
Quantitative Modeling in Adult Development and Aging: Reflections and Projections

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Quantitative research in aging is at an important crossroads. Theorists in a variety of substantive areas (e.g., cognition, memory, emotion) have tried to make important advances in the past few years, but the necessary empirical underpinnings tend to rely primarily on the methodological tools that have dominated the study of behavior, development, and change for the past several decades — the restrictive conceptions of static equilibrium, linearity, and additivity. It is time to push for the further development and adoption of alternative methodological approaches that embrace more dynamical and, when appropriate, nonlinear conceptions. It is also not amiss for methodologists to challenge the theorists to foster more dynamical concepts regarding the nature of aging. Looming in the interface of the method–theory collaboration is the fact that idiosyncrasies in stimulus perceptions and response patterns jeopardize analyses depending on the traditionally casual aggregation of data over experimental units. This matter will have to be addressed before the high levels of validity and precision we seek for lawful relationships can be attained.

1.1 Reflections

I am honored to participate in this inaugural conference of the Notre Dame Series on Quantitative Methodology with its focus on adult development and aging.¹ In psychology, in general, we have a very long history of concern with change and how to quantify it. Some of us are concerned primarily with developmental change, some with the outcome of structured interventions, and still others with processual change such as in the likely voting behavior during the waning days of political campaigns. I have been laboring in the vineyard of change measurement, primarily in personality and ability attributes, for over 40 years and I can still vividly recall how impressed I was by the magnitude, thoughtfulness, and no small amount of passion characterizing the literature on the topic that was already available back in the early 1960s when, as a graduate student in Raymond B. Cattell’s laboratory, I first started examining it. For example, papers by the likes of Bereiter (1963); Cattell (1963, 1966); Fiske and Rice (1955); Flugel (1928); Manning and DuBois (1962); Thouless (1936); Woodrow (1932) and many others intrigued me then and are still very much worth reading.

For me, one of the more stimulating pieces of work early on was the chapter by Bereiter (1963), titled “Some Persisting Dilemmas in the Measurement of Change”. I wrestled with the issues raised by Bereiter, some of which I found to be quite abstract, such as the subjectivism versus physicalism dilemma.² I would think I understood the dilemmas and then, reading the chapter again to make sure, would become uneasy about one or another aspect. Still, I strug-

¹I have a strong feeling of kinship with the University of Notre Dame Department of Psychology, having been a member of a reviewing committee several years ago that came to know the department rather well and made a number of recommendations designed to help strengthen an already fine program. One of those recommendations was to initiate a regular methodological dialogue such as that in which we are now participating. I am also proud to say that I had a hand in the graduate training of several of the current faculty members. In keeping with my intimate regard for the program and its faculty, I’ve purposefully injected a personal perspective into my comments. I hope they do not come across as overly egocentric.

²In 1964, Bereiter was slated to be the second reader on my master’s thesis so I felt obliged to continue working on mastering his chapter. Bereiter left Illinois to take a position elsewhere, but I persisted.

gled with these and many other issues regarding the representation and measurement of psychological and behavioral change and carried these concerns with me when professional circumstances described elsewhere (Nesselroade, 2000) dictated that I become a life-span developmentalist. But that is another story. It didn't help my state of mind when, in 1970, Cronbach and Furby seemed to be saying not to bother any more with trying to measure psychological change at the individual level.

In the late 1970s, Paul Baltes and I made an attempt to organize some of the literature as well as our own thoughts about studying developmental changes with a discussion of longitudinal research methods. In preparing for this undertaking, to our surprise, we found such “comforting” thoughts as: “There is no hard and fast definition of what constitutes a longitudinal study. (Hindley, 1972, p. 23) and Zazzo's (1967) identification of *longitudinal* as a general term describing a variety of methods. To try to bring some closure to our own thinking regarding the term *longitudinal*, we concluded that “longitudinal methodology involves repeated, time-ordered observation of an individual or individuals with the goal of identifying processes and causes of intraindividual change and of interindividual patterns of intraindividual change [in behavioral development]” (Baltes & Nesselroade, 1979, p. Unknown). We observed that there is one sine qua non of longitudinal research, namely “the entity under investigation is observed repeatedly as it exists and evolves over time,” and enunciated five reasons or rationales for why one would conduct longitudinal research. They are:

1. Direct identification of intraindividual change
2. Direct identification of interindividual differences in intraindividual change
3. Analysis of interrelationships in behavioral change
4. Analysis of causes (determinants) of intraindividual change, and
5. Analysis of causes (determinants) of interindividual differences in intraindividual change analysis.

