrease across time (exogenous influences that followed a stochastic exponential function).

More recently, Cole and Maxwell (2009) considered the issue of appropriate time lags in risk—outcome clinical psychology research. They found that "When the experimenter selects anything other than the optimal lag, the common statistical approaches will grossly underestimate the relation between risk and the outcome" (p. 80).

Cole and Maxwell (2003, Figure 7) demonstrated that lagged effects vary with time, and that the shape of the distribution of these effects over time varies, too. A model with similar assumptions will be discussed later, and we will extend this model by demonstrating *when* lagged effects become strongest.

Finally, Voelkle et al. (2012) described the shape of cross-lagged effect sizes of a variable X on Y that one would obtain for varying time lags. Their findings are entirely in line with the findings presented later. However, Voelkle et al. did not address the issue of optimal time lags; rather, they demonstrated how stochastic differential equations could be used to relate discrete time models (D-Models) to continuous time models (C-Models).

The above studies using simulations provide insight into optimal time lags but also involve some limitations. First, they do not provide specific guidance about the length of optimal time lags. Second, they are based on assumptions that do not meet the typical situation for panel studies in psychology (cf. Finkel, 1995; Kessler & Greenberg, 1981). For example, Cole and Maxwell (2009) assumed that the independent variable (X) and the dependent variable (Y) would be perfectly correlated if the optimal time interval was chosen ($\rho = 1.0$), and there was no autoregressive effect. This assumption is not valid for many psychological variables (cf. Finkel, 1995; Kessler & Greenberg, 1981). Cole and Maxwell (2009) were also particularly concerned with interindividual differences in optimal time lags.

Although no general conclusion can be readily drawn from existing research, a rule of thumb has emerged suggesting that effects decline as time lags become longer (J. Cohen, Cohen, West, & Aiken, 2003). This rule of thumb is inconsistent with nonmonotonic effects over time shown in simulated data and algebraic proofs. However, it is consistent with observations of actual panel studies, in which several meta-analyses have shown that effects erode as the time lag between two measurements increases (e.g., Atkinson et al., 2000; A. Cohen, 1993; Griffeth, Hom, & Gaertner, 2000; Holden, Moncher, Schinke, & Barker, 1990; Hom, Caranikas-Walker, Prussia, & Griffeth, 1992; Hulin, Henry, & Noon, 1990; Riketta, 2008; Steel, Hendrix, & Balogh, 1990; Steel & Ovalle, 1984; see also Cronbach, 1970, p. 137). Most of these earlier meta-analysis either split the studies into two groups using longer versus shorter lags (e.g., Holden et al., 1990) or conducted linear correlation or regression analyses of effect sizes and time lags (e.g., Steel et al., 1990). Both methods can reveal linear trends only. Later, we discuss why the time lag of most panel studies is likely to be longer than the optimal time lag, explaining the steady decline in effects shown in these meta-analyses. Some more recent meta-analyses investigated nonlinear effects of time lag on effect sizes. One did not reveal linear or nonlinear effects (after excluding an outlier study; Sowislo, & Orth, 2013), whereas another showed that lagged effects may increase across time, then decrease, and eventually level out (e.g., Ford et al., 2014).

Overall, researchers have little guidance for defining and selecting an optimal time lag in panel studies. J. Cohen et al. (2003, p. 571) concluded that no generalizations could be made about "the optimal interval for examining causal effects of one variable on another." We agree with Cohen et al. that little has been explicitly said about optimal time lags, even if rules of thumb are available. Furthermore, we concur with them that particular emphasis must be placed upon causal effects, rather than upon simple correlations. The reason is that panel models that do not include the lagged counterpart of the dependent variable Y as an additional independent variable are frequently misspecified (cf. Finkel, 1995).

To address the concerns of J. Cohen et al. (2003), we show that some generalizations can be made, and we present a statistical theory of optimal time lags that is applicable to a wide array of substantive theories. In the next section, we discuss the types of variables and models to which the question of optimal time lags applies. Then, we show how cross-lagged effects vary with time and how optimal time lags can be determined.

Optimal Time Lags in Regression-Based Designs

We define an optimal time lag as the lag that is required to yield the maximum effect of X predicting Y at a later time, while statistically controlling for prior values of Y in a 2w2v design. This definition corresponds to frequently applied approaches for investigating possible causal effects of X on Y using a panel design. For instance, it was the most common approach in the longitudinal studies reviewed by Zapf, Dormann, and Frese (1996; see also de Lange et al., 2003).

The optimal time lag represents the time lag across which researchers would ideally measure variables X and Y. That is, researchers would aim to match the time lag of a specific study with the optimal time lag. We define the actual time between two measurement periods as the specified unit (SU) of time.

The approach presented in this article is algebraic, rather than based on simulations. The analysis can be accomplished using ordinary least square regression analysis or latent variable structural equation modeling (SEM). To simplify presentation, in the present article, path diagrams without measurement models will be used. Note, however, that it is assumed that variables are measured without error when algebraically deriving optimal time lags. When using empirical data, measurement error will need to be taken into account. In the remainder of the article, we also make the assumption that models are correctly specified, that is, that all relevant third variables are included. As always, if a researcher fails to include all relevant third variables, parameter estimates of effects, including estimates of optimal time lags, might be seriously biased. Although the assumption of correct model specification is not realistic, it is always implicitly made when causally interpreting relations among variables. Hence, the approach to calculating optimal time lags proposed in the present article has the same deficiencies as other nonexperimental designs.

Conceptually, panel designs capture snapshots of a process that unfolds continually over time. We refer to the ongoing relationship between variables as a "continuous time" model and depict this process in Figure 1A. A fundamental assumption in many panel studies is that a cause (e.g., work conflict) and an effect (e.g., feelings of anxiety) unfold continuously over time. In the work stress literature, this assumption is reflected in the *stress reaction model* (Frese & Zapf, 1988, p. 389). According to this model, stressors reduce psychological well-being, and if the stressor is removed, an improvement in psychological well-being occurs. The ups and downs in the level of stressors could vary among individuals. Therefore, there is no discrete point at which the effect occurs. As this continuous process unfolds, the size of the observed effect also varies continuously, but because measures are gathered at a specific point in time, our goal is to provide guidance around this timing.

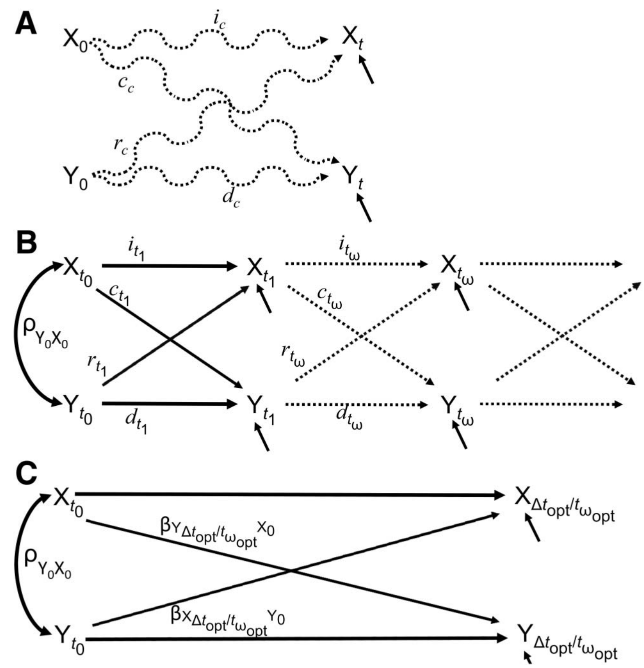

Figure 1. Continuous time model and discrete time panel analysis. Figure 1A depicts the ongoing relationship between variables as a "continuous time" model (C-Model), in which time can take on an infinite set of values for which t indicates the exact time point of a particular observation. The dotted paths indicate that the effect of prior variables on later variables is not necessarily direct; rather, these paths represent the overall effects that occurred across time. The C-Model reflects the data generation process in this article. Figure 1B depicts the relations between variables measured at discrete time points (D-Model), and t_0, t_1 denote Time 0, Time 1, and so forth of data collection and the index $\omega = 1 \ldots, T$ denotes the rank of an observation. Figure 1C shows the final model (F-Model) used to test substantive hypotheses. It depicts a regression of $Y_{\omega_{opt}}$ on X_0, while controlling for Y_0, and vice versa, with ω_{opt} being a multiplier of the lag that was used in the D-Model.

In C-Models, time can take on an infinite set of values for which t indicates the exact time point of a particular observation (cf. Voelkle et al., 2012). The C-Model in Figure 1A reflects the theoretical model that creates the data generation process we assume to operate in this article, and also fits the intuitive notion of change used by researchers adopting panel designs to evaluate lagged effects. If the parameters of the C-Model were known, one could extrapolate the parameter estimates one would obtain when choosing any particular time lag.

