Longitudinal research design definition pdf

Show page numbers A longitudinal layout is one that measures the characteristics of the same individuals in at least two, but ideally more, occasions over time. Its purpose is to directly address the study of individual change and variation. Longitudinal studies are expensive in both time and money, but provide many significant advantages in relation to cross-cutting studies. In fact, longitudinal studies are essential to understand changes related to development and aging because they allow direct evaluation of change within the person over time and provide a basis for assessing individual level differences as independent of rate and pattern of change, as well as the treatment of selection effects related to population wear and mortality. Traditional longitudinal designsAventurainal designs can be classified in several ways, but are mainly defined ... DatabasesLISRELMBESSNVivoRSASSoftware, FreeSPSSStatisticaSYSTATWinPepi All A B C D E F G H I J K L M N O P Q R S T U V W X Y Z Inputs per page: 20 40 60 A longitudinal study is an observational research method in which data are collected for the same subjects repeatedly over a period of time. Longitudinal research projects can be extended over years or even decades. In a longitudinal cohort study, the same individuals are observed during the study period. Cohort studies are common in medicine, psychology and sociology, where they allow researchers to study changes over time. Here are some examples of longitudinal studies: The Framingham Heart Study, which began in 1948 with 5,209 adult subjects from Framingham, Massachusetts, is the source of a wealth of current knowledge about heart disease. The study has resulted in most of what is known about the effects of diet, exercise and common medications such as aspirin on heart disease. The Framington Heart Study now follows the third generation of participants. In 1971, the British Bureau of Census and Population Surveys began following a sample of 1% of the British population. The study has correlated several outcomes, such as cancer mortality and incidence, with variables such as employment and housing. The Terman Study of the Gifted, formerly known as the Genetic Studies of the Genie, is the oldest and longest lasting longitudinal study in the world. Lewis Terman began the study in 1921, at Stanford University, to observer the development and characteristics of gifted children throughout life. Its initial purpose was to refute the then prevalent belief that gifted children were physically delicate and inclined to be Inept. Terman's initial findings were that, apart from intelligence and a tendency to be short-sighted, gifted children were not significantly different from their less gifted peers. The Canadian Longitudinal Study on Aging (CLSA) was designed to follow approximately 50,000 men and women between the ages of 45 and 85 for at least 20 years. CLSA researchers collect information about social, lifestyle and economic factors. The purpose is to gain knowledge about the effect of these factors, both separately and in combination, on the development of disease and disability as people age. In order to continue enjoying our site, we ask you to confirm your identity as a human. Thank you very much for your cooperation. The design of the study depends to a large extent on the nature of the research question. In other words, knowing what kind of information the study should collect is a first step in determining how the study will be conducted (also known as a methodology). Let's say we want to investigate the relationship between daily walking and cholesterol levels in the body. One of the first things we'd have to determine is the kind of study that will tell us the most about that relationship. Do we want to compare cholesterol levels between different populations of walkers and non-walkers at the same point in time? Or do we want to measure cholesterol levels in a single population of daily walkers over an extended period of time? The first approach is typical of a cross-sectional study. The second requires longitudinal study. To make our decision, we need to know more about the benefits and purpose of each type of study. Cross-sectional study Both transverse and longitudinal studies are observational studies. This means that researchers record information about their subjects without manipulating the study environment. In our study, we would simply measure the cholesterol levels of daily walkers and non-walkers along with any other features that might be of interest to us. We would not influence non-walkers to attend this activity, nor do we advise daily walkers to change their behavior. In short, we would try not to interfere. The defining characteristic of a cross-sectional study is that you can compare different population groups at a single point in time. Think about it in terms of taking a snapshot. The findings are extracted from what fits within the framework. To return to our example, we could choose to measure cholesterol levels in daily walkers in two age groups, over 40 and under 40, and compare them to cholesterol levels among non-walkers in the same age groups. We could even create subgroups for the genre. However, we would not consider past or future cholesterol levels, as these would fall out of the frame. We would like to look only at cholesterol levels at a time in time. The benefit of a cross-sectional study design is that it allows researchers to compare many different variables at the same time. We could, for example, see age, gender, income and educational level in relation to walking and cholesterol levels, with or no additional cost. However, cross-cutting studies may not provide definitive information on cause-and-effect relationships. This is because these studies provide a one-time snapshot over time; they don't take into account what happens before or after taking the snapshot. Therefore, we cannot know for sure whether our Walkers had low cholesterol levels before taking their exercise regimens, or if daily walking behavior helped reduce cholesterol levels that were previously high. Longitudinal study A longitudinal study, such as a cross-sectional study, is observational. So, once again, investigators don't interfere with their subjects. However, in a longitudinal study, researchers make several observations of the same subjects over a period of time, sometimes lasting many years. The benefit of a longitudinal study is that researchers are able to detect developments or changes in the characteristics of the target population both at the group level and at the individual level. The key here is that longitudinal studies extend beyond a single moment in time. As a result, they can set sequences of events. To return to our example, we could choose to see the change in cholesterol levels among women over the age of 40 who walk daily over a 20-year period. The design of the longitudinal study would explain cholesterol levels at the beginning of a walking regimen and as walking behavior continued over time. Therefore, a longitudinal study is more likely to suggest cause-and-effect relationships than a cross-sectional study by virtue of its scope. In general, research should drive design. But sometimes, the progression of research helps determine which design is most appropriate. Cross-sectional studies can be performed faster than longitudinal studies. That's why researchers could start with a cross-sectional study to first establish whether there are links or associations between certain variables. They would then create a longitudinal study to study the cause and effect. Source: At Work, Issue 81, Summer 2015: Institute for Work & Health, Toronto This column updates an earlier column that describes the same term, originally published in 2009. Skip Navigation Destination PDF Split View Article Content Figures and Tables Complementary Video Audio Data The purpose of this article is to clarify the conceptual, methodological, and practical problems that frequently appear when conducting longitudinal research, as well as in the journal review process. Using a panel discussion format, current authors address 13 questions associated with 3 aspects of longitudinal research: conceptual issues, research design and statistical techniques. These questions are intentionally framed at the general level so that authors can address them from their various perspectives. The authors' perspectives and recommendations provide useful guidance for conducting and reviewing longitudinal studies in retirement work, aging and research. A meta-trend at work, aging and retirement research is the greatest appreciation of the temporal nature of phenomena under investigation and the important role that longitudinal study designs play in their understanding (e.g. Heybroek, Haynes, & Baxter, 2015; Madero-Cabib, Gauthier, & Le Goff, 2016; Wang, 2007; Warren, 2015; Weikamp & Gritz, Gritz, This echoes the trend in more general research on work and organizational phenomena, where the discussion of time and longitudinal designs has evolved from the explanation of conceptual and methodological issues involved in the evaluation of changes over time (e.g. McGrath & Rotchford, 1983) to the development and application of analytical data techniques (e.g. McGrath & Rotchford, 1983) to the development and application of analytical data techniques (e.g. , Chan, 1998; Chan & Schmitt, 2000; DeShon, 2012; Liu, Mo, Song, & Wang, 2016; Wang & Bodner, 2007; Wang & Chan, 2011; Wang, Zhou, & Zhang, 2016), theoretical representation (e.g. Ancona et al., 2001; Mitchell & James, 2001; Vancouver, Tamanini, & Yoder, 2010; Wang et al., 2016), and methodological decisions in conducting longitudinal research (e.g. Beal, 2015; Bolger, Davis, & Rafaeli, 2003; Ployhart & Vandenberg, 2010). Given the importance and repeated call for longitudinal studies to investigate work, aging and retirement-related phenomena (e.g. Fisher, Chaffee, & Sonnega, 2016; Wang, Henkens, &gt a Solinge, 2011), more relevant conceptual and methodological issues need to be discussed. Such discussions would help researchers make more informed decisions about longitudinal research and conduct studies that would strengthen the validity of inferences and avoid misleading interpretations. In this article, using a panel discussion format, the authors address 13 questions associated with three aspects of longitudinal research: conceptual issues, research design and statistical techniques. These questions, as summarized in Table 1, are intentionally framed at the general level (i.e. not just in ageing-related research), so that authors can approach them from a variety of perspectives. The objective of this article is to clarify the conceptual, methodological and practical issues that often arise in the process of conducting longitudinal research, as well as in the related journal review process. Therefore, the authors' perspectives and recommendations provide useful guidance for conducting and reviewing longitudinal studies, not only those dealing with aging and retirement, but also in broader fields of work and organizational research. Table 1.Questions concerning longitudinal research addressed in this article Conceptual Issues 1. Conceptually, what is the essence of longitudinal research? 2. What is the state of time in longitudinal research? Is time a general notion of temporal dynamics in phenomena, or is time a substantive variable similar to other focal variables in longitudinal study? 3. What are the procedures, if any, to develop a changes over time in longitudinal research? Since longitudinal research supposedly addresses the limitations of cross-cutting research, can the findings of cross-cutting studies be useful for the development of a theory of change? Research Design 1. What are some of the main considerations to consider before deciding to use a longitudinal study design? 2. They are Son any design advantage of cross-cutting research that could make it preferable to longitudinal research? I mean, what would be lost and what could be gained if a moratorium was placed on cross-cutting research? 3. In a longitudinal study, how do we decide the length of the interval between two adjacent time points? 4. As events occur in our daily lives, our mental representations of these events may change as time goes on. How can we determine the points in time when the representation of an event is appropriate? How can these issues be addressed through design and measurement in a study? 5. What are the biggest practical obstacles to conducting longitudinal research? What are the ways to overcome them? Statistical techniques 1. With regard to evaluating changes over time in a latent growth modeling framework, how can a researcher address different conceptual issues by encoding the slope variable differently? 2. In longitudinal research, are there other issues of measurement error to which we must pay attention, which are beyond those applicable to cross-cutting research? 3. When analyzing longitudinal data, how should we handle missing values? 4. Most of the existing longitudinal research focuses on the study of quantitative change over time. What if the variable of interest is categorical or if the changes over time are qualitative in nature? 5. Could you speculate on the next big deal in conceptual or methodological advances in longitudinal research? Specifically, describe a novel idea or a specific data analysis model that is rarely used in longitudinal studies in our literature, but could serve as a useful conceptual or methodological tool for future science at work, aging and retirement. QUESTIONS ON CONCEPTUAL PROBLEMS Conceptual Question Question 1: Conceptually, what is the essence of longitudinal research? Vancouver This is a fundamental question to ask given the confusion in literature. It is common to see authors attribute their high confidence in their causal inferences to the longitudinal design they use. It is also common to see authors attribute greater confidence in their measurement due to the use of a longitudinal design. Less common, but with increasing frequency, the authors claim to be examining the role of time in their theoretical models through the use of longitudinal designs. These different assumptions from the authors illustrate the need to clarify when specific attributions on longitudinal research are appropriate. Therefore, a discussion of the essence of longitudinal research and what it provides is in Interestingly, longitudinal research definitions are rare. One exception is a definition of Taris (2000), who explained that longitudinal data are collected for the same set of research units (which may differ from sampling units/defendants) for (but not necessarily on) two or more occasions, in principle allowing intraincesal comparison over time (pp. 1–2). Perhaps more directly for the current discussion of longitudinal research related to the work and phenomena of aging, Ployhart and Vandenberg (2010) defined longitudinal research as research that emphasizes the study of change and contains at least three repeated observations (although more than three is better) in at least one of the substantive constructions of interest (p. 97; italic in original). Compared to Taris (2000), the ployhart and Vandenberg definition (2010) explicitly emphasizes change and encourages the collection of many waves of repeated measures. However, the definition of Ployhart and Vandenberg may be excessively restrictive. For example, it prevents designs from being often classified as longitudinal, such as prospective design. In a forward-looking design, some criterion (i.e. presumed effect) is measured in Times 1 and 2, so that the change in criteria can be examined based on the events (i.e. the alleged causes) that occur (or do not) occur between data collection waves. For example, a researcher may use this design to evaluate the psychological and behavioral effects of retirement that occur before and after retirement. That is, psychological and behavioral variables are measured before and after retirement. Although not as valid internally as an experiment (which is not possible because we cannot randomly assign participants to retirement and non-retirement conditions), this prospective design is a substantial improvement over the typical design where the criteria are only measured at once. This is because it allows you to further examine the change in a criterion based on the differences between events or person variables. Otherwise, inferences based on retrospective accounts of the change of judgment should be drawn along with retrospective accounts of events; In addition, it is concerned that the covariance between the criterion and the person's variables is caused by changes in the criterion that are also changing the person. Of course, this design does not eliminate the possibility that changes in the criterion may cause differences in events (for example, observed