Emergent Theories of Aging

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The Impact of Research Methodology on Theory Building in the Developmental Sciences*

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It is well recognized that theory building in science will often proceed inductively by observations of empirical phenomena that suggest sufficiently orderly patterns or sequences. These then permit construction of a schematic that allows predictions regarding the patterns that should be found in other related phenomena or data as yet unobserved. Inductive theories are essentially efforts to organize empirical observations in such a way that possibly isolated phenomena can be assigned to a more comprehensive supraordinate and overarching schema. Such schemas then allow a better understanding of each of the singular and infraordinate observations. By contrast, a deductively adduced theory begins with the elucidation of central assumptions and corollaries. The theory is then tested by investigating data sets that might fit the a priori theoretical propositions.

Many developmentalists, moreover, subscribe to a dialectic view of scientific inquiry. This view specifies the dynamic interplay of empirically derived inductive models, which are then expanded or differentiated deductively and

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applied to new data sets. Revised models are then once again generated in an inductive fashion. An implicit assumption of this process is the notion that the dialectic interplay is informed primarily by substantive empirical observations and the search for data sets that could provide substantive tests of theory (cf. Riegel, 1976).

The purpose of this chapter is to explore how theory building in the developmental sciences is informed and impacted by advances in research methodology (cf. Baltes & Willis, 1977). I will begin this exploration with some brief remarks on how methodological innovation leads to different views of extant data bases and the subsequent revision of theoretical models and developmental theory. To provide concrete illustration of this process I will then exercise the reader through two rather different examples illustrating the impact of methodology upon theory construction and theory testing. My first example involves primarily the impact of research-design issues related to the age-period-cohort problem upon the formulation of models for the study of development. The second example considers how the availability of a new method of analysis, confirmatory factor analysis, has impacted theorizing in work on adult development.

METHODOLOGICAL INNOVATION AND THEORY CONSTRUCTION

Important recent innovations in research methodology in the developmental sciences have been essentially of three varieties: advances in (1) instrumentation, (2) research design, and (3) techniques of analysis. The first involves the level of instrumentation and sophistication of measurement devices or scales. For example, introduction of computer-assisted tomography and direct measurements of blood flow have led to the obsolescence of earlier indirect approaches to the assessment of the integrity of cortical structures. Given the availability of these methods, it becomes increasingly implausible to formulate theories of neuropsychological aging that make assumptions about neural structures that can now be examined noninvasively. Likewise, moving from a strictly descriptive electroencephalographic description of the resting brain to an analysis of evoked potentials must lead to a paradigmatic shift in conceptualizing the role of the cortex and its structures as they interact with an active environment.

Advances in instrumentation may also involve a shift from mere categorical description of the presence or absence of a phenomenon to the development of scales that have ordinal or interval properties. An advance of this kind was represented by the realization that psychological scales could be developed for subjective properties, as evidenced, for example, by the method of comparative judgment and the more modern techniques of multidimensional scaling (Cliff, 1982). Given the scalability of subjective phenomena, it then becomes possible to specify theories that in addition to directly observed antecedents may include mediator variables based upon experiential evidence.

A second class of methodological innovations that may impact both theory building and the manner in which theory is tested concerns the specification of novel schemas for collecting data and for evaluating the validity of theoretical constructs. For example, specifications for determining the internal and external validity of experiments and quasi-experiments (Campbell & Stanley, 1963; Cook & Campbell, 1979) and the application of these concepts to developmental studies (Schaie, 1977, 1978) require the theorist to explicate the additional corollaries needed for existent theories to denote the side conditions under which propositions derived from theory can be expected to hold. Methodological discussions that specify alternate models for the structuring of developmental data collections (cf. Schaie, 1965), moreover, will impact theory by specifying the required properties of the data set to be explained by the theory or to be employed for testing the theory.