Continuous time parameters are typically not known, and researchers rely on discrete measures to infer the underlying relationships. Figure 1B depicts a typical discrete panel model (D-Model). The D-Model may comprise several waves of data collection. In this model, t_0, t_1, and so forth, are used to denote Time 0, Time 1, and so forth, of data collection. Thus, the index $\omega = 1 \ldots, T$ denotes the rank of an observation. The discrete lag between two consecutive measurement occasions is defined as the SU of time. Researchers would typically prefer to match the SU of a specific study with the optimal time lag. In the D-Model, the goal would be to match the length of the SU (e.g., from t_0 to t_1) to the optimal time lag. Because the optimal time lag is usually not known a priori, we propose the SU should be reasonably short, to increase likelihood that it will be shorter than the optimal time lag. Only then will researchers have the chance to collect another wave of data at a later point coinciding with the optimal lag. Put differently, if the SU used in a specific D-Model turned out to be longer than the optimal time lag as estimated next, the researcher could not travel back in time to collect additional data. Depending on the nature of the continuous change process, an SU could correspond to 2 days, 1 week, or 100 hr.

We describe the estimation of optimal time lags in three steps. First, we derive an estimate of optimal lag for two waves of data from the D-Model, which is easier to demonstrate compared with C-Models. Although multiple waves can be applied in a D-Model, two waves are algebraically sufficient to outline the basic principles for deriving optimal lag values. Second, we show how the estimates from the C-Model can be used to calculate optimal lags. In general, calculations of optimal time lags using the D-Model or the C-Model will yield identical results. Third, we outline a final F-Model to depict a two-wave panel model using the optimal time lag, and show how this final model relates to both the D-Model and C-Model.

The F-Model is depicted in Figure 1C and shows the regression of $Y_{\omega_{opt}}$ on X_0, while controlling for Y_0 and vice versa. If the optimal time lags are derived from D-Models, ω_{opt} represents a multiplier of the SU that was used in the D-Model. For example, if the D-Model used an SU of 1 week, ω_{opt} could reflect the second wave in a study that is separated by ω_{opt} weeks from the initial wave. If optimal time lags are derived from C-Models, Δt_{opt} reflects the optimal real time difference to Time 0. The F-Model is used to test substantive hypotheses using optimal time lags.

In general, we propose researchers aim to use a SU in the D-Model that is likely to be shorter than the optimal time lag. We explain this issue in more detail later, but simply note here that the main goal of estimating an optimal time lag is to ensure panel studies implement measurement waves at the most appropriate times. This outcome is most readily achieved if the multiplier of the optimal lag is greater than 1; that is, if the optimal time lag is longer than the lag in the D-Model.

Figure 1B is also useful to introduce the major terms used in our analysis of optimal time lags. In the D-Model shown in Figure 1B, X and Y are correlated at Time 0; these correlations reflect their relations, which have emerged through prior causal effects among X and Y. Each variable is stable over time to some extent. The *stabilities* of the variables are the horizontal autoregressive paths shown in the D-Model. The stability coefficients are i_{t_ω} and d_{t_ω}, to denote the autoregressive effects of the independent (i) and the dependent (d) variable, respectively. In Figure 1B, it is further assumed that X has a *direct cross-lagged effect* on Y (c_{t_ω}) one wave later. The *reversed* cross-lagged effect of Y on X is labeled r_{t_ω} in the D-Model. When referring to X and Y measured at particular waves (e.g., X_{t_2} and Y_{t_4}), the indirect cross-lagged effect of X_{t_2} on Y_{t_4} is sometimes called *time-specific indirect (cross-lagged) effect* (Gollob & Reichardt, 1991). Over the course of a study with multiple waves, there are usually many time-specific indirect effects. Altogether, they contribute to the unfolding of the *overall indirect (cross-lagged) effect* of X on Y over time (Gollob & Reichardt, 1991). Like Voelkle et al. (2012), we speak of autoregressive and cross-lagged effects when referring to discrete time analysis, compared with auto-effects and cross-effects in continuous time analysis. We will use the terms *stability*, *lagged causal effect*, and *lagged reversed effect* when we make general statements that apply to both discrete and continuous time.

As noted earlier, taking these terms into account, an optimal time lag is the time lag that is required to yield the *maximum overall effect* of X predicting Y at a later time, while statistically controlling for prior values of Y. The optimal time lag will be derived in terms of the number of SUs required to yield the maximum overall indirect effect of X on Y.

It is noteworthy that the definition of an optimal time lag in terms of maximum effect size is different from previous conceptualizations. It has been implicitly assumed by many authors that the "exact point in time when the cross-lagged relationship is greatest" has to "coincide with the true time lag" (e.g., Sims & Wilkerson, 1977, p. 633). However, when the underlying process is continuous, the concept of a "true time lag" is not particularly meaningful. In a correctly specified model, a cross-lagged effect over any period Δt reflects the "true effect" for that specific time lag because it comprises all direct and indirect causal effects that have occurred during the lag. Therefore, our goal is not to identify a "true time lag" but to identify the optimal time lag in terms of maximum effect size.

Furthermore, it has sometimes been assumed that cross-lagged effects fade as a function of time (see J. Cohen et al., 2003). In the present article, it will be shown that cross-lagged effects may also increase as a function of time. Our goal is neither to augment the list of examples demonstrating that conventional wisdom can be wrong, nor to criticize authors for mistaken definitions or derivations of optimal time lags. Of course, there are many potentially useful definitions of optimal time lags. Similarly, there are potentially useful alternatives to the basic model shown in Figure 1, and there are different ways to represent causal relations. Other models—such as those with nonlinear causal effects (Sims & Wilkerson, 1977), or models with causal effects that vary between individuals (Cole & Maxwell, 2009)—and other definitions may be even more appropriate under certain circumstances. However, models like the one shown in Figure 1, which will be used to derive optimal time lags, are frequently found in the literature, and thus probably reflect common assumptions inherent in many psychological theories and in models used to test these theories. Similarly, the definition of optimal time lags in terms of maximum effect size is probably in line with most researchers' aims to find large effects, which are statistically significant. Therefore, the goal of the present article is to derive conclusions that can be used to improve the design of time lags for panel studies that will be analyzed with regression-based models. To accomplish this goal, we will show how optimal time lags can be calculated after we have discussed the concept of stability and stationarity, which are essential for the calculation and design of optimal time lags.

Stability Versus Test–Retest Correlation

As will be shown later, the stability of a variable is crucial in determining an optimal time lag. Many authors have conceptualized stability in term of test–retest correlations, which are not based on any particular causal model. For the purpose of deriving optimal time lags, stability is better defined as the effect that a variable has on itself across time (autoregressive effect or auto-effect). This model-based definition has some distinct advantages.

For example, the test–retest correlation of X_{t_0} and X_{t_2} in the D-Model is larger than the autoregressive effect i_{t_ω}. The test–retest correlation is also larger than $i^2_{t_\omega}$, which reflects the autoregressive effect of X_{t_2} on X_{t_0} via X_{t_1}. This is so because X_{t_0} has a further indirect effect on X_{t_2} via Y_{t_1}. Because variables are connected over time not only by their stabilities but also by other variables that contribute to their test–retest correlation, it is important to distinguish test–retest correlations from stabilities. Stabilities are usually lower than test–retest correlations.

The meaning of the stability parameters i_{t_ω} and d_{t_ω} is not easily determined. For example, if one considers wealth, and if the rich were to become richer and the poor were to become poorer over time, i_{t_ω} could be considered a direct causal effect of X_{t_0} on X_{t_1} in its literal sense. Even if substantive reasons for such direct effects are lacking, stabilities should be routinely considered (e.g., Finkel, 1995). In psychological, social, biological, and other human systems, extreme scores at Time 1 are usually less extreme at Time 2, which is known as regression to the mean (RTTM). RTTM is always present if the test–retest correlation of a variable is less than 1.0 and if its variance remains constant over time (cf. Finkel, 1995; Kessler & Greenberg, 1981). A RTTM is appropriately captured when a stability parameter is estimated; conversely, when the relation of a variable across time is analyzed, for most variables investigated in psychological research, a model without a stability parameter is misspecified (cf. Finkel, 1995; Gollob & Reichardt, 1987).

It is important to distinguish stability from test–retest correlations because only the stability of a variable "carries" causal effects over time. If a variable was completely unstable, any change imposed on this variable would completely decay immediately. Causal effects over a period of time can only be observed because the causal effect occurs with some delay (a causal lag), or because the stability of the variable is not zero, or both. Stabilities might be negative in some rare cases, but the present article is limited to positive stabilities because they are much more common.

Stationarity

The stability of a variable is given by the autoregression parameters of X or Y, which are i_{t_ω} and d_{t_ω}, respectively, in the D-Model in Figure 1 (and i_c and d_c in the C-Model). The sizes of i and d may vary over time; however, in the reminder of the present article, it is generally assumed that the stabilities between each pair of identically spaced time lags are identical. On the one hand, this assumption is made as an expedient to the mathematical derivations that follow. On the other hand, this probably reflects what is typical in most instances in which there is no substantive theoretical reason for supposing that these parameters might change across time (e.g., Voelkle et al., 2012).

The causal effects c_{t_ω} and the reversed effects r_{t_ω} in the D-Model (and the corresponding effects c_c and r_c in the C-Model) are assumed to be lagged. Again, the C-Model assumes that the substantive theory does not include changes in the causal effects over time. A model with invariant stabilities, invariant causal effects, and invariant residuals across time is a so-called stationary model (e.g., Kenny, 1979). With invariant residuals over time, the variance of X and Y may increase across time. This may apply if developmental processes are investigated, but it does not apply to most other variables investigated in the social sciences, which have variances that remain relatively constant over time (Kessler & Greenberg, 1981). Therefore, in this article, it is assumed that residuals may change over time. If they change to an extent that keeps that the variances of X and Y over time invariant, all stabilities and causal effects can be interpreted in standardized terms. This assumption is made through the remainder of the article, unless otherwise stated. If the reader prefers to assume that residuals are invariant across time, stabilities and cross-lagged effects should be interpreted as unstandardized effects. Although stationarity may not always apply, this assumption is made in most regression-based causal models (Cole & Maxwell, 2003). Without stationarity assumptions, optimal time lags can be probably derived for particular sets of stabilities and causal effects, but generalizations are difficult to make.

