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Selective Attrition Effects in a Fourteen-Year Study of Adult Intelligence[1]

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Selective attrition effects in subjects born between 1885 and 1935 (age from 21 to 75 years of age at first test) are examined in a sequential design involving the components of cohort, occasion, retest-participation, and sex, 490 male and female subjects tested in 1956 on ten cognitive variables were classified into retest-participants and nonparticipants on the basis of a follow-up in 1963. An additional 867 subjects first tested in 1963 are also grouped into those who were available for retesting in 1970 and those who dropped out. Strong effects of participation were obtained on all measurement variables, with retest-participants scoring higher than dropouts on all variables. Indications of cohort-related and age-related differences in participating behavior were also found. These results further substantiate the contention that cohort differences, and age and cohort-related differences in volunteering behavior, seriously jeopardize the validity of conventional longitudinal and cross-sectional studies.

TRADITIONAL interpretations of intellectual development in adulthood and old age have recently been challenged seriously when investigators turned their attention to the development of methodologies suitable for the analysis of ontogenetic change phenomena. As Baltes (1968) and Schaie (1965) have pointed out, most traditional descriptive developmental research suffers from a severe lack of appropriate controls designed to assure internal and external validity (Campbell & Stanley, 1963) of the resulting data. Thus, a number of recent studies have demonstrated that phenomena such as generational change (e.g., Baltes & Nesselroade, 1973; Nesselroade, Schaie, & Baltes, 1972; Schaie, 1970; Schaie & Labouvie, 1972; Schaie & Strother, 1968a, b), volunteering behavior (e.g., Baltes, Schaie, & Nardi, 1971; Britton & Britton, 1969; Riegel, Riegel, & Meyer, 1967; Rose, 1965; Rosenthal & Rosnow, 1969; Streib, 1966) and selective survival (e.g., Berkowitz, 1965; Britton & Britton, 1969; Davies, 1954; Riegel & Riegel, 1972; Riegel et al. 1967) call for a reinterpretation of most well-known age-performance functions for the intellectual abilities (e.g., Baltes & Labouvie, 1973; Schaie, 1970).

The present study examines the effect of selective sampling and, in particular, subject attrition upon age-related intellectual patterns in a large-scale longitudinal study. Subject loss, or experimental mortality (Campbell & Stanley, 1963) becomes an important factor in restricting the generalizability of research in cases where such loss appears to be correlated with the behavior under study. Indeed, an impressive array of investigations comparing volunteering subjects with nonparticipants or retest-resisters shows that adult subjects who continue their cooperation in research are selected with regard to such factors as interests and attitudes (e.g., Riegel et al., 1967; Streib, 1966), social and educational level (e.g., Britton & Britton, 1969; Rose, 1965; Streib, 1966), and intelligence (Baltes et al., 1971; Riegel et al. 1967). Participating subjects, therefore,

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appear to comprise a subpopulation that is positively selected in terms of intellectual status and may be quite unrepresentative of the total population.

Moreover, experimental mortality appears to operate in a similar direction regardless of whether subject loss is due to such psychosocial reasons as refusal, lack of interest, and mobility on the one hand, or terminal illness and/or death on the other. A number of studies recently have compared surviving and nonsurviving subjects and reported a positive correlation between length of life-span and a number of cognitive variables (e.g., Berkowitz, 1965; Britton & Britton, 1969; Davies, 1954; Riegel & Riegel, 1972; Riegel et al., 1967). Since mortality itself is correlated with age, there is therefore reason to assume that positive selection factors may be of particular importance in the later parts of the life-span.

Attrition effects do not necessarily become invalidating for a given research effort. To the extent that subject loss is comparable in all age groups considered, the resulting problem is merely one of external validity, or generalizability of a positively biased sample to the population at large. However, the problem is of a more serious nature in cases where there is an interaction between subject loss and age. In this instance, age gradients may indicate differential subject attrition rather than ontogenetic change patterns, thus jeopardizing the internal as well as the external validity of research.

Few studies have heretofore addressed themselves to an analysis of experimental mortality effects over a wide age range, thus precluding convincing evidence concerning participation by age interactions (see, however, Baltes et al., 1971). The present study, in contrast, is geared toward examining the effect of subject loss including samples from all parts of the adult life-span.

