Can the Longitudinal Method be Applied to Psychological Studies of Human Development?[1]

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Abstract

Careful analysis of the assumptions underlying the application of the conventional longitudinal method shows that it is inappropriate for the analysis of age functions on most psychological variables and that the findings of most single cohort studies of such variables are therefore unlikely to stand replication. This extreme statement is based on the fact that age level is not an independent demographic variable but depends upon the points in time when an organism enters the environment and when it is measured. Findings from longitudinal studies would therefore be generalizable only if development is solely attributable to maturation or where the environment is totally stable. The first assumption is reasonable only for physiological functions and the second assumption is likely to hold only in infrahuman studies. Neither assumption seems justified for studies of human development beyond the first few weeks of life. It is suggested also that the longitudinal method, where applicable, must suffer from the effects of repeated measurement upon the organisms studied, from regression effects wiping out sub-group differences and from the effects of experimental mortality.

Study of the same individual over time should, therefore, be confined to case analysis and hypothesis generation but should be avoided in hypothesis-testing studies. Generalizability of age functions must properly be examined by modified longitudinal studies in which successive samples from a pre-defined population cohort are measured only once upon each measurement occasion. Replication of such longitudinal studies permitting assessment of each age level for successive cohorts at different points in time will permit the necessary distinction between the effects of cultural change and of age-related change.

Studies of adult development, in particular, need to abandon exclusive preoccupation with age and instead concern themselves with inter-generation differences. Since age-related changes on psychological variables are likely to be washed out by environmental impact, it is proposed that the traditional longitudinal study could do no more than trace the history of such impact. A more salient understanding of the relationship between age and the state of the environment, however, can best be obtained by replicated cross-sectional studies using the proper sequential methods. Illustrative data from the sequential study of age and generation differences on measures of intelligence are discussed.

Introduction

It is the purpose of this paper to lay to rest once and for all the traditional longitudinal method as a scientifically respectable approach to the study of human development on psychological variables. I will be even more restrictive than the previous speaker (Dr. Hindley) and wish to advocate unequivocally that the single cohort longitudinal study be used for no other purpose than that of the historian, the case history reporter or at best as anecdotal material for the purpose of generating hypotheses. This does not necessarily mean that the cross-sectional method should be substituted in all cases, but rather that in those instances where Dr. Hindley postulates the longitudinal method to be essential, we will need to substitute research designs which involve the successive independent sampling from pre-determined population cohorts or designs involving the replication of longitudinal studies over more than one cohort. I will outline some of these alternatives later on, but first will briefly document the sweeping indictment of the traditional longitudinal method made in this paragraph.

Development and Change in the Environment

The fundamental reason why the traditional longitudinal method is inappropriate may be found in the incontrovertible fact that age level is not a functionally independent variable. Instead age is directly dependent upon the points in time at which an organism enters the environment and at which it is measured (Baltes, 1968; Schaie, 1965, 1967, 1970; Wohlwill, 1970). Findings from longitudinal studies with respect to performance at any given age level can, therefore, be taken as a generalizable age function only if the charted performance is the sole consequence of maturationally determined development or under the even stronger assumption of a totally stable environment. These assumptions are necessary because the difference in performance for an individual over two times of measurement, in addition to other confounds to be discussed later, will consist of a component related to developmental change, that is the change from one developmental level to the next, plus a component which is a treatment effect due to environmental impact uniquely determined by the status of the environment during the historical time period under study, which component may bear no relationship whatsoever to the developmental status of the organism. The assumption that age changes are synonimous with maturation alone may be reasonable for some physiological age functions and perhaps for selected psychological phenomena of early infancy. The assumption of total environmental stability would at best be tenable for psychological studies of human development during the first few weeks of life. As a consequence, any report of change in a longitudinal study will leave up in the air the question whether it was the individual or the environment which has changed, and a report of no change will not tell us whether the behavior remains stable or whether maturational changes and environmental impact have cancelled out one another.

The traditional longitudinal study cannot answer questions of interest to the developmental psychologist with respect to age functions because it confounds changes

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the major components of the unrecognized treatment effects probably results from changes in the attitudes of the subjects about themselves and the research procedures, as well as changes in the attitudes of the researcher himself towards his study which are likely to effect his outcome.

If the researcher then is serious in conducting developmental inquiries over time, he will be obliged to consider a mixed model which will permit the study of interactions between age level, experimental treatment effects and naturalistic treatment effects. Such a model, however, requires that all age levels be examined at more than one time (eliminating in most instances the simple cross-sectional approach (Schaie, 1971a) and that for each age level and time of measurement samples are examined under experimental and control conditions. In purely descriptive research, the investigator may be willing to assume that there has been no systematic intervention for which controls are required. But this does not take care of the fact that there may nevertheless be a confounding of the effects of measuring his variables and general changes in the environment. Indeed, for those variables where environmental impact is expected (and this would include most variables of interest to the psychologist) it will therefore be necessary to use the mixed models which I have called sequential designs (Baltes, 1968; Schaie, 1965) in order to be able to distinguish between age changes and the temporally unique generalized input from the environment.

