# Longitudinal Studies of Adult Psychological Development

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# 1 What Can We Learn from the Longitudinal Study of Adult Psychological Development?

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## Introduction

Developmental psychologists have long felt that the understanding of lawful relationships pertaining to the developmental processes must sooner or later require that the same organisms be observed over that period of time during which the developmental phenomena of interest are thought to occur. But since developmental phenomena in humans occur relatively slowly, except during some critical periods in early infancy and shortly before death, it has often been found impractical to follow the same subjects over the entire developmental period. In fact, some researchers who simply cannot see the enormous investment of time and effort required for longitudinal studies have been willing to make strong assumptions to support arguments that developmental phenomena can be adequately modeled by cross-sectional designs.

Good longitudinal studies develop because they occur in a collaborative network that develops its own social system. And often a major study may have been instrumental in providing the training for young scientists, who will then carry on the study and in turn use it as a training vehicle for a further generation. Indeed, the existence of this volume is testimony to the possibility of carrying on longitudinal studies across long periods of time and to the fact that much can be learned from these studies. Nevertheless, it seems appropriate to orient the reader by specifying as succinctly as possible those aspects of developmental research that can only be gleaned from longitudinal inquiry. It seems appropriate also to alert the reader to some of the methodological pitfalls in the data from the older

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studies. This is not done to criticize these endeavors; all of the studies in the volume represent the best of the state of the art available at their inception. But the reader should know the limitations of the data bases, which are summarized in this chapter. These issues have recently appeared elsewhere; the reader is referred to Nesselroade and Baltes (1979) and Schaie and Hertzog (1982) for greater technical detail. In addition, the purposes of this introductory chapter are to attempt to chart, at least briefly, the manner in which the various studies reported here reach common ground or diverge from one another; to provide a chronology that may be useful for relating them to one another; and to indicate those themes and findings that to the editor seem to weave a common thread.

## Advantages of Longitudinal Studies

The principal advantage of the longitudinal approach to the study of human development is, of course, the possibility of gaining information about intraindividual change (IAC). By contrast, cross-sectional studies can provide data and make inferences only about interindividual variability (IEV). Even in the case where successive independent samples are longitudinally drawn from the same cohort, the emphasis is on change within the population examined rather than on the typical cross-sectional comparison of samples possibly coming from different populations. In spite of this emphasis on IAC, most longitudinal studies do, of course, also permit analyses of IEV.

It is possible to identify five distinct rationales for the longitudinal study of behavioral development. Three of these involve developmental descriptions, while the remaining two are explanatory in nature (cf. Baltes & Nesselroade, 1979).

### *Direct Identification of Intraindividual Change*

Intraindividual change can be quantitative and continuous, or it can involve qualitative change, such as the transformation of one behavior into another. Alternatively, changes may occur in the pattern of observed variables as they relate to or assess theoretical constructs. For a determination of any of these changes, observations based on a single occasion are simply inappropriate. To be even more explicit, if cross-sectional data were to be used to estimate IAC, the necessary assumptions to be met would include that (1) different-aged subjects come from the same parent population at birth, (2) subjects be matched across age levels, and (3) different-aged subjects have experienced identical life histories. It is clear that such assumptions cannot be met in human studies.



### *Identification of Interindividual Variability in Intraindividual Change*

Longitudinal studies permit assessment of the degree of variability displayed by different individuals in their behavioral course over time. Determination of typologies of growth curves requires the examination of similarities and differences in developmental patterns, data that require the availability of measures of longitudinal change within *individuals*. Barring such data, it would not be possible to answer the question whether or not group parameters are descriptive of the development of any particular individual. In addition, the valuable hypothesis-generating source of single-subject research must depend on longitudinal analyses (cf. Shontz, 1976).