In addition, the present design incorporates another feature that has thus far been neglected. In the light of recent evidence demonstrating the overriding importance of cultural/generational change components on modifying ontogenetic patterns, it is further desirable to attempt a separation of age and cohort (generation)-related attrition effects. As Baltes (1968) and Schaie (1965) have argued, the separation of age-related and generational components of change requires the application of sequential designs consisting in the testing of multiple cohorts at multiple measurement occasions (age levels). Since the present study covers three measurement occasions, it is then possible to study the effects of attrition within a sequential design involving cohorts, measurement occasions, and retest-participation.

Method

Subjects

The present study is part of a large-scale 14-year study on adult intellectual development. Detailed descriptions of the sampling procedure have been reported previously (Schaie, 1958; Schaie & Labouvie, 1972; Schaie & Strother, 1968a). To summarize, subjects were drawn at three measurement occasions (1956, 1963, 1970) from the population of approximately 18,000 members of a prepaid medical plan. In 1956, quotas of 25 males and 25 females were drawn for each 5-year interval from 21 to 70 years of age. These quotas subsequently were reorganized into 7-year intervals with mean birthyears (cohorts) ranging from 1889 to 1931. In 1963, the residual 1956 sample was retested, and a new stratified sample was drawn including all of the original cohort as well as the next younger one (mean birthyear: 1938). This procedure was again repeated in 1970. Thus, the second follow-up involved the retesting of the residual 1956 and 1963 samples, as well as the testing of a new independent sample now involving nine cohorts (1889 to 1945).

For the purpose of the present study, subjects from the independent 1956 and 1963 samples were classified as to whether or not they had participated in the retesting after 7 years (i.e., in 1963 for the 1956 sample and in 1970 for the 1963 sample). Only subjects from the seven original cohorts (1889 to 1931) are included in the present design. In specific, there are 490 subjects in the 1956 sample, 300 of which could be retested after a 7-year interval. The 1963 sample covers 867 subjects. Of these, 375 could be retested in 1970. Table 1 gives an overview of the participants and nonparticipants by birthyear, sex, and time of measurement.

Measurement Variables

Ten dependent measures are included in the present study. Five of these are the factors of Thurstone and Thurstone's (1949) Primary

Mental Abilities (PMA) test: Verbal Meaning (V), Space (S), Reasoning (R), Number (N), and Word Fluency (W). Two further PMA variables are the composite scales of Intellectual Ability (IQ=V+S+2R+2N+W) and Educational Aptitude (EQ=2V+R). The remaining three measurement variables are the factor scales of Schaie's (1960) Test of Behavioral Rigidity (TBR). In specific, the factors are: Motor-Cognitive Rigidity, Personality-Perceptual Rigidity, and Psychomotor Speed.

Design and Data Analysis

Two alternative ways of classifying subjects are possible within the present context: a breakdown by age or a breakdown by cohorts. The present study utilizes a breakdown by cohorts since it has been demonstrated that cohort membership, rather than chronological age, is the most important determiner of an adult individual's intellectual level (e.g., Nesselroade et al., 1972; Schaie & Labouvie, 1972; Schaie & Strother, 1968a, b). Thus, the 1956 and the 1963 sample were divided into seven cohorts, and each cohort was further subdivided into subjects who had participated or not 7 years later.

To permit cross-scale comparisons, raw scores were converted to T-scores (mean: 50, standard deviation: 10), using as reference the first-occasion data for all three times of measurement. Subsequently, separate 2 (Participation vs. Nonparticipation) × 7 (Cohorts) × 2 (Time of Measurement: 1956, 1963) × 2 (Sex) analyses of variance were computed for each of the ten measurement variables.

Table 1. Distribution of Retest-Participants and Retest-Resistors.

Mean Birthyear	1956 Participants Males	1956 Participants Females	1956 Non-participants Males	1956 Non-participants Females	1963 Participants Males	1963 Participants Females	1963 Non-participants Males	1963 Non-participants Females
1889	24	23	14	15	8	6	28	32
1896	13	25	21	11	18	23	44	38
1903	21	23	13	12	20	19	37	40
1910	22	18	13	11	28	44	30	35
1917	23	24	13	11	36	37	42	38
1924	19	25	12	12	34	43	34	35
1931	19	21	17	15	21	38	29	30
All Subjects	141	159	103	87	165	210	244	248

Results

Summary results of the analyses of variance on the ten dependent measures are presented in Table 2. Only the main effects of Participation, Cohort, Time of Measurement, and Sex, as well as the interaction effects of Cohort × Participation, Time of Measurement × Participation, and Cohort × Participation × Time of Measurement are listed, since none of the remaining effects yielded significant results (the only exception being a four-way interaction of Cohort × Participation × Time of Measurement × Sex for Psychomotor Speed, $p < .05$).