changes in psychological and behavioral variables lead people to decide to retire). In addition to longitudinal designs that potentially have only two data collection waves for a variable, there are certain types of criterion variables that need only an explicit measure in time 2 in a 2-wave study. Retirement (or, similarly, turnover) is an example. I say explicitly because retirement is implicitly measured in time 1. That is, if the units are in the work sample at time 1, they have not been removed. Therefore, retirement in time 2 represents a change in working status. On the other hand, if retirement intentions the criterion variable, the repeated measurements of this variable are important for evaluating the change. Repeated measures also allow simultaneously to assess the change in retirement intentions and their alleged precursors; it could be that a variable such as job satisfaction (an alleged cause of retirement intentions) is after retirement intentions are formed, perhaps in a process of rationalization. That is, people first intend to retire and then evaluate over time their attitudes towards their current work. This type of reverse causality process would not be detected in a design that measures job satisfaction over time 1 and retirement intentions over time 2.Given the above, I opt for a much more direct definition of longitudinal research. Specifically, longitudinal research is simply research where data is collected over a significant period of time. One difference between this definition and that of Taris (2000) is that this definition does not include the clause on the examination of intra-individual comparisons. These designs can examine intra-individual comparisons, but again, this seems overly restrictive. That said, I add a constraint to this definition, which is that the time interval should be significant. This term is necessary because time will always pass, that is, it takes time to complete questionnaires, do tasks or observe behavior, even in cross-sectional designs. However, this passage of time probably does not provide any validity benefits. On the other hand, the measurement interval could last only a few seconds and remain significant. To be meaningful you have to support the inferences that are being made (i.e. improve the validity of the investigation). Therefore, the essence of longitudinal research is to improve the validity of inferences that cannot otherwise be achieved using cross-cutting research (Shadish, Cook, & Campbell, 2002). Inferences that longitudinal research can potentially improve include those related to measurement (i.e. the validity of construction), causation (i.e. internal validity), generalization (i.e. external validity) and quality of effect size estimates and hypothesis tests (i.e. the validity of the statistical conclusion). However, the ability of longitudinal research to improve these inferences will depend to a large extent on many other factors, some of which could make

inferences less valid when using a longitudinal design. Increased inferential validity, in particular of any specific type (e.g. internal validity), is not an inherent quality of longitudinal design; is a design goal. And it is important to know how some forms of longitudinal design do not target that goal for some inferences. For example, consider a case in which a measure of an alleged cause precedes a measure of an alleged effect, but for a period of time in which one of the buildings in question probably does not change. In fact, it is often questionable whether several months gap between observations of many variables examined in the research would change significantly during the interim period, let alone that the change in one preceded the change in the other (for example, the intention to withdraw is an example of this, as people may maintain a stable intention to retire for years). Therefore, the design typically does not provide any real improvement in terms of internal validity. In the probably improves the validity of construction and statistical conclusion because it probably reduces the bias effects of the common method found between the two variables (Podsakoff et al., 2003). Also, consider the case of predictive validity design, where a selection instrument is measured from a sample of job applicants and performance is evaluated some time later. In this case, common method bias is usually not the problem; external validity is. Longitudinal design improves external validity because time measure 1 is taken during the application process, which is the context in which the selection instrument will be used, and time measure 2 is taken after a significant time interval (i.e. after sufficient time has elapsed for performance to be stabilized for new job owners). Again, however, internal validity is not greatly improved, which is well since prediction, not cause, is the main concern in the context of selection. Another clear improvement in the validity of the construction obtained through the use of longitudinal research is when one is interested in measuring change. An accurate version of the change measurement is to evaluate the exchange rate. When evaluating the rate, time is a key variable in the analysis. To evaluate a rate, only two repeated measures of the variable of interest are needed, although these measures should be taken from several units (e.g. individuals, groups, organizations) if measurement and sampling errors are present and perhaps under various conditions if a systematic measurement error (e.g. test effect) is possible. In addition, Ployhart and Vandenberg (2010) advocate at least three repeated measures because most exchange rates are not constant; therefore, more than two observations will be needed to assess whether and how the rate changes (that is, the shape of the growth curves). In fact, three is not enough given noise in the measurement and similarity of complex processes (i.e., consider the example of the opposing process below). Longitudinal research designs can, with certain precautions, improve confidence in inferences about causation. When this is the purpose, time does not need to be measured or included as a variable in the analysis, although the interval between measurements should be reported because the rate of change and cause are related. For example, intervals may be too short, so given the rate of an effect, the cause may not have had enough time to register for the effect. Alternatively, if the intervals are too long, an effect might have triggered a compensation process that exceeds the original level, reversing the cause effect sign. An example of the latter process is the process opposition (Solomon & Corbit, 1974). Figure 1 describes this process, which refers to the response to an emotional stimulus. Specifically, the emotional response provokes an opposing process that, at its peak, returns emotion to the baseline and beyond. If the emotional response is collected when an opponent's response peak occurs, like stimulus is having the opposite effect of what it's really having. Most longitudinal research designs that improve internal validity are quasi-experimental (Shadish et al., 2002). For example, interrupted time series designs use repeated observations to evaluate trends before and after some manipulation or natural experiment to model possible maturation or maturation effects by selection (Shadish et al., 2002; Stone-Romero, 2010). Similarly, discontinuous regression designs (RDDs) use a pre-test to assign participants to pre-handling conditions and can therefore use the pre-test value to model the selection effects (Shadish et al., 2002; Stone-Romero, 2010). Interestingly, the RDD design is not explicitly evaluating the change and is therefore not susceptible to threats of maturations, but it uses measurement synchronization in a meaningful way. Special group designs (i.e. cohorts) are also typically considered longitudinal. These designs measure all variables of interest during each wave of data collection. I think it was this kind of design that Ployhart and Vandenberg (2010) had in mind when they created their definition of longitudinal research. In particular, these designs can be used to evaluate exchange rates and can improve causal inferences if done right. In particular, to improve causal inferences with panel designs, researchers almost always need at least three repeated measures of hypothetical causes and effects. Consider the case of job satisfaction and intent to retire. If an investigator measures job satisfaction and intends to retire in Times 1 and 2 and discovers that time 2 measures of job satisfaction and intent to retire are negatively related when controlling time 1 states of variables, the researcher still cannot know which one changed first (or if any third variables cause both to change in between). Unfortunately, three observations of each variable is only a slight improvement because it could be a difficult thing to get enough variance in changing attitudes and change the intentions with only three waves to find something meaningful. In fact, the researcher might have better luck looking at real retirement, than as mentioned, he just needs one observation. Still, two job satisfaction observations are needed before retirement to determine whether changes in job satisfaction influence the likelihood of retirement. Finally, in this regard, I would add that significant variance over time will often mean case-intensive designs (i.e. many observations of many variables over time on a case-by-case basis; Bolger & 2013; Wang et al., 2016) because we will be increasingly interested in evaluating feedback and other compensatory processes, reciprocal relationships and how dynamic variables change. In these cases, the covariance within the unit will be much more interesting than the covariance between units. Wang It is important to note that true experimental designs are also a type of longitudinal research design by This is because in experimental design, a separate variable is manipulated before the dependent variable measurement occurs. This priority of time (or delay) is critical to the use of experimental designs to achieve stronger causal inferences. Specifically, since random allocation is used to generate experimental and control groups, researchers can assume that before manipulation, the average levels of dependent variables are the same across all experimental and control groups, as well as the average levels of independent variables. Therefore, when measuring the dependent variable after manipulation, an experimental design reveals the change in the dependent variable as a change function in the independent variable as a result of manipulation. As such, the time delay between manipulation and the measurement of the dependent variable is really significant in the sense of achieving causal inference. Conceptual question Question 2: What is the situation of time in longitudinal research? Is time a general notion of temporal dynamics in phenomena, or is time a substantive variable similar to other focal variables in longitudinal study? Chan In longitudinal research, we are concerned about conceptualizing and evaluating changes over time that can occur in one or more substantive variables. A substantive variable refers to a measure of a construction of interest envisaged in the study. For example, in an adaptation study for newcomers (e.g. Chan & Schmitt, 2000), substantive variables, whose changes over time interest us in tracking, could be the frequency of information search, job performance, and social integration. We could examine the functional shape of the change path of the substantive variable (for example, linear or quadratic). We could also examine the extent to which individual differences in a path growth parameter (for example, the individual slopes of a linear path) could be predicted from the initial values (i.e. at time 1 of repeated measurement) in the substantive variable, the values in an invariant predictor over time (for example, personality trait) , or the values of another variable variable that varies over time (for example, individual slopes of the linear path of a second substantive variable in the study). Substantive variables are measures used to represent the constructions of the study. As building measures, they have specific substantive content. We can assess the validity of the construction of the measure by obtaining relevant validity evidence. Evidence could be the extent to which the content of the measure represents the conceptual content of the construction (i.e., the validity of the or the extent to which the measure is correlated with another established measure of criteria that represents a criterion construct that is theoretically expected to be associated with the measure (i.e., validity related to criteria). Time, on the other hand, has a different ontological state from the substantive variables in the longitudinal study. There are in three ways to describe how time is not a substantive variable similar to other focal variables in longitudinal study. First, when monitoring a substantive construction in a longitudinal study for changes over time, time is not a substantive measure of a study construction. In the previous example of the adaptation study for The NewComers of Chan and Schmitt, it is not significant to talk about assessing the validity of time construction, at least not in the same way that we can talk about assessing the validity of the construction of work performance or social integration measures. Second, in a longitudinal study, a time point in the observation period represents a temporary measurement instance. The time point per se, therefore, is simply the time marker of the state of the substantive variable at the measuring point. The time point is not the state or value of the substantive variable that we are interested in to track changes over time. Changes over time occur when the state of a substantive variable changes over different measurement points. Finally, in a longitudinal study of changes over time, time is different from the substantive process underlying change over time. Consider a hypothetical study that repeatedly measured the levels of job performance and social integration of a group of newcomers over six points of time, at 1-month intervals between adjacent time points over a 6-month period. Suppose the study found that the change observed over time in your work performance levels was best described by a trajectory of monotonous increase at a declining rate of change. The observed functional form of the performance path could serve as empirical evidence for the theory that a learning process underlies performance level changes over time. Let us also assume that, for the same group of newcomers, the change observed over time in their levels of social integration was best described by a positive linear trajectory. This observed functional form of the social integration trajectory could serve as empirical evidence for a theory of the social adjustment process that underlies changes in the level of integration over time. In this example, there are two distinct substantive processes of change (learning and social adjustment) that can underlie changes in the levels of the two respective study constructs (performance and social integration). There are six time points at which each substantive variable was measured over the same time period. Time, in this longitudinal study, was simply the means through which the two substantive processes occur. Time was not an explanation. The it did not cause the occurrence of the different substantive processes and there is nothing in the conceptual content of the temporal construction that could, nor was expected, explain the functional form or nature of the two different substantive processes. Substantive processes occur or develop over time, but did not make time exist. How growth shaping analyze longitudinal data is consistent with the previous conceptualization of time. For example, in latent growth modeling, time per is not represented as a substantive variable in the analysis. Instead, a specific time point is encoded as a temporary marker of the substantive variable (for example, as base coefficients in a latent growth model to indicate the time points in the repeated measurement sequence at which the substantive variable was measured). The temporal variable nature of the substantive variable is represented at the individual level as individual slopes or at the group level as the variance of the slope factor. It is the slopes and variance of the slopes of the substantive variable that are being analyzed, and not the time per se. The nature of the trajectory of the change in the substantive variable is represented descriptively by the specific functional form of the trajectory observed within the time period of the study. We can also include in the latent growth model other substantive variables, such as invariant