Technical Flaws of the Longitudinal Method

For the reasons just explicated it follows that the results of a single repeated measurement study cannot be interpreted unless it can be assumed that there will be no socio-cultural change effects upon the measurement variables and unless it is known that the effect of one measurement upon the subsequent measurements will be zero. When these assumptions cannot be met it follows that the repeated measurement longitudinal research design does not represent a permissible developmental strategy. This prohibition, however, does not apply to the use of repeated measurements in the mixed model sequential methods. But there are other technical flaws which are sufficient to discourage use of repeated measurement strategies in developmental psychology, and to require rather elaborate designs under situations where a logical argument can be made that repeated studies of the same individuals must be conducted.

The technical problems to be discussed involve (1) experimental mortality, (2) misleading regression effects, (3) design complications in handling the effects of prior measurement, (4) lowered efficiency of repeated measurement designs, and (5) changes in factor structure over time in multivariate studies.

Experimental mortality. Perhaps the most serious limitation of the repeated measurement of the same individuals is the fact that there will inevitably be non-random attrition of subjects, or what Campbell and Stanley (1963) have termed experimental mortality. The effects of experimental mortality result in serious restrictions of the generalizability of research findings, and cannot readily be solved by limiting one's analysis to those members of the sample on whom data are available at all measurement points (cf. Schaie 1971a for a more complete discussion of this problem). Two types of experimental mortality may be distinguished. The first source of attrition involves psychological and/or sociological reasons which may, but need not be, related tothe researcher's skill in maintaining his sample. Examples of such attrition would be subjects' lack of interest, their active refusal to participate further, or their change of residence resulting in disappearance. The second kind of experimental mortality involves biological causes over which the researcher has virtually no control. Examples would be subject loss due to physical illness and individual differences in longevity.

The problem of experimental attrition in longitudinal studies is most serious since it is known that there are age-related differences in attrition which moreover interact differently with other population characteristics. For example, it is known that brighter subjects have greater longevity (Blum, Jarvik and Clark, 1970; Jarvik and Falek, 1963; Riegel, Riegel and Meyer, 1968), that attrition is lower for subjects from higher socio-economic and educational levels (Rose, 1965; Streib 1966), and that favorable selection on longitudinal study with respect to intellectual level is known to occur (Baltes, Schaie, and Nardi, 1971). As a consequence, the internal and external validity of longitudinal studies is seriously impaired whenever subject loss over time is more than minimal.

Misleading regression effects. Orthodox test theory would suggest the presence of strong regression effects in any study where the same subjects are measured repeatedly and there has been considerable controversy as to how change should be measured under such circumstances, or whether it should even be measured at all (Campbell and Stanley, 1963; Cronbach and Furby, 1970). The principal effect of regression follows from the assumption that there will be a positive correlation between the magnitude of the error of measurement and that of the observed score, while the error components within subjects over different occasions will be uncorrelated. The effect of regression will differ depending upon the nature of the sample. That is, due to regression effects, spurious drops in performance (or in children under-estimates of growth) will be reported for samples of high level subjects, while the reverse will be true for samples scoring low upon initial measurement.

The effects of regression may be tolerable for the estimation of population parameters, but will be fatal in studies contrasting ability or performance levels. There seems strong evidence, that regression effects may be responsible for the reported differential development of intelligence by ability level frequently reported in the literature (e.g., Miles and Miles, 1932; Owens, 1966; Riegel, Riegel and Meyer, 1967). By applying time reversal techniques suggested by Campbell and Stanley (1963), it has been shown that ability-level related differences in ontogenetic patterns of adult intelligence found in a cross-sequential repeated measurement study (Schaie and Strother, 1968) were actually a function of statistical regression effects (Baltes, Nesselroade, Schaie and Labouvie, 1971). These regression effects, as suggested by Lord (1963), turn out to be greatest for the least reliable measures.

Unless the investigator employs time-reversal or similar techniques he will have no way of knowing whether changes reported from longitudinal repeated measurement data represent actual changes in true scores or whether they represent a simple statistical artifact.

Design complications in handling the effects of prior measurement. It is possible to specify an extension of the traditional longitudinal method which permits unconfounding developmental change from temporally unique environmental input. This extension called the cohort-sequential method (Schaie, 1965) consists of following two or more successive cohorts drawn from the same population over equivalent age ranges, in which case age changes can be differentiated from differences between cohorts. Alternately an approach called the cross-sequential method, in which two or more successive cohorts are followed over equivalent time periods permits differentiation of generation differences from nonspecific environmental input characteristic for a particular point in time. The first approach requires the assumption that there are no differences in unique temporal input (too strong for psychological variables) while the second requires the assumption of the absence of maturational change (strong for early development, but acceptable for many variables over the adult life span). A third method, the time-sequential, under the assumption of no generation differences, can separate temporally unique input from maturational variance, but is not feasible for repeated measurement data since it requires data on all ages at all times of measurement.[2]

The sequential strategies separate out the components of developmental change but like the traditional longitudinal method also confound the effects of prior measurement. Only under the additional assumption of zero age changes could the effect of prior measurement be estimated in the cohort-sequential approach, or with the assumption of zero environmental change in the cross-sequential method. If these assumptions do not hold then prior measurement effects will have to be confounded with either the age or the environmental impact effects.

