R. Corsini (Ed.), *Encyclopedia of psychology* New York: Wiley

The Rotter Incomplete Sentences Blank (ISB) is the most rigorously standardized, and so is described here at considerable length. Designed for the specific purpose of assessing the personality adjustment of college students, it contains 40 short stems, mostly in the first person. A manual by J. B. Rotter and J. E. Rafferty (1950) provides scoring instructions, sample responses, and normative data on college students. The scoring consists of classifying the responses in three categories—conflict, neutral, and positive—and assigning them weighted scores. For example, to the stem "My mother ______________," the response "hates me" is given a high score on *conflict* whereas the response "is wonderful" rates a high score on *positive*. The scores on the 40 stems are summed to obtain an overall adjustment total. Interscorer reliability is high.

Interpretation of the ISB is based upon these total scores obtained for the content analysis. A cutting score of 135 correctly identified over 68% of the maladjusted students and over 80% of the well-adjusted students in a study by Rotter et al. (1949). Although impressionistic or objective content analysis is the typical method of interpretation, formal analyses of the length of completions, use of personal pronouns, verb–adjective ratios, and so forth are sometimes used.

P. A. Goldberg (1965) has summarized the results of 50 studies of validity using 26 different forms of the sentence completion test, including 15 validity studies of the ISB. Of the latter, a clear majority showed significant relationships to the validation criteria, which consisted of case histories, interviews, adjustment ratings, and the presence of psychiatric complaints. The best validities occurred when standardized scoring systems such as Rotter's were used. The most consistent success has been in assessing the psychological adjustment of adults and the severity of psychiatric disturbance. The success of the ISB can be attributed to its single-minded purpose. It was planned and scored to measure the adjustment of college students and was validated by behavioral data related to adjustment in college.

There has been considerable research on the many variables that influence the nature of sentence completions. Goldberg provides an extensive review of this research on such variables as the effects of instructions and sets, variations of the sentence stem and stem structure, and type of person reference.

There is little agreement on the important issue of what level of the personality is revealed by the subject's sentence completions. Do they reveal conscious, preconscious, or unconscious aspects of the psyche? Goldberg also reviews this issue at length and observes that the lack of agreement may be related to differences in the instructional set given the subject by the various forms of the test. Some clinicians and forms (e.g., Sachs & Levy, 1950; Stein, 1947; Forer, 1950) stress speed and immediacy of responding while others (e.g., Holsopple & Miale, 1954; Rotter & Rafferty, 1950) allow freedom from time pressure. The latter consider sentence completion to represent a conscious level of responding controlled by the subject, whereas the former assume the inclusion of deeper levels of responding. This observation is supported by the research of E. Siipola (1968). She found a direct relation between the amount of ego-alien content elicited by a sentence completion test and the amount of time pressure imposed. Despite the lack of agreement on the exact level of awareness at which the sentence completion test should be positioned, most clinicians agree that the bulk of the material elicited is closer to conscious control than that obtained in the TAT and Rorschach.

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CLINICAL ASSESSMENT
INCOMPLETE SENTENCES
PROJECTIVE TECHNIQUES

E. M. Siipola

SEQUENTIAL METHODS

Sequential methods are quasi-experimental research designs (Campbell & Stanley, 1967) applied to problems in developmental psychology and other developmental sciences designed to deal with the confounds implicit in the study of phenomena that involve the dimensions of chronological age, time, and generations (cohorts).

TRADITIONAL DESIGNS IN DEVELOPMENTAL PSYCHOLOGY

Traditional research on the age variable in developmental psychology has employed either cross-sectional or longitudinal approaches. Both are fraught with internal validity problems (cf. Campbell & Stanley, 1967) that limit inferences that can be drawn from studies employing such designs. When individuals are compared with themselves or with others at different developmental stages, three parameters must be considered: the chronological age (A) of the individual when observed, the birth cohort (C) of that individual (date of entry into the environment, and the time (T) of measurement when the individual is observed (Schaie, 1965). The traditional developmental research designs confound these parameters.