Although the major concern of the present study is the examination of effects involving Participation, it is instructive first to discuss Cohort effects. There are significant Cohort main effects for all of the ten measurement variables ($p < .01$). As Figures 1 and 2 show, most of the variables yield an almost linear drop from the youngest to the oldest cohort level. Only on Number and Word Fluency is this trend considerably less marked, with

Table 2. Summary of Analyses of Variance: Significant F Ratios.

Variable	Participation (P)[a]	Cohort (C)	Time (T)	CXP	PXT	CXT	CXPXT	Sex
Verbal Meaning	135.4**	56.1**		3.7**	4.3*			8.5**
Space	57.2**	56.9**						120.2**
Reasoning	121.5**	96.0**				2.2*		6.4*
Number	19.4**	16.6**		4.4**		3.4**		15.2**
Word Fluency	63.5**	17.7**	52.2**	2.8*		3.0**	3.1**	12.4**
Intellectual Ability	112.0**	70.0**		4.7**		3.0**		
Educational Aptitude	152.8**	73.5**		3.5**	5.6*			9.3**
Motor-Cognitive Rigidity	59.6**	58.3**	12.8**		4.3*			
Personality-Perceptual Rigidity	40.8**	32.5**	3.9*		10.9**			
Psychomotor Speed[b]	85.8**	39.5**	48.7**					63.2**

[a] Source. The degrees of freedom associated with effects involving Cohorts are 6, 1301; all others are 1, 1301. Only effects for which significant F ratios are obtained are included in Table.

[b] On Psychomotor Speed, in addition, there is a significant Participation X Cohort X Time X Sex interaction ($p<.05$).

*Significant at or beyond the 5% level of confidence. **Significant at or beyond the 1% level of confidence.

peaks occurring as late as at the middle cohort level for Number, and at the third oldest cohort level for Word Fluency.

Since cohort level covaries with age, these differences traditionally would be interpreted as evidence for age-related variations. In fact, however, a more valid index for age-related change is given by the Time of Measurement main effects, which in this design indicate within-cohort gains over a 7-year period. Only one of the PMA variables yields a significant Time of Measurement effect (Word Fluency, $p < .01$), while there are no 7-year gains on the remaining six. Thus, these data further substantiate the notion that cross-sectional age differences (as reflected in most age-performance functions) indicate generational differences rather than ontogenetic change for most intellectual abilities. This, however, is not true for the fluency dimensions of Word Fluency, Motor-Cognitive Rigidity ($p < .01$), Personality-Perceptual Rigidity ($p < .05$), and Psychomotor Speed ($p < .01$): On these dimensions, higher performance is obtained in 1956

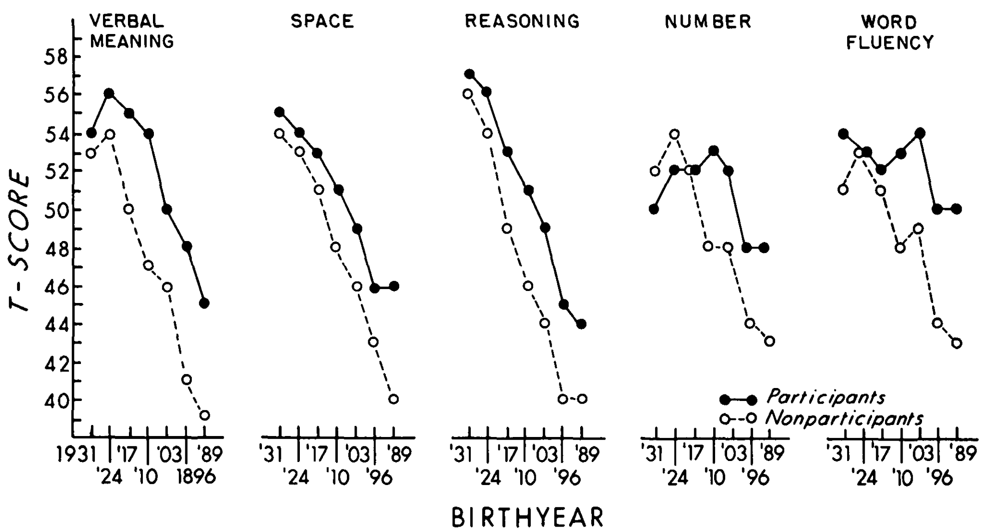

Fig. 1. Cohort differences on the primary mental abilities.