predictors over time or variable correlations over time, to evaluate the strength of their associations with the variance of the individual slopes of the path. These associations serve as validation and explanation of the substantive process of change in the focal variable that is occurring over time. Newman Many theories of change require articulation of a construction of change (e.g. learning, social adjustment, inferred from a slope parameter in a growth model). When you specify a change construct, the time variable is only used as a marker to track a substantive change or growth process. For example, when we say Extraversion to × the effect of time interaction on the social integration of newcomers, we really mean that Extraversion relates to the construction of change in social adjustment (i.e., where social adjustment is operated as the slope parameter of a model of growth of people's social integration over time). Similarly, when we say, consciousness × time2 quadratic interaction effect on the performance of the task of newcomers, we really mean that consciousness is related to the construction of learning changes (where learning is operated as the nonlinear slope of task performance over time). This view of time poses a number of problems with scaling and calibrating the time variable to properly evaluate the underlying substantive change construction. For example, should work experience be measured in the number of years at work versus the number of completed assignments (Tesluk & Jacobs, 1998)? Should the construction of change be considered an effect of the age of development, effect of the historical period or effect of the birth cohort (Schaie, 1965)? Should the equipment time study reflect development time instead of clock time, and therefore calibrate to the lifespan of each equipment (Gersick, 1988)? As such, although time is not a substantive variable in itself in longitudinal research, it is important to ensure that the time measurement matches the theory you specify construction of changes that is under study (e.g. aging, learning, adaptation, social adjustment). Vancouver agrees that time is not usually a substantive variable, but can serve as a proxy for substantive variables if the process is well known. The example of learning by Chan is an example. Of course, well-known temporal processes are rare and I have often seen the substantive power erroneously given to time: For example, it is the oxidation process, not the passage of time that is responsible for oxidation. However, there are cases where time plays a substantive role. For example, the temporary discount (Ainslie & Haslam, 1992) is a theory of behavior that depends on time. Similarly, Vancouver, Weinhardt and Schmidt's (2010) theory of multi-target search implies time as a key substantive variable. Undoubtedly, in the latter case the perception of time is a key mediator between time and its hypothetical effects on behavior, but time plays an explicit role in theory and should therefore be considered a substantive variable in the theory tests. Chan I referred to the objective time in explaining that time is not a substantive variable in longitudinal research and that it is instead the temporal medium through which a substantive process or a substantive variable develops changes its state. When we discuss theories of substantive phenomena or processes involving temporary constructions, such as temporary discounts, urgency of time, or polychronic related to multitasking or multiple objective searches, we actually refer to subjective time, which is the psychological experience of the individual's time. Subjective constructions of time are clearly substantive variables. The distinction between target time and subjective time is important because it provides conceptual clarity to the nature of temporal phenomena and guides methodological options in the study of time (for more details, see Chan, 2014). Conceptual question Question 3: What are the procedures, if any, to develop a theory of changes over time in longitudinal research? Since longitudinal research supposedly addresses the limitations of cross-cutting research, can the findings of cross-cutting studies be useful for the development of a theory of change? Vandenberg To address this question, what follows is largely an application of some of the ideas presented by Mitchell and James (2001) and by Ployhart and Vandenberg (2010) in their respective publications. Therefore, credit should be given for the following to these authors, and it is strongly recommended that their articles be consulted on the details. Before specifically addressing this question, it is important to understand our reason Apply. Namely, as Mitchell and James (2001), and repeated by, among others, Bentein and his colleagues (2005), Chan (2002, 2010) and Ployhart and Vandenberg (2010), there is an abundance of research published in the main organizational chemical and organizational sciences in which authors are not operating through their research designs causal relationships between their independent, dependent, moderating and mediating focal variables, even though the introductory and discussion sections involve such causality. Mitchell and James (2001) used the pieces published in the most recent issues (at the time) of the Academy of Management Journal and Administrative Science Quarterly to pin this point. In the crun of the problem you are using designs in which time is not a consideration. As so succinctly stated: At the simplest level, when examining whether an X causes a Y, we need to know when X occurs and when Y occurs. Without theoretical or empirical guidance on when to measure X and Y, we run the risk of measurement, analysis and ultimately inferences about the strength, order and direction of causal relationships (in aggregate italics, Mitchell & James, 2001, p. 530). When it is key because it is at the heart of causality in its simplest form, as in the cause must precede the effect (James, Mulaik, & Brett, 1982; Condition 3 of 10 for inferring causality, p. 36). Our informal look at literature published over the decade since Mitchell and James (2001) indicates that not much has changed in this regard. Therefore, our reason for asking the current question is quite simple: perhaps it is time to put these issues in front of us once again (expected pun), particularly given the growing criticisms of the significance of the findings published from studies with weak methods and statistics (e.g. statistical myths and urban legends, Lance & Vandenberg, 2009). The first part of the question asks: What are the procedures, if any, to develop a theory of change over time in longitudinal research? Before addressing the procedures per se, it is first necessary to understand some of the problems when incorporating the change into the investigation. Doing so provides context for procedures. Ployhart and Vandenberg (2010) identified four theoretical issues that should be addressed by incorporating change into variables of interest over time. These were: As far as possible, specify a theory of change by observing the specific shape and duration of the change and the predictors of change. Clearly articulate or graph the hypothetical form of change in relation to the observed form of change. Clarify the level of change of interest: average group change, intraunit change or differences between units in intraunit change. Note that cross-cutting theory and research may be insufficient to develop the theory of change. You should focus on explaining why the change occurs (p. 103). The interested reader is encouraged to consult Ployhart and Vandenberg (2010) on the details four issues, but were strongly reported by Mitchell and James (2001). Please note that, as a means of implementing time, Mitchell and James (2001) focused on time very broadly in the context of strengthening causal causal inferences over time in focal variables. Thus, ployhart and Vandenberg's argument (2010), with their only emphasis on change, is nested within the Mitchell and James perspective (2001). Consider this point because it is in this regard that the four theoretical issues presented above are based on the five theoretical issues addressed by Mitchell and James (2001). Specifically, first, we need to know the delay between X and Y. How long after X occurs does Y occur? Second, X and Y have durations. Not all variables occur instantly. Third, X and Y may change over time. We need to know the exchange rate. Fourth, in some cases we have dynamic relationships in which both X and Y change. The exchange rate of both variables must be known, as well as how the X-Y relationship changes. Fifth, in some cases we have reciprocal causality: X causeS Y and Y causes X. This situation requires an understanding of two sets of delays, durations and possibly fees. The main point of both groups of authors is that these theoretical issues must first be addressed in the sense that they must be the key determinants in the design of the general study; that is, decide on the procedures to be used. Although Mitchell and James (2001, see p. 543) focused on reporting procedures through theory in the broader context of time (e.g. resorting to studies and research that may not be in our specific area of interest; going to the workplace and actually observing causal sequence, etc.), our specific question focuses on change over time. In this regard, Ployhart and Vandenberg (2010, Table 1, p. 103) identified five methodological procedural issues and five analyses that must be reported by the nature of the change. These are: Methodological problems1. Determine the optimal number of measurement occasions and their intervals to properly model the hypothetical form of change.2. Whenever possible, choose the most likely samples to exhibit the hypothetical form of change, and try to avoid samples of convenience.3. Determine the optimal number of observations, which in turn means addressing the issue of wear and tear before conducting the study. Prepare for the worst (for example, up to a 50% drop from the first to the last measurement time). Also, whenever possible, try modeling the hypothetical cause of the missing data (ideally theorized and measured a priori) and consider the planned missing approaches to data collection.4. Introduce time delays between intervals to address causality issues, but make sure that the delays are not too long or too short.5. Evaluate the measurement properties of the variable for invariance (for example, configurator, metric) before testing whether produced the change. Analytical problems1. Be aware of possible violations in the statistical assumptions inherent in longitudinal designs (e.g. correlated waste, not independence).2. Describe how time is encoded (for example, polynomials, orthogonal polynomials) and why.3. Report why you use a particular analytical method and strengths and weaknesses for the particular study.4. Report all relevant effect sizes and adjustment rates to sufficiently assess the form of change.5. It is easy to 'overfit' the data; to develop a parsimonistic representation of change. In short, the main point of the above is to encourage researchers to develop a comprehensive conceptual understanding of time in defining causal relationships between focal variables of interest. We recognize that researchers are generally good at conceptualizing why their x variables cause some impact on their Y variables. What is asked here goes beyond the simple understanding of why, but forcing us to be very specific about the timing between variables. Doing so will result in stronger studies and where our inferences of findings can confidently include statements about causation, a level of confidence that is very lacking in most studies published today. As Mitchell and James succinctly claim (2001), with an impoverished theory of issues such as when events occur, when they change or how quickly they change, the empirical researcher is in a dilemma. Decisions about when to measure and how often to measure critical variables are left to intuition, chance, convenience, or tradition. None of them are particularly reliable guides (p. 533). This last quotation serves as a segue to address the second part of our question, Since longitudinal research supposedly addresses the limitations of cross-cutting research, can the findings of cross-cutting studies be useful for the development of a theory of change? Obviously, the answer here is up to it. In particular, it depends on the design contexts around which the cross-sectional study was developed. For example, if the study was developed strictly following many of the principles for designing quasi-experimentations in field environments written by Shadish, Cook and Campbell (2002), then it would be very useful to develop a theory of change about the phenomenon of interest. The results of these studies could inform decisions about how much the change should occur over time in the independent variable to see a measurable change in the dependent variable. Similarly, it would help inform decisions about what the baseline of the independent variable should be and how much change with respect to this baseline is required to affect the dependent variable. Another useful set of cross-cutting studies would be those developed in order to verify within field environments the findings of a series of well-designed laboratory experiments. Again, knowing problems such as thresholds, minimum/maximum values and intervals or timing of the start of the x would be very useful in informing a theory of change. A design context that would be of little use in developing a theory of change is the case in which a single cross-sectional study was completed to evaluate conceptual premises of interest. Theory theory the study may be useful, but the findings themselves would be of little use. Vancouver Few theories are not theories of change. Most, however, are not sufficiently specified. I mean, they leave a lot to the imagination. In addition, they often leave to the imagination the implications of behavioral theory. My personal bias is that change theories should generally be computationally represented to reduce vagueness, provide a test of internal coherence, and support the development of predictions. An immediately obvious conclusion that will be drawn when trying to create a formal theoretical computational model is that we have little empirical data on exchange rates. The procedures for developing a computational model are as follows (Vancouver & Weinhardt, 2012; also see Wang et al., 2016). First, take variables of (a) existing theory (verbal or static mathematical theory), b) qualitative studies, (c) deductive reasoning, or (d) some combination of these. Second, determine which variables are dynamic. Dynamic variables have memory in the sense that they retain their value over time, changing only as a function of processes that move the value in one direction or another at a speed or at a changing rate. Third, describe the processes that would affect these dynamic variables (if existing theory is used, this probably involves other variables in theory) or the rates and direction of change to dynamic variables if the processes affecting rates are beyond theory. Fourth, formally represent (for example, mathematically) the effect of variables on each other. Fifth, simulate the model to see if (a) it works (for example, no value is generated outside the bounds), (b) produces phenomena that the theory is supposed to explain, (c) produces data patterns over time (paths; relationships) that match (or could match) data, and (d) determine whether variance in exogenous variables (that is, those not presumably affected by other variables in the model) affect paths/relationships (called sensitivity analysis). For example, if we create a computational model to understand the timing of retirement, it will be critical to simulate the model to ensure that it generates predictions in a realistic way (for example, simulation should not generate too many cases where retirement occurs after the person is 90 years old). It will also be important to see if the predictions generated from the model match the actual empirical data (for example, the average retirement age based on the simulation should match the average retirement age in the target population) and whether the predictions are solid when the input factors of the model take a wide range of values. Newman As mentioned above, Change theories require articulation of a construction of change (e.g. learning, aging, social adjustment, inferred from a slope parameter in a growth model). A change construct must be specified in terms of its: (a) theoretical content (for example, what is changing, when we say learning or (b) form of change (linear versus quadratic versus cyclic) and (c) exchange rate (does the exchange process occur significantly for minutes versus weeks?). A major problem is how to develop the theory of the