

Problematics of Time and Timing in the Longitudinal Study of Human Development: Theoretical and Methodological Issues

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Key Words

Individual development · Intraindividual change · Longitudinal research ·
Time and timing of observations

Abstract

Studying human development involves describing, explaining, and optimizing intraindividual change and interindividual differences in such change and, as such, requires longitudinal research. The selection of the appropriate type of longitudinal design requires selecting the option that best addresses the theoretical questions asked about developmental process and the use of appropriate statistical procedures to best exploit data derived from theory-predicated longitudinal research. This paper focuses on several interrelated problematics involving the treatment of time and the timing of observations that developmental scientists face in creating theory-design fit and in charting in change-sensitive ways developmental processes across life. We discuss ways in which these problematics may be addressed to advance theory-predicated understanding of the role of time in processes of individual development.

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Development involves intraindividual change and the study of development seeks to describe, explain, and optimize the course (trajectories) of such changes and, as well, interindividual differences in intraindividual change across the life span [Baltes & Nesselrode, 1973; Baltes, Reese, & Nesselrode, 1977; Nesselrode & Baltes, 1979; Nesselrode & Ram, 2004]. In other words, there are three goals of developmental science: (1) describe the nature of development for individuals and

Table 1. Features of key instances of longitudinal designs

Single-cohort designs

A group of individuals (e.g., members of the same birth cohort, people born in 1990) is studied repeatedly, that is, at multiple occasions (2 or more times, e.g., 2000 and 2001).

Panel designs

A cohort (e.g., people born in 1990) is studied at 3 or more times (e.g., 2000, 2001, and 2002). Collins [2006] defines these designs as involving 8 or fewer times of measurement that are separated by at least 6 months.

Intensive designs

A person or group is studied on a large number of occasions. Collins [2006] defines these designs as involving at least 20, relatively closely spaced times of measurement. However, Nesselroade [e.g., Nesselroade & Ford, 1987] indicates that, when a single person is studied, there may be 100 or more times of measurement.

Sequential designs

Multiple cohorts of individuals are studied repeatedly, typically at 3 or more times. Attrition and retest control groups are often part of sequential designs.

Accelerated designs

Multiple cohorts of individuals are studied repeatedly, and the cohorts differ in age at time 1 of measurement. However, the initial ages of the cohorts (e.g., 10, 12, 14, and 16 years of age), and the points in time at which they are measured (e.g., 2000, 2001, and 2002), are selected to assure that members of the different cohorts will be of at least one identical age at different time points. The overlap of ages enables change across years to be estimated in less than elapsed calendar time.

groups; (2) explain the nature of development for individuals and groups, and, because of the interest in using developmental science to improve the human condition, (3) optimize, as much as possible, the trajectories of development.

To study development, therefore, one needs to conduct research that appraises intraindividual changes. One must, therefore, conduct research that is longitudinal in design [Baltes, et al., 1977; Collins, 2006; Little, Card, Preacher, & McConnell, in press; Nesselroade & Baltes, 1979; von Eye, 1990a, b].

However, the term *longitudinal design* is a generic label pointing to an array of different temporal designs used to observe change phenomena. Just as there are numerous designs for controlled experiments (e.g., the two-group test and control design, or the Solomon four-group design) [Solomon & Lessac, 1968], there are also various longitudinal or temporal designs [Collins & Graham, 2002; McGrath & Tschan, 2004]. For instance, Collins [2006] differentiated between panel and intensive longitudinal designs and, in turn, Nesselroade [Corneal & Nesselroade, 1991; Nesselroade & Ford, 1987] and Collins provided different specifications for the features of intensive longitudinal designs. Schaie [1965], Schaie and Strother [1968], and Baltes et al. [1977] contrasted single-cohort longitudinal designs with sequential design strategies and, for instance, described several variations of the latter methods (e.g., cohort sequential, time sequential, and cross-sectional sequential designs). In

addition, Collins discussed accelerated longitudinal designs, which are special cases of cohort sequential methods. Table 1 provides some of the defining features of key instances of longitudinal designs.

Any of these temporal designs may be useful, depending on the theory-predicated questions asked by an investigator. However, our purpose in this paper is not to review these designs (a task recently addressed by Little et al. [in press]), but rather to explore several important issues that arise when *any* temporal or longitudinal design is selected on the basis of fitting *any* theory-derived developmental question. That is, this paper focuses on issues pertinent to several interrelated problematics that arise when investigators are making decisions about theory-design fit. These problematics involve the treatment of time and the timing of observations that developmental scientists face when seeking to chart in change-sensitive ways changes in developmental processes across life, and they are not specific to any particular longitudinal research design.

A key reason why consideration of these problematics is important is that decisions about which temporal design to use should not be based on what Lerner and Overton [2008] discussed as mindless methodologism, that is, allowing the preference for one or another research design to dictate or constrain the questions one asks about the substance of human development. Moreover, in the ideal world (although not typically in the practical world of conducting research with human participants), these decisions should not be based on convenience, staff resources, or funding. As explained below, theory should be the primary basis for selecting the methods used in developmental research.

Three Bases of Methodological Decisions in Longitudinal Research

As discussed by Collins [2006], Little et al. [in press], Lerner [2002], and others [see Nesselroade & Baltes, 1979; von Eye, 1990a, b], decisions about designs should be based on three things. First, and most important, methodological choices should be based on theory. For instance, Collins noted that only when there is a ‘well-articulated theoretical model of change’ (p. 507) can longitudinal research begin to approach the ideal of providing ‘clear and unambiguous’ answers to questions about the nature of human development. In addition, Little et al. emphasized that ‘longitudinal data are best suited for testing hypotheses derived from well articulated models of change. In this regard, theory is the [data] analyst’s best friend’ (p. 6). In short, the developmental question one is asking and the theoretical conception of the substantive phenomenon/process one is studying should be the basis of any decisions about methodology.

The second key question thus becomes, ‘what design will elucidate the theoretical issues I am addressing?’ That is, the investigator must seek to optimally match the theory about the substantive phenomenon of interest with the design that will best elucidate changes in the phenomenon. In developmental science, this question becomes, ‘what temporal (longitudinal) design affords optimal fit with the theory-predicated question of the developmental process being addressed?’

Third, the investigator must match the design that is selected with statistical procedures that will best exploit the data that will be collected. The results of statistical analyses, and the resulting conclusions drawn about the study hypotheses, can

then be used to refine or extend theory and to design intervention programs to promote successful development. The study of development therefore requires a dynamic interplay among theory, research design, and statistical analysis [Collins, 2006].

These ideas are not new, as evidenced by the dates of many of the above-cited works. However, recent chapters by Collins [2006] and Little et al. [in press] highlighted the need to continue to push these ideas to the forefront of developmental science. Our goal in this paper is to support this effort by highlighting a set of issues that we call problematics. These issues are topics that must be addressed in longitudinal research if it is going to be able to serve the three important functions of developmental science that we listed above – describing, explaining, and optimizing developmental trajectories.