It is possible to obtain independent estimates of practice effects if the investigator is prepared to employ conjointly repeated measurement and independent random sampling designs. To do this it will be necessary to examine all levels for one of the sequential designs at two or more levels of practice. But this is not an inexpensive procedure since the minimal data needed to assess the effects of measurement will involve at least three successive test occasions for the cross-sequential and time-sequential and at least four test occasions for the cohort-sequential method. Sampling plans and analysis of variance designs for the identification of measurement effects have been presented elsewhere (cf. Schaie, 1971a).

Lowered efficiency of repeated measurement designs. Whether or not the intent of the investigator has been to follow individual subjects over time, the argument has been advanced frequently that variability can be significantly reduced by use of the matched group design, of which repeated measurement is an application, whenever experimental error is large and the changes to be detected are small. What has not always been recognized is the fact that this truism when extended to developmental study follows only when measures have high reliability or where it is possible to take measures either at many ages or many times of measurement. This must be so since the degrees of freedom available for the error term to test mean differences, given an equal number of observations, will always be less for replicated than for independently derived measurements. Consequently, the gain in sensitivity must exceed the

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[2]The latter method is actually the method of choice for studies during the growth period. It requires the selection of successive independent random samples of two or more population cohorts. A comparison of the three sequential methods using the independent random sampling approach with child data is provided elsewhere (Schaie, 1971d).

differences accounted for at least one third of the total variance and must therefore be assumed to have practical as well as theoretical implications.

Alternate Strategies

Our discussion up to this point has tried to demonstrate why the single cohort longitudinal study is unsuitable for the study of psychological variables and to demonstrate further that even sophisticated extensions, such as replicated longitudinal studies, are probably more trouble than they are worth, since their outcomes can be accepted only subject to various collateral studies. Although the longitudinal method may be inapprorpiate, this does not mean that some of the questions traditionally asked via longitudinal designs are also inappropriate. We will now examine how some of these questions can perhaps be better answered by alternate strategies.

The previous speaker (Hindley, 1971) properly distinguished between two kinds of longitudinal studies, those which involve estimation of population parameters and those where antecedent-consequent relationships are to be appraised. I take it that he wishes to consider cross-sectional methods appropriate to the former but reserve the longitudinal approach to the latter. I believe he comes to this conclusion by following the common error of accepting the identity of longitudinal and repeated measurement designs. I must continue to insist upon the limited usefulness of repeated measurement studies in developmental psychology, and differ with Dr. Hindley in that I would accept cross-sectional or longitudinal data as estimates of population parameters only given certain assumptions which I shall enumerate in a moment. I would further differ with him in that I must propose that whenever antecedent-consequent relationships are to be studied, it is not the traditional longitudinal study which is the method of choice. The investigator in this instance must study independent random samples drawn from the same population cohort at successive stages of development, but where each sample is measured either only once, or where comparison is made between samples at successive life stages who have been assessed an equivalent number of times.

As I have previously stated both the traditional longitudinal and cross-sectional methods yield generalizable data, if and only if, at least two of the following three assumptions apply. Even simple data interpretation is possible only if at least one of the assumptions is valid. These assumptions are (1) that development is unaffected by environmental input, (2) there are no changes in level of performance over successive generations, and (3) that maturational events do not affect development. We shall now examine how these assumptions affect our strategy to respond to the six types of developmental questions which Dr. Hindley has posed for us.

First of all we have the question of developmental change as a function of age. For psychological variables, since assumption (1) is unlikely to hold, this question must be investigated by replicated cross-sectional studies unless assumption (2) is known to hold also.

The second type of developmental question raised concerns the relationship between developmental status and other population facotors. If assumption (2) holds this question can be handled by cross-sectional data alone, otherwise again cross-sectional replication at different points in time will be required.

The third, add perhaps stickiest issue, relates to the study of individual differences in the course of development. Here the longitudinal method would be appropriate if assumptions (1) and (2) could be met, which is most unlikely indeed. Consequently it will be necessary to study independent random samples drawn from at least two successive cohorts, each sample studied once, with one sample for each developmental stage of interest. Only in this matter can one unambiguously determine whether the range of individual differences changes over age. If different patterns of development are suspected for different levels in the range of talent or other demographic factors it will moreover be necessary to define cohorts separately for each level of the demographic factor for which differential development is hypothesized. The same design must also be applied to any question where we are concerned about the relationship between earlier and later status. The proper measure here, of course, would then be an intra-class rather than a Pearsonian correlation.