I do believe that these rationale points were helpful in clarifying some of the purposes of developmental research and also in helping to demystify longitudinal research in general, although some of its mystique is clearly still alive today.

Along the way, one of the important views that students of change gradually came to accept, probably during the 1980s, is the perspective that, by and large, the effective study of change requires more than two occasions of measurement. Whether this realization helped to fuel the interest in growth curve modeling or vice versa, I'm not sure. In either case, because of this shift in perspective, we have been able to worry less about the use of simple measures of change, such as difference scores, and concentrate instead on the specification of more extensive change functions. Several of the chapters in this volume reflect this multi-occasion orientation.

It was about the time that Baltes and I were working on these ideas that I became better acquainted with the late Joachim F. (Jack) Wohlwill whom I had known since the late 1960s when he came to the first West Virginia Conference on Life-Span Development and delivered a paper titled, “Methodology and Research Strategy in the Study of Developmental Change.” I was struck by Wohlwill’s grasp of methodological issues pertinent to the study of developmental change as well as his knowledge of developmental theory.³ In one of his last papers, a chapter published in 1991 in the *Annals of Theoretical Psychology*, Wohlwill once again turned to the relationship between theory and method in developmental research. He identified his preferred view of this relationship as the partial-isomorphism relationship between method and theory.

This flexible, loose sort of linkage between theory and method will serve as a counterforce to sterile pursuit of methodology for its own sake, divorced from and uninformed by theory, such as would be encouraged if methodology were to be considered as completely independent of

³Ten years later, when Wohlwill and I were colleagues at Penn State, I relished our many lunch discussions concerning the study of change and development. These continued until his untimely death in 1987. Jack could and would “hold your feet to the fire” until he was satisfied that your case was stated unambiguously. Accepting your statement of the problem certainly did not mean that he would agree with your solution, as I learned over and over.

theory. At the same time the conception likewise avoids the excesses of theorizing without regard to methodological approach, or of subordinating method entirely to theory, which is apt to ensure the preservation of the theory in isolation from rival ones, and thus lead eventually to its dying on the vine. (Wohlwill, 1991, p. 91)

Wohlwill elegantly made the point that theory is not always in the driver’s seat, nor, indeed, should it be, although many of us have been “hammered,” over and over, with the idea that theory should drive method and not vice versa. Rather, Wohlwill was describing a productive tension between theory and method such that the one reinforced and pulled along the other. Indeed, theory may sometimes have to wait on method. But that does not mean that theory should contentedly rely on inadequate method. In a similar vein, the developer of method does not need to delay, until theory demands new products, promulgating something novel that may, in turn, elicit more advanced theoretical contributions.

In the second edition of the *Handbook of Multivariate Experimental Psychology* (Nesselroade, 1988), I had occasion to refer to Wohlwill’s view of the theory-method interface and used the metaphor of a dance between two strong partners. I wrote:

First, substance, often in the form of elaborate but untested theory, takes a step and then methodological developments follow. Subsequently, methodology may glide out ahead, even far ahead of substantive gains. The partners in this seemingly cumbersome dance likely will never blend in a graceful pas de deux. Nor should we wish them to. Rather, a continuing imbalance seems to enable each in turn to elicit new steps from the other.⁴ (p. 643)

Obviously, there are many instances in which theory has challenged, to good effect, the prevailing methodology. Looking back over

⁴I still remember a remark from my coeditor, Raymond B. Cattell, on reading this bit of prose, “Kind of abstract, isn’t it?” This from the person who once described the hyperplane as “the footprint of a causal influence.” I kept it in, anyway.

our history of the past 100 years, the development of factor analysis as a tool to help Spearman elaborate his concept of g , Thomson his “sampling bonds” theory, and subsequently, the development of multiple factor analysis by Thurstone to aid his theoretically guided search for multiple factors of human ability are cases in point.

Method Leading Theory

But this is a methodological conference, so I want to spend a little time on examining the other phase of this dance: how methodology has and must continue to challenge theories of adult development and aging. The chapter by Bergeman and Wallace thoughtfully addresses some of these key matters.

Modeling.

One pertinent example of methodological concerns forcing theory to do better can be seen in the aftermath of the presentation of Schaie’s general developmental model (see also Baltes, 1968) and parallel developments in the life-course research of some sociologists. Once researchers became convinced of the importance of identifying cohort and time of measurement effects, for example, it didn’t take long for theoretical concerns to force researchers to begin to grapple with the “unpacking” of these generalized combinations of influences into their key, distinct components. The detailed identification by Baltes, Cornelius, and Nesselroade (1978), for example, of age-graded, history-graded, and nonnormative life events illustrates an advance in the theory of cohort and time of measurement effects that was elicited, in large part, by the methodological tension created by the appearance of the general developmental model.