Methodological innovations in methods of analysis, finally, will lead to the possibility of performing tests of theory that were previously not feasible (e.g., the estimation of complex causal models à la Jöreskog, 1979), as well as to major paradigmatic shifts. An example of the latter impact would be the shift from theories that relate directly observable variables to theories that account relationships among latent (unobserved) variables. This shift occurred as a consequence of the general acceptance of confirmatory factor analysis and canonical correlation approaches as state-of-the-art techniques in the behavioral and social sciences.

In each of the above instances, the introduction of methodological advances has at a minimum permitted the testing of theoretical propositions that were previously not amenable to empirical study. Beyond that, however, the discovery of constraints identified by the methodological information requires expansion of theoretical formulations, and new models must be derived that can explain and include the attributes of the phenomena that methodological innovations have uncovered. There probably is no uniform path in which this intersection of methodological innovation and modification of theoretical thinking operates in every instance. To obtain a better understanding, it seems necessary then to examine some relatively detailed examples. These will be provided in the remaining sections of this chapter.

RESEARCH DESIGN AND THEORY: THE CASE OF THE AGE-COHORT-PERIOD PROBLEM

One of the more prominent examples of the impact of research methodology on theory formation and testing in studies of adult development has been the controversy engendered by the debate about appropriate methodologies involved in the study of change over time, commonly referred to as the age-cohort-period problem (cf. Mason, Mason, Winsborough, & Poole, 1973; Palmore, 1978; Ryder, 1965; Schaie, 1965, 1973, 1977, 1984, 1986). It presents a ready illustration of the dialectic interaction between puzzling data sets, the examination of the appropriateness of standard research design, and the specification of alternate designs leading to the collection of new data sets that would fit the new paradigms. Of equal importance, for our present purposes, is the impact that the resulting methodological innovations have had with respect to theories of how adult aging might best be conceptualized.

Some Historical Context

The work to be referred to here began with the realization that data on the adult development of mental abilities showed wide discrepancies between cross-sectional and longitudinal data collected on the same subject population over a wide age range. In particular, it became evident that for some dependent variables substantial age differences obtained in cross-sectional studies could not be replicated in the longitudinal data, while for other dependent variables longitudinal age changes reflected more profound decrement than was shown in the comparable cross-sectional age difference patterns (Schaie & Strother, 1968).

In an inductive effort to explain these discrepancies a general model for the study of developmental change was developed that explicated the relationship between the cross-sectional and longitudinal methods (Schaie, 1965).[1] From this model it became possible to show that cross-sectional data involve the description of age differences at a single point in time; that they represented a separate samples design à la Campbell and Stanley (1963). Designs of this type suffer from the problem that maturational change (age) is confounded with cohort acting as a selection factor (Schaie, 1984). Longitudinal data, by contrast, involve a time series assessing the same individuals at two or more points in time. Here maturational change (age) is confounded with historical (secular) trends. Specification of the general model in a deductive manner then allowed the specification of a third approach to the collection of developmental data, for which the term "time-lag" was coined (see also Palmore, 1978). This latter approach involves the comparison of two samples at the same chronological age but at different calendar times, as would be the case, for example, in the comparison of SAT scores for successive classes of high school graduates. In this design, cohort differences are confounded with historical trends. Consideration was then given to the possibility of deducing more complex designs, termed "sequential methods," that proposed to permit estimates of the magnitude of specific components of developmental change by controlling for the confounds mentioned above (Schaie, 1965).

The sequential designs were applied to empirical data sets obtained as part of the Seattle Longitudinal Study (Schaie, 1983). From these applications it soon became apparent that there are specific patterns of data acquisition that lend themselves most readily to optimal utilization of the sequential analysis strategies (Schaie & Willis, 1986a). It also became apparent that different sequential designs are appropriate for different developmental questions (Schaie, 1973) and that there is a need to specify design complications that allow for the control of some of the validity threats specified by Campbell and Stanley (1963). Design variations were therefore explicated that permit controls for reactivity, practice, and experimental mortality (Schaie, 1977).