I would argue further that the remaining questions enumerated by Dr. Hindley will similarly require a replicated cross-sectional (time-sequential for children, cross-sequential for adults) approach whenever population parameters are to be studied, or the independent random sampling cohort-sequential approach whenever antecedent-consequent relationships are to be investigated.

Estimation of Longitudinal Gradients

As part of the above discussion we should perhaps have called attention to the fact that so-called cross-sectional age differences will actually represent differences among performance between samples from different generations when no maturationally determined age change occurs. Similarly so-called longitudinal age changes may represent changes in environmental impact over time when no true maturation has occurred. For the purpose of generating developmental population parameters it is therefore possible to construct at least three different types of developmental gradients provided one has available two successive cross-sectional samples randomly drawn from the same population, preferably tested at a time interval equivalent to the interval used to segregate age levels within each sample.

It is now possible to construct an age gradient for each cross-sectional sample on the assumption that there are no generation differences. If that assumption is questionable one can next construct a composite longitudinal age gradient centered upon any one of the cohorts for whom two measurement points are available. Over the age range for which information is available one can then compute the observed age change over the time interval between the two cross-sectional studies and use these segments to construct the projected longitudinal gradient or gradients. Finally, one can project the level of performance for each cohort at a specified age. Such a gradient assumes that environmental input will be approximately equal at past and future time intervals to that prevailing over the period of the study upon which data have been collected. The proper cross-sectional gradient to be compared to the longitudinal composite would be a composite of the two sets of cross-sectional data also.

I would like to illustrate the above procedure with some data from our long-term study of changes in adult cognitive behavior (Schaie, 1971c). The example uses data on the Verbal Meaning subtest from the Primary Abilities Test for seven seven-year age groups from 32 to 74 years of age tested for the first time in 1963 and 1970 respectively. Figure 3 shows the two cross-sectional gradients (broken lines) as well the six short-term longitudinal gradients (solid lines) based on the comparison of independent random samples of the same cohorts at successive measurement times. Figure 3 shows evidence of generational differences, i.e., the cross-sectional gradients for 1963 and

1970 do not agree, with apparent positive differences. However, we cannot tell from the cross-sectional data whether this change is a function of greater ability for successive generations at identical ages or whether the change is the result of some general environmental change occurring from 1963 to 1970. The longitudinal data in Figure 3 suggest that the former rather than the latter interpretation seems warranted. Inspecting the solid lines it will be noted that there was no difference over seven years for the cohorts aged 32 and 39 at the base point, marked gain for the cohorts who were 46 and 53 at base point, and loss for those cohorts aged 60 and 67 upon the first measurement occasion.

Figure 4 gives the comparable projected cross-sectional and longitudinal gradients which can be constructed from the time-sequential data matrix. We contrast here the cross-sectional data averaged over the two measurement occasions with a longitudinal gradient constructed for the mid-age sample (cohort born in 1917) in our study. The pre- and post-dicted segments of that gradient come from the seven year age changes actually observed for the other cohorts in out study over the time interval from 1963 to 1970. The discrepancy between these curves portrayed in Figure 4 shows explicitly that the cross-sectional data give evidence of substantial generation differences in performance on the Verbal Meaning Test. Within generation performance on the other hand seems to increase into the sixties with only minimal decrement thereafter. Incidentally, referring back to Figure 1, it might be noted that the effect of prior measurement appears to have positive effects for the younger but negative effects for the older Ss. On the other hand as seen from the cross-sectional data in Figure 1, prior testing may have the effect of reducing generation differences, at least with respect to the older cohorts.

In order to focus upon the importance of generation differences a further projection is made of the pre-and post-dicted performance levels of all cohorts in our study at age 53 (the median age of our subjects). Figure 5 graphs the performance level at age 53 for eight cohorts born from 1889 to 1938. The cohort gradient shows a dramatic rise from the '96 cohort to the '24 cohort and apparently has now reached an asymptote. If this cohort can be projected into the future we will then expect that future cross-sectional studies will no longer yield the dramatic decrement form now all too apparent.

It is hoped that the listener will agree that data of this kind are useful as well as necessary to uncover the parameters of human development. I do not know whether I have convincingly pleaded for the abandonment of the old-fashioned longitudinal method in developmental research, but at least hope that I have made a case for the strong suggestion that the sequential designs be substituted whenever possible.