The cross-sectional (age-comparative) approach draws samples of individuals at different ages and compares their performance on a given dependent variable, assuming that samples are comparable in prior life history and all other variables except age. Groups of individuals measured at the same point in time but differing in age must by definition be born at different points in time also. Hence, cross-sectional studies confound the influences of chronological age and cohort. When comparisons are made over a wide age range, it has been argued that the resulting comparisons are most likely to reflect cohort (generational) variation rather than influences of chronological age (Schaie, 1973, 1977, 1986). Cross-sectional studies are, however, appropriate to define age differences at a particular historical time for purposes of immediate policy decisions, and studies of limited age ranges in children or the very old may provide a first estimate of age change when cohort effects are unlikely to prevail.

The purpose of the classic longitudinal design, by contrast, is to study development within the same individuals. As such, the design explicitly represents a time series, with an initial pretest, a subsequent intervention (those maturational events that occur over time), and a posttest, that is applied to 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. The longitudinal design has usually been applied to a single group of individuals of relatively homogeneous chronological age when first tested and thus to a single birth cohort.

Several of the internal validity threats enumerated by Campbell and Stanley (1967; also see Schaie, 1973, 1977, 1988a) may be plausible alternative explanations for the observed behavioral change (or lack thereof) attributed to age in studies employing the traditional designs. In a single-cohort longitudinal study, time-of-measurement (period) and aging effects must be confounded, and the presence of period effects in the dependent variable 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.

The single-cohort longitudinal design does not directly control for, or allow assessment of the magnitude of, other internal validity threats. For example, pains are generally taken to eliminate the confound of instrumentation by taking steps to ensure that the measurement procedures remain as consistent as possible throughout the course of a study. Statistical regression effects are minimized at times by including at least two, and often more, retest occasions. Nevertheless, unless collateral control samples are employed for this very purpose, single-cohort longitudinal studies cannot circumvent the confounds of testing and experimental mortality. Single-cohort longitudinal studies were necessary and appropriate in the early stages of the developmental sciences. 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 cohorts and measurement occasions. Single-cohort studies may also be useful in applications such as defining typologies of developmental patterns in a specifically targeted population.

SEQUENTIAL STRATEGIES

To reduce the limitations inherent in both the cross-sectional and the single-cohort longitudinal designs, several alternative sequential strategies have been suggested (Baltes, 1968; Schaie, 1965, 1973, 1977, 1988a). The term *sequential* implies that the required sampling frame involves a sequence of samples observed across several measurement occasions (periods). Sequential strategies can best be understood by differentiating between sampling design and analysis design (Schaie & Baltes, 1975), although both are closely related. Sampling design refers to the cells of a cohort-by-age matrix that are to be sampled in a particular developmental study. Analysis design refers to the manner in which the cells that have been sampled may be organized to analyze for the effects of age, cohort, and time of measurement. Figure 1 shows a typical cohort-by-age matrix, indicating all of the sequential designs and also illustrates the confounding of the three parameters of interest. *A* and *C* appear as the rows and columns of the matrix, whereas *T* is the parameter contained within the matrix cells. There has been a debate on whether and how these effects should be unconfounded (cf. Adam, 1978; Buss, 1979/80, Horn & McArdle, 1980; Mason, Mason, Winsborough, & Poole, 1973; Schaie, 1986; Schaie & Hertzog, 1982). The issues involved are quite complex, highly technical, and beyond the scope of this introductory treatment.

SAMPLING DESIGNS

Two types of sequential sampling designs may be distinguished: those observing a panel of individuals repeatedly to fill the cells of the matrix and those using independent samples of individuals (each observed only once) drawn from the same cohorts to do so. The matrix shown in Figure 1 could have been produced by either approach. Employing Baltes's (1968) terminology, it is possible to denote the two designs as longitudinal and cross-sectional sequences, respectively. A cross-sectional sequence implies the replication of a cross-sectional study, the same age range being assessed for at least two different time periods, obtaining the estimate for each age level across multiple cohorts, where each sample is measured only once. A longitudinal sequence, by contrast, 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 change within groups individuals as well as individual differences in such change. An example of a data set containing both cross-sectional and longitudinal sequences may be found in Schaie and Hertzog (1983).