Measurement.

I want to say a word or two regarding measurement issues and describe a situation where I would like to see method exert more of an influence on theory development. It is in this context that I have come to reinterpret the contemporary phrase, “thinking outside the box.” I will illustrate this in a moment but first, let me explain a bit more. The past two decades have witnessed an increased interest in

modeling short-term, intraindividual variability in a variety of substantive domains, including temperament and human abilities. Coincidentally, developments in dynamic factor analysis (e.g., Browne & Nesselroade, in press; McArdle, 1982; Molenaar, 1985; Nesselroade & Molenaar, 1999), as well as other kinds of dynamic modeling such as those presented in the chapters coauthored by Boker and by Wenger and Schuster, have blossomed. The appearance and preliminary application of these methods have given us some important new insights into the nature of behavior and behavior change and, I very much believe, have raised some promising possibilities about the way we conceptualize and measure variables.

Consider, for example, variables such as rhythmicity (a temperament dimension measured in young children) and rigidity (a personality characteristic studied at many age levels). Typically, these are measured by assigning a person a score indicating how much or how little of the attribute is manifested. For example, how much rhythmicity does a participant have? How high does someone score on rigidity? This kind of conceptualization is the traditional thinking within the “box,” as represented in Fig. 1.1.

But, it is possible to think outside the “box” with such variables and many others, I’m convinced, and to conceptualize and measure them with actual intraindividual variability in the pertinent behavior rather than an estimate of a static amount. Consider the concept of socialization, for example. *Socialized* behavior is behavior that varies appropriately from situation to situation while still falling within acceptable limits; it is not behavior that is so highly constrained and repetitive as to be considered pathological. It is my expectation that work on intraindividual variability that has been largely methodologically oriented will challenge substantive researchers to consider these implications as they conceptualize their variables and build measurement devices for them.

There are many other aspects of measurement that space constraints preclude addressing here. The chapter by Schuster examines some additional critical aspects.

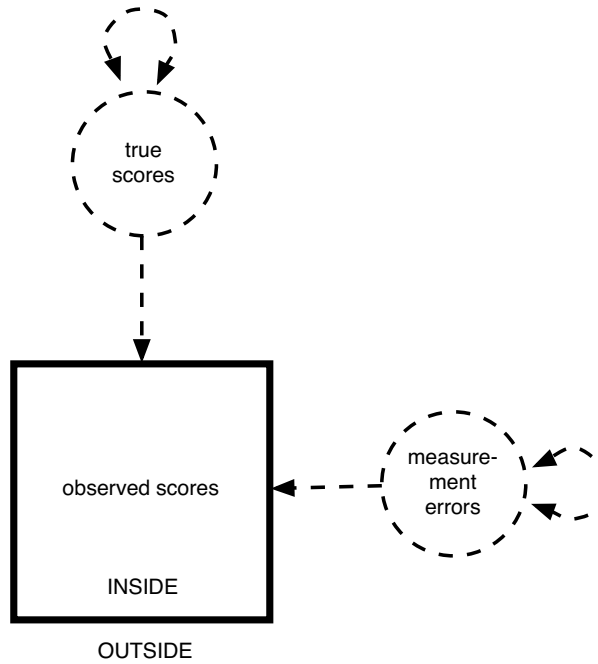


Figure 1.1: The “box.”

Design.

In a similar vein of methodological emphases running ahead of theoretical concerns, about 15 years ago I argued, on the strength of results from a series of intraindividual variability modeling studies, in favor of incorporating “bursts” of measurement into longitudinal designs. It took a while, but that methodologically leading step was followed by compelling theoretical rationale, and now, for example, the Victoria Longitudinal Study (Hultsch, Hertzog, Dixon, & Small, 1998) includes such a design feature.

Method And Theory In Aging Research

Despite a number of interesting and promising methodological developments of the past couple of decades, there are still many aspects of the study of aging for which theory has led the dance for a long time, perhaps long enough that it is time for a change. Developmental systems theorists (e.g., Ford, 1987; Ford & Lerner, 1992) have pushed some interesting substantive ideas well beyond the capabilities of the currently popular methods. Theorists in lifespan development and aging, such as Paul Baltes and Margret Baltes, Laura Carstensen, Gisela Labouvie-Vief, and others, have advanced the development and presentation of theory to a considerable level, one involving complexities and abstractions such as multidimensionality, multidirectionality, and gains and losses and process notions of continuing adaptation such as selection, optimization, and compensation, socioemotional selectivity, and so forth. But the tools by which many of these conceptions are being empirically tested are the “work horses” of yesteryear, such as multiple regression, with some extensions.