Accordingly, the purpose of this article is to build on the work of Collins, Little, Nesselrode, Baltes, von Eye, and others, and to discuss the second issue that must be addressed in conducting longitudinal studies of human development, that is, the fit between theory and type of longitudinal design. We will neither review the array of theories of development that exist in contemporary developmental science [for such a discussion, see the chapters in Lerner, 2006], nor discuss in detail the statistical procedures useful for elucidating developmental data [for such appraisals, see Singer & Willett, 2003; Walls & Schafer, 2006]. By focusing on theory-design fit, however, we believe we can illuminate several issues that must be addressed when selecting a temporal design to observe change phenomena, issues that arise independent of whether one is conducting research framed by theoretical models that focus on either qualitative change (e.g., stage theories of development [Piaget, 1970]), on quantitative change (e.g., in scores for fluid or crystallized mental abilities [Horn, 1968]), or on both quantitative and qualitative changes (e.g., dynamic system models of infant motor development [Thelen & Smith, 2006]). In our view, chief among these issues are problematics of conceptualizing time and of the timing of observations. These two interrelated problematics must be addressed during the decision making process of matching theory about the specific substantive phenomenon with the temporal design used to observe changes in the phenomenon. These problematics underscore the tension, or dynamism, between theory and the choices about longitudinal designs. Moreover, the problematics that we raise here are applicable regardless of the specific longitudinal research design selected. Defining and operationalizing time, for example, is essential to single-cohort designs as well as to accelerated, multiple-panel, and cross-sequential designs.

We should clarify the ways in which we will use the terms *study design*, *methodology*, and *statistical analysis*. Study design refers to the specific longitudinal approach adopted, as well as the problematics of how time will be conceptualized and spaced. Methodology refers to specific issues within the design selected, such as the measures chosen to assess the study constructs, the mode of assessment, and procedures to ensure participant retention. We will pay less attention to these specific methodological decisions in this article and will focus primarily on larger study design issues. *Statistical analysis* refers to the analytic methods used to summarize and interpret the data generated in the study. Although some of the problematics we cover here, such as aggregation of data points, have important implications for statistical analyses, issues of which specific analytic method to use, how to parameterize the analysis, and how to handle attrition and missing data, for example, are be-

yond the scope of the present article. We refer readers to recent sources in statistical methodology, such as Singer and Willett [2003] and Walls and Schafer [2006], for thorough treatments of longitudinal data analysis.

Dividing (or Divining) the x-Axis: The Temporal Spacing of Observations

Arguably, the temporal division (spacing) of the x-axis in longitudinal research is the most unrecognized problematic in developmental science. Both researchers and the funding agencies that support their work have shown little recognition that the use of calendar time to divide the axis is not necessary, that age may not be the best way to represent time, or that equal interval spacing along the axis may not be appropriate in all situations [Collins, 2006; Little, et al., in press]. Moreover, there has not been awareness that annual, semi-annual, or even shorter temporal divisions may not be appropriate conceptually. In other words, time-related changes in developmental processes may occur at the same rate across units of time arrayed across the x-axis or, alternatively, at different rates across adjacent or nonadjacent demarcations of the x-axis. As such, we believe that when, and how often, to measure (observe) should be the primary questions – and that theoretical understanding of the process of change must be used as the frame for addressing these questions. That is, the frequency and rate of change specified by theory should be used to guide the selection of assessment points for any given research study.

If, as we argue, a given developmental process may not change at a constant rate across all of ontogeny, then it is also the case that the variation observed could occur for both qualitative and quantitative facets of development. Qualitative change is by definition discontinuous; qualitative change means that something new, not reducible to prior forms, has emerged [Lerner, 2002; Werner, 1957]. Qualitative change may emerge in gradual and continuous ways (e.g., as in the notion of stage mixture discussed by Turiel [1969], when the transition between developmental stages is marked by the increasing presence of the structures, or qualities, of a new stage and the decreasing presence of the structures of prior stages) or marked by an all-or-none type change, as in Anna Freud's [1969] idea that the rapid emergence of the sex drive creates a developmental disturbance in early adolescence. In turn, quantitative change is continuous when the slope of change remains the same across a set of sampled points in ontogeny, and is discontinuous when the slope changes across adjacent ontogenetic points (a phenomenon that Werner [1957] labeled abruptness). For either qualitative or quantitative changes, then, the developmental process may be faster, slower, or take different forms across a set of sampled points in ontogeny.

For instance, feelings pertinent to emotional well-being (e.g., self-esteem) may fluctuate rapidly among adolescent girls during the transition from elementary school to middle school [e.g., Simmons & Blyth, 1987]. In the spring of the pre-transition school year (i.e., fifth grade) self-esteem may be high, but it may decrease precipitously in the fall of the post-transition school year (sixth grade), show some positive increase by mid-year (but still be substantially lower than the pre-transition point in time) and, by the spring of the post-transition year, self-esteem may return to the pre-transition level. Whereas annual assessments of self-esteem may have been useful to mark the level of this construct during the elementary school years, and

annual spacing along the x-axis may again be useful for the second and third years after transition, such a division during the first post-transition school year would mask the curvilinear character of the course of self-esteem change during this period. Within this transition period, finer divisions of the x-axis would be needed to index with sufficient sensitivity the actual course or form of change in this construct.

Accordingly, divisions of the x-axis – the point in ontogeny when observations are taken (when data are collected, as in the example of adolescent girls' self-esteem, the spring of grade 5, grade 6, or grade 7, etc., for a particular cohort of participants) – do not need to be spaced evenly, and spacing them evenly may actually misrepresent the developmental process being investigated. Inappropriate aggregation can result in misrepresenting the form of the trajectory, such as fitting a linear slope to a nonlinear trajectory. Although this can happen regardless of how the time points are spaced, arbitrarily deciding to space them evenly (which is the most common x-axis spacing decision in longitudinal research) represents one way in which this problem can be created. Ideally, spacing should be dictated by theoretical understanding of the form of the developmental process (e.g., regarding when there are 'spurts' in growth, that is, increases in y-axis amounts of a variable that are greater than prior y-axis levels of increase, i.e., that are associated with a marked change – or, in the terms of Werner [1957], with a quantitative discontinuity – in the slope, y intercept, of a developmental function) or, as illustrated by the work of Simmons and Blyth [1987], at the least, by empirical generalizations derived from prior research regarding when such quantitative discontinuities tend to occur. Although advanced statistical methods are useful in summarizing trends in the data, they cannot 'save' a data set where data points were collected too far apart or too closely together.

For example, if the theoretically consistent trend in the data is linear, but the data collection intervals were too fine, a discontinuous trend might be suggested by the results – regardless of the analytic techniques used. Similarly, if the data are not collected frequently enough, then the true patterns in the data may be missed, and no statistical technique will be able to recover the true pattern. As a result, theory should be consulted first during the design phase of a study; although theory must also guide analytic decisions, advanced analyses cannot compensate for problems in the study design.

Another example of such an empirical generalization might be the spurts in height in adolescence. Of course, there is not a lot of theory that is sufficiently temporally nuanced to allow investigators to know when to time observations. Issues of stasis, constancy of rate of change increases, or of irregularly timed spurts in growth are not documented or even theoretically conceptualized for many developmental processes. How can this problematic be addressed, therefore?