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Figure Captions

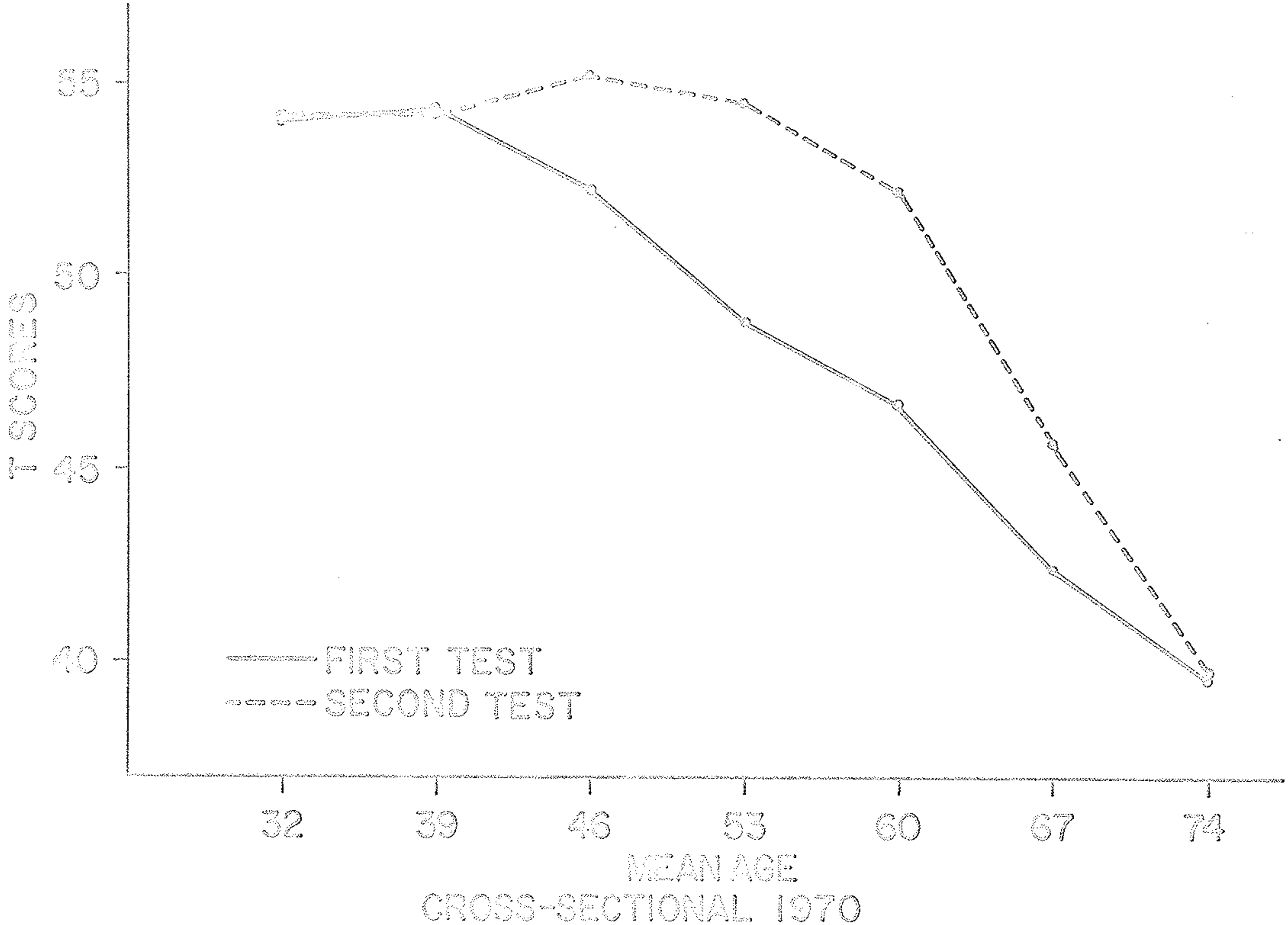
Figure 1. Comparison of means of subjects tested for the first or second time on the Verbal Meaning Test, in a cross-section study over the age range from 32 to 74 years.

Figure 2. Longitudinal age gradient for the Verbal Meaning test projected for a cohort born in 1917, comparing the effects of the presence or absence of prior measurements for the age ranging from 32 to 74 years.

Figure 3. Comparison of data collected in 1963 and 1970 for the Verbal Meaning Test. Broken lines represent the cross-sectional studies over the age range from 32 to 74 years. Solid lines represent the six seven-year longitudinal studies.

Figure 4. Longitudinal age gradients for the Verbal Meaning test projected for cohort born in 1917 compared with he average cross-sectional gradient based on data collected in 1963 and 1970, both over the age range from 32 to 74 years.

Figure 5. Cohort gradient for the Verbal Meaning test projecting expected performance at age 53 for cohorts born from 1889 to 1938.