### *Interrelationships among Intraindividual Changes*

Modern developmental psychology has recognized that it must operate within a multivariate domain of variables. When individual behaviors are followed over time, it is then possible to discover constancies and change for the entire organism, especially when the theoretical model followed is of a holistic or structural persuasion (e.g., Riegel & Rosenwald, 1975). Longitudinal studies alone, by virtue of multiple observations over time, permit the discovery of structural relationships among behavior changes. The multivariate longitudinal approach is essential for the identification of progressive differentiation processes and for any type of systems analysis (cf. Lund, 1978; Urban, 1978).

### *Analysis of Determinants of Intraindividual Change*

In the inferential realm, longitudinal studies are required to permit the identification of time-ordered antecedents and consequents as necessary, albeit not sufficient, conditions for causal interpretations. Specifically, it is the longitudinal approach alone that can provide requisite data to show that a causal process involves discontinuities, such as the so-called sleeper effects, or where causal chains are multidirectional or contain multivariate patterns of influence (see also Baltes, Reese, & Nesselroade, 1977; Heise, 1975).

### *Analysis of Interindividual Variability in the Determinants of Intraindividual Change*

Finally, longitudinal data permit inferences concerned with the fact that many individuals can show similar patterns in IAC that may be determined by different change processes. Such individual differences are found for persons at different levels in the range of talent or other behavioral attributes. Alternatively, interindividual differences in patterns of change may be attributable to the operation of alternative combinations of causal sequences.

## Longitudinal Studies as Quasi-Experiments

Although the longitudinal approach has many advantages over studies based on one-time observations, it is also beset with many methodological problems, some of which have led to a variety of design refinements which may be noted as the reader follows the accounts in this volume from the earlier to the later studies. In this section, I attempt to alert the reader to some of these issues by discussing internal and external validity, the traditional single-cohort design, sequential strategies in general, and the longitudinal sequence in particular; finally, I suggest some possible solutions to the remaining difficulties, which were applied to one or another of these studies.

### *Internal and External Validity*

Longitudinal studies do not conform to the rules for true experiments since age is a subject attribute that cannot be experimentally assigned. Consequently, they are subject to all the problems inherent in what Campbell and Stanley (1967) term "quasi-experiments." These problems may be threats to the internal validity of a study; that is, factors analyzed in a given design that are thought to be measures of the hypothesized construct may be confounded by other factors not explicitly included in the design. Alternatively, the problems are threats to the external validity



of a study, which limits the extent to which valid generalizations from the sample can be applied to other populations (see also Cook & Campbell, 1975).

#### Internal Validity

Eight different threats to the internal validity of quasi-experiments such as longitudinal studies have been enumerated (Campbell & Stanley, 1967): maturation, effects of history, testing, instrumentation, statistical regression, mortality, selection, and the selection–maturation interaction. The first two of these, history and maturation, have, for the developmental psychologist, special meaning beyond their threat to the internal validity of any pretest–posttest design. *Maturation*, quite obviously, is not a threat to the validity of developmental studies, but rather is the specific effect of interest to the investigator. Nevertheless, its measurement is not always unambiguous, since, given a specific developmental model, it may be necessary to go beyond a test of the null hypothesis negating maturational effects in order to test instead some explicit alternative hypothesis that specifies direction and magnitude of the expected maturational effect.

On the other hand, *historical effects* are the primary concern regarding internal validity problems for the developmental scientist. History is directly involved in both cohort and time-of-measurement (period) effects. "Cohort" is here defined as a group of individuals born in the same historical period, who consequently share similar environmental circumstances at equivalent points in their maturational sequence. "Time-of-measurement effects," by contrast, represent those events that affect all members of a population, regardless of cohort membership, living through a given period of history. The specific threat to longitudinal studies is that historical effect may threaten the internal validity of designs attempting to measure the effects of maturation.