One solution is to return to the ideas of Wohlwill [1970] and include time in the definition of the dependent variable. For our purposes, we will simplify Wohlwill's ideas and place time on the y-axis, that is, make time the dependent variable. Wohlwill's work helps elucidate whether temporal divisions arrayed across the x-axis reflect with veridicality the rate and form of developmental processes. Wohlwill suggested that developmentalists assumed that processes unfolded in equally timed intervals and, as a consequence, organized the x-axis based on this assumption which, he pointed out, was rarely, if ever, tested. However, if the levels, or

Table 2. Scores for an indicator of a developmental process

Participant	Age			Mean
	5	10	15	
1	8	9	10	9.0
2	0	4	5	3.0
3	1	4	7	4.0
4	1	2	6	3.0
5	0	1	2	1.0
Mean	2.0	4.0	6.0	

phases (stages), of a process did unfold in a manner commensurate with a linear and quantitatively continuous progression of time, then one could invert the x- and y-axes and place the levels of the process along the x-axis and see whether, in fact, the assumption of linear and quantitatively continuous time progression was warranted.

In short, such a test would provide heretofore unavailable information about the quantitative continuity of the temporal parameter and the form of a given change process. In other words, our use for methodological purposes of Wohlwill's [1970] ideas involves plotting the variable in question as the x-axis, and plotting the time associated with each given score as the y-axis. Just as the traditional plotting method requires specifying the mean scores for each age group (or other demarcation of time), our adaptation of Wohlwill's work implies specifying the mean age/time for each score on the variable in question. To illustrate the application of Wohlwill's [1970] ideas for the analysis of longitudinal data, consider the imaginary data for a study of a developmental process presented in table 2. Figure 1a presents a graph showing the typical way in which these data are depicted in developmental analyses. Differences in scores for the developmental process are presented as a function of equal interval age differences, as if age itself is responsible for the changes. As shown in figure 1a, the data suggest that the process linearly increases across ages 5–15 years. However, if the developmental process actually has the time-ordered characteristics depicted in figure 1a, then the process should appear the same when scores for age are presented as a function of different scores for the developmental process.¹

¹ The data represented in the figure pertain to an aggregate growth curve. In contemporary developmental science, views about best practice in growth curve analysis emphasize the advantages of methods of individual growth curve fitting [Newell & Molenaar, 1998]. Our purposes in this example are not related to a discussion of these methods or of their advantages in comparison to aggregate growth curve analysis. Our goal here is more fundamental, that is, to discuss the issue that theory affects the treatment of data and thus the findings (the curves) one observes. That is, whether one is fitting an individual or an aggregate curve, one may in some way code, organize, aggregate, etc. that is shaped primarily by theory and not by the empirical array with which one is working. We only seek in the present discussion, then, to show that in any of these steps theory affects data treatment.

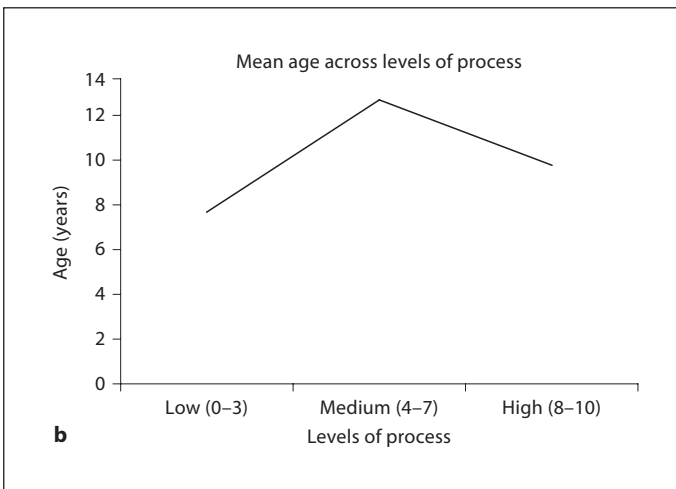
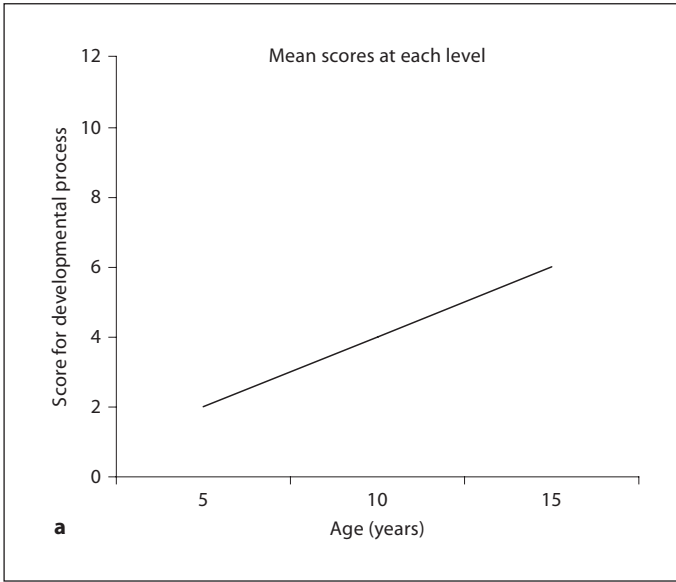


Fig. 1. **a** Scores for a developmental process as a function of equal interval age differences. **b** Age as a function of different levels of a developmental process.

As we have noted, if divisions of the new x-axis cannot be established on the basis of theory or past research, then, with recognition of cautions about generalizability beyond a given sample, x-axis divisions could be made empirically (e.g., on the basis of quartile scores).

In any case, scores for the developmental process should be arranged in ascending order, and the participant ages that correspond to each score (or set of scores)

should be plotted on the y-axis. If the same curve emerges for the inverted and non-inverted forms of the plot, then equal intervals of chronological age may well serve as an adequate index of time. However, it is also possible that when such inversion of the x- and y-axes is done, a very different form for developmental change will be evident. This difference is illustrated in figure 1b. The data presented in table 2 are again used. However, the developmental process appears curvilinear, as an inverted U-shaped function.

Which depiction of the process is correct? To answer this question, theory, once again, should take precedence. In cases where theory is weak or even absent, developmental scientists can pursue another, iterative path based again on Wohlwill's [1970] work. Data sets can be explored for divisions of both time and process until the fit between the inverted and noninverted (traditional) depictions of the developmental function is maximized. Specifically, one could create an index of fit between the inverted and noninverted curves, and the x-axis partitioning scheme for which this fit index is maximized would be selected tentatively as the 'correct' metric and scaling for time. Note that this maximization of fit does not necessarily imply that the original and inverted curves are equivalent – just that they can be reconciled with one another in a logical and meaningful way. Of course, scaling decisions reached through this method would need to be cross-validated with another sample. Nevertheless, through such iteration and cross-validation, the nuanced understanding of developmental change may be advanced and, as well, the extent to which time in general, or specific portions of ontogenetic time in particular, represent important parameters of the form of change may be identified.