Calculating Optimal Time Lags

An optimal time lag is the lag that is required to yield the maximum effect of X predicting Y at a later time, while statistically controlling for prior values of Y. This definition corresponds to the cross-lagged regression coefficient in a regression analysis (or SEM) in which later Y_{t_ω} is predicted by prior Y_{t_0} with the regression weight $\beta_{Y_{t_\omega}Y_{t_0}}$ and prior X_{t_0} with $\beta_{Y_{t_\omega}X_{t_0}}$. Thus, $\beta_{Y_{t_\omega}X_{t_0}}$, which is visualized Figure 1C (F-Model), is an estimate of the overall cross-lagged effect of X_{t_0} on Y_{t_ω}.

In the following sections, we will derive the optimal number ω of SUs (the optimal discrete time lag) that yields the strongest overall cross-lagged effect ($\beta_{Y_{t_\omega}X_{t_0}}$). First, we derive the optimal discrete time lag for a simple D-Model without reverse causation. We then derive an estimate for the continuous C-Model. Finally, we extend the logic of these derivations to incorporate reciprocal causation.

Optimal Time Lags in One-Directional Discrete Time Models

To determine the optimal discrete time lag (i.e., the number ω of SUs), we have to determine the components of $\beta_{Y_{t_\omega}X_{t_0}}$. This can be quite complicated, and to facilitate understanding we begin with a simplified model in which the reversed causal effect is absent (i.e., $r_{t_\omega} = 0$). As already noted, we also make the assumption that all $i_{t_\omega} = i$, all $d_{t_\omega} = d$, and all $c_{t_\omega} = c$. Then, $\beta_{Y_{t_\omega}X_{t_0}}$ can be derived following the rules of path analysis (cf. Kenny, 1979). The sum of all direct effects of X on Y, that is, the sum of all traces that do not involve a correlation, is

$$\beta_{Y_{t_\omega}X_{t_0}} = i^0 c\, d^{\omega-1} + i^1 c\, d^{\omega-2} + i^2 c\, d^{\omega-3} + \ldots + i^{\omega-2} c\, d^{\omega-(\omega-1)} + i^{\omega-1} c\, d^{\omega-(\omega)} \tag{1}$$

Applying the general binomial formula yields

$$\beta_{Y_{t_\omega}X_{t_0}} = c\frac{d^\omega - i^\omega}{d - i}, \tag{2}$$

which shows that the sum of all effects of X on Y equals the causal effect c multiplied with a fraction; this fraction is composed of the stabilities of X (i) and Y (d).

The first derivative of $\beta_{Y_{t_\omega} X_{t_0}}$ with regard to ω is

$$\beta'_{Y_{t_\omega} X_{t_0}} = \frac{c(d^{\omega}\ln(d) - i^{\omega}\ln(i))}{d - i}. \quad (3)$$

This first derivative equals 0 if

$$\omega_{opt} = -\frac{\ln\left(\frac{\ln(d)}{\ln(i)}\right)}{\ln(d) - \ln(i)} \quad (4)$$

When SU stabilities for X and Y, i and d, are equal ($s = i = d$), then Equation 4 is not identified. Appendix A derives the proper formulas for this case.

Suppose we had determined the month-to-month SU stability in a pilot study (e.g., monthly sales, X, and salary bonus, Y), for example, a SU stability of $i = .83$ and $d = .82$ for a 1-month period (SU), then inserting these values in Equation 4 results in

$$\omega_{opt} = -\frac{\ln\left(\frac{\ln(.83)}{\ln(.82)}\right)}{\ln(.83) - \ln(.82)} = 5.1995.$$

Thus, the optimal time lag is 5.20 SU, that is, 5.20 months. If six waves of data (5.20 rounded) separated by monthly intervals are available for analysis, a model with five one-wave lagged effects can be tested. The overall indirect effect of monthly sales at Time 0 on salary bonus at Time 5 will become the largest of all possible overall indirect effects; time lags that span 4 months or less, or 6 months or more, will yield smaller overall indirect effects. Because collecting six waves of data is inefficient, collecting two waves separated by 5.20 months would be sufficient.

Note that the size of the causal effect c does not influence the optimal time lag. Similarly, the correlation at the onset of the causal process, that is, ρ_{Yt0Xt0}, does not affect the optimal time lag either. Thus, the optimal time lag does not depend on whether a causal system has just been initiated, or whether it has achieved a state of equilibrium, or whether it has begun dissolving for whatever reason. This also applies for the subsequently presented cases. Also note that when the effect $c = 0$, there is no optimal time lag because Equation 3 is then $\beta_{Y_{t_\omega} X_{t_0}} = 0$ for all possible time lags.

Figure 2 shows an asymmetric distribution of cross-lagged effect sizes (lagged standardized partial regression coefficients) for $c = .005$, .050, and .100, with $i = .99$, .98, and .90, and with $d = .995$, .97, and .90, respectively. In cases of superstable variables ($i = .99$ and $d = .995$; the dashed line in Figure 2), the issue of an optimal time lag is of less importance: The curve is relatively flat and wide. It is not until 3 years have gone by (if ω refers to a 1-day interval) that effects vanish. Superstable variables (i.e., $i \geqq 0.99$) are unlikely to be seen in practice; however, the example demonstrates that using long time lags exceeding 2 years may be inappropriate.

For slightly less stable variables, however, the situation changes dramatically. If $i = .98$ and $d = .97$ (the dotted line in Figure 2), a cross-lagged effect size would approach zero when the lag is approximately 350 SUs (e.g., 1 year if the SU is 1 day). Thus, if a researcher knows that X and Y have, for example, day-to-day (SU) stabilities that fall short of .98 (after correcting for measurement error), finding a cross-lagged effect spanning one year or more becomes a matter of chance.

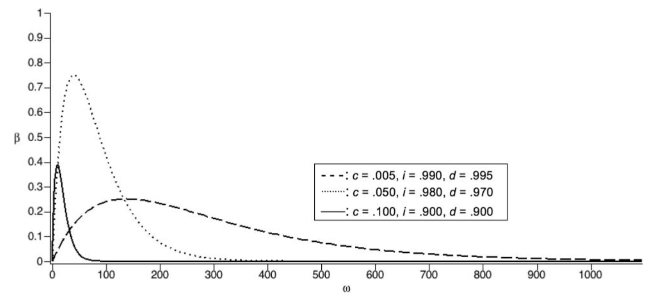

Figure 2. Effect sizes (cross-lagged regression coefficients of Y_{ω_i} on X_0, while controlling for Y_0) across time depending on the stability of the independent variable X (i), the stability of the dependent variable Y (d), and the cross-lagged effect of Y_{ω_i} on $X_{\omega_{i-1}}$ (c).

When variables become more unstable (e.g., $s = i = d = .90$; the solid line in Figure 2), the issue of optimal time lags becomes even more important. Figure 2 shows that time intervals, which are less than optimal, lead to a sharp decline in effect sizes. This decline is slightly more marked for time lags that are too short compared with time lags that are too long. For example, the optimal time lag in this case is 9.49 SU with $\beta_{Y_{t_{9.49}} X_{t_0}} = 0.3880$, and at 5 SU below the optimal lag $\beta_{Y_{t_{4.49}} X_{t_0}} = 0.3109$, whereas at 5 SU above the optimal lag $\beta_{Y_{t_{14.49}} X_{t_0}} = 0.3498$. Thus, because effect sizes are positively skewed, time lags that are too short lead to slightly stronger decreases in effect size compared with time lags that are too long.

Note that the equations for the optimal time lag (i.e., Equation 4 for $i \neq d$ and Equation 17 for $i = d$) may result in $\omega_{opt} < 1.0$. For example, when a pilot study has revealed identical stabilities for X and Y (i.e., $i = d$) that are lower than .3678, Equation 17 shows that the optimal time lag is shorter than the time lag used in the pilot study. For example, if $s = .30$ across 1 month,

$$\omega_{opt} = -\frac{1}{\ln(.30)} = .8306.$$

Thus, the optimal time lag is .83 SU, that is, .83 months, which is 24.92 days (assuming 30-day months). Although stabilities <.3678 are rarely reported in empirical studies, they do occur. For instance, the stability of mental distress among men was .30 in the 1-year-lag study by Mäkikangas and Kinnunen (2003). Stabilities typically become smaller if a model includes many causes of the Time 2 variable (e.g., overall, 15 causes of distress at T1 in the study of Mäkikangas & Kinnunen, 2003). As Coleman (1968) already asserted, "as the formal system becomes more complete, this coefficient should approach zero" (p. 441). Note, however, that Coleman did not claim that the stability of a variable is entirely made up by stable underlying third variables. He also viewed the stability coefficient as a placeholder for mediators that cause a negative feedback loop over time, which we usually observe as the RTTM. A further reason why low stabilities are rare in the literature is measurement error related to omitted third variables, because "omitted variables in panel models may lead to *autocorrelation* in the endogenous variable's error term over time. This in turn produces . . . inconsistent OLS estimates of the effects of Y_{t-1} on Y_t" (Finkel, 1995, p. 229; italics in the original).