The traditional longitudinal design is a special case of the pretest–posttest design in that it repeatedly measures the same individuals over time; as a result it is affected also by the other six threats to internal validity proposed by Campbell and Stanley. There are actually two different effects of *testing*: practice and reactivity. Reactivity involves the possible effect upon subsequent behavior of being exposed to a certain procedure. Longitudinal study subjects might respond to a second test in a very different manner than if they had not been previously tested, a behavior change that could be confused with the effects of maturation. Practice effects, on the other hand, may simply mean that, upon subsequent tests, subjects have to spend less time in figuring out items previously solved and thus can improve their overall performance.

The internal validity threat of *instrumentation* refers to differences in measurement techniques that covary with measurement occasions. In long-term longitudinal studies, such differences may occur when study personnel changes, or when records regarding study protocol on previous occasions are lost, and slight variations are introduced inadvertently. Again, such changes may lead to the wrong inference of having found maturational trends or may tend to obscure reliable, but small, developmental changes actually occurring.

*Statistical regression* involves the tendency of variables containing measurement error to regress toward their mean from one occasion to the next. This problem is of particular importance in two-occasion longitudinal studies. As has recently been shown, regression effects do not necessarily cumulate over extended series (Nesselroade, Stigler, & Baltes, 1980).

Since human panels cannot be forced to continue participation in long-term studies, another serious threat is that of *experimental mortality*. This term describes the attrition of subjects from a sample between measurement occasions, whether such attrition be due to biological mortality, morbidity, other psychological and sociocultural factors, or sheer experimenter ineptness. Most empirical studies of experimental mortality suggest that attrition is nonrandom at least between the first and second measurement occasion (cf. Gribbin & Schaie, 1979; Schaie, Labouvie, & Barrett, 1973).

*Selection* refers to the process of obtaining a sample from the population such that the effect obtained is a function of the sample characteristics rather than of the maturational effect to be estimated. The *selection-maturation interaction*, of course, refers to the case where maturational effects may be found in some samples but not in others.

### External Validity

As quasi-experiments, longitudinal studies also share certain limitations with respect to the generalizability of their findings. Four major issues can be identified here. The first concerns *experimental units*, that is, the extent to which longitudinal data collected on one sample can permit inference to other populations. The second involves *experimental settings*, or the extent to which findings have cross-situational validity (cf. Scheidt & Schaie, 1978). The third is concerned with *treatment variables*, that is, the limitations imposed by specific settings or measurement-implicit reinforcement schedules (cf. Birkhill & Schaie, 1975; Schaie & Goulet, 1977). Finally, external validity may be threatened by certain aspects of the *measurement variables*, with regard to the extent to which task



characteristics remain appropriate at different developmental stages as a longitudinal study progresses (cf. Schaie, 1977/1978; Sinnott, 1975).

### *Traditional Single-Cohort Designs*

The purpose of the classical longitudinal design is to estimate development of IAC within the same individuals. As such, the design explicitly represents a time series, with an initial pretest, a subsequent intervention (the maturational events occurring over time), and a posttest, all on the same individual organisms. If there is more than one time interval, then there is a succession of alternating treatments (further maturational events) and posttests. Traditionally, the longitudinal design was applied to only one group of individuals of relatively homogeneous chronological age at first testing and thus to a single birth cohort. In principle, the first two studies reported in this volume are of this kind; two of the latter also began in this manner.

In reviewing the single-cohort studies, the reader needs to keep in mind that several of the threats to internal validity just enumerated may be plausible alternative explanations for the observed behavioral change (or lack thereof) reported as a function of age for these studies. To be explicit, in a single-cohort longitudinal study, time-of-measurement (period) and aging effects must be confounded, and the presence of period effects related to the dependent variable of interest will render estimates of age effects internally invalid. These period effects may either mimic or suppress maturational changes occurring over a particular age span, depending on whether age and time-of-measurement effects covary positively or negatively.

It must also be noted that the single-cohort longitudinal design does not directly control for the other internal validity threats. The reader should be alert to the fact that some of the latter threats can be and have been controlled by careful researchers such as those represented in this volume. That is, great pains were generally taken to eliminate the confound of instrumentation by taking steps to ensure that the measurement procedures remained as consistent as possible throughout the course of the studies. Statistical regression effects were minimized in all these studies by including at least two, and often more, retest occasions. But clearly, except in those cases where collateral control samples were studied for this very purpose, there is no way for single-cohort longitudinal studies to circumvent the confounds of testing and experimental mortality.