Moreover, as is implicit in the above imaginary example, the x- and y-axis inversion procedures we have illustrated can be used to test whether theoretical ideas about the time-ordered character of a process are correct. This use of the inversion procedure we have described can be illustrated by reference to data from an actual longitudinal data set, in this case the 4-H Study of Positive Youth Development [Lerner et al., 2005; Phelps et al., 2007], a large, national study involving, to date, more than 4,000 youth from 30 states across the United States. Puberty data from the 4-H Study are used here to illustrate the theory testing use of the inversion procedure.

Beginning in grade 5 (about 10 years of age for most of the participants) measures of pubertal development were gathered, using the Petersen [1983; Petersen, Tobin-Richards, & Boxer, 1983] measure. Height data were collected as part of this assessment. If pubertal change proceeds as a developmental process, then the height measure should be correlated with time. Accordingly, if we use gradations of this measure as divisions of the x-axis and the mean age of attaining a particular height as the dependent variable, evidence can be produced about whether puberty, as indexed by height changes, is a time-related change process. Figure 2 shows the results of this analysis for male and female participants in the 4-H Study (in all graphs in this figure, data points are randomly sampled to make the graphs readable). The relation between age and the height index of puberty remains essentially the same when height is the dependent variable (fig. 2a) or when age is placed on the y-axis (fig. 2b). They both show that for girls and boys, there are age ranges where weight and age are linearly related and other ranges where growth is 'flat'. However, by putting a potential causal (and age-related) factor, pubertal level, on the x-axis as shown in figure 2c, a linear pattern emerges for boys and girls. When height is plotted

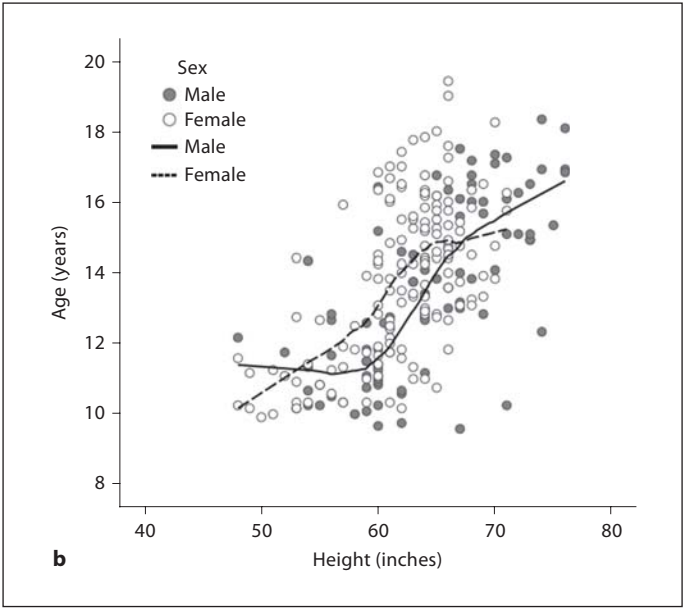
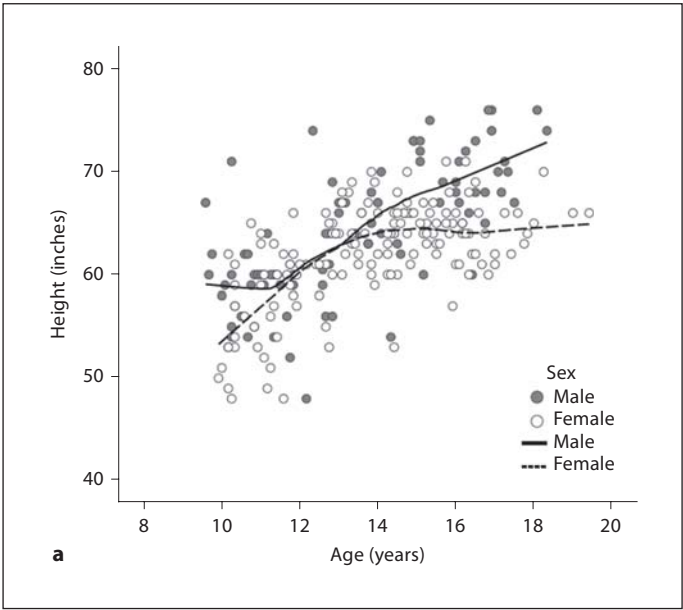


Fig. 2. a Height for male and female adolescents as a function of age. **b** Age for male and female adolescents as a function of height.

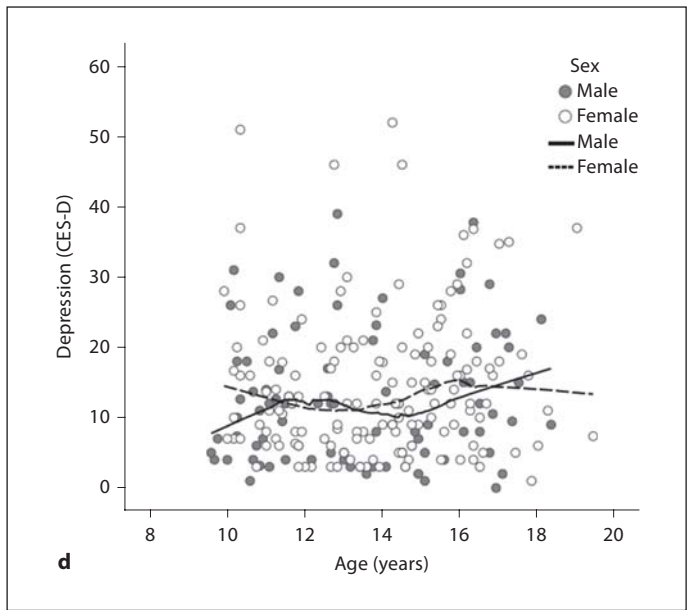
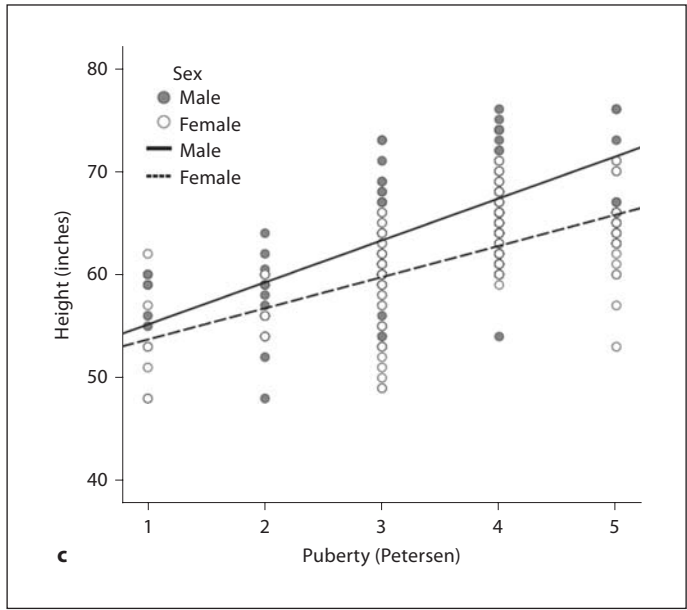


Fig. 2. c Height in male and female adolescents as a function of differences in pubertal development scores (as measured by the Petersen scale). **d** Depression in male and female adolescents as a function of age.