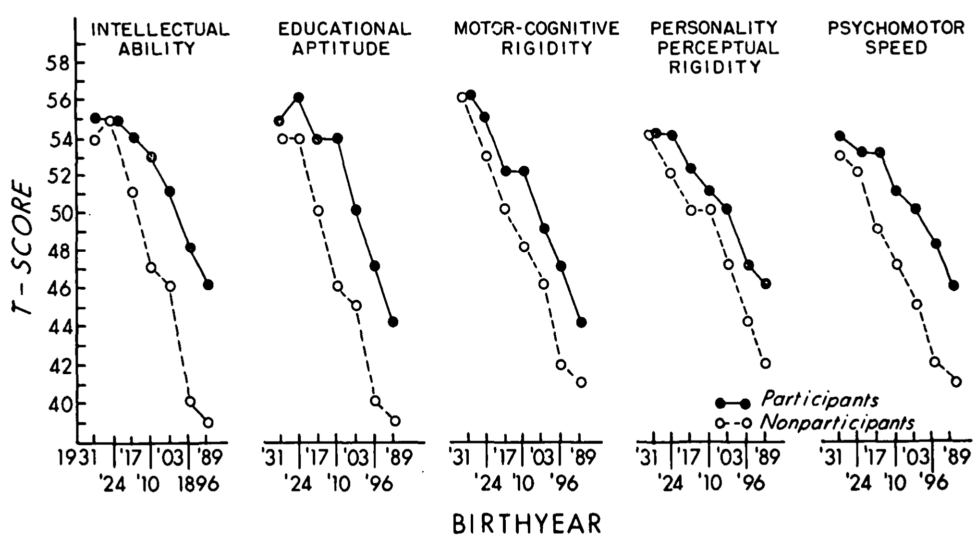

Fig. 2. Cohort differences on global indices of mental ability and on measures from the test of behavioral rigidity.

than in 1963. For four of the variables there are, moreover, significant Cohort by Time of Measurement effects (Reasoning: $p < .05$; Number, Word Fluency, and Intellectual Ability: $p < .01$). As would be expected, these interaction effects indicate that 7-year changes are largest in the oldest cohort levels.

Of major interest in the context of the present study are effects involving Participation. Significant main effects of Participation are obtained on all ten variables ($p < .01$). As Figure 1 shows, Participants consistently obtain higher scores on all variables; the only exception, in fact, is Number where the performance of Nonparticipants exceeds that of Participants in the two youngest cohort levels. It may be concluded, therefore, that those subjects who could be retested after 7 years are positively selected on cognitive variables in comparison to the total sample.

In general, the difference between Participants and Nonparticipants appears to be more pronounced in the older cohorts than in the younger ones, a trend already noted by Baltes et al. (1971). In the present study, significant Cohort by Participation effects are obtained for Verbal Meaning ($p < .01$), Number ($p < .01$), Word Fluency ($p < .05$), and the composite variables Intellectual Ability and Educational Aptitude ($p < .01$). The same trend is distinguishable for the remaining variables, as well. Thus, there is indication that participation effects are of differential import in different cohort/age groups.

Participation by Time of Measurement effects are, in general, not very pronounced. They too, however, are consistent. All of the ten variables exhibit a more marked difference between Participants and Nonparticipants in 1963 than in 1956. This interaction is statistically significant for four of the variables ($p < .05$ for Number, Educational Aptitude, and Motor-Cognitive Rigidity, $p < .01$ for Personality-Perceptual Rigidity). The same trend, however, is found for all of the measurement variables. Thus one may advance the interpretation that attrition effects are of a more severely biasing nature in 1963 than in 1956. For Word Fluency, morever, this trend is specific to selected cohort levels and indicated by a significant Cohort by Participation by Time of Measurement interaction ($p < .01$).