Although I have generally argued that it would be unwise to start new single-cohort longitudinal studies (e.g., Schaie, 1972), I do believe that such studies were necessary and appropriate in the early stages of the developmental sciences. Moreover, there continue to be instances when a single-cohort longitudinal design may be the best approach to providing preliminary evidence for developmental functions, which can later be replicated for additional cohort and measurement occasions. Single-cohort studies may also prove useful in particular applications such as defining typologies of developmental patterns in a specifically targeted single-cohort population.

The reader will see in this volume that early longitudinal studies served the preceding purposes well, but also that those investigations started more recently were soon found to require buttressing by conversion to a multiple-cohort approach (e.g., the Baltimore, Bonn, Duke, and Seattle longitudinal studies).

### *Sequential Strategies*

To reduce the limitations inherent in the single-cohort longitudinal design, several alternative sequential strategies have been suggested (Baltes, 1968; Schaie, 1965, 1973, 1977). The term "sequential" merely implies that the required sampling frame involves a sequence of samples taken across several measurement occasions. Sequential strategies can best be understood by first differentiating between sampling design and analysis design (Schaie & Baltes, 1975), although both are closely interrelated. Sampling design refers to the particular cells of a cohort-by-age (time) matrix that are sampled in a developmental study. Analysis design refers to the manner in which the cells that have been sampled may be organized in order to analyze for the effects of age (*A*), cohort (*C*), and time of measurement. (*T*). Figure 1.1 gives a typical cohort-by-age matrix showing sequential designs. This figure also illustrates the confounding of the three parameters of interest. *A* and *C* appear as the rows and columns of the matrix, while *T* is the parameter contained within the matrix cells. The reader interested in the debate on whether and how these effects should be unconfounded is referred to papers by Adam (1978); Horn and McArdle (1980); Mason, Mason, Winsborough, and Poole (1973); and Schaie and Hertzog (1982). The issues involved are quite complex, highly technical, and beyond the scope of this introductory chapter.

### Sampling Designs

Two types of sequential sampling designs may be distinguished: those using the same panel of individuals repeatedly to fill the cells of the

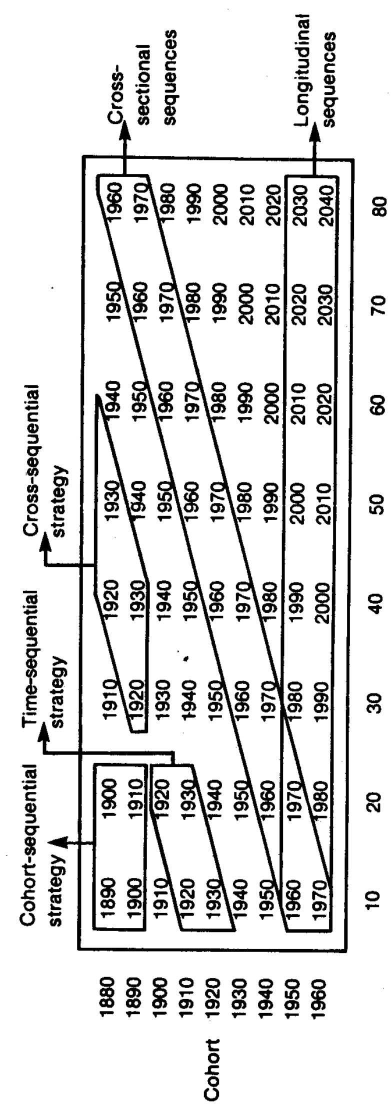


FIGURE 1.1. Schematic showing cross-sectional and longitudinal sequences and the modes of analysis deduced from the general developmental model. Table entries represent times of measurement (period).