ANALYSIS DESIGNS

Data collected according to schemes such as those indicated in Figure 1 permit a variety of alternative analysis strategies (Schaie, 1965, 1977, 1986). 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, unfortunately, do not permit complete disentanglement of all components of the $B = f(A, C, T)$ function due to the obvious linear dependency of the three factors. Nevertheless, it may be suggested that, given the model, there exist three distinct analysis designs are available by considering the separate effect of any two of the components while assuming the constancy or irrelevance of the third on theoretical or empirical grounds.

Given the minimum designs displayed in Figure 1, Schaie suggests that the cohort-sequential strategy will permit separation of age changes

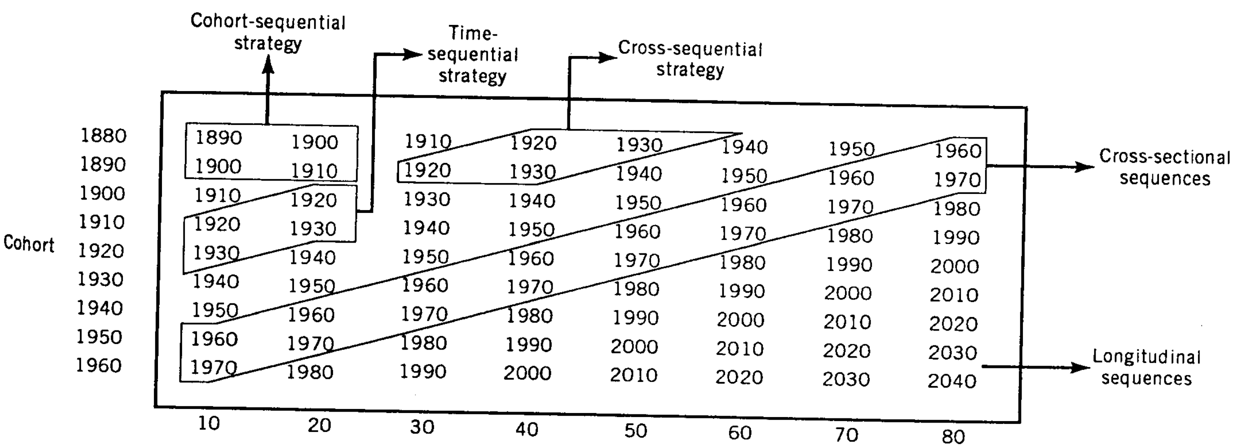

Figure 1. Schema of cross-sectional and longitudinal sequences and possible analytic designs derived from the general developmental model (Schaie, 1965). Entries in this schema represent times of measurement (from Schaie, 1983).

from cohort differences, under the assumption of trivial time-of-measurement effects. The time-sequential strategy will permit the separation of age differences from period differences, assuming only trivial cohort effects. Finally, the cross-sequential strategy will permit separation of cohort differences from period differences. The time-sequential strategy, of course, does not allow a repeated measurement 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 also is an appropriate strategy for estimating time-of-measurement (period) effects in studies covering a wide range of age/cohort levels.

Each of the sequential analysis strategies can be expanded further to control for the effects of experimental mortality (attrition) and for testing or experimentation (cf. Schaie, 1988a). One additional data collection will be required for each additional parameter to be controlled for (Schaie, 1977, 1988a).

When data are collected in the form of longitudinal sequences to examine intraindividual differences in change over time, it is possible to apply both cohort-sequential and cross-sequential strategies for data analysis. Developmental psychologists are likely to find the cohort-sequential design of greater interest because it explicitly differentiates intraindividual change within cohorts from average individual differences between cohorts (cf. Baltes & Nesselroade, 1979; Schaie, 1990b; Willis, 1989). 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.