Implications for Theory Building

Three different aspects of the design considerations as they have implications for theory building will now be considered. First, we will indicate how the manner in which different designs confound sources of individual-difference variances may influence theories about the normative course of aging. Second, implications for the structuring of theory-guided data collection will be noted; and third, we will discuss implications for the explanation of phenomena involving change over time and age.

Implications for Aging Theories

Many theories of aging, whether involving wear and tear, cumulations of waste products, or successive loss of neurons, implicitly or explicitly include the assumption of irreversible decrement. The design specifications resulting from the general developmental model, however, clearly indicate that in behavioral data irreversible decrement would represent only one of a number of observable patterns of aging. The plausibility of the irreversible decrement model, like that of any other model, can be tested only under certain conditions; a multicohort longitudinal study would be needed to protect against the possibility that adverse historical effects could either inflate or suppress maturationally determined change. As part of the specifications of the theoretical model, it would therefore seem necessary to indicate under what conditions the predicted age effects could validly be observed (cf. Baltes, Cornelius, & Nesselroade, 1979).

Consideration of the design confounds involving developmental data suggests that alternative models of aging should be considered and tested. For example, the recognition that positive secular trends may indeed suppress observable aging effects over some portion of the life span would lead us to give more serious considerations to models that posit stability across most of adulthood and involve decrement with compensation, positive age trends, or even recursive or cyclical phenomena (cf. Schaie, 1973).

The availability of designs that allow the modeling of theories that imbed multidimensional forms of the aging process also can be fruitful in extending existing theoretical systems into considerations of adulthood and aging. I have done so, for example, in proposing an outline for a stage theory of cognitive aging that is essentially an extension of Piagetian thinking (cf. Schaie, 1977/1978). The essential basis of this theorizing was the design-guided data acquisition that showed the need to posit life stages representing growth, stability, and decremental change for different cognitive traits. In another extension, I was able to show how developmental designs introduced in aging studies might be useful in disaggregating environmental and genetic factors that influence development (Schaie, 1975).

In examining the contributions of aging research methodology it is important to distinguish between models that serve to help interpret the results of data acquisitions from models that explicitly specify how data should be acquired (cf. Schaie & Baltes, 1975). This is obviously the distinction between explanation and description to which we will next turn.

Implications for Models of Data Collection

If alternative models of aging are to be tested, it becomes necessary to embed theory into data-acquisition plans. Given the confounds described above, few theory-based questions are likely to be answerable by studying a single cross-sectional data set or even a single sample followed longitudinally over time. A logical consequence then is to conceptualize data-collection plans so that extensions of the initial acquisition can be suitably added. This involves data acquisitions that are structured as cross-sectional or longitudinal sequences (cf. Baltes, 1968; Schaie & Baltes, 1975).

One important derivative from the theoretical analysis of the general developmental model has been the specification of recommendations for an optimal data-collection approach that permits flexible application of sequential data-analysis strategies, as well as the provision of controls to protect against major threats to the internal validity of developmental studies. Noting the fact that all longitudinal studies must begin somewhere with a single first-measurement occasion, I have long been convinced that it is always prudent to commence with an age-comparative cross-sectional design. However, in those instances where such a design cannot answer the questions of interest, it would prove reasonable to collect additional data across time.

A hypothetical data collection of this kind, which I have previously identified as the "most efficient design," requires the identification of a population frame that provides a reasonable representation of the full range of dependent variables to be studied (Schaie, 1965; Schaie & Willis, 1986a). Optimally, the population frame should be a natural one, such as a school system, a health plan, a broadly based membership organization, or the like. If the population frame is reasonably large, it is then possible to assume that members leaving the population will on average be replaced by other members with similar characteristics (sampling with replacement). An age range of interest is defined at Time 1 and is sampled randomly at intervals that are optimally identical with the time chosen to elapse between successive times of measurement. At Time 2, previous participants are retrieved and restudied, providing short-term longitudinal studies of as many cohorts as there were age intervals at Time 1. At the same time, a new random sample is drawn from the population frame over the same age intervals as in the Time 1 sampling, with one additional sample at the age level currently attained by the oldest subsample assessed at Time 1. The whole process can be repeated again and again with retesting of old subjects (adding to the longitudinal data) and initial testing of new samples (adding to the cross-sectional data). Three assessment points provide maximum design benefits, although one or two additional measurement points allow additional design refinement.