matrix, and those using independent samples of individuals (each observed only once) from the same cohorts to do so. The matrix shown in Figure 1.1 could have been produced by either approach. In this volume, the Bonn study uses the former approach, and the Baltimore, Duke, and Seattle studies employ both. Using Baltes's (1968) terminology, we can call the two designs longitudinal and cross-sectional sequences, respectively. Typically, a cross-sectional sequence involves the replication of a cross-sectional study such that the same age range of interest is assessed at least for two different time periods, obtaining the estimate for each age level across multiple cohorts, where each sample is measured only once. By contrast, the longitudinal sequence represents the measurement of at least two cohorts over the same age range. Again, estimates from each cohort are obtained at two or more points in time. The critical difference, however, is that the longitudinal sequence provides data that permit the evaluation of IAC and of IEV in IAC (see preceding discussion in this chapter).

## Analysis Designs

Matrices like Figure 1.1 contain data permitting a variety of alternate strategies of analysis (Schaie, 1965, 1977). To be specific, each row of this matrix can be treated as a single-cohort longitudinal study, each diagonal as a cross-sectional study, and each column as a time-lag study (comparison of behavior at a specific age across successive cohorts). Sequential sampling designs do not permit complete disentanglement of all components of the $B=f(A, C, T)$ function because of the obvious linear dependence of the three factors. Nevertheless, I have suggested that, given this model, there exist three distinct analysis designs, which are created by considering the separate effect of two of the components while assuming the constancy or irrelevance of the third on theoretical or empirical grounds.

As exemplified by the minimum designs shown in Figure 1.1, I have suggested that the *cohort-sequential* strategy will permit separation of age changes (IAC) from cohort differences (IEV), under the assumption of trivial time-of-measurement effects. Further, the *time-sequential* strategy will permit the separation of age differences from period differences (both IEV), assuming only trivial cohort effects. And finally, the *cross-sequential* strategy will permit separation of cohort differences (IEV) from period differences (IAC). The time-sequential strategy, of course, is not a truly longitudinal approach (i.e., the same individual cannot be the same age at two different points in time), but it does have merit for the estimation of age differences for social policy purposes, for those dependent variables



for which cohort effects are likely to be minimal. It is also an appropriate strategy to use in estimating time-of-measurement (period) effects for studies including a wide range of age/cohort levels.

### *Longitudinal Sequences*

When data are collected in the form of longitudinal sequences in order to examine IAC, it is possible to apply both the cohort-sequential and cross-sequential strategies for data analysis. Developmental psychologists find the cohort-sequential design of greatest interest because it explicitly differentiates IAC within cohorts from IEV between cohorts (cf. Baltes & Nesselroade, 1979; Schaie & Baltes, 1975). This design, in addition, permits a check of the consistency of age functions over successive cohorts, thereby offering greater external validity than would be provided by a single-cohort longitudinal design.

As was noted earlier, a critical assumption of the cohort-sequential design is that there are no time-of-measurement effects contained in the data. Although this assumption may be parsimonious for many psychological variables, others may still be affected by "true" period effects or because of the confounds presented by occasion-specific internal validity threats such as differences in instrumentation or experimenter behavior across test occasions. The question arises, then, how violations of the assumptions of no time-of-measurement ($T$) effects would be reflected in the cohort-sequential analysis. Logical analysis suggests that all estimated effect will be perturbed, albeit the most direct evidence would be shown by a significant $C$ (cohort)-by-$A$ (age) interaction (cf. Schaie, 1973). However, absence of such an interaction does not guarantee the absence of $T$ effects; such effects might localize in a small subset of occasions in extensive studies, in which case all our effect estimates would be biased.