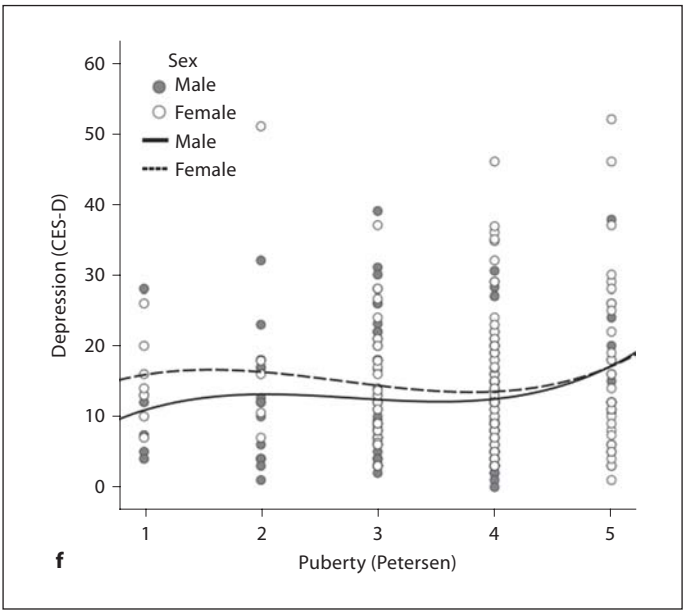
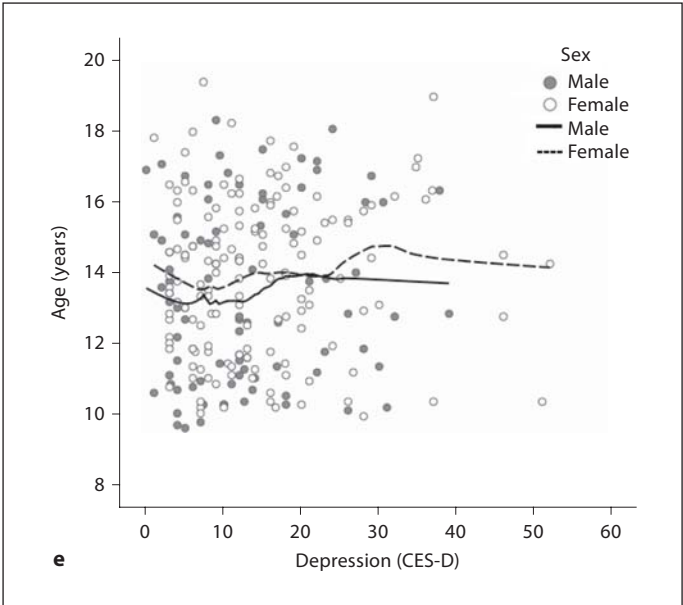


Fig. 2. e Age for male and female adolescents as a function of differences in scores for depression. **f** Depression scores for male and female adolescents as a function of differences in pubertal development scores (as measured by the Petersen scale).

against pubertal stage, the relationship is linear and represents our common understanding that boys steadily gain on girls during adolescence. This pattern is much clearer than the relationship between height and age shown in figure 2a. As a result, in this case, age is *not* the best measure of developmental time.

The example of pubertal change may be an ideal sample case to demonstrate the use of Wohlwill's [1970] ideas for validating the time-ordered nature of a purported developmental process, in that there is in fact abundant physiological and morphological data indicating the time-ordered sequence of pubertal maturation and that, although there are atypical conditions where pubertal changes may be reversed (e.g., in anorexia), for the preponderant majority of people, maturational change is ordered in an invariant sequence [Susman & Dorn, in press; Tanner, 1991]. However, the theoretical challenges of dividing the x-axis in ways appropriate to the rate of change in a phenomenon may be more complicated in regard to phenomena associated with cognitive development [e.g., Feldman, 1980; Piaget, 1970], moral development [Damon, 1988; Eisenberg, Morris, McDaniel, & Spinrad, in press], or personal identity development [Côté, in press; Erikson, 1950; Schwartz, 2005]. Simply, there is no reason to believe that stage (phase) progression may proceed evenly across time and that equal temporal division of the x-axis is warranted.

Data from the 4-H Study can also be used to illustrate this point. Figure 2d presents scores for depression among girls and boys as a function of age. Figure 2e presents age as a function of differences in depression scores. Developmental system theories (DST) have been used to understand adolescent development. These ideas stress that girls show higher levels of depression across the middle school transition as a consequence of the accumulation of stresses associated with the confluence of pubertal, school, and social relationship transitions [e.g., Simmons & Blyth, 1987; Susman & Dorn, in press]. However, when age is placed along the x-axis (fig. 2d) less evidence for this expectation is found than when age is the dependent variable (fig. 2e). Similarly, plotting depression against pubertal stage (fig. 2f) provides a clearer picture of the course of depression and its relationship to concurrent changes during adolescence than when only considering age-related changes.

In short, then, not only may the x-axis be divided in diverse ways when time (age, stage, etc.) is placed on this axis but, as well, there can be substantial substantive benefits for exploring the use of time as a dependent (y-axis) variable. Consistent with the links between method and theory stressed by Collins [2006] and Little et al. [in press], such inversion of the x- and y-axes can have important implications for testing or extending developmental theories. Other problematics of time and timing also illustrate the significance of the theory-method link.

Selection of Appropriate Ontogenetic Points to Index a Change Process

What does theory tell us about when in ontogeny a particular process may unfold? Independent of how we space observations along the x-axis, we need to address this question by selecting the correct points in the life span to optimize the identification of changes in a given process. The key idea here is that atheoretical selec-

tion of ontogenetic observational points may lead to unrepresentative – if not distorted – conclusions about the presence or form of change [Lerner, Bornstein, & Smith, 2003].

A prime example of such a potential error is the literature on identity development. Based on his clinical observations, Erikson [1950, 1968] theorized that the majority of identity formation occurs during the adolescent years. Moreover, Erikson argued that the emergence of the identity crisis is coupled with the advent of puberty and, as such, ontogenetic points within the early portion of the adolescent period should be selected for observation. However, the vast majority of identity research studies have been conducted using college student samples [Schwartz, 2005].

Moreover, although social-structural changes, including the uncoupling of the realities of adolescence from the realities of adulthood (e.g., the separation of education from work, or the separation of family of origin from family of procreation), have extended the identity stage into the late teens and the twenties [Arnett, 2000; Côté, in press; Schwartz, Côté, & Arnett, 2005], this *extension* of the identity development process does not imply that identity work in adolescence is *replaced* with identity work in emerging adulthood. As a result, the majority of work on identity has examined the ways in which identity is consolidated in emerging adulthood [Schwartz, 2006, 2007] – but has attended far less to the ways in which the task of developing an identity is initiated in early adolescence [Schwartz, Pantin, Prado, Sullivan, & Szapocznik, 2005] and continued in middle adolescence [Reis & Youniss, 2004].