Overall, there is good reason to believe that stabilities typically reported in the literature are biased upward.

Optimal Time Lags in One-Directional C-Models

Thus far we have used estimates of the stabilities based on an SU. Of course, the lag that is assumed in the SU affects the stabilities obtained. In C-Models, there is no SU. Thus, any estimate must be made with reference to the real time difference on which it is based. Because the time lag in C-Models can take any set of values, a time lag may be infinitesimally short. The change in a variable across an infinitesimally short interval is the derivative. This change, either from its counterpart earlier in time or from its cause earlier in time, can be predicted using so-called drift parameters (cf. Voelkle et al., 2012). To discuss this issue in a more compact way, we use matrix notation. Instead of X and Y, we now use x_1 and x_2; $\mathbf{x}(t)$ no longer represents a single variable at Time t, but a 2×1 vector of variables. The derivative of $\mathbf{x}$ can be predicted by a so-called drift matrix $\mathbf{A}$ with 2×2 dimensions:

$$\mathbf{A} = \begin{pmatrix} i_c & r_c = 0 \\ c_c & d_c \end{pmatrix},$$

in which the diagonal elements are the auto-effects and the off-diagonal elements are the cross-effects. Because we still assume there is no reversed lagged effects, $r_c = 0$. This leads to a (nonstochastic) differential equation:

$$\frac{\mathrm{d}\mathbf{x}(t)}{\mathrm{d}t} = \mathbf{A}\mathbf{x}(t) \qquad (5)$$

Fortunately, this is a well-known differential equation, and the only function that satisfies this equation is

$$\mathbf{x}(t) = e^{\mathbf{A}(t-t_0)}\mathbf{x}(t_0). \qquad (6)$$

Equation 6 can now be used to express the exact relation between discrete and continuous time (for details see Voelkle et al., 2012) as follows:

$$\mathbf{A}(\Delta t_i) = e^{\mathbf{A}\cdot\Delta t_i} \qquad (7)$$

where $\mathbf{A}(\Delta t_i)$ is the matrix relating the outcome variables over time and contains the autoregressive effects in the main diagonal and cross-lagged effects in the off-diagonals, and $e^{\mathbf{A}\cdot\Delta t_i}$ is the matrix exponential of the drift matrix $\mathbf{A}$ multiplied by the change in time. The matrix exponential provides expressions, in which the continuous time drift coefficients are expressed as functions of i, d, and c. These are as follows (with the subscript "c" indicating the continuous drift parameters and letters without subscript the discrete stabilities and causal effects):

$$i = e^{i_c}, \qquad (8)$$

$$d = e^{d_c}, \qquad (9)$$

$$c = c_c \frac{e^{d_c\cdot\Delta t_i} - e^{i_c\cdot\Delta t_i}}{i - d} \qquad (10)$$

For example, if the continuous time parameters are $c_c = .15$, $i_c = -.19$, and $d_c = -.20$ for 1 week, the corresponding 1-week discrete time parameters are $c = .12$, $i = .83$, and $d = .82$, and, for example, the corresponding 4-week discrete time parameters are $c = .28$, $i = .47$, and $d = .45$. Note that drift parameters for stabilities in continuous time are usually negative, and the more negative they are, the lower is the stability of the variable. Also note that one could use continuous time parameters to extrapolate discrete time parameters for any time lag. However, correctly estimating continuous parameters using available discrete time parameters requires simultaneous estimation of continuous time and F-Models (for details, see Voelkle et al., 2012).

The derivative of the Equation 10, in which Δt_i refers to the real time difference between two possible measurement occasions, equals zero if

$$\Delta t_{opt} = -\frac{\ln\left(\frac{d_c}{i_c}\right)}{d_c - i_c}, \qquad (11)$$

whereas for the discrete case in which ω referred to the number of SU, it was

$$\omega_{opt} = -\frac{\ln\left(\frac{\ln(d)}{\ln(i)}\right)}{\ln(d) - \ln(i)}.$$

The optimal time lag now is obtained by inserting the continuous time stabilities into Equation 11:

$$\Delta t_{opt} = -\frac{\ln\left(\frac{-.19}{-.20}\right)}{-(-.19) - (-.20)} = 5.1995.$$

For the D-Model using $i = .83$ and $d = .82$, we also obtained that the optimal time lag is 5.1995 weeks. Both approaches yield identical values for optimal time lags at the population level. Although the C-Model requires no assumption about the lags between variables, in practice, we must assess individuals with a certain lag, regardless of the choice of underlying models. Using an D-Model, rather than a C-Model, to estimate parameters and to calculate optimal lags also has some additional advantages. Estimation problems, in particular with small sample sizes, are likely to be less of a problem when using the D-Model, and parameter estimates are probably more efficient. Although we are not aware of an elaborated investigation of the statistical properties of the estimators, our reanalysis of the examples used by Voelkle et al. (2012) revealed that the ratios of the estimates and their associated standard errors were larger for discrete estimates.[1]

Optimal Time Lags in Models With Reciprocal Effects

In panel analyses, authors are frequently interested in possible reciprocal effects between X and Y. So far, we have assumed no causally reversed effect of X on Y, that is, all $r_{t_\omega} = 0$. This was in order to facilitate understanding by simplifying the calculations. In addition, models with reciprocal effects generate some problems for discrete time models. Consider Figure 3, which depicts a

[1] We thank an anonymous reviewer for suggesting these possible advantages of using the D-Model.

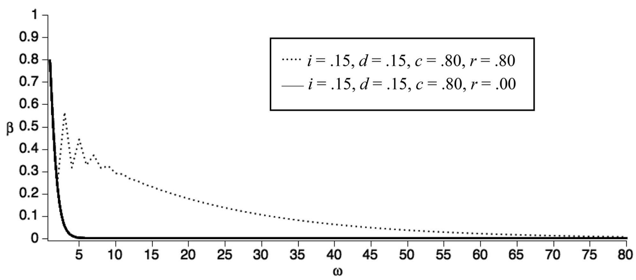

Figure 3. Example of erratic effect sizes (cross-lagged regression coefficients of Y_{ω_i} on X_0, while controlling for Y_0) in models with reciprocal causation, in which cross-lagged effects exceed autoregressive effects. In this example, the stability of the independent variable X is $i = .15$, the stability of the dependent variable Y is $d = .15$, the cross-lagged effect of Y_{ω_i} on $X_{\omega_{i-1}}$ is $c = .80$, and the reversed cross-lagged effect of X_{ω_i} on $Y_{\omega_{i-1}}$ is $r = .80$ (dotted line) and $r = .00$ (solid line).

reciprocal process with very low stabilities (.15) and very strong causal effects (.80). These parameters cause erratic, saw-toothed variations in effect sizes during the first couple of time lags. This occurs because the effects of X_{t_0} on Y_{t_ω} are only strong when ω is odd, because then the trace between X_{t_0} and Y_{t_ω} does not mainly go along the (weak) stability paths, but also involves the (strong) *c* and *r* paths in a zig-zag pattern. Although not impossible, we believe most variables in psychology and the social sciences are likely to have higher stabilities across short time intervals compared with their causal impact on other variables. Such erratic behavior is probably limited to very short time intervals and to very few content areas. Nevertheless, the example in Figure 3 shows that there is no general "real" solution of possible optimal time lags. Therefore, the closed-form solutions provided subsequently could imply optimal time lags that are "complex" rather than "real" numbers, but complex numbers will usually not result if causal effects are smaller than stabilities. Also, Figure 3 shows that the optimal time lag in reciprocal systems in which $c_c > 0$ and $r_c > 0$ is longer than in unidirectional systems in which $r_c = 0$.

For C-Models, we demonstrated that the derivation of optimal time lags required determining the root of the first-order derivative of the matrix exponential of the drift matrix. This is still valid if reciprocal effects exist, that is, $r_c \neq 0$. The optimal continuous time lag then is

$$\Delta t_{opt} = \frac{\ln\left(\frac{1}{2}\,\frac{d_c\sqrt{d_c^2 - 2d_ci_c + i_c^2 + 4r_cc_c} + i_c\sqrt{d_c^2 - 2d_ci_c + i_c^2 + 4r_cc_c} - d_c^2 - 2r_cc_c - i_c^2}{r_cc_c - d_ci_c}\right)}{\sqrt{d_c^2 - 2d_ci_c + i_c^2 + 4r_cc_c}}. \tag{12}$$

Compared with models with unidirectional effect, the optimal time lag no longer depends solely on the stabilities i_c and d_c; the sizes of the causal effects c_c and r_c also matter. This is because in reciprocal systems, c_c and r_c, do not only carry a causal effect from one point in time to the next; rather, they carry a causal effect to infinity. Note that the optimal time lag to estimate the effects of X on Y, or of Y on X, is identical because *c* and *r* in Equation 12 are tied together in product terms.