A critical assumption of the cohort-sequential design is that one postulates the absence of time-of-measurement effects contained in the data. This assumption may be parsimonious for many psychological variables, but others may still be affected by "true" period effects or the confounds presented by occasion-specific internal validity threats of differences in instrumentation or experimenter behavior across test occasions. The question arises of how violations of the assumptions of no time-of-measurement (*T*) effects would be reflected in a cohort-sequential analysis. Logical analysis suggests that all estimated effects will be perturbed, albeit the most direct evidence would be shown by a significant *C* by *A* interaction (cf. Schaie, 1973). However, lack of a significant interaction does not guarantee the corresponding absence of *T* effects. Such effects could be localized in a small subset of occasions in extensive studies, in which case all effect estimates would be biased.

The essential consequence of the interpretational indeterminacy in sequential analysis is that all effect estimates will be inaccurate to some degree if the assumptions that justify the specific design are violated. The interpretational problem may be reduced, however, in estimating the relative likelihood of confounded *T* effects, given a strong theory about the nature and direction of estimated and confounded effects. The practical application of strong theory to sequential designs requires specification of confounds in an invalid design to obtain estimates of these confounded effects.

An important example of a planned violation of assumptions is the use of the cross-sequential design under the assumption of no *A* effects, an assumption that most developmental psychologists might consider quite unreasonable. Such a design is useful, however, when longitudinal data are available for only a limited number of measurement occasions for a wide range of cohort (age) groups. The cross-sequential design can be implemented after only two measurement occasions, while a cohort-sequential design requires at minimum of three. In addition, the number of measurement occasions that are required to estimate cohort-sequential designs that span a wide age and/or cohort range would be prohibitive if one were to insist that no data analyses should be performed until the entire cohort-sequential design appropriate for the research question being investigated had been accomplished. If a strong developmental theory about the nature of the confounded age effects is available, then a misspecified cross-sequential design can yield useful information about the significance of the age components represented in both the time-of-measurement main effect and the $C \times T$ interaction. Shaie's early work on the sequential analysis of intelligence began with such misspecification in cross-sequential design to permit drawing preliminary inferences concerning the relative importance of cohort and age effects before the availability of data would permit the direct and 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 as a temporary expediant or because of the nature of the phenomenon that is being studied. The preferred design to be employed in a given study must, therefore, be guided both by the substantive literature as well as the investigator's theoretical assumptions.

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COHORT DIFFERENCES
CONTAMINATION (STATISTICAL)
DOUBLE-BLIND RESEARCH
EXPERIMENTAL DESIGNS
LIFE EVENTS
LONGITUDINAL STUDIES
RESEARCH METHODOLOGY

K. W. Schaie

SET

The term *set* is used in a number of different contexts and problem areas in psychology—in areas as diverse as motor behavior, perception, and problem solving. There are many variants and synonyms of the term, although each tends to convey a specific theoretical orientation or to specify a particular application. Typical variants include the German word *Einstellung,* as well as more common and descriptive terms *readiness, determining tendency,* and *propensity.* A more problem-oriented variant sometimes encountered is *functional fixity.* The common kernel in each of these labels is the sense of some form of behavioral predisposition, inclination, or bias on the part of the individual that makes it more likely that a particular type of response will emerge in a given situation.

Before discussing how the concept of set is applied to complex cognitive problems, consider a simple example that illustrates the sense in which the term is used. In most athletic races, the official prepares the runners prior to the starting gun by calling out "Ready," and then, a few moments later, "Set." Following this second command, the athletes assume their starting positions. At this point, it is obvious, even to the casual observer, that the competitors are primed for only one class of activity—running. This is visible from observation of the runners' postures, of course, but also may become more graphically clear if some stray stimulus, such as a shout or a loud grunt, accidently arises. At