Accordingly, a fuller understanding of identity would involve longitudinal work tracking the development of identity from early adolescence through emerging adulthood [Schwartz, 2005]. Moreover, assessments might be conducted more frequently in early adolescence, when the process may be initiated by some adolescents at some times and by other adolescents at earlier or later times, and during emerging adulthood, when the consolidation of a sense of identity may occur more quickly for some adolescents and more slowly for others.

In the absence of theory, selection of appropriate ontogenetic observation points should be rationalized on the basis of empirical generalizations. For example, if a longitudinal study of the initiation of alcohol use were to be conducted in the United States, it would be necessary to plan to have an initial observation point prior to entrance into about the sixth grade. By this grade level, estimates are that between 3 and 5% of youth have begun to use alcohol and, by the eighth grade, 41% of American children have initiated alcohol use [Johnston, O'Malley, Bachman, & Schulenberg, 2007; Lerner et al., 2005]. Similarly, observation points after the end of high school would be of relatively little value for the collection of information about the initiation of alcohol use, given that more than 90% of youths have initiated use by this time [Johnston et al., 2007].

In short, theory or, at the least, inferences from past research should be used to select the particular times in life when one observes development and, as well, the spacing – that is, the intervals between, or the density – of times of observation. Depending on theoretical understanding of the process under study and/or empirical knowledge of rates of change in the process, interobservational points may involve any ontogenetic time scale, for example, days, weeks, months, or years. As well, theory of prior research should influence the density of observational points along the

temporal axis, for example, precise descriptions of nonlinear trajectories may require greater density of measurement during periods of curvature.² These bases of the selection of observation points and of the spacing of these points along the temporal axis may also suggest that during some ontogenetic periods (e.g., early adolescence in the case of identity development) several different times of observation (separated perhaps by intervals of 6 months) should be considered because nonlinearity and rapid fluctuation are likely, whereas at other times in ontogeny (e.g., middle adolescence) fewer points in time (with longer, say annual, separations between them) or, perhaps, the aggregation of data points would suffice to chart developmental change because it is more likely to be linear and/or to change less rapidly. These recommendations would likely apply regardless of the specific longitudinal design used. However, the topic of aggregation raises additional issues, and is also a problematic that must be considered.

The Nonequivalent Temporal Metric: Relations among Levels within the Developmental System

Today, DST represents the superordinate theoretical frame for the study of human development (for discussions of the ‘family’ of DST within contemporary developmental science, see Damon & Lerner [2006, 2008]). As emphasized by Overton [2006], DST specifies that relations among variables constitute the basic unit of analysis in developmental research. Although the basic processes of development always involves links between intraindividual changes and the other levels of organization within the ecology of human development, the temporal metric of change may vary across these levels [Lerner, Skinner, & Sorell, 1980]. Such inter-level relations may involve covariance among variables from temporally commensurate levels of organization (e.g., pubertal change and cognitive change are both individual level processes and may change across x-axis divisions that are closely aligned, e.g., months or years). However, such relations may involve covariance among variables from temporally incommensurate levels of organization, for example, the individual (weeks, months, or a year) and the society or culture (e.g., decades).

For example, Bronfenbrenner [1979; Bronfenbrenner & Morris, 2006] provides an instance of a DST that elucidates the problematics of attempting to interrelate changes with disparate temporal change parameters. Bronfenbrenner described multiple systems within the ecology of human development. The microsystem refers to the specific individual-context relation involving the person at a particular point in time; the mesosystem is the ecological set of all of a person’s microsystems. In turn, the exosystem involves settings within which a person does not interact but that can nevertheless impact the person (e.g., the work place of the parent of an infant). Finally, the macrosystem contains the cultural and societal institutions, including social policies, within which all other portions of the ecology of human development are embedded. Bronfenbrenner and Morris [2006] explained that each

² We are grateful to one of the anonymous reviewers of this paper for suggesting this point to us.

system within this ecology is both inextricably interconnected and, at the same time, changing in accordance with its own chronosystem, that is a temporal metric with which changes in the system can be detected.

For instance, to index changes in infant sensorimotor or neuromuscular development (which involves changes in the microsystem), the x-axis (the chronosystem) may need to be divided by weeks. In turn, to index changes in the impact of US government policies aimed at improving infants' health and neuromuscular development (which involves changes in the macrosystem), x-axis divisions should arguably be no finer than 1 year and, more likely, 2, 4, or 6 years apart, depending on the nature of governmental changes being indexed.

As another example, Elder [1998] illustrated the impact of such chronosystem variation in describing the differences among individual, family, and generational change. The individual chronosystem is typically gauged in years, because the person measures his or her time through counting years since birth. However, family time may be indexed by a chronosystem involving life events (marriage, birth of a first child, the occurrence of an 'empty nest'), whereas generational time involves a chronosystem where, as family members are born, marry, or die, cohorts may change from being, for instance, the children, to the parents, to the grandparents.

To illustrate the methodological challenges of indexing the different meanings of time in order to understand the coactions among the levels of organization within the dynamic, developmental system, it is useful to return to the example of changes in infant neuromuscular development. As we have noted, such changes might best be measured in weeks. However, changes in the factors and processes that interact with infant neuromuscular development may have very different temporal metrics. For example, changes in parenting styles or practices in infancy might best be measured in months whereas, as noted, changes in government policies toward young children might best be measured in division of 2 or more years. In cases such as these, when research involves measures of all of these constructs using the same temporal metric to chart changes, a clear limitation exists in the potential to identify relationships between infant neuromuscular development and its influences. Measuring all variables using weeks as the temporal metric would result in parenting practices and government policies remaining constant (or fluctuating randomly) as infant neuromuscular development changes along its appropriate metric. Conversely, measuring all variables using months or years as the temporal metric would result in the trajectory of infant neuromuscular development being misestimated (or falling 'between the cracks' of the x-axis divisions).

The noncommensurate nature of time across these levels of organization within the ecology of human development makes the study of trajectories of intraindividual changes difficult to index empirically. In such circumstances, relationships between intraindividual development and its influences cannot be directly charted. As such, developmentalists may need to utilize cohort comparative designs, such as cross-sectional sequential methods [Baltes et al., 1977; Schaie, 1965].

However, there may be ways in which other temporal designs may be used to understand the role of macrolevel events or changes on intraindividual change. One possibility is to use multiple panels, or multiple birth cohorts in an accelerated longitudinal design, that are selected on the basis of the presence or absence of a major historical event (e.g., the advent of the Great Depression [Elder, 1974], or the advent

of the Vietnam War [Nesselroade & Baltes, 1974]), differentiating the experiences of a birth cohort during particular times in life (e.g., childhood, adolescence) from otherwise normative developmental patterns. Another possibility is to use an intensive measurement design where sufficient data points are gathered prior to or after some expected macrolevel event (e.g., new social policies affecting support to parents with dependent children or the level of retirement benefits provided to adults ending their work lives) to assure sufficient measurement occasions to judge whether there has been a change in the developmental trajectory.