The second data collection converts the original cross-sectional study into a series of C (number of cohorts studied at T_1) longitudinal studies, and the additional cross-section provides a cross-sectional sequence over two occasions. A series of C time-lag comparisons is also available. In addition, it is now possible to examine the repeated measurement data as a cohort × time matrix allowing cross-sequential analyses. The cross-sectional sequence can be further examined as an age × time matrix (time-sequential strategy) or as an independent measures cohort × time matrix (cross-sequential strategy). A cross-sectional experimental mortality analysis can be done by comparing T_1 data for those individuals who were successfully reexamined and those who were not retrieved at T_2 (cf. Costa et al., 1987, for a recent application of this design).

The third data collection, in addition to replicating longitudinal findings upon the samples first tested at T_2, permits the analysis of age × cohort data matrices for $C-1$ data sets for both repeated measurements on the same subjects or independent samples from the cross-sectional sequence. In addition, age × time and cohort × time matrices can be analyzed for a $3 \times C$ data matrix. Experimental mortality analyses, classifying dropout to occur after both the first and second occasion, can now be conducted, using either an age × time × dropout model or a cohort × time × dropout model. Alternatively, it is possible to estimate effects of practice for either an age × time × practice level design or a cohort × time × practice level design (cf. Schaie & Parham, 1974).

All of the sequential paradigms can be estimated by three measurement occasions, but there are some additional options available if further extensions of the data collection are possible. A fourth measurement occasion permits three replications of longitudinal data over one time segment, two replications over two time segments, and an estimate of longitudinal change over three time segments. The study of experimental mortality data can then be extended to an age × cohort × dropout model, and it would be possible to cross experimental mortality and practice effects within an age × time × practice × dropout paradigm or a cohort × time × practice × dropout paradigm. A fifth measurement occasion, finally, allows estimating an age × cohort × practice × dropout model, in addition to allowing four replications of longitudinal data over one time segment, three replications over two time segments, two replications over three time segments, and an estimate of change over four time segments (cf. Schaie, 1977).

Implications for the Study of Age and Time

More recent analyses of the age-cohort-period problem have also provided a better understanding of what needs to be done if the dependency of chronological age upon calendar time is to be broken in a meaningful manner. Models that contain parameters, one of which is wholly determined by the others, are well known in science (e.g., the attributes of volume, pressure, and temperature in physics). Just as in the case of confounded physical parameters, there are many reasons why one would want to examine the relative contributions of any of the three possible sets of two developmental parameters. The recent literature on the analysis of sequential data matrices, moreover, would finesse the problem of invalid parametric assumptions by promoting regression models that estimate simultaneously the effects of age, period, and cohort under an additivity assumption that allows for no interaction among the factors (e.g., Buss, 1979/1980; George, Siegler, & Okun, 1981; Horn & McArdle, 1980; Mason et al., 1973; Winsborough, Duncan, & Read, 1983). Regression models for the study of sequential data sets do represent a step forward in the modeling of average developmental functions by employing sophisticated applications of the general linear model. They are prone to errors of inference, nevertheless, whenever the assumption of additivity, or other parametric assumptions needed to identify the model, are violated (Glenn, 1976, 1981). None of these efforts, however, satisfied the original objective of providing unambiguous estimates of the variance accounted for by each of the individual components of developmental change. This observation leads to the conclusion that there is no purely statistical solution (Schaie & Hertzog, 1982).