form of change (linear versus nonlinear/quadratic) and the rate of change (how fast?) For example, a quadratic/nonlinear time effect may be due to a substantive process of decreasing returns to time (for example, a learning curve), or to ceiling (or floor) effects (i.e., hitting the top end of a measuring instrument, after which it becomes impossible to see continuous growth in dormant construction). In fact, only a small fraction of the processes we studied would turn out to be linear if we used more widespread time frames in longitudinal design. That is, most seemingly linear processes result from the researcher approaching a nonlinear process in a way that truncates the time frame. This problem is directly related to the alleged rate of change of a phenomenon (for example, a process that appears nonlinear in a 3-month study might seem linear in a 3-week study). So when we are asked to theoretically justify why we hypothesize a linear effect rather than a nonlinear effect, we should derive a theory of what the passage of time means. This would involve three steps: a) naming the substantive process for which time is a marker (e.g. see answers to question #2 above), b) theorizing the rate of this process (e.g. for weeks versus months), which will be more fruitful if it depends on related empirical longitudinal research, that if it depends on armchair speculation about time (i.e., the sequence of development of the appropriate theory here is: data passed to theory → new data, and not simply, theory → to new data; the empirical origins of theory are → an essential step), and (c) disallow nonlinear forces (e.g. decrease in returns to time, periodicity), within the chosen time period of the study. RESEARCH DESIGN QUESTIONS Research design question 1: What are some of the main considerations to consider before deciding to use a longitudinal study design? Vancouver As with all research, design should allow the researcher to address the issue of research. For example, if you are trying to evaluate an exchange rate, you have to ask if it is safe to assume that the form of change is linear. If not, more than two waves will be needed or continuous sampling will be required. A computational model could also be used to assess whether linearity assumption violations are important. The researcher also needs to have an understanding of the likely period of time of which the processes under review occur. Alternatively, if the time frame is unclear, the researcher should continuously take samples or use short intervals. If one wants to know the shape of the change, then one will need enough waves of data collection in which to capture the changes in an integral way. If one is interested in evaluating causal processes, more issues need to be considered. Issues. for example, what are the processes of interest? What are the factors that affect processes or process rates? What is the form of the effect of these factors? And perhaps most importantly, what alternative process could be responsible for the observed effects? For example, consider proactive socialization (Morrison, 2002). The processes of interest are those involved in determining the proactive search for information. One observation is that the rate of proactive information you are looking for decreases with an employee's tenure (Chan & Schmitt, 2000). In addition, the shape of the fall is asymptomatic to a floor (Vancouver, Tamanini et al., 2010). The uncertainty reduction model predicts that proactive search for information will decrease over time because knowledge increases (i.e. uncertainty decreases). An alternative explanation is that ego costs grow over time: One feels he will look dumber asking for information the longer it lasts (Ashford, 1986). To distinguish these explanations for a drop in the search for information over time, one might want to examine whether transparency of the reason for seeking information would moderate the negative change trend of the search for information. For the uncertainty reduction model, transparency should not matter, but for the ego-based model, transparency and the legitimacy of reason must matter. Of course, both processes may be working. As such, the researcher may need a computational model or two to help think through the effects of the various processes and whether the forms of relationships depend on hypothetical processes (e.g. Vancouver, Tamanini et al., 2010). Research Design Question 2: Is there any design advantage of cross-cutting research that could make it preferable to longitudinal research? I mean, what would be lost and what could be gained if a moratorium was placed on cross-cutting research? Newman's cross-sectional research is easier to conduct than longitudinal research, but often estimates incorrect parameters. Interestingly, researchers often emphasize too much/talk too much about the first fact (ease of cross-cutting research), and little emphasis/talk too little about the latter fact (which cross-cutting studies estimate the wrong thing). Cross-cutting research has the advantages of enabling wider sampling of participants, due to faster and cheaper studies involving less load from participants; and a wider sampling of constructions, due to the possibility of anonymity of participants in cross-cutting designs, allowing a more honest and complete measurement of sensitive concepts, such as counterproductive work behavior. In addition, when the theoretical process in you have a very short period of time (for example, minutes or seconds), cross-sectional designs can be completely appropriate (for example, for factor analysis/measurement modeling, because it could only take a moment for a latent construction to be reflected in a survey response). In addition, the descriptive first-stage models of (e.g. gender pay differences; intercultural differences in attitudes; and other black box models that do not specify a psychological process) can be suggestive even with cross-cutting designs. Cross-sectional research can also be tolerated in the case of a 2-study design in which cross-sectional data is supplemented by lagged/longitudinal data. But in the end, almost all psychological theories are theories of change (at least implicitly) [Contrary to Ployhart and Vandenberg (2010), I tend to believe that cross-cutting theory does not really exist—theories are inherently longitudinal, while models and evidence can be cross-cutting.]. Therefore, longitudinal and time-lagged designs are indispensable because they allow researchers to begin answering four types of questions: a) causal priority, b) future prediction, (c) change, and (d) temporary external validity. To define and compare cross-sections with longitudinal and time-time designs, I refer to Figure 2. Figure 2 shows three categories of discrete-time designs: transverse (X and Y measured at the same time; Figure 2a), delayed (Y measured after X by a delay of duration t; Figure 2b), and longitudinal (And measured at three or more points in time; Figure 2c) designs. First, note that in all time designs, a1 denotes the cross-sectional parameter (that is, the correlation between X1 and Y1). In other words, if X is job satisfaction and Y is retirement intentions, a1 denotes the cross-sectional correlation between these two variables in t1. To understand the value (and limitations) of cross-sectional research, we will examine the role of the cross-sectional parameter (a1) in each of the models in Figure 2. Open in new tabDownload slideTime-based layouts for two constructions, X and Y. (a) cross-sectional design (b) delayed designs (c) longitudinal designs. To assess causal priority, delayed models and the panel model are more relevant. The time-delayed parameter b1 (i.e. the correlation between X1 and Y2; for example, predictive validity) helps in future prediction, but tells us little about causal priority. In contrast, the b1' regression parameter of the cross-delayed panel regression panel (in Figure 2b) and the cross-delayed panel model (in Figure 2c) tells us more about the causal priority from X to Y (Kessler & Greenberg, 1981; Texas, 1985), and is a function of parameter b1 and cross-sectional parameter a1 [b1'-(b1-a1rY1,Y2)/1-a12]. To test theories that X spawns Y (i.e. X→Y), the delayed parameter b1' can be extremely useful, while the cross-sectional parameter a1 is the wrong parameter (in fact, a1 is often negatively related to b1'). That is, a1 does not estimate X→Y, but so negatively related to that estimate (via the above formula for b1'). Using the example of job satisfaction and retirement intentions, if we want to know the causal priority from job satisfaction to retirement intentions, we should at least measure both job satisfaction and intent on t1 and then measure retirement intentions at t2. Deriving the estimate of b1' involves regressing retirement intentions in t2 on job satisfaction in t1, while controlling the effect of retirement intentions on t1. For future prediction, the self-regulating model and growth model in Figure 2c are more relevant. An illustrative empirical phenomenon is the degradation of validity, which means that the X-Y correlation tends to decrease as the time interval between X and Y increases (Keil & Cortina, 2001). The degradation of validity and stability patterns have been explained through simplex self-regulating models (Hulin, Henry, & Noon, 1990; Humphreys, 1968; Fraley, 2002), which expresses the X-Y correlation as rX1,Y1+k-a1gk, where k is the number of time intervals separating X and Y. Note that the cross-sectional parameter a1 in this formula serves as a multiplicative constant in X-Y correlation with time delays, but is usually very different from the X-Y correlation with time delay. Using the example of extraversion and retirement intentions, the degradation of validity means that the effect of extraversion on t1 to the extent of retirement intentions is likely to decrease over time, depending on what stable retirement intentions are. Therefore, relying on a1 to measure how well extraversion can predict future retirement intentions is likely to overesesy the predictive effect of extraversion. Another relevant model is the latent growth model (Chan, 1998; Ployhart & Hakel, 1998), which explains longitudinal data using an interception of time and a slope. In the linear growth model in Figure 2, the cross-sectional parameter a1 is equal to the relationship between X1 and the Y intercept, when t1 is 0. I also note that from the perspective of the growth model, the phenomenon of validity degradation (e.g. Hulin et al., 1990) simply means that X1 has a negative relationship with the Y slope. Therefore, again, the cross-sectional a1 parameter simply indicates the initial state of the X and Y ratio in a longitudinal system, and will only provide a reasonable estimate of future Y prediction under the rare conditions when g ≈ 1.0 in the self-regulating model (i.e. Y is extremely stable), or when i ≈ 0 in the growth model (i.e. , X does not predict the Y slope; Figure 2c). To study the change, I mean the growth model (where both X and intercept Y explain the change in Y [or Y-slope]) and the coupled growth model (where X-intercept, Y-intercept, change in X, and change in Y all interrelate) in Figure 2c. Again, in these models the cross-sectional parameter a1 is the relationship between the X and Y intercepts, when the slopes are specified over time t1 to 0 (where t1 arbitrarily refers to any point of time when cross-cutting data was collected). Just as interceptions tell us very little about slopes (despite the effects of roof and floor), the cross-sectional X1 parameter tells us almost nothing about the change parameters. Again, using the example of relationship of job satisfaction and retirement intentions, to understand the change in retirement intentions over time, it is important to measure the effects of the initial state of job satisfaction (i.e. interception of job satisfaction) and change in job satisfaction (i.e. pending job satisfaction) on the change in retirement intentions (i.e. pending retirement intentions). Finally, temporary external validity refers to the extent to which an effect observed at any given time generalizes on other occasions. This includes longitudinal measurement equivalence (for example, if the concept measurement metric or concept meaning may change over time; Schmitt, 1982), stability of bivariate relationships over time (e.g. job satisfaction is more weakly related to turnover when the economy is bad; Carsten & Spector, 1987), stationary parameters of cross delays on all measurement occasions (b1'-b2', see the cross-delayed panel model in Figure 2c; for example, Cole & Maxwell, 2003), and the ability to identify change as an effect of the participant's age/tenure/development, is not an effect of the birth/hire cohort or historical period (Schaie, 1965). Obviously, cross-sectional data has nothing to say about temporary external validity. Should there be a moratorium on cross-cutting research? Since any single wave in a longitudinal design is itself cross-sectional data, a moratorium is not technically possible. However, there must be (a) an explicit recognition of the different theoretical parameters in Figure 2, and (b) a general moratorium on the treatment of the cross-sectional parameter a1 as if it were causal priority (cf. regression parameter b1'), future prediction (cf. panel regression, self-regressive and growth models), change (cf. growth models) or temporary external validity. This recommendation is equivalent to a moratorium on cross-cutting research, because almost all theories involve the lagging and/or longitudinal parameters in Figure 2. As noted above, cross-sectional data is easier to obtain, but estimates the wrong parameter. Vancouver agrees with Newman that most theories have to be about change or should be (i.e., we are interested in understanding processes and, of course, processes happen over time). I also agree that cross-cutting designs have almost no value in evaluating theories of change. Therefore, I am interested in getting to a place where most research is longitudinal, and where top magazines rarely publish articles with only a cross-cutting design. Without as Newman points out, some research questions can still be addressed using cross-cutting designs. Therefore, it would not support a moratorium on cross-cutting investigative documents. Research Design Question 3: In a longitudinal study, how do we decide the length of the interval between two adjacent time points? Chan This question should be addressed along with the question of how many time measurement points to administer in a longitudinal study. It is. established that intra-individual changes cannot be properly evaluated at just two points of time because (a) a two-point measurement per need produces a linear trajectory and is therefore unable to empirically detect the functional shape of the true change path and (b) the time-related measurement error (random or correlated) and the true change over time are confused by the observed change in a two-point measurement situation (for details, see Chan, 1998; Rogosa, 1995; Singer & Willett, 2003). Therefore, the minimum number of time points for evaluating intra-individual change is three, but more than three is better to obtain a more reliable and valid assessment of the change path (Chan, 1998). However, this does not mean that a higher number of time points is always better or more accurate than a smaller number of time points. Because the total time period of the study captures the change of interest process, the number of time points must be determined by the appropriate location of the time point. This then leads us to the current practical question about the choice regarding the appropriate length of the interval between adjacent time points. The correct length of the time interval between adjacent time points in a longitudinal study is critical because it directly affects the observed functional shape of the change path and in turn the inference we make about the true pattern of change over time (Chan, 1998). So what should be the correct length of the time interval between adjacent time points in a longitudinal study? Simply put, the correct or optimal length of the time interval will depend on the phenomenon of specific substantive change of interest. This means that it depends on the nature of the substantive construction, its underlying process of change over time, and the context in which the change process is occurring that includes the presence of variables that influence the nature and rate of change. In theory, the time interval for data collection is optimal when the time points are properly spaced in such a way that it allows you to observe the true pattern of change over time during the study period. When the observed time interval is too short or too long compared to the optimal time interval, true change patterns will be masked or false change patterns will be