The point, then, is that to study the relations within the developmental system that influence, that is, that determine the rate and form (trajectory shape) of developmental processes, developmental scientists must be sensitive to the different meanings of time across levels of organization or analysis within the system. They may need to pursue programs of research that include combinations of intraindividual and interindividual research designs, and/or that use macrolevel events or changes to differentiate developmental trajectories in order to fully elucidate the course of developmental change. In addition, although not a focus of the present paper, it is useful to underscore that there are exciting innovations in statistical methodology that can be used to innovatively exploit the data derived from such designs.³

Complicating this challenge, however, is that in any design used in such a developmental research program, decisions must be made about the specific ontogenetic observation points that will be used for *each* variable or level of analysis. Whether a researcher chooses to divide the x-axis according to a fixed or variable number of weeks, months, or years, one may raise questions about whether the selections are theoretically optimal for elucidating the true rate and form of developmental change for each of the developmental processes being studied. When several variables are assessed in a given study – and especially when the variables being assessed are at different levels of analysis – the decisions regarding research design and analytic strategy can become quite complex.

Aggregating Observational Points

One way in which developmentalists have dealt with differing temporal metrics across distinct substantive phenomena (e.g., infant development and parent behaviors), or sought to maintain equal intervals of change across the x-axis, has been to aggregate data across two or more observational points. Although aggregation⁴ may seem to be an appropriate way of reliably handling variables with differing temporal metrics [see Rushton, Brainerd, & Pressley, 1983], it has the effect of summing over

³ As noted by one of the two anonymous reviewers, recent developments in statistics, such as mixed model approaches [Muthén & Muthén, 2000; Nagin, 2005] can be used to model these kinds of relations, and the field of dynamic systems [Boker & Wenger, 2008; Thelen & Smith, 2006] offers much promise for sophisticated models of multilevel phenomena.

⁴ We refer to aggregation here as a method of handling and understanding data, rather than as a statistical strategy for summing over different trajectories that may be present within a given sample. More in-depth treatments of statistical aggregation and disaggregation can be found in Muthén and Muthén [2000] and in Nagin [2005].

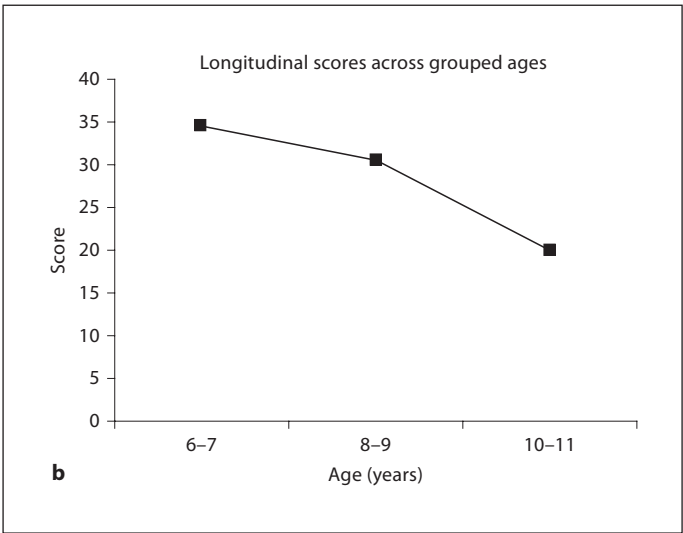
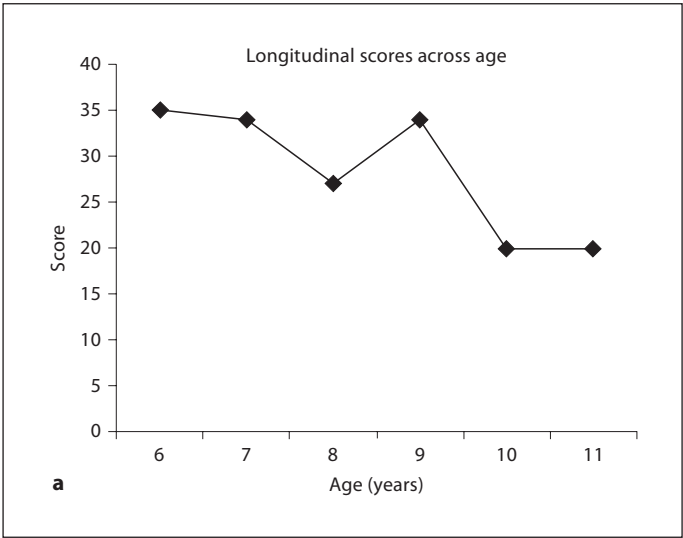


Fig. 3. a Scores for aggression as a function of year-by-year differences in age. **b** Scores for aggression as a function of grouped differences in age.

potential subtleties of developmental variations. As a consequence, aggregation may have the effect of recasting a pattern characterized by fluctuations from one time (e.g., a month) to the next as linear, if the monthly measurements are aggregated into yearly averages. As a result, a complex nonlinear trajectory may be mistaken for a simple linear trajectory, and the conclusions drawn about the developmental process in question are likely to be incorrect. To avoid such distortions of data, whether and

how one should aggregate should be determined by explicit specification of the theory of the process under study.

For example, suppose that a researcher wants to study the level of aggression in the play situations of girls from the ages of 6–11. The researcher develops a measure of aggression that is applicable to girls throughout this age range, studies groups of girls at each age level, and obtains scores for each girl. Now, let us imagine that the researcher has a theory about the development of aggression that predicts that the trajectory of aggression in girls of this age range should be discontinuous. Thus, the researcher might specifically expect to see abrupt (i.e., quantitatively discontinuous [Werner, 1957]) changes in the levels of aggression, and he or she accordingly would graph the results of the study so that any year-by-year fluctuations in aggression levels are evident.

Such a graph is shown in figure 3a. These hypothetical results would reveal abrupt fluctuations in measured aggression levels in play situations within the age range studied, and the researcher could use these findings to support the notion that aggression in play situations is a discontinuously developing phenomenon in girls.

On the other hand, the researcher's theory might hold that the development of aggression is a continuous phenomenon in girls of this age range. Accordingly, the researcher might not expect any abrupt changes in levels of aggression with age; instead, he or she might expect such development to be a gradual process. Thus, for ease and clarity in the analysis and presentation of the results of the study (and/or because the aggregation of data may lead to a more reliable estimation of data points), the researcher might use the average scores for a combination of the 6- and 7-year-olds as one data point on the graph, for the 8- and 9-year-olds as another data point, and so on. Such a graph is seen in figure 3b. The researcher could now use these results to support the contention that aggression in play situations decreases rather gradually over time with girls, and that the development of such aggression is therefore a continuous phenomenon.

Thus, in this example (which is intentionally extreme to make a point about the role of theory in decisions about aggregating data to describe developmental processes), the same data could be used to support quite disparate representations of the change trajectory being studied. Of course, this example of aggregation would not be pertinent to experienced, competent researchers, who would invariably be sensitive to such major trends in their data. However, this is precisely the point. Most often, trends in data are *considerably* more subtle than those in figure 3. As such, the impact of one's theoretical orientation on the collection and handling of data is not so readily obvious. The complexity of the data sets typically generated in developmental research not only requires vigilance about how scientists – because of their theoretical biases – may affect the nature of the *realities* that they *discover* [Kurtines, Azmitia, & Gewirtz, 1992]. This complexity also highlights the need to be aware of how depictions of data elucidating developmental processes relate primarily to theoretical, rather than empirical, issues.