An alternative approach may be suggested, however, that can reorient our thinking and offer a possible way out of what has become a methodological impasse. The concern with methodologies designed to separate age, cohort, and period effects arose in essence from our preoccupation with the role of age as the independent variable of prime interest to students of development (Featherman, 1985; Slife, 1981). It can be shown, however, that cohort and period may have more interesting explanatory properties than age. This is done by conceptually separating cohort effects and historical time from calendar time. *Cohort* then becomes a selection variable (Nesselroade, 1983) that characterizes the common point of entry for a group of individuals into a given environment, and *period* becomes a measure of event density (for further elaboration see Schaie, 1984, 1986). Similarly, *chronological age* is reconceptualized as a functional age dimension. Given such reconceptualizations it is then possible to reformulate the general developmental model, utilizing indicators that have been freed from the calendar restrictions that have thus far been accepted as virtually immutable. In this approach, chronological age, or other age functions related to calendar time, rather than serving as explanatory concepts, emerge as useful scalers that measure the amount of time elapsed within the lives of individuals, over which developmental phenomena have occurred. The dependencies implicit in the age-cohort model, moreover, can be shown to have been resolved, and all three effects can be directly estimated, whenever one of the dimensions is redefined in terms other than calendar time.[2]

METHODOLOGICAL INNOVATIONS THAT INFORM THEORY: THE CASE OF CONFIRMATORY FACTOR ANALYSIS

Some Historical Context

Most direct observations conducted by psychologists are of interest only to the extent that such observations represent reliably measured markers of latent (unobserved) psychological constructs. Theory-guided research requires the specification of the psychological constructs of interest, as well as the observations that will be used to estimate the constructs. Notable examples of theoretical models (inductively derived) that specify the relationship between latent constructs and observable measures are the structure of human ability models developed by Thurstone (1938) and by Guilford (1967).

More early concerns in the study of aging were related to the estimation of development as a function of change in performance level on directly observable measures (cf. Dixon, Kramer, & Baltes, 1985). More recent work has extended these concerns to the comparison of structure (i.e., the regression of observables upon latent constructs) across different age groups and within cohorts over time (Cunningham, 1980, 1981; Cunningham & Birren, 1980; Hertzog & Schaie, 1986; Schaie & Hertzog, 1985, 1986; Schaie, Willis, Hertzog, & Schulenberg, 1987). This work was made possible as a direct consequence of the methodological developments in restricted factor analysis most notably represented by the work of K. G. Jöreskog and his associates. Jöreskog (1971) and other methodologists such as Bock and Bargmann (1966) and Bentler (1980) demonstrated that hypotheses about the relationships among latent constructs that underlie empirically observed variables can be formulated as structural models. They furthermore showed that maximum-likelihood methods can be used to test the plausibility of such structural models and to estimate the parameters of the linear model. Further extensions of this work allow the testing of alternative, hierarchically nested models as well as contrast the fit of empirical data to a model of theoretical interest with that attained for its falsification.

The original objective of confirmatory factor analysis was to permit the fitting of data to specific measurement models depicting the relationship between latent constructs and observed variables. It soon became apparent, however, that confirmatory factor analysis could also be used to test the equivalence of factor structure across and within groups (Jöreskog, 1979). In addition to the estimation of measurement models, it was also shown that covariance structures representing the relationships between two or more sets of latent constructs could be formulated as structural models.

Implications for Theory Building

An important role of confirmatory factor analysis in theory-guided research, and one that requires attention by those who do cross-sectional or longitudinal work, is the applicability of structural equation models to the demonstration of measurement equivalence (Hertzog, 1986; Schaie & Hertzog, 1985). Structural equation models may also be of particular utility in aging studies because the unidirectionality of time permits sounder guides for the specification of causal paths than is possible in studies using single observation points only. Longitudinal factor analysis may be a particularly useful approach to the modeling of individual differences in intraindividual change, the central focus of any individual-differences approach to aging (cf. Hertzog & Schaie, 1986; Jöreskog, 1979; Jöreskog & Sörbom, 1977). Finally, confirmatory factor analysis turns out to be the method of choice to test the stability of latent constructs under conditions of serendipitous or planned interventions in the aging process (cf. Schaie & Willis, 1986b).