Because calculating the continuous drift parameters is a relatively recent development, which cannot be carried out using the most common SEM computer programs, we also provide the formulas for determining optimal discrete time lags based on the SU of the D-Model. Compared with solving the matrix exponential of the drift matrix, and determining the Δt for which the matrix exponential has a root, for continuous time parameters, in the discrete time case one has to determine the matrix power of the discrete empirical coefficients, and determine for which ω it has a root. As with C-Models, the optimal discrete time lag for *c* and *r* is identical. This root is

$$\omega_{opt} = -\frac{\ln\left(\dfrac{\ln\left(\frac{1}{2}d + \frac{1}{2}i + \frac{1}{2}\sqrt{d^2 - 2di + i^2 + 4cr}\right)}{\ln\left(\frac{1}{2}d + \frac{1}{2}i - \frac{1}{2}\sqrt{d^2 - 2di + i^2 + 4cr}\right)}\right)}{\ln\left(\frac{1}{2}d + \frac{1}{2}i - \frac{1}{2}\sqrt{d^2 - 2di + i^2 + 4cr}\right) - \ln\left(\frac{1}{2}d + \frac{1}{2}i + \frac{1}{2}\sqrt{d^2 - 2di + i^2 + 4cr}\right)}. \tag{13}$$

Thus far, we have shown how estimates of optimal time lags for correctly specified continuous and discrete time models can be calculated. We have limited ourselves to models with only two variables that are unidirectionally or reciprocally related across time. In principle, the same approach applies to more complex models. However, the complexity may increase exponentially if further variables are added to the model. Formulas to calculate optimal time lags become particularly unwieldy if further variables are involved that are reciprocally related to X and Y. Fortunately, if such third variables cause X or Y in a unidirectional fashion only, Equation 13 is still valid. This is because unidirectional third variables do not carry forward the causal effects between X and Y.

Note that Equations 12 and 13 remain valid only if the C-Model and D-Model, respectively, are correctly specified; that is, if all relevant third variables are actually included in the models. For instance, suppose X is the number of hassles experienced by children and Y is their level of depression. If the number of siblings affect X, and genes affect Y, and the effects of number of siblings and genes are explicitly modeled in the C-Model and in the D-Model, then the estimates obtained of c_c, d_c, i_c, and r_c can be inserted into Equation 12 (and c, d, i, and r into Equation 13) to calculate the optimal time lag. If the number of siblings and genes are excluded from the C-Model and the D-Model, inserting the estimates of stabilities and causal effects into Equations 12 and 13 would yield biased calculations of the optimal time lag.

Designing Optimal Time Lags

A key goal of the present research is to provide guidance on the design of optimal time lags. In many cases, neither discrete nor continuous parameter estimates are known before a study has been carried out. Fortunately, because increasing numbers of repeated-measure studies are being published, researchers now have a better chance of finding examples that have used repeated measures of the variables that they wish to study. One could use the reported coefficients and then calculate optimal time lags for a new study. However, there are potential problems that should be considered when using prior research to calculate optimal time lags, which we discuss in the next section.

Using Existing Data to Calculate Optimal Time Lags

To demonstrate calculation of optimal time lags using existing data, we use data reported by Steca et al. (2014), who conducted a study to investigate whether children develop depressive symptoms when facing daily hassles. We leave out the moderators they investigated, and focus on the cross-lagged relations between depression and hassles. Using a sample of 554 male children aged 7 to 9 years, the authors used four waves of data collection (Time 0 to Time 3). The time lag between each pair of subsequent measurement occasions was 2 months. The correlations reported are shown in Table 1.

In setting up a D-Model, we started by using data from Time 0 to Time 1. We obtained the following discrete time parameter estimates (stabilities in the diagonal, casual and reversed cross-lagged effect in the off-diagonal, where *** $p < .001$):

$$\mathbf{B}_{(\Delta t=2months,\, t_0=Time0)} = \begin{pmatrix} .530^{***} & .033 \\ .159^{***} & .597^{***} \end{pmatrix}.$$

Using Equation 13 yields the optimal time lag in terms of a multiplier of the SU:

$$1.7485 = -\frac{\ln\left(\dfrac{\ln\left(\frac{1}{2}.53 + \frac{1}{2}.60 + \frac{1}{2}\sqrt{.53^2 - 2\cdot .53 \cdot .60 + .60 + 4 \cdot .16 \cdot .03}\right)}{\ln\left(\frac{1}{2}.53 + \frac{1}{2}.60 - \frac{1}{2}\sqrt{d^2 - 2\cdot .53 \cdot .60 + 60^2 + 4\cdot .16 \cdot .03}\right)}\right)}{\ln\left(\frac{1}{2}.53 + \frac{1}{2}.60 - \frac{1}{2}\sqrt{.53^2 - 2 \cdot .53 \cdot .60 + .60^2 + 4 \cdot .16 \cdot .03}\right) - \ln\left(\frac{1}{2}.53 + \frac{1}{2}.60 + \frac{1}{2}\sqrt{.53^2 - 2\cdot .53\cdot .60 + .60^2 + 4\cdot .16\cdot .03}\right)}$$

Thus, the optimal time lag is 1.7485 times the SU for an SU of 2 months. Because $2 \times 1.7485 = 3.497$ is closer to 4 months than to the 2-months lag realized in the D-Model, we used an F-Model comprised of Time 0 and Time 2 data, which were separated by 4 months. We obtained the following discrete parameter estimates (where *** $p < .001$):

$$\mathbf{B}_{(\Delta t=4months,\, t_0=Time0)} = \begin{pmatrix} .353^{***} & .015 \\ .176^{***} & .434^{***} \end{pmatrix}$$

Indeed, the effect of hassles Time 0 on depression Time 2 is now .176 and .017 larger than in the initial D-Model, which used an SU of 2 months. Unexpectedly, the reverse effect of depression Time 0 on hassles Time 2 decreased by .018 to .015.

Assuming a correctly specified model, our conclusion is that the optimal time lag for the effect of hassles on depression is indeed between 2 and 4 months. This conclusion is consistent with the calculated optimal lag of 3.497, and with the increase in the lagged effect from .159 to .176 for the effect of hassles on depression. In fact, the effect of .176 was very close to expectation, which is .1809 (this value results from taking the matrix power of the discrete empirical coefficients, which we did not show for reasons of space). In Figure 4, we show the expected effect sizes of the lagged and reversed lagged effect over time, based on the results

Table 1
Correlations (N = 554) of Depression and Hassles From Time 0 to Time 3 Reported by Steca et al. (2014)

No.		1	2	3	4	5	6	7	8
1	Depression T0	1							
2	Depression T1	.59	1						
3	Depression T2	.42	.67	1					
4	Depression T3	.49	.62	.70	1				
5	Hassles T0	.38	.36	.31	.29	1			
6	Hassles T1	.26	.39	.28	.27	.61	1		
7	Hassles T2	.18	.29	.34	.31	.44	.59	1	
8	Hassles T3	.16	.26	.24	.29	.35	.50	.61	1

Note. T0 = Time 0; T1 = Time 1; T2 = Time 2; T3 = Time 3.

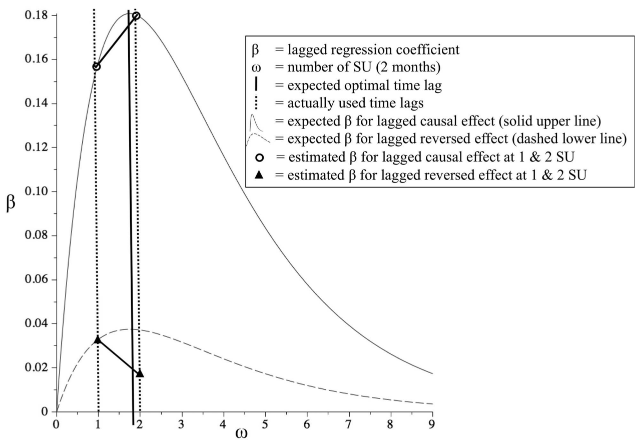

Figure 4. Observed lagged causal effects of hassles on depression (circles) and of depression on hassles (triangles) from Time 0 to Time 1 (left circle/triangle) and from Time 0 to Time 2 (right circle/triangle) using correlations reported by Steca et al. (2014) and controlling for Time 0 counterparts of the dependent variable. Dashed and solid lines represent the expected effects based on extrapolating from the analysis of Time 0 and Time 1 data. Further information is provided in the text.

from the initial D-Model. Note again that the formulas to compute the expected effects sizes are not reported in the present article for reasons of space.

The declining effect of the reverse path from depression to hassles contrasts with the increasing effect from hassles to depression, yet can also be consistent with an optimal time lag between 2 and 4 months. However, the size of the lagged reverse effect from depression to hassles did not confirm expectations. This result might tempt researchers to conclude that the optimal time lag for effects of hassles on depression differs from the optimal time lag for the reverse path. Note, however, that Equation 13 shows that for correctly specified models, the optimal time lag is identical for both effects. The problem, rather, is the assumption of correct model specification. We will discuss this issue in the next section.

Up to this point, we conclude that a 2-month lag was too short for hassles to have its strongest effect on depression, whereas the 4-month lag was too long for depression to have its strongest effect on hassles. Therefore, the optimal time lag to investigate the impact of hassles on depressive symptoms among children aged 7 to 9 years might be between 2 and 4 months.

Issues With Using Existing Data

The previous analyses show how existing data might be used to investigate optimal time lags. However, our calculations were based on assumptions that are probably not appropriate in most practical applications. In particular, measurement error, restrictions in variance, and unmeasured third variables are likely to influence the observed correlations that we used. In this section we explore how these factors might influence the estimation of optimal time lags.