Considering, finally, the Sex differences obtained, it is found that females score higher than males on Verbal Meaning ($p < .01$), Reasoning ($p < .05$), Word Fluency ($p < .01$), Educational Aptitude ($p < .01$), and Psychomotor Speed ($p < .01$) and Space ($p < .01$). These Sex differences are consistent with those reported previously (e.g., Schaie & Labouvie, 1972; Schaie & Strother, 1968a, b). Note, however, that except for Psychomotor Speed, Sex does not interact with the variables of principal interest in this study.

Discussion

The present data further substantiate the importance of selective sampling effects in the study of adult cognitive behavior. In line with research previously reported, pronounced differences exist between subjects who continue their cooperation in a long-term project and those who drop out. Moreover, there appear to be cohort-related differences in the effects of attrition. In general, selectiveness appears to be pronounced in the middle-adult and old-age ranges in the present study. In the younger cohort levels, differences between participants and nonparticipants are less dramatic. There is also some indication that attrition becomes a more important factor in the older age groups, since larger differences between Participants and dropouts are obtained in 1963 where the mean age of the sample is 7 years higher than that in 1956. This might be expected due to an age-related increase in mortality.

What are some of the implications of the present data for the interpretation of conventional cross-sectional and longitudinal age-performance functions? At first glance, it might appear that selection effects such as those reported here are of most import in longitudinal research. Selective sampling appears to be of importance in such long-term projects even from the onset of a study, resulting in an initial bias of the samples (e.g., Baker, Sontag, & Nelson, 1958; Rose, 1965; Streib, 1966). With repeated testing, moreover, the sample will become progressively more biased and the external validity will be increasingly restricted. In fact, this high selectivity of longitudinal samples may be one factor in accounting for the finding that longitudinal data often suggest less age-related decline than do cross-sectional studies (cf. also Schaie, 1972a).

Thus, in longitudinal research, one of the major problems is the accounting of changes in the composition of the parent population as the study progresses, in order to determine to which part of the total birth cohort the resulting age-performance functions can be generalized. A cross-sectional study, in contrast, suffers from restrictions of a more severe nature. On the one hand, the existence of strong differences between cohorts on most cognitive variables clearly demonstrates that the various cohorts do not represent samples from the same parent population. Instead, people from different cohorts appear to differ so dramatically that the construction of cohort-specific age gradients is warranted. On the other hand, the cohort by participation and occasion (age) by participation interactions obtained in the present study suggest, moreover, that the composition of the original cohort populations represented in a study may differ from the outset and may be differentially affected by selection effects as each cohort grows older. Thus the triple confound of cohort differences and cohort and occasion-related differences in volunteering behavior renders any inference about ontogenetic change from a one-shot cross-sectional study extremely hazardous.

Such an outcome suggests, in fact, that it will be necessary to chart age gradients not only for separate cohorts, but also for different subgroups within cohorts (such as individuals who differ in the length of their life-spans, volunteering vs. nonvolunteering individuals, individuals who differ in cognitive and socioeconomic characteristics, etc.) (see Baltes et al., 1971). It is obvious, of course, that such an enterprise acquires truly formidable proportions; and although an internally valid description of ontogenetic change will require a more and more specific breakdown into gradients for particular subsamples, one may easily wonder whether such an endeavor is at all practicable.

It has been argued elsewhere (e.g., Baltes & Labouvie, 1972; Labouvie, 1973) that the motivation for a more and more refined description of intellectual change in specific subsamples appears to be rooted in the conviction that, once all invalidating components are controlled for, it will be possible to arrive at "true," "normal" age-performance functions of intelligence. At the same time, however, the necessity of constructing age gradients for more and more specific fractions of the total population leads to a state of indeterminacy where generalization and prediction become virtually impossible, and the very notion of normality leads to a paradox. These restrictions are, however, inherent in any purely descriptive effort where a construct—such as intelligence—is not well-defined in terms of its antecedents and process variables.[4]

One might argue, therefore, that the development of adequate descriptive methodologies needs to be supplemented by a concentration on process-oriented, analytic research aimed at isolating those environmental and experiential components that monitor the course of intellectual ontogeny. To the extent that a sample could be defined in terms of the relevant experiential parameters—which themselves need to be defined within a theoretical system—the problem of struggling with sampling effects and *a posteriori* considerations of generalizability would be greatly alleviated.

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[4] Some simplifying assumptions may, however, limit this dilemma somewhat. For more detailed discussion of such assumptions see Schaie (1972b).

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