For example, running through most of the theoretical arguments referred to earlier (and rightly so) is the notion of process, and that notion, I believe, challenges both the methodologist and the theorist more importantly now than at any point in our history. One of the arenas where method is already daring theory is growth curve modeling (McArdle & Nesselroade, 2003). Since the papers by Rao (1958) and by Tucker (1958) with key followups by McArdle and Epstein (1987); Meredith and Tisak (1984, 1990); Rogosa, Brandt, and Zimowski (1982), and others, the technology of fitting growth curves

can be argued to have grown, to some extent, somewhat beyond the features of many of the data to which these models are fitted—the methods are more interesting than the data, in many cases. Theorists will do well to strengthen their conceptions and measures to take advantage of the benefits of these recently developed technologies. The chapter by Curran, Bauer, and Willoughby explores some key aspects and novel applications of growth curve modeling. But the theoretical arguments involving process concepts are crying out for other methodological approaches as well, including the damped linear oscillator discussed by Boker and latent change models presented by McArdle.

Projections

I want to look down the road a bit farther and prod you, the reader, perhaps even irritate you a little, regarding the current state of the method–theory interface. To do this, I am going to use theory to challenge measurement, design, and modeling methodology.

I call attention to a specter that I see slowly growing but not yet substantial; hulking but not yet sharply defined; advancing but not yet truly threatening. Yet, there are signs of it in the titles and content of many papers that are found in today’s literature. I do not believe that this specter can be ignored.

I will cast this particular devil within the framework of the so-called idiographic versus nomothetic debate. The psychological literature contains an old distinction between idiographic and nomothetic concerns (e.g., Allport, 1937; Lamiell, 1981, 1988; Rosenzweig, 1958, 1986; Zevon & Tellegen, 1982) pertaining especially to the study of personality, but the concerns hold for any domain studied via differences among persons, I would argue. In developmental science, for example, the distinction between person-centered and variable-centered approaches to the study of behavior and its development (Bergman, Magnusson, & El-Kouri, 2003; Magnusson, 1997) is, in part, an acknowledgment of some of these same ideas. Valsiner’s (1984) discussion of typological versus variational modes of thought also bears on the topic. Recent discussions by Lamiell (1998) and van Kampen (2000) illustrate those features of the debate of most centrality to the present discussion. Idiographic concerns center on the

uniqueness of the individual, whereas nomothetic concerns emphasize the generality of lawfulness in behavior.

These two conceptual domains are often regarded as antithetical from the standpoint of building a science of behavior, but I subscribe to the spirit of rapprochement expressed by authors such as Lamiell (1981), who argued for integrating the two into an “idiothetic” approach, and Zevon and Tellegen (1982; see also Nesselroade & Ford, 1985), who proposed that idiographic information can and should be put to the service of developing nomothetic relationships (see also Molenaar, Huizinga, & Nesselroade, 2003). These and other writers have dared to raise questions regarding the validity of some of our most cherished group-analysis concepts, including means, variances, and covariances or correlations. All of us are familiar with examples in which the mean is not a very workable concept; where it applies to no one. For example, statistically speaking, the average number of bedrooms in single family dwellings implies a lot of unfinished houses, just as the average number of children living in these houses implies a lot of partial children. Far more serious, I contend, are the questions being raised regarding variances and covariances because those are the “stuff” that many of us study the most intensively. Correlations, and the statements of structure derived from them, are group, not individual representations. Just what is the role of the individual in these kinds of group modeling efforts? This is one of the key questions that is being asked.

Behavior patterns have both idiosyncratic and general features. To illustrate the basic idea more concretely, two speakers find themselves in front of large audiences, preparing to deliver addresses. One is painfully aware of the size of the audience, the fact that many of its members are well-dressed, professional-looking people, and that they seem to be a serious, humorless bunch. Waiting to be introduced, he feels his heart start to pound and his hands begin to tremble as his breathing becomes more and more shallow. The other speaker, somewhat by contrast, is also painfully aware of the size of his audience, notices that they appear to be “organized” into small groups of seemingly intimate acquaintances, and many of them have their eyes on the clock. His hands begin to sweat, his shirt collar feels very tight, and his heart begins to pound. In stimulus-response terms, both speakers are experiencing a stress response to a threatening situa-

tion. Clearly, some of what is happening is common to the two of them. But, there are also substantial idiosyncratic elements in both the perception of the stimulus situation and the pattern of response.