If one adopts developmental system models of human development, aggregation should not be pursued merely because it affords ease and clarity in the analysis and presentation of data or because it may lead to more reliable estimation of data points. Instead, from the perspective of these individual-context models of human development, the most accurate representation of the complexity of development

should be a priority in all research studies. For instance, achieving reliability through aggregation of time points assumes stability across observational points, which may not accurately represent either the data or the theory on which the study is based. Within a developmental analysis framed by DST, the presence of the stability of interindividual differences in intraindividual change is an empirical question, not an assumption to be made in the service of data analysis. Thus, in this theoretical context, aggregation procedures may obscure the true form of changes or the ontogenetic point where interindividual differences may appear.² Again, then, use of aggregation should be undertaken with recognition of its embeddedness within theory and in light of understanding what is both gained and lost through aggregation.

We would argue that aggregation is most useful when the data have been gathered at too fine a temporal metric, and where the data are aggregated so as to achieve the optimal metric as suggested by theory. Aggregating beyond this optimal metric is likely to obscure important patterns and trends in the data. Attempts to avoid the problematic of aggregation by not using all observations available in a data set may be where the problematic of aggregation is most pronounced (e.g., relating to selection bias and generalizability).

The problematic of identifying the appropriate level of aggregation for elucidation of a given developmental process has also emerged in chaos theory [Thelen & Smith, 1998, 2006], in which solid objects break down into tiny, nonsolid, and seemingly random packets of energy when they are magnified millions of times. Thus, consistent with both developmental theory [Lerner, 2002] and with chaos theory, for any given developmental process, there is likely to be an optimal metric at which some orderly progression is evident only when a specific degree of aggregation is used. When aggregation is to be used, the challenge is to identify this optimal metric of aggregation.

However, even in cases where data are measured at too fine or intense a temporal metric, such as assessing adolescent depression several times per day over a series of weeks through event sampling methods [Csikszentmihalyi & Larson, 1984], statistical procedures may be available to identify an interpretable pattern of change. Although our focus in this article is not to review such methods, we would note that there are several extant and emerging approaches that enable fitting sophisticated models of change without resorting to aggregating observational points.³ Examples of such models include time series analysis [Little et al., in press], P-technique factor analysis⁵ [Corneal & Nesselroade, 1991; Nesselroade & Ford, 1987], and functional data analysis [Li, Root, & Shiffman, 2006; Walls & Schafer, 2006], some of which may render the issue of aggregation moot in some cases.

⁵ In P-technique, the person is held constant, and the covariance among variables is modeled across time. In contrast, the more typically used R-technique design involves time being held constant, with covariance among variables modeled across persons [Cattell, 1957]. In other words, in P-technique designs intraindividual change is assessed by considering how variables covary across time within a person. Because this person-centered design may involve 100 or more times of measurement [Corneal & Nesselroade, 1991; Nesselroade & Ford, 1993], it constitutes an instance of an intensive longitudinal design [Collins, 2006]. However, R-technique designs, even when reiterated at multiple times with the same sample, are not longitudinal designs; they are variable centered in nature, and assess how variables covary across people within time.

Although advanced statistical analyses that empirically model trends in the data may appear to decrease the importance of aggregation [Walls & Schaefer, 2006], different conclusions can be drawn depending on how data are aggregated, and the statistical procedures must be *set up* to match the theory being used. The level of aggregation used should match the theoretical understanding of the phenomenon, which will then maximize the likelihood of obtaining results that will bear directly on the theory in question. Theory must play a prominent role in deciding whether data gathered at too fine a temporal metric are to be analyzed with or without aggregation.

However, there remains an additional problem that must be addressed at the level of theory, rather than at the level of analysis. Recall our earlier point that the *same* data can be interpreted as either continuous or discontinuous, depending on the theory in question (which, in turn, guides the aggregation procedure used). No statistical procedure is capable of replacing theory in making the determination of how data should be aggregated. Moreover, it may be that each developmental process has an optimal level of aggregation that matches a given theoretical perspective. Advanced statistical procedures may replace aggregation as the method of choice for handling data in some cases, but aggregation may still be necessary in other instances to facilitate a match between the spacing of time points in a data set and the theoretical mapping of change in the developmental processes under study.

Thus, vigilance in regard to theoretical biases regarding aggregation needs to be integrated with attention to the potential use of aggregation to identify emergent systematicity in one's data. Developmental scientists should remain aware that a given theoretical position might lead one to interpret a specific data set in one way (e.g., as consistent with a continuity position), whereas someone with a different theoretical position might interpret that same data set in another way (e.g., as consistent with a discontinuity position). Indeed, as emphasized by both Collins [2006] and Little et al. [in press], developmental scientists would do well to attend to the role of theory in both their choice of longitudinal design and, as well, to how their theoretical ideas do or might shape their selection, spacing, and treatment of the multiple times of observation involved in the design they elect to use. Statistical analyses cannot replace theory in making these decisions.

Conclusions

The study of change across the human life span is an arduous enterprise. Good developmental theory must be coupled with change-sensitive longitudinal designs and – although we did not explicitly address them in this article – to change-sensitive data analytic methods to maximally exploit the data set generated through the synthesis of good theory and good research design. For more than three decades, developmental scientists have discussed the links between theory and the methodological choices involved in longitudinal research [Baltes et al., 1977; Collins, 2006; Little et al., in press; Nesselroade & Baltes, 1979; von Eye, 1990a, b]. Our primary goal in this article was to extend this literature by discussing some, but certainly not all, of the problematics associated with appropriately addressing the theory-design link. We have sought to both raise awareness of, and to suggest, ways to address some of

the key problematics of time and timing that remain incompletely understood by developmental scientists.

A key implication of the discussion we have presented is not the resolution of all of the problems we have raised but, rather, an underscoring of the complexity of the theory-methodology choices facing longitudinal researchers. Some of these problems do not have definitive answers, but rather must be addressed on a case-by-case basis in light of the theory and issues under study. Our hope is that we will sensitize scientists and funders that the important call for longitudinal research should not be answered by an insistence that we go out in the field and collect data every year, twice a year, or every other year. How the x-axis of longitudinal research is conceptualized, spaced, and aggregated depends critically, fundamentally, and primarily on a scientist's rationalized and operationalized theoretical understanding of the change process being investigated.

It is important to appreciate that the issues raised in this article about the study of developmental processes are not simply theoretical abstractions. Research on human development is used to shape public policies [Balsano, Theokas, & Bobek, in press], prevention and treatment programs [Schwartz, Pantin, Coatsworth, & Szapocznik, 2007], classroom curricula [Eccles & Roeser, in press], and other applied arenas for children and adolescents. Indeed, decisions about how to measure developmental phenomena have already influenced – and will continue to influence – decisions in these applied domains. More nuanced and theoretically predicated study of developmental change processes will enable us to make more refined, theoretically predicated, and empirically sound recommendations about what should be done vis-à-vis policies and programs to work with, treat, educate, teach, and enhance the lives of people across the life span.

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