observed. The problem is that we hardly ever know what this optimal time interval is, even if we have a relatively strong theory of the phenomenon of change. This is because our theories of research phenomena are often static in nature. Even when our theories are dynamic and focus on the processes of change, they almost always keep about the specific length of the temporal dimension through which substantive processes occur over time (Chan, 2014). In practice, researchers determine their choice of time interval length along with choosing the number of time points and choosing the duration of the total time period of the study. Based on experiences as an author, reviewer and editor, I suspect that these three options are influenced by the resource-specific constraints and opportunities researchers face in designing and conducting the longitudinal study. The deviation of optimal time intervals probably occurs more often than we would like, as decisions about the time intervals between measures in a study are often pragmatic and atheoretic. When interpreting the findings of longitudinal studies, we should consider the possibility that the study has produced patterns of results that led to erroneous inferences because the study did not reflect the real changes over time. Since our theories of phenomena are not at the stage where we could specify optimal time intervals, the best thing we could do now is explain the nature of change processes and the effects of influence factors to serve as guides for decisions about time intervals, number of time points, and total study time period. For example, in research on meaning-building processes in adaptation to the newcomer, the total study period often ranged from 6 months to 1 year, with 6 to 12 time points, equally spaced in 1 or 2 month time intervals between adjacent time points. A much longer time interval and a total time period, ranging from several months to several years, would be more appropriate for a change process that should take longer to manifest, such as developing cognitive processes or acquiring skills that require extensive practice or accumulation of experiences over time. At the other end, a much shorter time interval and total time period, ranging from several hours to several days, will be appropriate for a change process that should take a short time to manifest itself as the activation or inhibition of mood states prepared by experimentally manipulated events. Research Design Question 4: As events occur in our daily lives, our mental representations of these events may change as time goes on. How can we determine the points in time when the representation of an event is appropriate? How can these issues be addressed through design and measurement in a study? Beal In some cases, longitudinal researchers will want to know the nature and dynamics of immediate experiences. In these cases, items included at any given time will simply ask participants to report on states, events, or behaviors that are relatively immediate in nature. For example, one might be interested in an employee's immediate affective experiences, of tasks or help behavior. This approach is particularly common for intensive and short-term longitudinal designs, such as experience sampling methods (ESM; Beal & Weiss, 2003). In fact, the main objective of the ESM is to capture a representative sample of points within a day to help understand the dynamic nature of the immediate experience (Beal, 2015; Csikszentmihalyi & Larson, 1987). Longitudinal designs that have a longer measurement you can also capture immediate experiences, but more often ask participants to provide some kind of summary of these experiences, usually over the entire interval between each measurement occasion. For example, a panel design with a 6-month interval can ask participants to report affective statuses, but include a time frame such as since the last survey or over the past 6 months, requiring participants to mentally add their own experiences. As you might imagine, there are also several designs and approaches ranging from the endpoints of the immediate experience to the aggregated experiences throughout the entire range. For example, an ESM study might examine experiences since the last survey. These intervals are obviously close together in time, and are therefore conceptually similar to the immediate state of one; however, they require both higher levels of memory and some degree of mental aggregation. Similarly, studies with a longer time interval (e.g. 6 months) might, however, ask about relatively recent experiences (e.g. affecting over the past week), requiring less in terms of recovery and mental aggregation, but only partially covering events throughout the intermediate interval. As a result, these two approaches and the many variations between form an abstraction continuum that contains a number of differences worth considering. Differences in stability Perhaps the most obvious difference in this abstraction continuum is that different degrees of aggregation are captured. As a result, the elements will reflect more or less stable estimates of the phenomenon of interest. Consider the hypothetical temporal breakdown of the help behavior described in Figure 3. No matter how unstable the most disaggregated level of help behavior, aggregations of these behaviors

will always produce greater stability. Therefore, asking about how to help behavior during the last hour will produce greater observed variability (that is, across the entire scale) than aid behavior averages during the last day, week, month, or overall level of one. Although it is well known that individuals do not follow a strict averaging process when asked directly about a higher level of aggregation (e.g. help this week; see below), such deviations from a straight average are very unlikely to result in less stability at higher levels of aggregation. The reason why this increase in stability is likely to occur regardless of the actual process of mental aggregation is that presumably, as you go from shorter to longer periods of time, increasingly stable aspects are being estimated level of disposition of a building individual, or increasingly stable characteristics of context (e.g. a coherent working environment). As it moves from longer to shorter time frames, it is increasingly estimating the immediate instances of construction or context that are influenced not only by more stable predictors, but also dynamic trends, cycles and intermediate events (Beal & Ghandour, 2011). In particular, this stabilizing effect exists regardless of the differences in memory and mental aggregation described below. Differences in Fundamental memory to determine how people will respond to these different forms of questions is the nature of memory. Robinson and Clore (2002) provided an in-depth discussion of how we rely on different forms of memory by answering questions in different time frames. Although these authors focus on emotional experience reports, their findings are likely to apply to a much wider variety of auto-inuimmos. At one end of the continuum, immediate experience reports are direct, requiring only the interpretation of what is happening and minimizing mental recovery processes. Lowering the continuum slightly, we find elements that ask about very recent episodes (for example, since the last survey or in the last 2 hours in the STUDIES studies). Here, Robinson and Clore (2002) point out that we trust what cognitive psychologists refer to as episodic memory. Although memory is involved, the specific details of the episode in question are easily remembered with a high degree of accuracy. As the elements move further down the continuum towards summaries of experiences over longer periods of time (for example, since the last survey in a longitudinal panel design), the details of particular relevant episodes are more difficult to remember and therefore responses are more tanned by semantic memory. This form of memory is based on individual characteristics (for example, neurotic individuals could offer more negative reports), as well as well-learned knowledge based on the situation (for example, my co-workers are generally good people, so I'm sure I've been satisfied with my interactions during this time period). As the time limit for which people report increases, the nature of the information provided changes. Specifically, it is increasingly informed by semantic memory (i.e. situation-based trait and knowledge) and less and less informed by episodic memory (i.e. particular details of the experiences themselves). Therefore, researchers should be aware of the memory-related implications when choosing the time frame for their measurements. Differences in the process of summarizing Apart from the role of memory in determining the content of these reports, people also summarize their experiences in a complex way. For example, psychologists have shown that even during a single episode, people tend not to subjective summaries of the episode in its typical or average characteristics. Instead, we focus on particular notable moments during the experience, such as its peak or final state, and pay little attention to some aspects of the experience, such as its duration (Fredrickson, 2000; Redelmeier & Kahneman, 1996). The result is that a mental summary of a given episode is unlikely to reflect the actual averages of the experiences and events that In addition, when considering reports spanning multiple episodes (for example, during the last month or the interval between two measurements in a longitudinal panel study), the summaries become even more complex. For example, recent evidence suggests that people naturally organize experience streams into more coherent episodes largely based on the relevance of the objectives (Beal, Weiss, Barros, & MacDermid, 2005; Beal & Weiss, 2013; Zacks, Speer, Swallow, Braver, & Reynolds, 2007). Therefore, the way we interpret and analyze what is happening around us is strongly connected to our goals at that time. Presumably, this process helps us make sense of our experiences and predict what might happen next, but it also influences the type of information we have been with us in the episode, thus affecting how we might report on this time period. Practical Differences What can researchers then remove from this information to help decide what kind of elements to include in longitudinal studies? One topic that arises from the previous discussion is that summaries over longer periods of time will tend to reflect more about the individual and the meanings he or she may have imparted to experiences, events, and behaviors that have occurred during this time period, while short-term summaries or more immediate occurrence reports are less likely to have been processed through this type of interpretive filter. Of course, this does not mean that the most immediate end of this continuum is completely objective, as immediate perceptions still harbor many potential biases (e.g. attribution biases usually occur immediately); rather, immediate reports are more likely to reflect the immediate interpretation of events rather than an interpretation that has been reflected and considered in the light of an individual's short- and long-term goals, provisions, and world view. The particular choice of element type (i.e. immediate experiences versus aggregate experiences) that will be of interest to a researcher designing a longitudinal study should, of course, be determined by the nature of the research question. For example, if an investigator is interested in what Weiss and Cropanzano (1996) referred to as judgment-driven behaviors (for example, a calculated decision to leave the organization), then capturing how people make sense of relevant work events is probably more appropriate, so elements that ask one to add experiences over time can provide a better conceptual match than the elements being asked about immediate states. For the effect-driven behaviors or other immediate reactions to an event will likely be best served by reports asking participants for minimal mental aggregations of their experiences (e.g. immediate or in small periods of time). Chan The question of mental representations of events at particular times should always be discussed and assessed in the context of research into conceptual questions about the and change the processes that can take into account response patterns over time. Many of these conceptual issues are likely to relate to construction-oriented issues, such as the location of substantive construction on the continuum of the state trait and the period of time through which short- or long-term effects on temporary changes in substantive construction (e.g. the effects of stressors on health changes) are likely to manifest. On the question of aggregation of observations over time, I see it as part of a more basic question as to whether an individual's subjective experience in a substantive construction (e.g. emotional well-being) should be evaluated using momentary measures (e.g., assessing a person's current emotional state, measured daily over the past 1 week) or global retrospective reports (e.g., assessing the person's current emotional state, measured daily over the past 1 week) or global retrospective reports (e.g. , ask the individual to report an overall assessment of their emotional status in the last week). Each of the two measurement perspectives (i.e. momentary and global retrospective) has strengths and limitations. For example, momentary measures are less likely to recall biases compared to global retrospective measures (Kahneman, 1999). Global retrospective measures, on the other hand, are widely used in various studies for the evaluation of many constructions of subjective experience with a large database of tests relating to the reliability and validity of the measure (Diener, Inglehart, & Tay, 2013). In a recent article (Tay, Chan, & Diener, 2014), my colleagues and I reviewed the conceptual, methodological and practical issues in the debate between the momentary and global retrospective perspectives applied to research on subjective well-being. We concluded that both perspectives could offer useful ideas and suggested a multi-method approach that is sensitive to the nature of substantive construction and the specific context of use, but also called for more research on the use of momentary measures to obtain more evidence of its psychometric properties and practical value. Research Design Question 5: What are the biggest practical obstacles to conducting longitudinal research? What are the ways to overcome them? Beal As noted above, practical obstacles are perhaps one of the main reasons why researchers choose cross-cutting rather than longitudinal designs. While we have already discussed a number of these issues that need to be addressed in conducting longitudinal research, the following discussion emphasizes two obstacles that are ubiquitous, often difficult to overcome, and are particularly relevant to longitudinal designs. Encouraging incentives to continuing To encourage participation is a practical issue that all studies are likely to face, regardless of design; however, longitudinal studies raise special considerations as participants must complete measurements on multiple occasions. Although there is a small literature that has examined this topic specifically (e.g. Fumagalli, Laurie, & Lynn, 2013; Grove Groves al., 2006; Laurie, Smith, & Scott, 1999), it seems that the relevant factors are quite similar to those observed for cross-sectional surveys. In particular, providing monetary incentives before completing the survey is a recommended strategy (although non-monetary gifts may also be effective), with an increase in amounts resulting in an increase in participation rates, particularly as the burden of the survey increases (Laurie & Lynn, 2008). The impact of participants' burden is directly related to the special considerations of longitudinal designs, as they are generally more burdened. In addition, with longitudinal designs, the nature of the incentives used may vary over time, and can be adapted to reduce wear rates throughout the survey period (Fumagalli et al., 2013). For example, if the total monetary incentive is distributed among survey waves so that subsequent waves have higher incentive amounts, and if this information is provided to participants at the beginning of the study, then wear rates can be reduced more effectively (Martin & Loes, 2010); however, some research suggests that a larger down payment is particularly effective at reducing wear and tear throughout the study (Singer & Kulka, 2002). Moreover, the fact that longitudinal designs reflect an implicit relationship between the participant and researchers over time suggests that incentive strategies that are considered less effective in cross-cutting designs (e.g. incentives contingent on completion) may be more effective in longitudinal designs, as