These idiosyncratic features of behavior are shaped by both genetics and experience. There is much in common to the two speakers' experiences. Both perceive the situation as threatening, both have heightened sympathetic nervous system activity, and both are subjectively aware of their discomfort. They have inherited a number of physical and physiological attributes common to human beings that influence their perceptions of the situation and their reactions to it. But, their perceptions and reactions also have unique characteristics that introduce considerable idiosyncrasy into the mix. One sees the crowd as hostile; the other sees it as aloof. One breaks into a cold sweat; the other's shirt collar seems to be choking him. These perceptions and behavior patterns are, in part, functions of the unique genetic makeup and histories of conditioning and learning each has undergone over the course of his lifetime. Thus, the two speakers' perceptions and behaviors are similar in some ways and different in others.

Now, in this simple example, it is not amiss to aggregate information over the two speakers at the level of “experiencing a stress response to a threatening situation.” However, there is much less justification for aggregation at the level of the speakers' self-reported perceptions of the stimulus situation and self-reported or objectively measured responses. For instance, “shallow breathing” holds for one, but not for the other. Aggregating over these kinds of “individual differences” might lead to relationships, but they will not be nearly as strong as is implied by the nomothetic components (e.g., anxiety response to a threatening situation).

Another example of the difficulties created by these kinds of individual differences comes from an earlier foray into *p-technique research*, this time with Linda Mitteness (Mitteness & Nesselroade, 1987). During debriefing, we found that two participants (a mother and daughter), whose daily emotion self-reports we were trying to relate, were responding quite differently to the stimulus, “Are you anxious?” One of the individuals was interpreting “anxious” to mean “anxious” and the other was interpreting “anxious,” to mean “eager.” Because of their unique phenotypic histories, these two individuals

had different ideas about what the item signified and responded according to their respective views. In analyzing such data, we typically ignore the possibility that the content of the item might have been construed differently by different respondents and proceed to aggregate the information they have supplied across persons as though it were perfectly meaningful to do so.

Elsewhere, Molenaar et al. (2003) explored these issues in considerable detail within the framework of modeling single subject and group data. Another important key is the breaking down of data into groups and levels. Multilevel models, for example, involve the systematic recognition of differences among subgroups that would obscure relationships if ignored. The chapters by Curran et al. (ch. 4), by Johnson and Raudenbush (ch. 5), and by Neale et al. (ch. 9) examine various aspects of this matter. Furthermore, the growing interest in “mixture models” offers additional evidence that some researchers are becoming aware of and are trying to deal with these matters. I underscore the seriousness of the implications for our science.

There may be even “tougher” issues here than many of us would like to confront at this time. Avoiding the extreme question. “What if we are all different from each other?” we may still ask, “Just which individual differences can we aggregate over in order to develop meaningful, powerful lawful relationships among theoretically interesting variables?” “How can we identify, measure, and model them?” Clearly, these are important theoretical issues. They are there, beckoning to the methodologist to catch up, perhaps even to glide on by in some new and radical steps.

Metaphors, like analogies, break down at some point. Just who is leading who and when they are ahead in the method and theory dance is not always clear. I am convinced that the key is in the tension; the dynamic that inheres in theories that demand stronger ways to evaluate their empirical implications and methods that can elicit from the theorists more precise statements of compelling relationships to be evaluated. I am looking forward to what the future brings in this regard. Until these issues are clarified, I cannot be optimistic regarding the probable rate of progress in building compelling explanatory systems regarding adult development and aging or behavior in general, for that matter.

Concluding Remarks

In their sweeping overview chapter, Bergeman and Wallace (ch. 2) set the stage for a discussion of methodological issues that is appropriately grounded in the key theoretical issues of human development and change. They identify a number of developmental methodology issues and convey a sound impression of the importance of design, measurement, and analysis or modeling concerns. Despite the frank overall orientation of the volume toward methodological issues, Bergeman and Wallace’s emphasis on the productive interplay between method and theory keeps the reader aware of the vital role that theory and theory development play in the generation of knowledge. No doubt methodologists need this kind of reminder on occasion—especially when several of them are brought together under one roof.

This first volume in what we hope will be a long and successful series is pointed toward the future; a future which I believe will witness the development and incorporation of powerful linear and nonlinear dynamical systems modeling tools that will first titillate only the younger theorists while offending the sensitivities of the older ones. But the dancing will continue. As it does, these newer methodologies will eventually gain the momentum and purchase to wrench adult development and aging theory out of its comfortable reliance on the methodologies that have reigned over the past century and lure it into trying out some new steps.

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