It is well recognized by now that the essential consequence of the interpretational determinancy in sequential analysis is that, if the assumptions that justify the specific design are violated, then all effect estimates will be inaccurate to some degree. The interpretational problem may be reduced, however, to estimating the relative likelihood of confounded $T$ effects, given a strong theory about the nature and direction of estimated and confounded effects. Indeed, a practical application of strong theory to sequential designs involves specification of confounds in an invalid design in order to obtain estimates of the confounded effects.

An important example of planned violation of assumptions is the use of the cross-sequential design under the assumption of no $A$ effects, an assumption most developmental psychologists might find quite unreasonable. Such a design may be useful when longitudinal data are available for only a limited number of measurement occasions but for a wide range of cohort groups. The cross-sequential design can be implemented after only two measurement occasions, whereas a cohort-sequential design requires at least three. Moreover, the number of measurement occasions required to estimate cohort-sequential designs that span a wide age and/or cohort range would be prohibitive if we insist that no data analyses be performed until the cohort-sequential design appropriate for the research question had been accomplished. Given a strong developmental theory about the nature of the confounded *A* effects, a misspecified cross-sequential design can provide useful information about the significance of the *A* effects represented in both the *T* and the *C*-by-*T* design. My early work on the sequential analysis of intelligence began with such misspecification in cross-sequential design in order to permit drawing preliminary inferences concerning the relative importance of *C* and *A* effects prior to the availability of data that could have permitted direct simultaneous assessment of these effects (cf. Schaie, Labouvie, & Buech, 1973; Schaie & Strother, 1968).

Although it is always preferable to estimate the "true" parameter effects from the appropriate design—one that makes the correct limiting assumptions—the developmental psychologist must often settle for something less than the optimal design, whether this be a temporary expedient or dictated by the nature of the phenomenon being studied. The studies represented in this volume individually and collectively provide good illustrations of the evolution of a field that has seen much methodological turmoil and change, the end of which is not yet in sight.

## Empirical Studies of Adult Psychological Development

This volume consists of fairly detailed descriptions of the design, methodology, and results of seven longitudinal studies, which probably represent the bulk of work on what is securely known about age changes in psychological variables from young adulthood into old age. The first two of these studies represent single-cohort designs, but their interpretability was enhanced by later collateral work with other comparison groups.[1]

[1]There are several other important longitudinal studies that could have been included but were not, because of either space limitations or study personnel changes, which made it difficult to commission adequate reviews for this volume. The most noteworthy omissions are probably the Berkeley Growth and Guidance Studies (Eichorn, Clausen, Haan, Honzik, & Mussen, 1981), the Terman Study of Genius (Sears, 1977; Terman & Oden, 1959), and the Boston Normative Aging Study (Bell, Rose, & Damon, 1972).



The remaining five studies either were conceptualized as multicohort studies or were designed in such a way that conversion to a multicohort format was possible, once the desirability of more extended data collections became obvious.

In this section, I will introduce the substantive accounts by calling attention to some of the highlights and reflect briefly on some unique aspects of each study.

### *Chronology of the Studies*

Table 1.1 summarizes the basic chronology of the studies reported here. With the exception of the American Telephone and Telegraph (AT&T) study, the oldest cohort includes persons born before or around the turn of the century. And with the exception of the Iowa State Study, data collections began after World War II. Although the Iowa State Study has precedence in order of first data collection, it did not really begin as a longitudinal study; its first longitudinal data point occurred in 1953.

The Aging Twins Study began in the mid-1940s as a special population study. All of the remaining studies began in the decade from the mid-1950s to the mid-1960s. Time frames covered a range from 20 to over 40 years, and age ranges investigated are from the early 20s to over 100 years. Except for the AT&T study, whose oldest subjects were born in the 1930s, studies examine wide cohort ranges: members of the oldest cohort were born in the last quarter of the 19th century, and those of the youngest cohort during the 1950s.