Equivalence of Psychological Constructs Across Time

Utilization of the same questionnaire or test apparatus does not guarantee measurement equivalence over time or different subject populations. Indeed, in the developmental sciences the broad dilemma must be faced that no two individuals, or groups of individuals, have identical characteristics at the same point in time, nor does a group of individuals retain identical characteristics over different points in time. Two fundamentally different aspects are at issue. The first includes the traditional problem of reliability of measures across occasion and regression to the mean when using fallible measures (cf. Nesselroade, Stigler, & Baltes, 1980). The second issue concerns the fact that

measurement equivalence would not be guaranteed, even if only perfectly reliable measures were used, because of systematic but nonuniform changes occurring among individuals over time.

In cross-sectional studies it is legitimate to ask whether a task that may be a good estimate of one construct in young adulthood remains so in late life or if it in fact becomes a measure of some other construct. What must be demonstrated then is the invariance of factor structure across multiple groups or subpopulations (cf. Alwin & Jackson, 1981). Similar issues occur when samples are followed longitudinally. Here, of course, we are concerned with demonstrating factorial invariance for the same individuals measured longitudinally. Structural equation analysis seems to be the approach of choice to assess measurement equivalence issues involving multiple groups and occasions (cf. Rock, Werts, & Flaugher, 1978; Schaie & Hertzog, 1985).

Availability of explicit statistical models for testing construct equivalence has profound effects for theory building and testing in aging research. For example, any discussion of a model that argues for the successive differentiation and dedifferentiation of human abilities (cf. Reinert, 1970) must not only specify the constructs that are involved in such a process but also must specify hypotheses on the manner in which the constructs may be represented by different abilities (or the same abilities weighted differentially) at different life stages. The issue of equivalence of constructs is also likely to impact future work in experimental gerontology. The experimental literature on age differences (cf. Salthouse, 1982) is largely an account of manipulations of single observable measures in the laboratory context. In order for such data to become meaningful in understanding age-related behavior in real-life contexts, it will certainly be necessary to formulate more complex structural models and to attempt manipulations at the latent-variable level. Recent discussions of the role of experience (Salthouse, 1987) and the relationship between cognition and everyday behaviors (Willis, 1985; Willis & Schaie, 1986) clearly indicate that methodological advances are beginning to transform our behavior in theory building and theory testing.

The Structuring of Causal Models in Aging Research

Age-comparative experimental research can at best test whether a given manipulation has a differential effect on samples of different age composition. If the age-by-treatment interaction is significant, if the main effect for age favors the younger group, and if the treatment is more effective for the older group, it may then be argued that the manipulation indeed accounts for the age difference. The investigation of structural causal models involving longitudinal data, by contrast, directly addresses the question of whether particular antecedent conditions that may represent multiple levels of causation significantly impact consequent behavior. In this approach it is possible to model correlations observed across time as indications of the direct influence of one latent variable upon another, as an indirect influence mediated through other variables, or as a causally spurious correlation (cf. Schaie & Hertzog, 1985).

Whenever a theoretical model can be represented as a path diagram that indicates the direction of causal influences, it then becomes possible to test variations of that model which allow for different mediators, make more or less stringent assumptions, or represent falsifications à la Popper. Causal models have been used in aging research to study the relationships among environmental influences, health status, and cognitive change, and in particular to evaluate the validity of the differentiation-dedifferentiation hypothesis (cf. Gilewski, 1982; Hertzog & Schaie, 1986; Stone, 1981).

Equivalence of Constructs in Age Simulation or Intervention Models

Theory construction designed to model the aging process frequently involves making assumptions about unobserved change processes (cf. Baltes & Nesselroade, 1973). To provide a reasonably coherent theoretical proposition it may often be necessary to assume age changes at the latent-construct level that have not reached levels critical enough to lead to change in observed behaviors. Likewise, changes in one behavioral marker of a major theoretical construct may well be compensated for by another, and it consequently becomes reasonable to specify behavioral stability at the construct level, albeit requiring change at the observable level. Again confirmatory factor analysis has provided us with the necessary tools to provide explicit formulation of the theoretical model in quantitative terms that allow tests of the model by examining relevant empirical data bases (cf. Hertzog & Schaie, 1986; Schaie & Hertzog, 1985).