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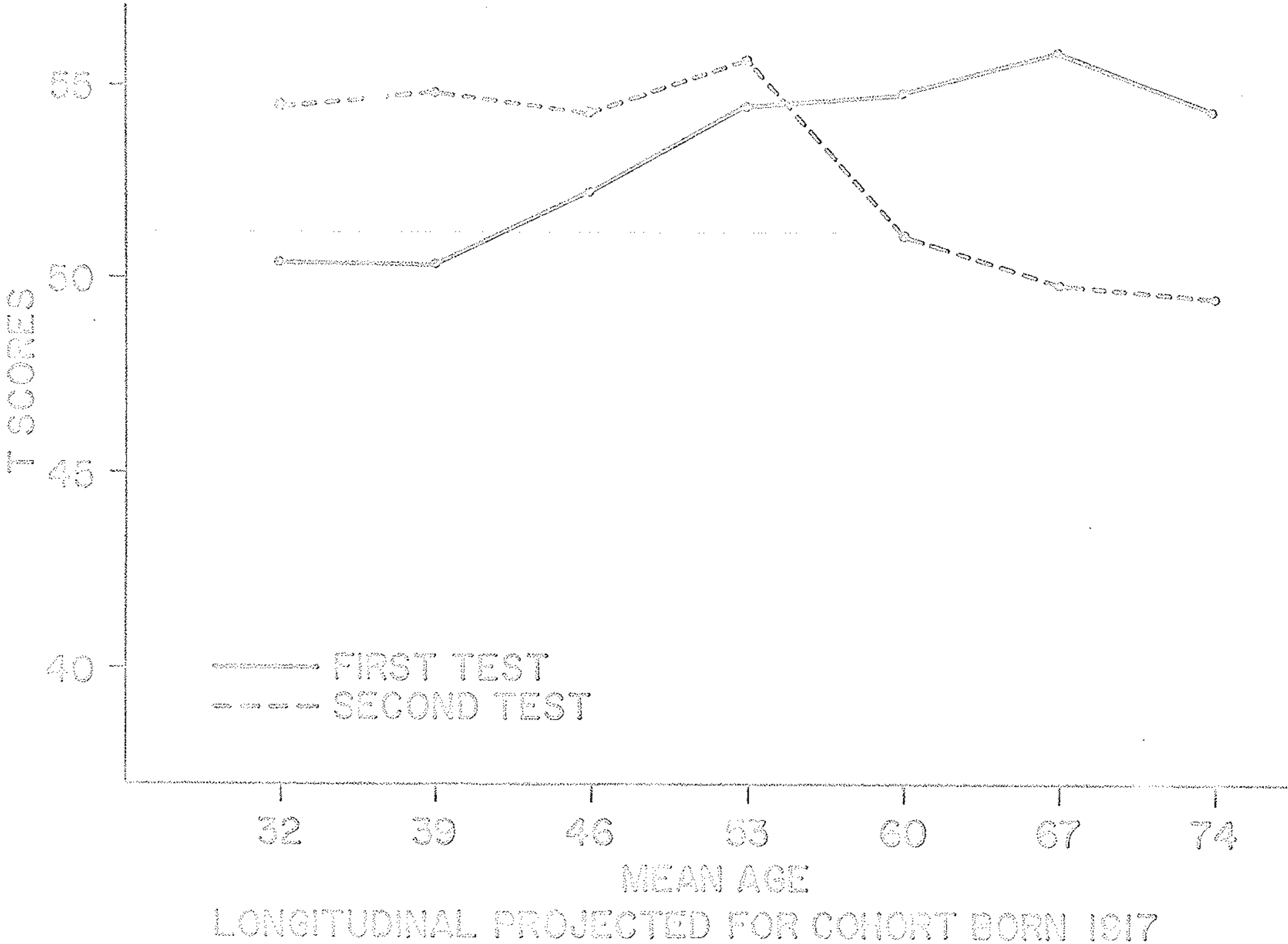