### *Specific Study Characteristics*

#### The Iowa State Study

This was the first carefully reported, long-term longitudinal follow-up of a group of people whose intellectual functioning had been studied first in young adulthood. It is a good model for illustrating how a sound longitudinal study can develop from an existing data base collected long ago, provided that subject identifications are carefully maintained and prospective subjects belong to a population whose whereabouts will likely be followed for other reasons (in this case, as university alumni). Indeed, although it is important for ethical reasons to protect the confidentiality of subjects' records, it is scientifically unsound to destroy subject name rosters. Without such records, the important data yielded by the Iowa study would not have been obtained. Substantively, the initial reports from the Iowa study (Owens, 1953) were important in stimulating

TABLE 1.1. Chronology and Age Ranges of Studies

| Name of study | Year study began | Reported data collections | Last reported data point | Age range covered | Birth year of oldest cohort |
|---|---|---|---|---|---|
| 1. Iowa State Study | 1919 | 3 | 1961 | 19–61 | 1900 |
| 2. Aging Twins Study | 1946 | 5 | 1973 | 60–87 | 1886 |
| 3. Seattle Longitudinal Study | 1956 | 4 | 1977 | 25–88 | 1889 |
| 4. Duke Longitudinal Studies | | | | | |
| I | 1955 | 11 | 1976 | 59–102 | 1874 |
| II | 1968 | 4 | 1976 | 46–77 | 1899 |
| 5. Bonn Longitudinal Study on Aging | 1965 | 7 | 1976/1977 | 60–86 | 1890 |
| 6. Baltimore Longitudinal Study of Aging | 1958 | 3 | 1978 | 17–97 | 1881 |
| 7. AT&T Longitudinal Studies of Managers | 1956 | 3 | 1977 | 25–45 | 1931 |





a critical reexamination of the inevitability of intellectual decline in adulthood. But other methodological advances introduced by Cunningham (see Chapter 2) are also of interest. The reader will find a good example of how a retrospective study can be strengthened by collecting a variety of additional data in matched samples, which permit more fine-grained exploration of possibly inconsistent findings. Substantively, Cunningham concludes that the Iowa data argue for peaks in performance in intellectual ability (at least in college men) to occur during the 40s and 50s, with decline thereafter not reaching practical significance until the 60s.

## The Aging Twins Study

In 1946 Franz Kallmann and Gerhard Sander became interested in the study of the hereditary aspects of aging and longevity. Both monozygotic and dizygotic twin pairs who had reached age 60 were included in this study, survivors of which were last examined in 1973. This study is of particular interest because it permitted at least limited assessment of the interaction between genetic and environmental factors. Although heavily emphasizing biochemical data, the study also included carefully collected psychometric data, which are featured here.

In the highly selected sample of survivors, cognitive functioning was maintained, at least on a nonspeeded test, until age 75. Psychological test scores and survival were positively associated, and women outscored men on most tests. Of particular interest are findings suggesting that hereditary factors are important in some of the Wechsler Adult Intelligence Scale (WAIS) tests and intriguingly, that, in women, chromosome loss in old age appears to be related to poorer psychological test performance.

## The Seattle Longitudinal Study

This study, begun in 1956, with the latest data collected in 1977, was limited primarily to tracking five of Thurstone's Primary Mental Abilities (PMAs) as well as some personality characteristics across the adult life span. It was during the course of this study that some of the formal relations between cross-sectional and longitudinal data became clear and were formalized in what has come to be known as "sequential methodology" (cf. Schaie, 1965, 1977). The study includes four cross-sectional waves and several longitudinal studies extending from 7 to 21 years.

Substantively, it was found that several of the abilities increase into midlife; show statistically reliable, but small-magnitude, age changes in the late 50s; and increasingly decline once the 60s are reached, although the decline does not reach substantial magnitude until the late 70s and early 80s. Patterns of substantial decrement differ across the abilities, with so-called fluid abilities beginning to decline earlier, but crystallized abilities declining more precipitously in advanced old age. Intriguing relationships are also reported among health, life-style, and personality factors in midlife, predicting maintenance or decline of intellectual ability in advanced age.