First, measurement error has to be taken into account. Measurement error attenuates relations among variables. Therefore, ideally, one should take parameter estimates from latent variable structural equation models reported in the literature. Unfortunately, a matrix covering the correlations of all latent constructs is usually not reported in published studies. Alternatively, one could use reported correlations and reliability estimates, and then estimate an initial D-Model using the disattenuated correlations, although this approach has some shortcomings (e.g., Schmitt, 1996).

Second, taking parameter estimates from an arbitrary sample to derive optimal time lags is conceptually problematic because parameter estimates depend on sample characteristics. Correlations will be reduced if the variances of the variables in a given sample are restricted. Hence, whether or not parameter estimates reported in the literature are useful depends on the degree to which variance restrictions are comparable.

Third, we argue later that many studies reported in the literature use time lags that are too long, and using these parameter estimates to compute the optimal time interval for one's own study will usually lead to further exaggerated time lags. For example, suppose we have used Time 0 and Time 3 data from the Steca et al. (2014) study, which are separated by 6 months, for estimating a D-Model. The discrete time coefficients here are (where *** $p < .001$, ** $p < .01$)

$$\mathbf{B}_{(\Delta t=6months,\, t_0=Time0)} = \begin{pmatrix} .444^{***} & .032 \\ .121^{**} & .338^{***} \end{pmatrix}$$

Notably, inserting these values in Equation 13 yields an optimal time lag that is longer than the 6 months (6 months × 1.0576 = 6.3456 months). Thus, the calculated optimal time lag increases with the length of the SU; it was 3.497 months when using an SU of 2 months, but 6.3456 months when using an SU of 6 months.

This longer period for the optimal time lag could occur if the D-Model is not correctly specified. In particular, it is likely that the stabilities of hassles and depression are influenced by causes that are not included in the model. Earlier, we speculated that the number of siblings might affect hassles, and that genetic determinants might affect depression. Unfortunately, these variables were not measured, or not reported, by Steca et al. (2014). However, one could model unmeasured third variables and then reexamine the stabilities and cross-lagged effects. This model is shown in Figure 5. The abbreviations UTV_X and UTV_Y represent

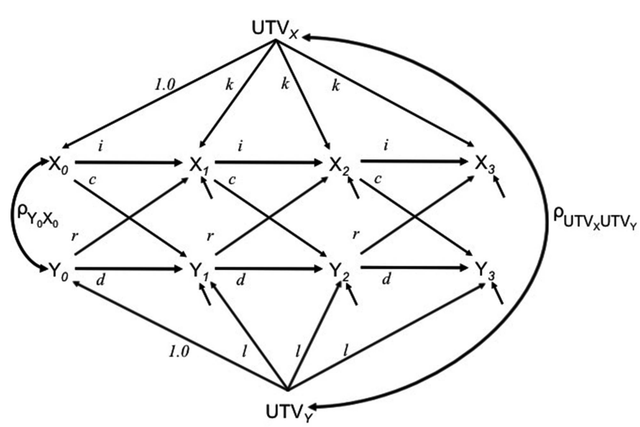

Figure 5. A discrete time model of X and Y across four time points with two correlated unmeasured third variables (UTVs), which reduce the stability estimates of X (*i*) and Y (*d*) compared with a model without unmeasured third variables.

unmeasured causes of X and Y, respectively, which might be correlated. Applying the model shown in Figure 5 (an Mplus syntax file can be found in Appendix B) to the Steca et al. data shown in Table 1, we obtained

$$\mathbf{B}_{(\Delta t=2 months,\ \text{UTVs included})} = \begin{pmatrix} .211^{***} & -.051 \\ .019 & .372^{***} \end{pmatrix}.$$

The stabilities are now much lower than before, and the cross-lagged effects both fail to become significant. The model fit was quite good (χ^2 = 34.96, df = 21, p < .029, RMSEA = .035, CFI = .993), and was significantly better than its counterpart without the two unmeasured third variables involved ($\Delta\chi^2$ = 93.65, Δdf = 5, p < .001). If the two stabilities i = .211 and d = .372 were used as fixed parameters in the counterpart model, χ^2 increases significantly ($\Delta\chi^2$ = 500.82, Δdf = 2, p < .001); thus, the stabilities estimated in the model with unmeasured third variables are significant lower than the values obtained from the first analyzed D-Model.

If the values for i = .211 and d = .372 are inserted into Equation 13, the optimal time lag is calculated as .8012 times the SU of 2 months, which is 1.6024 months, and therefore shorter than the shortest time lag available in the study by Steca et al. (2014). This and other examples in the literature (e.g., Dormann, 2001; Ormel & Schaufeli, 1991) show that stability estimates will be biased upward and, importantly, the upward bias increases with increasing time lags.

To summarize thus far, there are several limitations when using existing data to calculate optimal time lags: (a) correlations based on perfectly reliable (latent) variables are frequently not available; (b) possible range restriction in existing data limits the ability to extrapolate optimal time lags to a planned study; (c) most extant studies use rather long time lags, which are likely to provide stability estimates that are biased upward; and (d) only few extant studies use multiple waves that allow unmeasured third variables to be included when estimating stabilities and cross-lagged effects (the structural models are not algebraically identified when two waves are available only). Therefore, conducting a pilot study with rather short time lags for the initial D-Model could be a useful alternative to using existing data. We describe this approach as a "shortitudinal study" because we want to stress that panel studies with shorter time lags than usually applied could reveal important information about the unfolding of psychological processes over time, and about the optimal time lag for the process under study.

Discussion

We addressed the unresolved question of optimal time lags in panel studies. Our presentation of optimal lags leads us to recommend greater use of shortitudinal research. Some aspects of this recommendation perhaps seem counterintuitive, so we emphasize that the optimal time lag is one that will detect the *maximum effect size* across two waves of measurement.

Over time, a continuous causal process produces both increasing and declining effect sizes. Previous simulations (e.g., Cole & Maxwell, 2003; Sims & Wilkerson, 1977) and algebraic analyses (Voelkle et al., 2012) clearly demonstrated the distribution of effect sizes across time. The present article extends previous research by providing a general solution to the problem of how to calculate when the discrete and continuous effect becomes strongest in time. Interestingly, it turned out that in unidirectional causal models, the optimal time lag does not depend on the magnitude of the cross-lagged effect, but only on the stabilities of X and Y.

Second, when reciprocal relations between X and Y across time are considered, the results also confirm that the shape of the distribution of total cross-lagged effects is similar to that for unidirectional effects. The existence of reciprocal relations could make it easier to demonstrate empirically a causal effect in either direction. This is because the overall cross-lagged effects are always equal to or greater than those which would be observed if only unidirectional effects were operating. Reciprocal relationships "stabilize" a causal system by producing a better spread of causal effects. This stabilization has another side effect: When reciprocal effects exist, the optimal time lag is slightly longer compared with systems with unidirectional effects. Interestingly, if models are correctly specified, the optimal time lag for an effect of X on Y is identical to the optimal time lag for an effect of Y on X.

It should be noted that the optimal time lag might differ from the time it takes for a dependent variable to reach its maximum level when individuals are exposed to an independent variable. For example, a study of the link between exposure to a work stressor and subsequent reporting of depressive symptoms in a sample of employees would be expected to show a distribution of increasing, then decreasing, effect sizes over time, depending on the time of measurement. This result is distinct from the answer to the question of when the accumulation of work stressors and symptoms might result in a depressive disorder. This latter effect is a question of optimal time lags for estimating unknown hazard rates within a given cohort (e.g., Inoue & Parmigiani, 2002), but it is not related to the distribution of effect sizes across time, which captures the cause–effect relations that we are interested in.

The optimal time lag might also be different from the time one should allow to pass in order to make a precise prediction of future states (e.g., the exact level of stressors and stress symptoms). This is a question of optimizing forecasts (cf., Prasolov, 2001) by using several reasonable past variables to predict future variables, but does not address the question of when the effect of one particular past variable X reaches its maximum value in predicting future Y. Our study complements these questions by addressing the different, but important, question of the maximum lagged effect of X on Y.

Our call for researchers to use shorter time lags implies that the common lag of 1 year should be supplemented with shortitudinal studies of much shorter time lags. Again, this recommendation may seem counterintuitive, based on the observation that many 1-year stabilities fall between .40 and .70, and inserting these values in Equation 17 would yield estimates of 1.09 and 2.80 years. Although this result seems to be at odds with the need for shorter time lags, these stabilities do not take account of the upward bias in stabilities because of factors such as unmeasured third variables. Our analyses show that these factors add greatly to the observed stability values.

In addition, we believe that in the applied psychology literature, many substantive changes can be observed over reasonably short time frames. For example, many cause–effect relationships between work experiences and work attitudes might not take long to be expressed, but are likely to be obscured because they are very small.

Therefore, we suggest greater attention be given to shorter time lags in research that applies common longitudinal statistical methods (e.g., hierarchical regression analysis or SEM). This plea is not to be confused with a call for more diary or event sampling studies: These are worthy in their own right, but they tend to analyze cross-sectional effects (within individuals) and are based on a quite different concept of change. There is great potential, however, for diary studies to be useful when between-subjects analysis is applied. This is because diary studies employing short time intervals may be better suited for demonstrating cross-lagged effects than panel studies covering several months or years.