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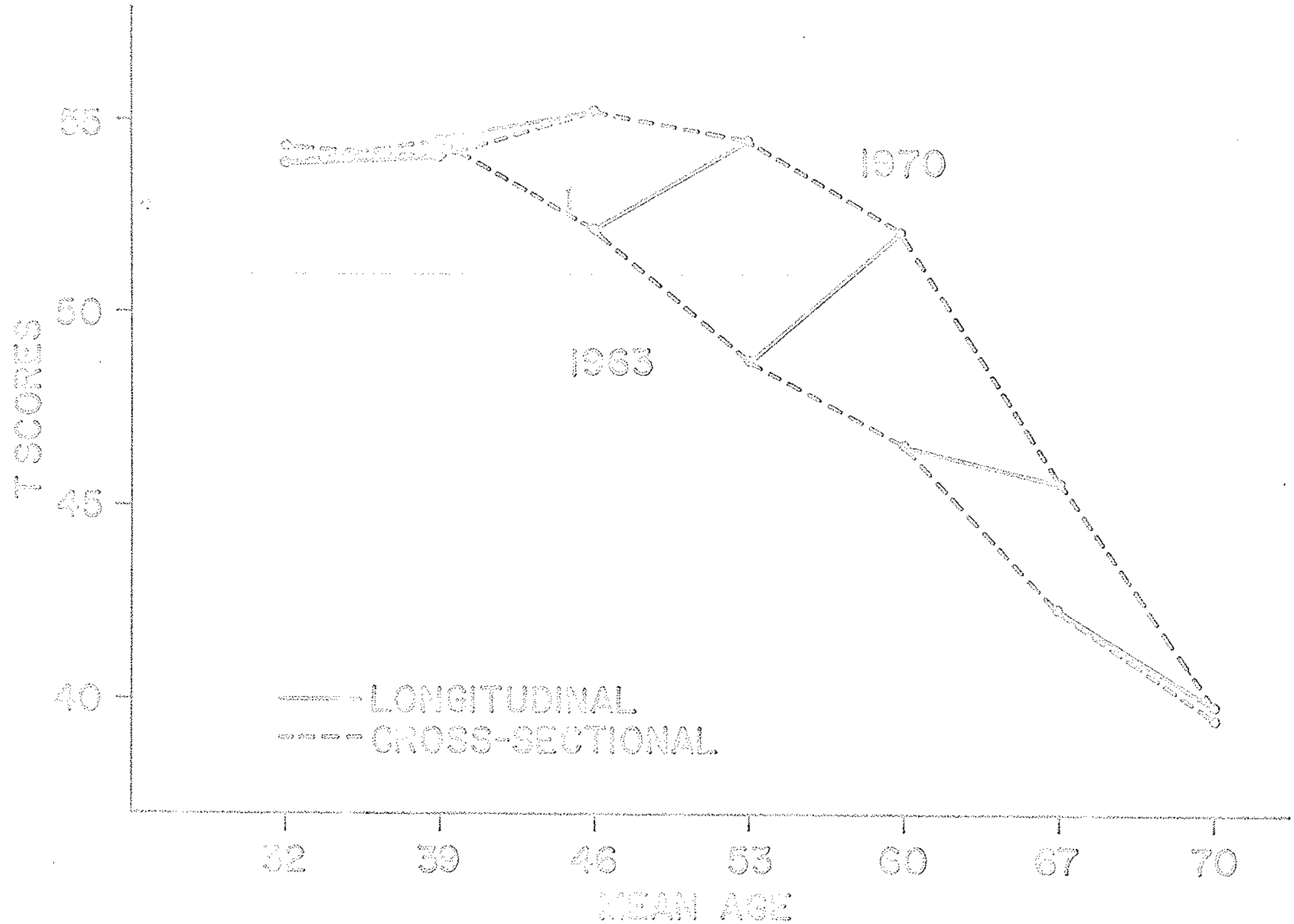

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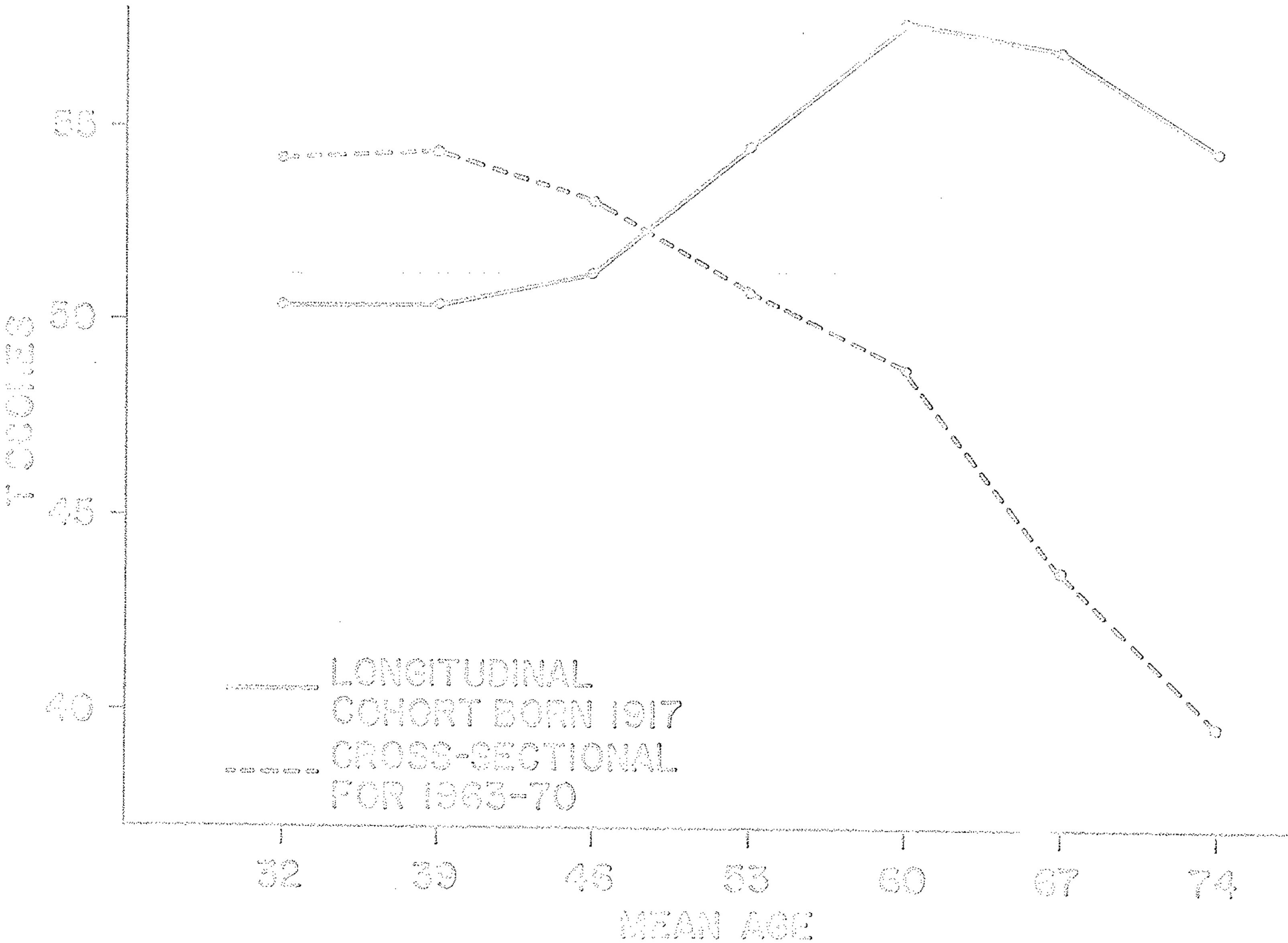