repeated evaluations reflect a continuous reciprocal relationship. In fact, there is some evidence that contingent incentives are effective in longitudinal designs (Castiglioni, Pforr, & Krieger, 2008). Taken together, a potential strategy to incentivize participants in longitudinal surveys would be to split the payment in such a way that a relatively large initial incentive has been provided before completing the first wave, followed by smaller but increasing amounts that are contingent on the completion of each successive panel. Although this strategy is consistent with the theory and evidence we have just discussed, it has not yet been explicitly proven. Continuous contact One thing that does seem true, particularly in longitudinal designs, is that incentives are only part of the image. An additional factor that many researchers have emphasized is the need to maintain contact with participants throughout the duration of a longitudinal survey (Laurie, 2008). Strategies here include obtaining multiple forms of contact information at the beginning of the study and continuously updating Information. Based on this information, researchers should make efforts to stay in touch with participants on measurement occasions (for panel studies) or some type of continuous basis (for ELDE or other intensive designs). Laurie (2008) referred to these efforts as Keeping Exercises In Contact (KITEs) and suggested that and perhaps a sense of commitment to the survey effort, and have the added benefit of getting up-to-date contact and other relevant information (e.g. job change). Data collection mode General considerations In panels designs, in relation to the intensive designs discussed below, only a limited number of surveys are sought and the interval between assessments is relatively large. Therefore, there is likely to be greater flexibility in the particular methods chosen to submit and record responses. Although the benefits, costs, and deficiencies associated with traditional paper and pencil surveys are well known, the use of Internet-based surveys has evolved rapidly, so the implications of using this method have also changed. For example, early survey design technologies for Internet administration were often complex and potentially costly. Simply adding items was sometimes a difficult task, and custom-formatted response options (for example, sliding scales with specific endpoints, ranges, and tick marks) were often unattainable. The web-based design tools currently available are often relatively inexpensive and increasingly customizable, but have maintained or even improved the level of ease of use. In addition, several studies have found that data collected using paper and pencil applications versus Internet-based applications are often comparable if not indistinguishable (e.g. Cole, Bedeian, & Feild, 2006; Gosling et al., 2004), although notable exceptions may occur (Meade, Michels, & Lautenschlager, 2007). One issue related to the use of Internet-based survey methods that is likely to be of increasing relevance in the years to come is the collection of survey data using a smartphone. From this writing (this area changes rapidly), smartphone options are in a development phase where there are some reasonably good options, but they have not yet matched the flexibility and standardized appearance that comes with most of the desktop or laptop web-based options they have just described. For example, it is possible to implement repeated surveys for a particular mobile operating system (OS; for example, Apple iOS, Google's Android operating system), but unless a member of the research team is competent in programming, there will be a not inconsiderable upfront cost to a software engineer (Uy, Foo, & Aguinis, 2010). In addition, as smartphone market share is currently divided into several mobile operating systems, a holistic approach will require software development for each operating system you can use it. There are a few other options, however, but some of these options are not entirely complete solutions. For example, survey management tools, such as Qualtrics, now allow you to test smartphone compatibility when creating web-based surveys. Therefore, one could create a survey using this tool and have people respond to it on their smartphone with little or no loss of fidelity. Unfortunately, these tools (again, at this point in time) do not offer elegant or flexible signaling capabilities. Para Para intensive designs of repeated measurements will often try to point to a reasonably large number (e.g. N to 50–100) of multiple random signals every day over several weeks. Achieving this task without the use of a built-in signaling function (for example, one that generates this pattern of random signals and alerts each person's smartphone at the right time) is not a small feat. However, there are several efforts underway to provide free or low-cost survey development apps for mobile devices. For example, PACO is a free Google app (currently) that is in the beta testing phase and allows great flexibility in the design and implementation of repeated surveys on Android OS and iOS smartphones. Another example currently being developed for Android and iOS platforms is Expimetrics (Tay, 2015), which promises flexible design and signaling features that are inexpensive for researchers who collect ESM data. These applications deliver the promise of high-access survey management and signaling and have the added benefit of quickly transmitting data to servers accessible to the research team. Ideally, these advances in the accessibility of survey administration will allow for higher response rates over the duration of the longitudinal study. Specific Issues of Intensive Designs All issues that have just been discussed with respect to the mode of data collection are particularly relevant for short-term intensive longitudinal designs, such as the EESM. As the number of measurement occasions increases, so do the needs to increase accessibility and reduce participants' burden whenever possible. Of particular relevance is the emphasis that the ESM places on obtaining on-site assessments to increase the ecological validity of the study (Beal, 2015). To maximize this benefit of the method, it is important to reduce the disruption introduced by survey administration. If the measurement frequency is relatively low (e.g. once a day), simple collection modes based on paper and pencil or on the web are likely to be sufficient without creating too much interference (Green et al., 2006). On the contrary, as measurements become increasingly intensive (for example, four or five times a day or more), it will be important to rely on the most accessible survey modes. Therefore, a format that allows the management of desktops, laptops, or smartphones should be more useful in these intensive designs. QUESTIONS ABOUT STATISTICAL TECHNICALS Statistical Questions 1: With regard to evaluating changes over time in a latent growth modeling framework, how can a researcher different conceptual issues by encoding the slope variable differently? Vandenberg As with many questions in this article, an in-depth answer to this particular question is not possible in the available space. Therefore, only a general treatment of the different encoding schemes of the slope or change variable is provided. Excellent detailed treatments of this topic can be found at Bollen Bollen Curran (2006, particularly chapters 3 and 4), and in Singer and Willett (2003, particularly Chapter 6). As Ployhart and Vandenberg (2010) pointed out, specifying the form of change should be an a priori conceptual effort, not a post hoc data-driven effort. This position was also previously declared by Singer and Willett (2003) in distinguishing between empirical (data-driven) strategies versus rational (theory-driven) strategies. Under rational strategies, on the other hand, theory is used to hypothesize a substantially significant functional form for the trajectory of individual change. Although rational strategies generally produce clearer interpretations, their dependence on good theory makes them somewhat more difficult to develop and apply (Singer & Willett[2003, p. 190). The last statement of the quotation simply reinforces the main topic throughout this article; that is, researchers must undertake the difficult task of introducing time (change is a form) into their conceptual frameworks to more appropriately examine the causal structure between focal variables within those frameworks. In general, there are three sets of functional shapes for which you can encode or specify the slope or change variable: (a) linear; (b) discontinuous; and (c) nonlinear. Sets emphasize that within each form there are different types to consider. The most commonly seen form in our literature is linear change (e.g. Bentein et al., 2005; Vandenberg & Lance, 2000). Linear change means that there is an expectation that the variable of interest should increase or decrease in a straight line function during study intervals. The simplest form of linear change occurs when measurement intervals are equal over time and observation units were obtained at the same time at those intervals. Assuming, for example, that there were four measurement occasions, the encoding of the slope variable would be 0 (Time 1), 1 (Time 2), 2 (Time 3), and 3 (Time 4). This encoding corrects the interception (initial value of the line) in the Time 1 interval, and therefore the conceptual interpretation of the linear change is performed relative to this starting point. Reinforcing the notion that there is a set of considerations, one may have a conceptual reason for wanting to set the interception to the last measurement occasion. For example, there may be an extensive training program anchored with a final exam on the last occasion, and one wants to study the development process resulting in the final score. In this case, the coding scheme can be 3, 2, 1 and 0 1 to time 4, respectively (Bollen & Curran, 2006, p. 116; Singer & Willett ,2003, 182). One may also have a conceptual reason to use half the time intervals to anchor the interception and look at the change above and below this point. Therefore, the coding scheme in the current example can be 1.5, 0.5, 0.5, and 1.5 for time 1 at time 4, respectively (Bollen & Curran, 2006; Singer & Willett, 2003). 2003). are other considerations in the linear set such as specifying linear changes in cohort designs or other cases where observation times vary individually (i.e., not all started at the same time, at the same age, at the same intervals, etc.). The latter may need to make use of missing data procedures, or the use of time variable covariates that explain differences in when observations were collected. For example, to examine how retirement influences life satisfaction, Pinquart and Schindler (2007) modeled life satisfaction data from a representative sample of German retirees who retired between 1985 and 2003. Due to differences in retirement time between participants (not all retirees at the same time or at the same age), different amounts of life satisfaction observations were collected for different retirees. Therefore, the missing observations annually were modeled as latent variables to ensure that the analyses were able to cover the entire time interval studied. Discontinuous change is the second set of functional ways with which the change in substantive focal variables could theoretically be described. Discontinuities are precipitated events that can cause the focal variable to accelerate rapidly (slope change) or dramatically increase/decrease the value (change in elevation) or both slope and elevation change (see Ployhart & Vandenberg, 2010, Figure 1 in p. 100; Singer & Willett [2003], 190–208, see Table 6.2 in particular). For example, according to stage theory (Wang et al., 2011), retirement can be a hasty event, because it can create an immediate honeymoon effect on retirees, dramatically increasing their energy level and life satisfaction as they pursue new activities and roles. This set of discontinuous functional form has also been referred to as part growth (Bollen & Curran, 2006; Muthén & Muthén, 1998-2012), but in general, represents situations in which all observation units are collected at the same time during time intervals and discontinuity occurs to all units at the same time. It is actually a variant of the linear set, and therefore it could have been presented earlier as well. To illustrate, let's say we're tracking individual performance metrics that had been steadily increasing over time, and suddenly the employer announces an upcoming overall bonus based on those metrics. A sudden increase (such as a change in slope) in those metrics could be expected based solely on reinforcement theory. Suppose, for example, that we had six measurement intervals, and the bonus announcement was made just Time 3 data collection. We could specify two slope or change variables and encode the first one as 0, 1, 2, 2, 2, and 2, and encode the second slope variable as 0, 0, 0, 1, 2, and 3. The latter specification would independently examine the linear change in each slope variable. Conceptually, the first pending slope variable change to the transition point (i.e. the last measurement before the announcement) while the second captures the change after the transition (Bollen & Curran, 2006). Regardless of whether the variables are dormant or observed only, if modeled using software such as Mplus (Muthén & Muthén, 1998–2012), the difference between the means of the slope variables can be statistically tested to assess whether the post-announcement slope is actually greater than the pre-announcement slope. You can also predict that your ad would also cause an immediate sudden elevation of the performance metric. This can be examined by including a dummy variable that is zero at all times before the announcement and one at all times after the announcement (Singer & Willett ,2003, 194-195). If the coefficient of this dummy variable is statistically significant and positive, then it indicates that there was a sudden increase (upward elevation) in the value after the transition. Another form of discontinuous change is one in which the discontinuous event occurs at different times for observation units (in fact it may not happen at all for some) and the intervals for data collection may not be evenly spaced. For example, suppose again that individual performance metrics are monitored over time for people in high-demand occupations with the first one collected on the hiring date. Let us also assume that these people are required to report when an external recruiter approaches them; that is, they are not prohibited from talking to a recruiter, but only have to report when it happened. Due to some cognitive dissonance process, people can begin to discount the current employer and reduce their inputs. Therefore, you can expect a change in slope, elevation, or both in performance. With regard to testing a possible change in elevation, the same mannequin-coded variable as described above is used (Singer & Willett, 2003). However, as to whether the slopes of performance metrics differ earlier than subsequent contact with the recruiter, it requires the use of a covariate that varies over time. The way this works specifically is out of reach here. However, Bollen and Curran (2006, pp. 192–218) and Singer and Willett (2003, pp. 190–208) offer excellent treatments on the subject. In general, a covariate that varies over time captures measurement intervals. In the current example, this can be the number of days (weeks, months, etc.) from the hiring date (when the reference yield was obtained) to the next measurement interval and all subsequent intervals. Person 1, for example, can have values 1, 22, 67, 95, 115, and 133, and was 3rd of the 72nd from the date of contracting. Person 2 can have values 1, 31, 56, 101, 141 and 160, and was contacted after time 2 on the 40th from the date of hiring. Referring to the reader's details beginning on Page 195 of Singer and Willett (2003), a new variable would be created from the where all values of this new variable before the hiring contact are set to zero, and the values after that to the difference on the days when contact was made with the measurement interval. Therefore, for Person 1, this new variable would have values 0, 0, 0, 23, 43, and 61, and for Person 2, the values would be 0, 0, 16, 61, 101, and 120. The slope of this new variable represents the increment (up or down) to what the slope would have been if the individuals had not been contacted by a recruiter. If it is statistically non-negligible, then there is no change in pre-slope contact in front of the post-recruiter. If it is statistically significant, then the slope after contact differed from that before contact. Finally, while much of the above is based on a multi-level approach to the implementation of change, Muthén and Muthén (1998-2012) offer a SEM approach to covariates that vary over time through their Mplus software package. The final functional shape to which you can encode or specify the nonlinear slope or change variable. As with the other shapes, there is a set of nonlinear shapes. The simplest in the set is when the theory states that the change in the focal variable can be quadratic (upward or downward curve). As such, in addition to the linear slope/change variable, a second change variable is specified in which the values of its slope are set to the squared values of the first linear change variable. Assuming five equally spaced measurement intervals encoded as 0, 1, 2, 3 and 4 in the linear change variable. The values of the second quadratic change variable would be 0, 1, 4, 9, and 16. The theory might state that there is also a cubic change. In that case, a third cubic change variable is entered with the values of 0, 1, 8, 27, and 64. One problem with the use of quadratics (or even linear change variables) or other polynomial forms as described above is that paths are unlimited functions (Bollen & Curran, 2006); that is, there is an assumption that they tend towards infinity. Most, if any, of the theoretical processes in the social sciences are unlikely to be truly limitless. If a nonlinear shape is expected, commissioning the change using an exponential path is probably the most realistic option. This is because exponential trajectories are limited functions in the sense that they approach an asymptote (either in growth and/or decay to the asymptote). There are three forms of exponential trajectories: a) simple where there is explosive growth of the asymptote; (b) negative when there is growth to an asymptote; and (c) logistics where this is asymptote at both ends (Singer & Willett, 2003). Obviously, of the slope or change variable would be attached to exponents that more closely represent the shape of the curve (see Bollen & Curren, 2006, p. 108; and Singer & Willett, 2003, Table 6.7, p. 234). There are other nonlinear considerations also that belong to this. For example, Bollen and Curran (2006, p. 109) 109) emission of cycles (recurring ups and downs, but which follow an overall upward or downward trend.) Again, the values of the change variable would be encoded to reflect those cycles. Similarly, Singer and Willett (2003, p. 208) address recoding when you want to eliminate nonlinearity in the change function through transformations to make it more linear. They provide excellent heuristics on page 211 to guide thinking on this topic. Statistical techniques question 2: In longitudinal research, are there additional measurement error problems that we should pay attention to, which are beyond those applicable to cross-cutting research? Wang's longitudinal research should pay particular attention to the issue of measurement invariance. Chan (1998) and Schmitt (1982) introduced the notion of alpha, beta and gamma change of Golembiewski and his colleagues (1976) to explain why measurement invariance is a concern in longitudinal research. When measuring a particular concept retains the same structure (that is, the same number of observed elements and latent factors, the same value and pattern of factor loads), the change in the absolute levels of the latent factor is called alpha change. Only for this exchange rate can we conclude that there is a specific form of growth in a given variable. When measuring a concept has to be adjusted over time (that is, different values or patterns of factor loads), a beta change occurs. Although the conceptual meaning of the factor remains the same about measurements, the subjective metric of the concept has changed. When the meaning of a concept changes over time (for example, having a different number of factors or different correlations between factors), a gamma change occurs. It is not possible to compare the difference in absolute levels of a latent factor when beta and gamma changes occur, because there is no longer a stable measurement model for construction. Notions of beta and gamma changes are particularly important to consider when conducting longitudinal research on aging-related phenomena, especially when using long time intervals in data collection. In such situations, the risk of finding beta and gamma changes is higher and can seriously jeopardize the internal and external validity of the investigation. Longitudinal analysis is often performed to examine how changes occur in the same variable over time. In other words, it operates on the alpha change assumption. Therefore, it is often important to explicitly test measurement invariance before modeling growth parameters. Without establishing the invariance of the measurement, it is unknown whether we are testing significant changes or comparing oranges. Several references have discussed procedures for testing measurement invariance under latent variable analysis (e.g. Chan, 1998; McArdle, 2007; Ployhart & Vandenberg, 2010). The basic idea is to specify and include measurement models in the with continuous or categorical indicators (see responses to statistical #4 below on categorical indicators). With the assumption of latent factor invariance, the factor loads between the measurement points must be constrained to be equal. Errors of different measurement occasions can be correlated, especially when measurement contexts are very similar over time (Tisak & Tisak, 2000). Therefore, error deviations for the same element over time can also be correlated to take into account common influences at the position level (that is, autocorrelation between positions). With the specification of the measurement structure, absolute changes in latent variables can be modeled by the mean structure. It should be noted that a stricter definition of measurement invariance also requires the same variance in latent factors. However, in longitudinal data this requirement becomes extremely difficult to meet, and factor variances can be sample-specific. Therefore, this requirement is often alleviated by testing measurement invariance in longitudinal analysis. In addition, this requirement may even be invalid when the nature of true change over time implies changes in latent variance (Chan, 1998). It is important to note that the mid-structure approach not only applies to longitudinal models with three or more measurement points, but also applies to simple repeated measurement designs (for example, the design of previous positions). Traditional paired sample t-tests and repeated measurements within subject ANOVAs do not take into account measurement equivalence, which simply uses the scores summed at two measurement points to perform a hypothesis test. The mid-structure approach provides a more efficient way to test changes/differences in a latent variable based on measurement errors (McArdle, 2009). However, sometimes it is not possible to achieve measurement equivalence by using the same scales over time. For example, in research on the development of cognitive intelligence in individuals from birth to late adulthood, different cognitive intelligence tests are administered at different ages (e.g. Bayley, 1956). In the applied settings, you can manage different domain knowledge or skill tests to assess employee competence at different stages of your career. Another possible reason to change measurements is the poor psychometric properties of the scales used in the previous data collection. Previously, researchers have used transformed scores (e.g. standardized scores within each measurement) before modeling growth curves over time. In response to criticism of these scaling methods, new procedures have been developed to model longitudinal data using modified measurement (e.g. rescoring methods, excessive time prediction, and modeling structural equations with convergent factor patterns). Recently, McArdle and his colleagues (2009) proposed a joint model approach that simultaneously estimated an element response theory (IRT) model and a latent curve model. They provided provided demonstrate how to effectively manage changing measurement in longitudinal studies using this proposed new approach. Vancouver is not sure that these measurement error issues are beyond cross-cutting issues as much as cross-cutting data do not provide mechanisms to address these problems, so they are simply ignored at the analysis stage. Unfortunately, this creates problems at the interpretation stage. In particular, the issues of random walking variables (Kuljanin, Braun, & DeShon, 2011) are a potential problem for longitudinal data analysis and interpretation of transverse or longitudinal designs. Random walk variables are dynamic variables that I mentioned earlier when describing the computational modeling approach. These variables have some value and move from that value. The expression of random walking comes from the image of a highly drunk individual, who is in some position but who staggers and swings from the position to the neighboring positions because alcohol has interrupted the stabilizers of the nervous system. This drunk individual might have a desired direction (called the trend if the individual can make any real progress), but there can be a lot of noise in that path. In the literature on aging and retirement, retirement savings can be seen as a random walking variable. Although the overall retirement savings trend should be positive (i.e. the amount of retirement savings should grow over time), at any given time, the exact amount added/earned in savings (or withdrawal/loss of savings) depends on a number of situational factors (e.g. stock market performance) and cannot be consistently predicted. Random walks (i.e. dynamic variables) have a non-independence between observations over time. In fact, one way to know if one is measuring a dynamic variable is if one observes a simplex pattern between the correlations of the variable with itself over time. In a simplex pattern, observations of the variable are more correlated when measured closer in time (for example, observations of time 1 correlate more highly with time 2 than time 3). Of course, this pattern can also occur if its proximal causes (rather than itself) is a dynamic variable. As noted, dynamic or random walking variables can create problems for poorly designed longitudinal research because you may not realize that the level of criterion (Y), for example, measured in time 3, was largely close to its level at time 2, when the alleged cause (X) was measured. In addition, in time 1 the criterion (Y) might have been busy moving the level of the causal (X) to the place where it is observed in time 2. That is, the criterion variable (Y) at time 1 is actually causing the alleged causal variable (X) at time 2. For example, actions can affect beliefs of self-efficacy in such a way that beliefs of self-efficacy end in alignment with performance levels. If one measures self-efficacy after it has been largely aligned, and then largely stable performance, a positive correlation between the two variables could be considered as a reflection of the influence of self-efficacy on performance due to measurement synchronization (i.e. measuring self-efficacy before performance). This is why the practice of measuring multiple waves is so important in passive observational panel studies. However, multiple measurement waves could still create problems for random walking variables, especially if there are trends and reverse causality. Consider self-efficacy to the performance example again. If performance is trending over time and self-efficacy continues along the back, a positive correlation between self-efficacy and subsequent performance (even if there is no or a weak negative causal effect) is likely to be observed because self-efficacy will be relatively high when performance is relatively high and low when performance is low. In this case, controlling the trend or past performance will usually solve the problem (Sitzmann & Yeo, 2013), unless the random walk has no trend. In the meantime, there are other issues that random walking variables can raise for both cross-sectional and longitudinal research, which Kuljanin et al. (2011) do a very good joint job. One problem related to longitudinal research is the non-independence of observations based on nesting within clusters. This issue has received great attention in multilevel literature (e.g. Bliese & Ployhart, 2002; Singer & Willett, 2003), so I'm not going to be more attracted to the point. However, there is one more non-independence issue that has not received much attention. Specifically, the problem can be seen when a variable is a delayed predictor of itself (Vancouver, Gullekson, & Bliese, 2007). With only three repeated measurements or observations, the correlation of the variable itself will average .33 at three points of time, even if observations are randomly generated. This is because there is a third chance that repeated observations are changing monotonically over the three time points, resulting in a correlation of 1, and a two-thirds probability that they are not changing monotonically, resulting in a correlation of .1, which has an average of .33. Therefore, on average it will appear that the variable is being caused negatively. Fortunately, this problem is quickly mitigated by more waves of observations and more cases (i.e. bias is largely eliminated with 60 pairs of observations). Statistical Techniques Questions 3: When analyzing longitudinal data, how should we handle missing values? Newman Revised by Newman (2014; see in-depth discussions of 2001, 2010; Little & Rubin, 1987; Newman, 2003, 2009; Schafer & Graham, 2002), there are three levels of lack of data (lack of data at the element level, lack of variable/level of construction and lack of level of person), two problems caused by lack of data (parameter estimation bias and low statistical power), three mechanisms of lack of data (missing completely in lack of chance/MAR, and is not random/MNAR), and a handful of common missing data techniques (list deletion, peer elimination, single allocation techniques, maximum probability, and multiple allocation). The next-generation advice is to use the highest probability (ML: EM algorithm, Complete ML information) or multiple allocation (MI) techniques, which are particularly superior to other missing data techniques under the missing sea mechanism, and work as well as, or better than, other missing data techniques under the MCAR and MNAR missing mechanisms (lack of MAR is a form of systematic misreliability in which the probability of missing data in one variable [Y] is related to the data observed in another variable [X]). Most of the controversy surrounding missing data techniques involves two misconceptions: (a) the misconception that list and peer-to-peer deletion are somehow more natural techniques involving fewer or fewer subt assumptions than ML and MI techniques, with the false belief that a data analyst can extract more secure inferences by avoiding newer techniques, and (b) the misconception that multiple imputation simply involves manufacturing data that was not observed. First, since all missing data techniques are based on particular assumptions, none is perfect. In addition, when it comes to selecting a missing data technique to analyze incomplete data, you should choose one of the above techniques (for example, in the list, in pairs, ML, MI). One cannot avoid the decision altogether, that is, abstinence is not an option. One must select the least among the evils. Because point deletion on the list and in pairs makes the extremely unrealistic assumption that missing data is completely randomly missing/MCAR (cf. Rogelberg et al., 2003), they will almost always produce a worse bias than ML and MI techniques, on average (Newman & Cottrell, 2015). List elimination can lead to even more extreme reductions in statistical power. Next, single allocation techniques (for example, mean substitution, stolastic regression allocation) (in which the missing data is filled in only once, and the resulting data matrix is analyzed as if the data had been completed) are seriously erroneous because they overestimate the sample size and underestimate standard errors and p-values. Unfortunately, researchers often get confused at the idea that multiple imputation suffers from the same problems as single imputation; it doesn't. In multiple account assignment, the missing data is populated several different times, and the resulting multiple allocated datasets are aggregated so that you realize the uncertainty in each account assignment (Rubin, 1987). Multiple imputation is not a composing data; it is an exercise to track the uncertainty of parameter estimates, examining the degree of variability through several vague guesses (given the available information). The operational word in multiple account assignment is multiple, not account assignment. Longitudinal modeling tends to a large amount of missing construction