### The Duke Longitudinal Studies

Two broadly multidisciplinary studies of normal aging were begun in 1955 and 1968, respectively, and were continued until 1976. Many aspects of the Duke studies have been reported elsewhere (e.g., Palmore, 1970, 1974). The chapter in this volume focuses on the psychological aspects. In the first study, these included measures of intelligence, memory, reaction time (RT), and sensory functioning; in the second study, measures of psychological well-being and complex psychomotor tasks were added. Of particular interest is that, at least in the first study, there were as many as 11 data collections, and consequently much was learned about interindividual patterns of change. Because of the multidisciplinary nature of the study, attention could also be focused upon the relationship between psychological variables and health factors. As in the Seattle Longitudinal Study, the findings suggest a complex pattern of interaction between cardiovascular disease (CVD) and the maintenance of intellectual competence.

As compared with other findings reported in this volume, substantive findings from the Duke study indicate somewhat later encounter with substantial intellectual decrement. In healthy individuals, performance-scale decrements do not occur until the 70s, and for the verbal scales not until the late 70s. Remarkable stability of personality patterns was seen, and the conclusion was reached that, for many psychological variables, sex is a much more important individual difference than is age.

### The Bonn Longitudinal Study on Aging

In contrast to most American studies, the Bonn study, conducted from 1965 to 1977, emphasized from the very beginning the phenomenological aspects of aging. That is, it was greatly concerned about chronicling the individual's perception of his or her own aging. Nevertheless, the study also included substantial objective measurements from intelligence and personality tests, as well as a broad sweep of motivational, biological, and biographical indexes. The particular focus of the Bonn study was on



defining differential patterns of constancy and change beyond age 60. Some of this material has been previously reported in segmented form (Thomae, 1976). The chapter in this volume provides an integrative overview of the entire study and its implications.

Perhaps the most important contribution of this study is its thorough coverage of topics often conspicuously absent in longitudinal inquiry. Personality data were collected by questionnaire as well as by observational techniques; other topics discussed include social participation, leisure-time activities, perceived life space, perceptions of self, and reactions to life stress, health problems, and family stress. Substantive results are too complex to summarize here. They strongly point, however, to the wide variety of adaptive patterns and to the dangers of premature identification of presumed normative models of aging.

### The Baltimore Longitudinal Study of Aging

This study is the longitudinal study on normative aging conducted over the past two decades by the intramural research program associated with the National Institute on Aging. The study originally focused on biological aspects of aging, with a variety of psychological variables successively being added. The chapter in this volume focuses more narrowly on the personality-change data acquired during that study, but seeks to set that material within the context of a broad discussion of the research issues involved in the study of personality change and aging.

The chapter presents an application of both traditional and sequential methodology to personality data. Substantively, persuasive evidence is provided supporting stability of adult personality as expressed by mean levels of dispositions, age-invariant personality structure, and consistency, over time, of individual differences. This evidence is bolstered further by a detailed analysis of the effects of response bias as a possible source of spurious stability across time. In addition, the chapter includes important discussion on the effects of mood states, predicting across periods of the life span, and identifying cause and effect in psychosomatic research.

### The AT&T Longitudinal Studies of Managers

The final chapter in this volume has a more applied bent. It is concerned with the longitudinal follow-up of industrial managers who were carefully studied in young adulthood as part of the selection process for entry managerial positions. A 20-year study follows the young managers into midlife and is supplemented by comparisons with the initial assessments of a new cohort of managers recruited 20 years after the initial cohort.

Longitudinal findings are presented on changes in abilities, attitudes, life interests, motivation, and personality as well as the relationships of these variables to career and personal success and happiness.

Of substantive interest in this chapter are the findings suggesting that intial emotional adjustment was an important predictor of career success. Although most members of the study tended to lower their expectations and become less positive about their careers, the emotionally healthy men changed less in that direction, as well as increasing their motivation to lead and direct others. Data on generational differences are also of considerable importance, suggesting much greater heterogeneity in the new generation of managers.

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