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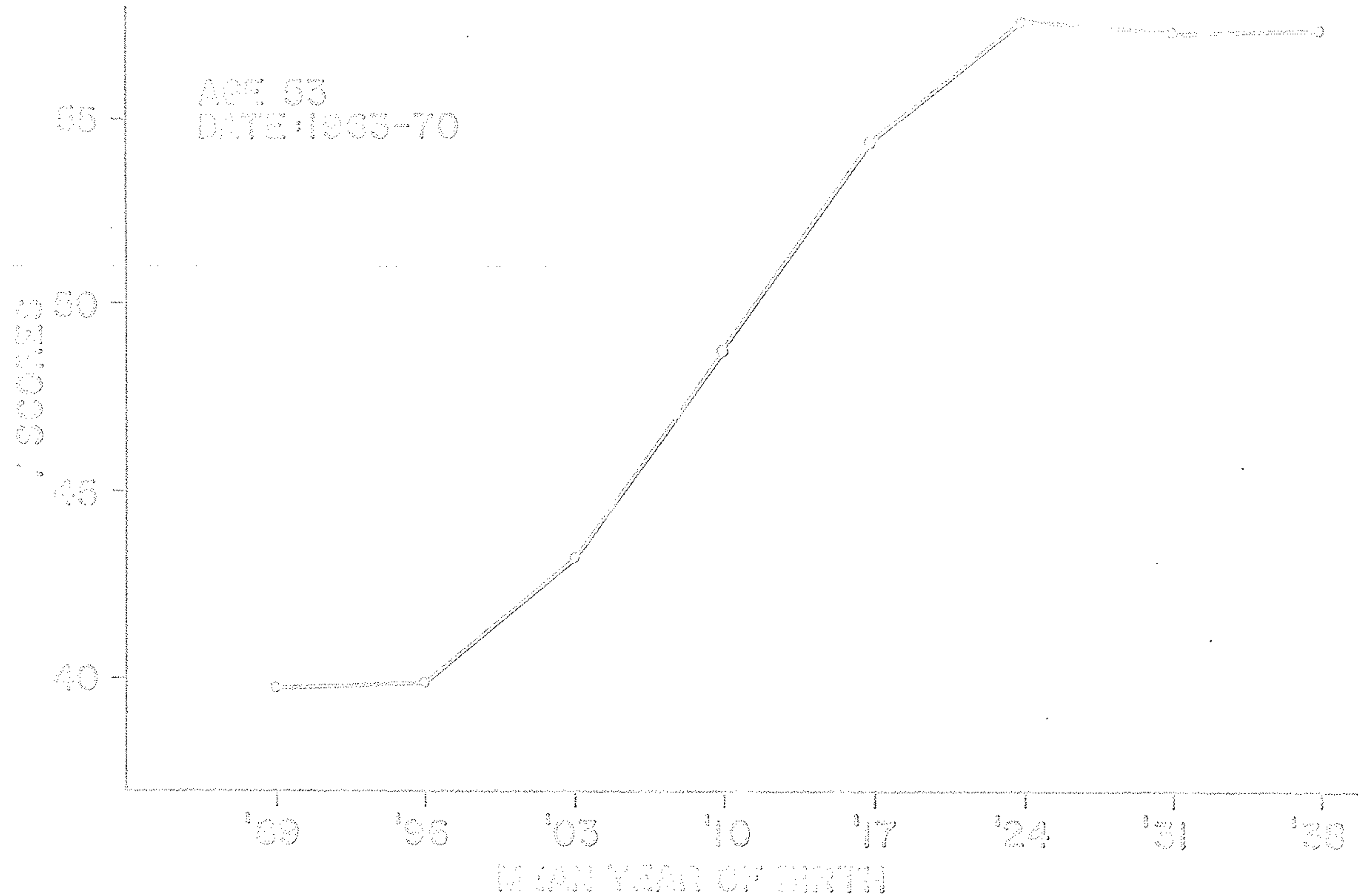

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