The call for more shortitudinal research need not be made for all areas of research. Certain disciplines, however, are particularly inclined toward using long time lags, and these disciplines might benefit from the present analysis of optimal lags. In particular, research in basic psychology applies quite short lags, whereas time intervals in industrial and organizational psychology research are usually very long. As an example, Glasman and Albarracín's (2006) meta-analysis of basic research into attitude–behavior relations reported time lags of less than one day (46 out of 51 studies). By contrast, the average time lag in Steel and Ovalle's (1984) meta-analysis of applied research into the relationship between behavioral intention and turnover was approximately two years. The stability of intention to quit and turnover, however, may not be very high even across shorter time lags. Therefore, we believe that it becomes likely that long lags yield noticeable cross-lagged effects only by chance, or because third variables are left out from analysis. Then, long time lags are perhaps also more likely to provide results that are contrary to theory, such as in a recent meta-analysis by Swider and Zimmerman (2014), in which the generally negative sign of the cross-lagged effect of performance on withdrawal behavior turned into a positive one when studies using time lags of 13 or more months were analyzed.

Compared with social sciences, other disciplines such as biology or chemistry have invested much more effort to determine optimal time intervals between assessments of independent and dependent variables (e.g., Cole & Maxwell, 2003). As we have shown for regression-based panel analysis, assessing the optimal time interval requires determining the stabilities for the variables under study in unidirectional causal systems, and of the (reversed) causal effects in reciprocal systems. Therefore, we are convinced that appropriately chosen, optimal time lags based on accurate, preliminary estimates of stabilities and causal effects, are most promising in demonstrating lagged effects of X on Y. The suggested D-Model to estimate stabilities and causal effects is quite easy to test, but it is probably too simplistic. More complex models that would yield more accurate stability estimates could be used when multiwave data are available, as we also demonstrated (cf. Kenny & Zautra, 1995, 2001).

Limitations

Our presentation of optimal time lags is based on assumptions that might limit application to other research contexts. An important assumption was that the model was correctly specified. For example, third variables that affect X and Y have to be included and statistically controlled for. The presence of a third variable Z does not influence the optimal time lag for finding an effect of X on Y if it is appropriately included in analyses, because estimates of stabilities and (reversed) causal effects will then be unbiased. However, leaving out third variables from analyses will certainly yield biased estimates and, therefore, biased calculation of the optimal time lag.

As noted in the introduction, another general assumption underlying all models discussed was that causal effects generally are linear. It was assumed that an increase and a decrease in an independent variable had identical effects on the dependent variable. Therefore, theoretical models that include cumulative effects were beyond the scope of the present article. In the future, such models need to be examined in more detail.

In the present article, stationarity was assumed, that is, invariant stabilities and causal effects across time. Thus, the findings of the present study may not apply to developmental processes in which stabilities and causal effects may change across time. Indeed, stationarity is a strong assumption, but it can be tested if multiple waves of data are available for analysis (e.g., Kenny & Zautra, 1995), ideally each separated by optimal time lags.

Furthermore, only variables without measurement errors were considered. Measurement errors should be corrected whenever possible, for instance, by modeling latent variables.

Our approach to the calculation of optimal time lags is directed to between-subjects regression-based designs. An extension to within-subject designs in which person-specific optimal time lags are determined was beyond the scope of the present article. Such an extension is more straightforward for multilevel models, in which time is implicit, than for approaches such as growth curves, in which time is explicit in the model (cf. Voelkle et al., 2012). We suspect this extension will reveal very short optimal time lags for most persons, based on our observation that stabilities are typically rather low in such studies. For example, the (unstandardized) stability for affect balance between the evening and the next morning was .26 in a study by Wrzus, Wagner, and Riediger (2014); for negative affect in the morning and before bedtime, it was .10 in a study by Dudenhöffer and Dormann (2013); and for positive mood after returning home from work and before bedtime, it was .26 in a study by Sonnentag and Bayer (2005). These low stabilities probably result from within-person centering; without this centering, longer optimal time lags might result. Future research is needed for a more informed answer to this question.

Implications and Recommendations

Because of the problems associated with using existing studies and their estimates, we recommend that researchers estimate the parameters of the D-Model themselves. In particular, we suggest the following steps for designing a shortitudinal pilot study:

1. Based on conceptual considerations, estimate a reasonably short interval across which a change in X and Y can be expected. This interval (the SU) will be the lag between Time 0 and Time 1 in the D-Model (see Figure 1B).

2. Consider important third variables (Z) that may impact on X and Y, and include them in the D-Model, too. This is, of course, always important in order to obtain unbiased estimates; it is also important for unbiased calculations of optimal time lags.

3. Randomly select a reasonably sized subsample from the target sample (i.e., the participants sampled at Time 0) to gather Time 1 data in a shortitudinal pilot study.

4. Estimate the D-Model using discrete time modeling. Furthermore, use measurement models to account for measurement error, because without them, structural relations may be biased. Also include error autocorrelations among errors of observed indicators—omitting them usually leads to overestimated stability estimates.

5. Insert the estimated parameter values in Equation 13 (for discrete time parameters) to determine the optimal ω_{opt} (i.e., optimal number of SUs). Based on this result, determine how much time you have to wait until Time 2 data should be gathered for testing the F-Model (see Figure 1C). Alternatively, if *c* (and *r*) is already significant using the initial D-Model, you may consider stopping here and publishing the results.

The sample size of the pilot and main studies should carefully follow the guidelines suggested by Kelley and Maxwell (2012). Researchers' intuition might be to use a small sample size for the pilot study (D-Model), then follow up with a main study (F-Model) using a larger sample. However, parameter estimates obtained from the initial D-Model will be imprecise because of the sampling error when only a small sample is used. A larger sample size for the pilot study will produce more accurate estimates of parameters for calculating the optimal time lag, which should then allow estimation of larger cross-lagged effects in the F-Model. Thus, for testing the F-Model a smaller sample size might be feasible compared with an F-Model without a shortitudinal pilot study.

We identify three further implications for empirical studies when repeatedly gathering data over time. First, researchers should provide not only the estimates of the effect sizes obtained, but also information concerning the exact time that Y was measured following the measurement of X. This is highly important because there is no such thing as a time-invariant effect size (e.g., Cole & Maxwell, 2009, 2003; Gollob & Reichardt, 1987). Ideally, researchers of future panel studies would use continuous time modeling, and report their estimated drift parameters, because they can then be more easily integrated in possible meta-analyses of cross-lagged effects.

Second, the logic of our approach extends directly to studies with more than two time points. In such cases, we suggest using varying time lags between the different measurement occasions, ranging from shorter to longer intervals. Of course, different discrete stabilities and discrete cross-lagged effects will then be estimated for the different time lags. Voelkle et al. (2012) have shown how to estimate the different discrete effects as functions of a single set of two underlying continuous auto-effects (i.e., stabilities) and two cross-effects (i.e., causal effects). Inserting the estimated continuous auto-effects and cross-effects in Equation 12 will yield the optimal time lag. The result from this calculation might explain why some of the discrete effects were small, and perhaps not significant, if they spanned measurement occasions that were not optimally spaced in time.

Third, researchers should clarify the theoretical and empirical time frame over which concepts relate to each other (see also Cole & Maxwell, 2003; Collins, 2006; Finkel, 1995). Problems arise when a construct and its measures do not align with the general time frame of relationships. For example, if overlap across measurement occasions exists, then a variable, which ought to be regressed on its *counterpart* at an earlier point in time, is rather partly regressed on *itself* (cf. Frese & Zapf, 1988). In this case, a clear interpretation of what has really changed between measurements becomes impossible. More importantly, without a clear reference to time when defining constructs and developing their measures, stabilities of the variables will vary. Consequently, effect sizes will also vary, as will cross-lagged effects, making it impossible to compare validly results obtained from different panel studies. Similarly, problems ensue for integrating findings from different panel studies using meta-analysis.

A concern that arises from these implications relates to the asymmetric distribution of effect sizes. It is possible that a shortitudinal pilot study might produce relatively small effects, and if the effect is not significant, researchers might decide that it is not worth collecting a further wave of data at the optimal time point. This situation is most likely when the stabilities of the variables are rather low, producing a sharp decline in effect sizes around the optimal time lag. Here, time lags that are too short are slightly more likely than time lags that are too long to yield effect sizes that are very low and not significant.

Despite this concern, there are two reasons why the time lag in the pilot study should ideally be short. First, given low stabilities, the likelihood of conducting a panel study that yields significant cross-lagged effects is—in absolute terms—rather low, because of the sharp decline of effect sizes around the optimal time lag. Second, if a researcher finds no significant cross-lagged effect in a shortitudinal pilot study, an extrapolation of the maximum effect that can be expected after an optimal time lag is possible. If this is still quite low, it might indeed be worth thinking about not doing another follow-up. However, if the pilot study was short enough, one can expect the effect sizes to increase thereafter.