or variable-level data (i.e., omitting full-scale responses, an entire construction, or an entire wave of observation, such as wear). These conditions create many partial non-responders, or participants for which some variables have been observed and some other variables have not been observed. Therefore, a large amount of missing data in longitudinal designs tends to be MAR (for example, because the lack of data in time 2 is related to the data observed in time 1). Because the lack of variable level under the MAR mechanism is the ideal condition for which ML and MI techniques were designed (Schafer & Graham, 2002), ML and MI techniques (compared to peer elimination, peer elimination, and single allocation techniques) will typically produce much less sesathed estimates and more accurate hypothesis tests when used in longitudinal designs (Newman , 2003). In fact, the missing data techniques in ML are now the default techniques in LISREL, Mplus, HLM, and SAS Proc Mixed. Therefore, it is no longer excusable to perform longitudinal analyses in discrete times (Figure 2) without using missing data techniques in ML or MI (Enders, 2010; Graham, 2009; Schafer & Graham, 2002). Finally, because these new missing data techniques incorporate all available data, it is now increasingly important that longitudinal researchers do not surrender to the first non-respondents. Wear doesn't have to be a permanent condition. If an aspiring respondent decides not to respond to a survey request at time 1, the researcher should still try to collect data from that person at time 2 and time 3. More data: More useful information that can reduce bias and increase statistical power. Applying this advice to longitudinal research on aging and retirement means that even when a participant does not provide answers at some measurement points, continuing to make an effort to collect more data from the participant on subsequent waves may still be worth it. It will certainly help combat the issue of wear and tear and allow more usable data to emerge from longitudinal data collection. Questions on Statistical Techniques 4: Most existing longitudinal research focuses on the study of quantitative change over time. What if the variable of interest is categorical or if the changes over time are qualitative in nature? Wang I think there are two questions here: How to model longitudinal data of categorical variables, and how to model discontinuous change patterns of variables over time. In terms of longitudinal categorical data, there are two types of data that researchers often find. A data comes from measuring a sample of participants in a categorical variable in a few points of time (i.e. panel data). The research question that drives data analytics is to understand the state change from one moment to the next. For example, researchers might be interested in whether a population of older workers would remain employed or switch between (e.g. Wang & Chan, 2011). To answer this question, the employment situation (at work or unemployed) of a sample of older workers could be measured five or six times over several years. When the transition between qualitative states is of theoretical interest, this type of panel data can be modeled through Markov string models. The simplest form of Markov chain models is a simple Markov model with a single chain, which assumes (a) the observed state at the time t depends on the state observed at the time t-1, (b) the observed categories are free of measurement error, and (c) the entire population can be described by a single chain. The first assumption is in the hands of most, if not all Markov chain models. The other two assumptions can be released by latent Markov chain modeling (see Langeheine & Van de Pol, 2002 for a detailed explanation). The basic idea of latent markov strings is that the observed categories reflect the true state in latent categorical variables to some extent (i.e. the latent categorical variable is the cause of the observed categorical variable). In addition, because observations may contain measurement errors, a number of different observed patterns over time might reflect the same underlying latent transition pattern in a qualitative state. In this way, a large number of observed patterns (for example, a maximum of 256 patterns of a categorical variable with four categories measured four times) can be reduced to reflect a small number of theoretically consistent patterns (for example, a maximum of 16 patterns of a latent categorical variable with two dormant states over four time points). It is also important to note that subpopulations in a larger population may follow qualitatively different transition patterns. This heterogeneity in latent Markov chains can be modeled using latent markov blend modeling, a technique that integrates Markov latent modeling and latent class analysis (see Wang & Chan, 2011 for technical details). Since latent Markov modeling of the mixture is part of the general framework of latent variable analysis (Muthén, 2001), latent Markov models per mixture can include different types of covariates and results (latent or observed, categorical or continuous) of subpopulation membership, as well as the transition parameters of each subpopulation. Another type of longitudinal categorical data comes from measuring one or a few study units on many occasions separated by the same time interval (for example, every hour, day, month, or year). Studies examining this type of data are primarily aimed at understanding the temporal trend or trend in a phenomenon. For example, you can examine the cyclical trend of daily stressful events (w or not) over several months among a few employees. The goal of research could be to reveal multiple cyclic patterns within repeated events at stressful events, such as daily, weekly and/or monthly cycles. Another example is the study of the performance of a particular player or a sports team (i.e., lost or tied) in hundreds of games. The research question might be to find out factors that vary the time they could explain the cyclical patterns of game performance. The statistical techniques normally used to analyze this type of data belong to the time series categorical analysis family. A detailed technical review is beyond the current reach, but interested readers can consult Fokianos and Kedem (2003) for an expanded overview. Regarding the modeling of discontinuous variable change patterns, Singer and Willett (2003) and Bollen and Curran (2006) provided guidance on modeling procedures using the multilevel modeling framework or structural equation modeling. Here I briefly discuss two additional modeling techniques that can achieve similar research objectives: spline regression models and catastrophes. Spline regression is used to model a continuous variable that changes its path at a certain point in time (see Marsh & Cormier, 2001 for technical details). For example, newcomers' satisfaction with co-workers could increase steadily immediately after entering the organization. Then, due to a critical organizational event (e.g., the reduction of the company, a newly introduced policy to eliminate poor artists in the cohort of newcomers), the satisfaction of the co-workers of the newcomers may begin to decrease. A spline model can be used to capture the dramatic change in the attitude trend of newcomers in response to the event (see Figure 4 for an illustration of this example). The

time points at which the variable changes its path are called spline nodes. On spline nodes, two regression lines are connected. The location of the spline knots can be known in advance. However, sometimes the location and number of spline nodes are unknown before data collection. Different spline models and estimation techniques have been developed to take into account these different spline knot scans (Marsh & Cormier, 2001). In general, spline models can be considered as models based on dummy variables with continuity constraints. Some forms of spline models are equivalent to linear part regression models and are fairly easy to implement (Pindyck & Rubinfeld, 1998). Open in the new tabDownload Slide Geographical illustration of spline regression: The discontinuous change in satisfaction of newcomers with co-workers over time. Catastrophe models can also be used to describe a sudden (i.e., catastrophic) discontinuous change in a system For example, some organization systems develop from a certain state to uncertainty, and then switch to another particular state (for example, perception of performance; Hanges, Braverman, & Rentsch, 1991). This pattern of nonlinear dynamic change can be described by a cusp model, one of the most popular catastrophe models in the social sciences. Researchers have applied catastrophe models to understand various types of behaviors at work and in organizations (see Guastello, 2013 for a summary). Estimation procedures are also readily available models of catastrophes appropriate to empirical data (see technical introductions in Guastello, 2013). Statistical Techniques Question 5: Could you speculate on the next big deal in conceptual or methodological advances in longitudinal research? Specifically, describe a novel idea or a specific data analysis model that is rarely used in longitudinal studies in our literature, but could serve as a useful conceptual or methodological tool for future science at work, aging and retirement. Vancouver In general, but especially conceptually, I think we will see greater use of computational models to evaluate theory, design and analysis. In fact, I think this will be as big as multilevel analysis in future years, although the speed at which it will happen cannot be predicted. The main factors slowing the pace of adoption are the knowledge of how to do it and ignorance of the cost of not doing so (cf. Vancouver, Tamanini et al., 2010). The factors that will accelerate adoption are easy-to-use modeling software and training opportunities. My co-author and I recently published a tutorial on computational modeling (Vancouver & Weinhardt, 2012), and provided more details on how to use a specific, free and easy-to-use modeling platform on our website (. At the methodology level I believe that research simulations (i.e. virtual worlds) will increase in importance. They offer great control and the ability to measure many variables continuously or frequently. At the analysis level I anticipate greater use of Bayesian and Bayesian hierarchical analysis, particularly to evaluate the adjustments of the computational model (Kruschke, 2010; Rouder, & Lu, 2005; Wagenmakers, 2007). Chan I predicts that significant progress will be made in several areas in the near future through the proper application of latent mixing modeling approaches. These approaches combine different latent variable techniques, such as latent growth modeling, latent class modeling, latent profile analysis, and latent transition analysis into a unified analytical model (Wang & Hanges, 2011). They could also integrate continuous variables and discrete variables, whether predictors or result variables, into a single analytical model to describe and explain simultaneous quantitative and qualitative changes over time. In a recent study, my co-author and I applied an example of a latent model mix to understand the retirement process (Wang & Chan, 2011). Despite or rather due to the power and flexibility of these advanced mixing techniques to suit various models to longitudinal data, I will repeat the caution I made more than a decade ago: that the application of these models for evaluating changes over time should be guided by appropriate theories and relevant previous empirical findings (Chan, 1998). Vandenberg My hope or desire for the next big deal is the use of longitudinal methods to integrate the micro and macro domains of our literature into work This will involve combining aspects of growth modeling with multi-tiered processes. Although I don't have a particular conceptual framework in mind to illustrate this, my reasoning is based on the simple notion that it's the people who make the place. Therefore, it seems logical that we can, for example, study the change in some aspect of the company's performance over time depending on the change in some aspect of individual behavior and/or attitudes. Another example might be that we can study the change in household well-being throughout the retirement process based on the change in the individual well-being of the two partners over time. There are analytical tools to carry out such analyses. What is missing at this point are the conceptual frameworks. Newman hopes that the next big thing for longitudinal research will be dynamic computational models (Ilgen & Hulin, 2000; Miller & Page, 2007; Weinhardt & Vancouver, 2012), which encodes the theory in an appropriately longitudinal/dynamic way. If most theories are in fact theories of change, then this breakthrough promises to revolutionize what goes through theory in the organizational sciences (i.e., a computational model is a formal theory, with much more specific, risky and therefore more significant predictions about phenomena, compared to the informal verbal theories that currently dominate and are somewhat vague about time). My preferred approach is iterative: a) authors first collect longitudinal data, so (b) inductively build a parsimonious computational model that can reproduce the data, then (c) collect more longitudinal data and consider its goodness of fit with the model, then (d) suggest possible modifications to the model, and then repeat steps (c) and (d) iteratively until some convergence is reached (e.g. Stasser, 2000, 1988 describes one of those efforts in the context of group discussion). Exactly how to implement all of the above steps is not currently well known, but developments in this area can potentially change what we think is a good theory. Beal I don't know if my next big thing really reflects the wave of the future, or if it simply reflects my own hopes of where longitudinal research should be directed in our field. I'm going to play it safe and treat it like the latter. In line with several other answers to this question, I hope that researchers will soon begin to incorporate much more complex process dynamics into both their theorization and analysis methods. While process dynamics can (and do) occur at all levels of analysis, I am particularly excited by the prospect of linking them across at least adjacent levels. For example, basic researchers interested in the of the effect have recently begun to theorize and model emotional experiences using various forms of differential structural equation or space-state models (e.g. Chow et al., 2005; Kuppens, Oravecz, & Tuerlinckx, 2010), and, like the resulting parameters that people-to-people dynamics can be added to higher levels of analysis (e.g. Beal, 2014; Wang, Hamaker, & Bergeman, 2012), are inherently multilevel. Another example of models that capture this complexity and are increasingly used in immediate and long-term longitudinal research are multivariate latent change scoring models (Ferrer & McArdle, 2010; McArdle, 2009; Liu et al., 2016). These models extend LGMs to include a wider range of sources of change (e.g. self-regulating and cross-delay factors) and therefore capture more than the complexity of changes that can occur in one or more variables measured over time. All of these models share a common interest in modeling the underlying dynamic patterns of a variable (e.g. linear, curvilinear or exponential growth, cyclic components, feedback processes), while considering shocks for the underlying system (e.g. affective events, organizational changes, etc.), allowing them to better evaluate the complexity of dynamic processes more accurately and flexibly (Wang et al. , 2016). Wang, I believe that the implementation of a dynamic systems framework will advance our research a lot. Implementation of the Dynamic Systems Framework (e.g. DeShon, 2012; Vancouver, Weinhardt, & Schmidt, 2010; Wang et al., 2016) forces us to more explicitly conceptualize how changes develop over time in a particular system. Dynamic system models can also better answer the question why by specifying how elements of a system work together over time to achieve observed system-level change. Studies on dynamic system models also tend to provide richer data and more detailed analysis of processes (i.e. black boxes not measured in traditional research) in a system. A number of research design and analysis methods are available relevant to dynamic system frameworks, such as computational modeling, EESM, event history analyses and time series analyses (Wang et al., 2016). RECOGNITIONS M. Wang's work in this article was supported in part by the Dutch Institute for Advanced Studies in Humanities and Social Sciences. References. (). Time and emotions at work. En (Eds.), (Vol. , pp. –). : . . (). 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