

THE FATE OF A PANEL OVER A FIVE-YEAR PERIOD - A CRITICAL REVIEW

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The attempt by Wheeler** to analyze the sources of attrition in two samples of older adults represents an all too rare attention to methodological detail in social science research. The information presented can be useful in planning future longitudinal interview studies with older adults.

As Wheeler has pointed out, it is difficult to determine the limits of generalization from his first sample to other populations. In generalizing these findings it is important to note that the main sample is restricted to the middle socio-economic range of white noninstitutionalized relatively healthy urban persons. The second sample (referred to in the report as a quasi-sample) consists of persons aged 70 through 90 selected by interviewers from among neighbors, acquaintances, and "other sources", obviously not necessarily a random sample. Therefore, a more detailed description of the distribution of these samples and the characteristics of dropouts on such variables as education, occupation, and financial condition could help the reader determine the applicability of these findings in other situations which require the anticipation of sample attrition in the designing of a study.

Wheeler and his associates are to be commended for attempting to identify with some precision the various sources of attrition. Thus they held constant social class while examining dropouts by age and by sex.

Unfortunately the number of cases available often practically precluded statistically significant differences between categories under examination. Thus on page 6 one reads, "Out of 174 persons completing Wave I, eight--four males and four females--were lost through moves. The females, who moved were, however, concentrated in the lower-middle class, while males who moved were from both the upper-middle and the upper-lower, but not the lower-middle, classes." This conclusion is based on the data presented in Tables 1a and 1b which show that among the men 2 of the 4 dropouts by moves were in the upper-middle and 2 in the upper-lower social class, while among women all 4 dropped because of moves were in the lower-middle class.

Larger samples would most likely have reflected considerably different distributions in the contribution to dropping out from age, sex and social class. This conclusion is supported by the observed consistency in direction of differences on the independent variables.

Both the analysis and the report might have been strengthened through use of the directional hypothesis and one tailed rather than two tailed tests of significance. From the report I was not

able to determine that this more powerful approach had been utilized.

I should like to encourage the investigator to give further attention to the quality of the interviewers. My own computation of the data presented in the report indicates that of the dropouts, 83 percent of the women and 58 percent of the men consisted of refusals. Death accounted for only 6 percent of the women and 28 percent of the men dropping out, while moves accounted for 11 and 14 percent respectively. This, in combination with the statement, "nearly all of the previously mentioned 'pick ups' were moved from the refusal category to the completion category through the efforts of this interviewer (i.e., an especially competent highly trained interviewer)".

I should like to congratulate the investigator for his careful analysis and clear report. At the same time I would suggest that it might be fruitful to investigate further, differences in dropouts between the major sample and the quasi-sample report.

Dr. Kleemeier* addressed himself to a significant substantive question, namely: is decline in intellectual performance during the senium caused by "factors related to death" rather than by age per se? He concludes, quite confidently, that, contrary to traditional expectations, "the decrement in intellectual performance may be accounted for to a substantial degree by . . . terminal drop" leading to death, rather than by normal age change. Therefore, a pertinent question for the reviewer is: Does available data warrant this degree of confidence, or even the conclusion? To answer, it is well to examine the quality of the data and its treatment.

The paper first points out the origin of its hypothesis. An examination of three studies of the mid-fifties suggested "continued (intellectual) growth rather than decline in the years of middle adulthood". This prompted the question of "whether or not intellectual deterioration occurs" at all normally, which generated the hypothesis investigated.

Kleemeier is to be commended for adding to his own carefully analyzed data those from other published reports. With the latter, it is necessary to recognize the need for caution in using data gathered for another purpose to test a given hypothesis.

To what extent do these studies confirm the terminal drop hypothesis? In my opinion, the studies cited lend support to the hypothesis, but not as firm support as might be thought from a reading of the paper reviewed. For instance, the 1957 study by Jarvis and others, listed as "most pertinent" in Kleemeier's paper, provides support only via secondary inference. That is, Kleemeier

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interprets the poorer performance of the cross-sectional sample than that of the longitudinal as reflecting terminal drop deterioration. He assumes that a number, sufficient to lower test results in the cross-sectional sample, were approaching death. This may have been true, and his reasoning may well be sound; nevertheless, the conclusion rests on secondary inference. Furthermore, both the longitudinal and cross-sectional data showed some progressive decline through the seventh and eighth decades. The cited directly supportive finding of the Jarvis et al study, that original scores of survivors exceeded those who died before the retest, is statistically nonsignificant. The increase in intra-pair differences between twins, although fairly consistent, is also statistically nonsignificant.

Turn next to the evidence cited from drop in EEG. The argument as presented is that, since EEG and intelligence were associated for the Moosehaven group where 26 percent were convalescent, but not for an independently living sample, there is evidence to suggest an organic base for terminal decline. This, as well as the clinical data showing some association between arteriosclerosis, measured intelligence and mean alpha frequency, would best be used to guide the refinement of the terminal drop hypothesis for further study rather than as proof of its correctness.

In the reviewer's opinion, the available literature cited does allow the logic and premise posed in Kleemeier's paper, but it offers mainly tangential (though supportive) evidence for testing the hypothesis in question.

Now note the original data presented. Results on two groups of the Moosehaven Study subjects are reported. The first group consists of 13 men, who, for reasons not stated, had been tested four or more times during the preceding 12 years. What biasing selectivity, if any, may have operated, is not noted. The four who died during the interval reportedly showed statistically greater decline on Wechsler-Bellevue scores than did the rest. The statistical test used is reported, but it is not clear whether the changes over all four tests, or only those between the first two and last two tests, were used in determining statistical significance.

These observations, that is, those data presented on the screen, quite correctly are presented not as a test but as generating a dual hypothesis: that (1) "factors related to death

cause a decline in intellectual performance and (2) may be detected in some instances several years prior to . . . death." (No data on the latter.)

A second pair of groups consisting of 37 living and 33 deceased persons as of January 1, 1961, provided the data to test the above hypothesis. These 69 were apparently the total of those who had been tested at least twice. The differences in changes on the verbal performance and the scores of the Wechsler-Bellevue between these 37 survivors and the 33 deceased are the sole original data presented to test the hypothesis. The report states "of the three only the performance score showed a significant difference". After indicating effective control on age and institutionalization, it is concluded "that we have here strong evidence for the existence of a factor (terminal drop) . . . which adversely affects intellectual performance and is related to impending death of the aged person".

Since the concept, "terminal drop", is central to the entire paper, it might have been desirable to have the investigator's definition of the concept. How long a period does it involve? From statements on page seven it may be deduced that terminal drop covers less than eight years, while according to page five that it may be detected several years prior to death. Is it meant to identify a strictly functional deterioration or does the writer view the concept as having an organic and physiological referent?

To this reviewer the terminal drop hypothesis is hereby hardly verified. However, it remains an hypothesis worthy of further careful testing. Kleemeier has set forth the research model to be used. More adequate samples can be expected to confirm the terminal drop hypothesis, help determine at what point before death terminal drop may be detected, and to what extent and in what way it is associated with death and may help predict it.

If it is finally demonstrated that the traditionally claimed decline in intellectual performance during middle age and through senescence is incorrect, the practical implications are manifold. Retraining of displaced or redundant labor, or of those required to change jobs for reasons of health; periodic return to formal education by professionals and technicians, less emphasis on "new blood" in an organization, and reduced excuses for poor performance by healthy oldsters, are but a few examples.

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