Confirmatory factor analysis methods also make it possible to formulate and test theoretical propositions about construct equivalence in intervention studies that involve multivariate effect models (cf. Schaie & Willis, 1986b). Much of the controversy regarding reversal of age decrement, whether by serendipitous environmental changes or by planned intervention, is concerned with the question of whether the intervention might result in changing the construct that is to be modified. For example, it has been argued that certain training paradigms might change the nature of fluid abilities to crystallized abilities (Donaldson, 1981). Any research design that incorporates both observed and latent constructs, of course, must be theory-guided (cf. Baltes & Willis, 1982). That is, the observable latent-construct relationship must arise from a theoretical model of the variable structure. It is then possible, however, to express such theoretical models by confirmatory factor analysis paradigms and to derive additional hypotheses that would test certain corollaries which must follow if latent constructs remain stable across interventions or are indeed perturbed by the training effort. Specifically, it is possible to determine

empirically whether structural change has been caused by intervention. The exact nature of such change, if any, can be tested for further by imposing suitable constraints regarding the structural equivalence of experimental and control groups prior and subsequent to intervention (cf. Schaie et al., 1987). Conclusions drawn from such studies, once again, will inform us on the plausibility of theoretical formulations regarding the aging process and can guide future theory building.

SOME CONCLUDING THOUGHTS

The purpose of this chapter has been to examine a number of ways in which developments in research methodology impact theory building in the aging-research community. Methodological innovations can require major changes in the way in which theories are tested, or can demand the addition of new corollaries and boundary conditions to retain the viability of existent theories. Other methodological innovations can directly produce paradigm shifts either by permitting the direct investigation of phenomena that were not previously specifiable by a theoretical model or by providing methods that allow investigation at different levels of conceptual specification.

There does not seem to be a single model, however, that would clearly specify how shifts in research methodology lead to modification of theory. Nevertheless, it is not difficult to provide substantive examples where shifts in methods have impacted theory. Such links between methodology and theory can be seen most clearly in cases where investigators have been forced to abandon previous methods of measuring, designing, or analyzing data in favor of conceptually superior innovations. We therefore proceeded to examine two distinct series of methodological developments that have had particular impact upon the thinking of gerontological researchers: the age-period-cohort model in research design, and confirmatory factor analysis in model testing.

The most immediate effect of methodological developments upon theory is the likely increase in effective ways of expressing theoretical thinking in explicitly testable terms. Theories of aging, in the past at least, have frequently not been well articulated, have lacked full specification of assumptions and corollaries, and thus have had little impact in guiding research. Methodological advances introduced via the wide acceptance of the age-cohort-period model, for example, have required the incorporation of formal specifications of assumptions regarding internal and external validity in many research plans. Explicit operationalization of theoretical models, moreover, including their measurement assumptions, become necessary when methodologies such as confirmatory factor analysis or linear structural analysis are utilized. Finally, introduction of new methodologies lead to paradigm shifts, whether by moving from theoretical models relating observables to systems specifying the relationship among constructs, or by introducing new constructs such as cohort or functional age. In conclusion, I would like to suggest that theory building that influences and guides the research enterprise will inevitably follow upon the incorporation of sound methodological innovation in any science. It is hoped that the examples provided in this chapter demonstrate the validity of this argument for theory development in the field of human aging.

NOTES

[1]Surfacing of these issues certainly represented the *zeitgeist*, as concurrent and quite independent discussions may be found in the sociological literature by Ryder (1965).

[2]Empirical examples of data sets that would allow direct estimation of age, cohort, and period effects, given the proposed reformulation of the general developmental model, are provided in Schaie (1986).

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