A final consideration is the question of what makes a time lag in a shortitudinal pilot study too short. This question cannot be answered without referring to theoretical reasons (cf. Collins, 2006). We believe that, for many psychological variables, a time lag shorter than 1 day is indeed too short. Within a single day, several processes may occur that are unrelated to the substantive change process being investigated. For example, researchers interested in the effect of exposure to work-related stressors on the development of stress symptoms have to consider work timetables and shift patterns that mean intensity of a stressor at Time 0 is not a very good predictor of the intensity of a stressor 15 hr later when work has finished. Consequently, across a time lag of 15 hr or so, the stability of the stressor is rather low, which would imply a very short optimal time lag. However, theoretically, it is assumed that stressors during each person's workdays cause stress symptoms. Therefore, it is more reasonable to use a time lag of greater than 1 day to cope with different working hours. Ultimately, theory rather than calculus determines the optimal lag for a pilot study, but calculus rather than theory determines the optimal lag for a final study.

Conclusion

We suggest that shortitudinal pilot studies using quite short lags will help researchers to design an optimally spaced panel study. When the substantive hypotheses tested are valid, such shortitudinal studies will provide essential information about the expected distribution of causal effects over time. In the introduction, we noted several meta-analyses that seem to support the incorrect rule of thumb suggesting that cross-lagged effects decline as a function of time. We suspect these meta-analyses are based on time lags that are longer than optimal, and future research is needed to provide better estimates of the effects identified by panel studies. It is possible that some results are derived from lags that are so far beyond the optimal time lag that they might fail to establish the expected cause–effect relations. We believe that estimating optimal time lags has the potential to enhance understanding of important causal relationships across multiple domains of psychology.

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(*Appendices follow*)

Appendix A

Optimal Time Lags in One-Directional Discrete Time Models With Identical Stabilities ($i = d$)

The partial regression coefficient relating $\mathbf{X_0}$ to $\mathbf{Y_\omega}$ is

$$\beta_{Y_\omega X_0} = i^0 c\, d^{\omega-1} + i^1 c\, d^{\omega-2} + i^2 c\, d^{\omega-3} + \ldots + i^{\omega-2} c\, d^{\omega-(\omega-1)} + i^{\omega-1} c\, d^{\omega-(\omega)}. \quad (14)$$

When stabilities for X and Y for the SU, that is, i and d, are equal ($s = i = d$), then Equation (14) can be rewritten as follows:

$$\beta_{Y_\omega X_0} = s^0 c\, s^{\omega-1} + s^1 c\, s^{\omega-2} + s^2 c\, s^{\omega-3} + \ldots + s^{\omega-2} c\, s^{\omega-(\omega-1)} + s^{\omega-1} c\, s^{\omega-(\omega)} = \omega c\, s^{\omega-1}. \quad (15)$$

The first derivative with regard to $\boldsymbol{\omega}$ is

$$\beta'_{Y_\omega X_0} = c\, s^{\omega-1} + \omega c\, s^{\omega-1} \ln(s), \quad (16)$$

which equals 0 if

$$\omega = -\frac{1}{ln(s)}. \quad (17)$$

Appendix B

Mplus Syntax for the Analysis of the Model in Figure 5 Using the Correlations Among Hassles and Depression Reported by Steca et al. (2014)

```
title: Steca et al., 2014;
data:
  file = Steca.cor;
  type = correlation;
  nobservations = 544;
variable:
  names = CStCon CStSlf AcSE SoSe Dep0 Dep1 Dep2 Dep3 Has0 Has1 Has2 Has3;
  usevar = Has0 Has1 Has2 Has3 Dep0 Dep1 Dep2 Dep3;
analysis: estimator = ML;
Model:
  ! Modeling the 2 unmeasured third variables
  TV_D BY Dep0 (Ld1);
  TV_D BY Dep1 Dep2 Dep3 (Ld1);
  TV_H BY Has0 (Lh1);
  TV_H BY Has1 Has2 Has3 (Lh1);
  TV_H WITH TV_D;
  ! Modeling the causes (auto & cross-lagged) of depression
  Dep1 ON Dep0 (a11);
  Dep1 ON Has0 (a12);
  Dep2 ON Dep1 (a11);
  Dep2 ON Has1 (a12);
  Dep3 ON Dep2 (a11);
  Dep3 ON Has2 (a12);
```

(*Appendices continue*)

```
    ! Modeling the causes (auto & cross-lagged) of hassles
    Has1 ON Has0 (a22);
    Has1 ON Dep0 (a21);
    Has2 ON Has1 (a22);
    Has2 ON Dep1 (a21);
    Has3 ON Has2 (a22);
    Has3 ON Dep2 (a21);
    ! Covariance of depression and hassles at Time 0
    Dep0 WITH Has0 (cov1);
    ! Covariances of latent disturbances at later times
    Dep1 WITH Has1 (cov2);
    Dep2 WITH Has2 (cov2);
    Dep3 WITH Has3 (cov2);
    ! Variance of depression and hassles at Time 0
    Dep0 (VDep0);
    Has0 (VHas0);
    ! Variances of latent disturbances at later times
    Dep1 (VDep1);
    Dep2 (VDep1);
    Dep3 (VDep1);
    Has1 (VHas1);
    Has2 (VHas1);
    Has3 (VHas1);
Output: sampstat stdyx;
```

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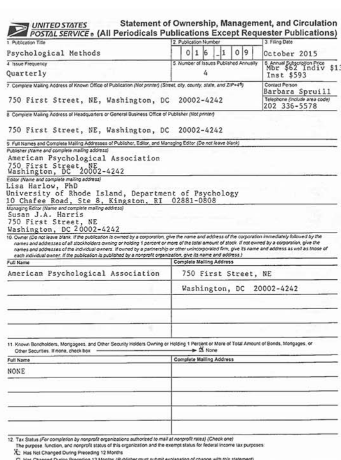

UNITED STATES POSTAL SERVICE® **Statement of Ownership, Management, and Circulation (All Periodicals Publications Except Requester Publications)**

1. Publication Title	2. Publication Number	3. Filing Date
Psychological Methods	0 1 6 - 1 0 9	October 2015
4. Issue Frequency	5. Number of Issues Published Annually	6. Annual Subscription Price
Quarterly	4	Mbr $62 Indiv $138 Inst $593

7. Complete Mailing Address of Known Office of Publication (Not printer) (Street, city, county, state, and ZIP+4®)
750 First Street, NE, Washington, DC 20002-4242
Contact Person: Barbara Spruill
Telephone (Include area code): 202 336-5578

8. Complete Mailing Address of Headquarters or General Business Office of Publisher (Not printer)
750 First Street, NE, Washington, DC 20002-4242

9. Full Names and Complete Mailing Addresses of Publisher, Editor, and Managing Editor (Do not leave blank)
Publisher (Name and complete mailing address)
American Psychological Association
750 First Street, NE
Washington, DC 20002-4242
Editor (Name and complete mailing address)
Lisa Harlow, PhD
University of Rhode Island, Department of Psychology
10 Chafee Road, Ste 8, Kingston, RI 02881-0808
Managing Editor (Name and complete mailing address)
Susan J.A. Harris
750 First Street, NE
Washington, DC 20002-4242

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Full Name	Complete Mailing Address
American Psychological Association	750 First Street, NE
	Washington, DC 20002-4242

11. Known Bondholders, Mortgagees, and Other Security Holders Owning or Holding 1 Percent or More of Total Amount of Bonds, Mortgages, or Other Securities. If none, check box → ☒ None

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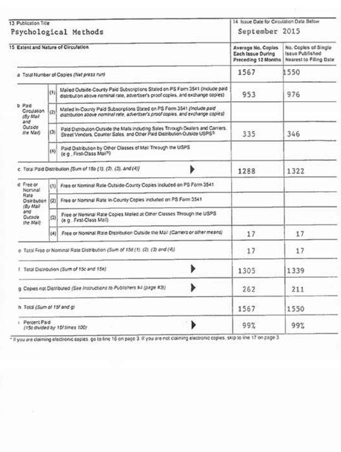

13. Publication Title			14. Issue Date for Circulation Data Below	
Psychological Methods			September 2015	
15. Extent and Nature of Circulation			Average No. Copies Each Issue During Preceding 12 Months	No. Copies of Single Issue Published Nearest to Filing Date
a. Total Number of Copies (Net press run)			1567	1550
b. Paid Circulation (By Mail and Outside the Mail)	(1)	Mailed Outside-County Paid Subscriptions Stated on PS Form 3541 (Include paid distribution above nominal rate, advertiser's proof copies, and exchange copies)	953	976
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f. Total Distribution (Sum of 15c and 15e)			1305	1339
g. Copies not Distributed (See Instructions to Publishers #4 (page #3))			262	211
h. Total (Sum of 15f and g)			1567	1550
i. Percent Paid (15c divided by 15f times 100)			99%	99%

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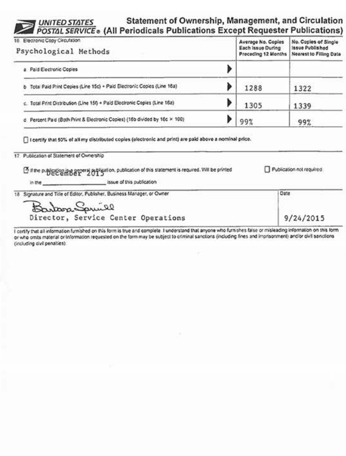

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Psychological Methods		
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☒ If the publication is a general publication, publication of this statement is required. Will be printed in the December 2015 issue of this publication. ☐ Publication not required.

18. Signature and Title of Editor, Publisher, Business Manager, or Owner
Barbara Spruill
Director, Service Center Operations
Date: 9/24/2015

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