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ABSTRACT

This study used five data sets to investigate the effects of measured cognitive skills on educational attainment, and the effects of cognitive skills and educational attainment on occupational status and earning among men with low test scores, as compared to men with high test scores, and among men with blue-collar fathers, as compared to men with white-collar fathers. Three of the five data sets were not available for the 1972 study by Christopher Jencks. None of the analytical approaches detected evidence that ability differences have larger effects in educational attainment among men with low scores than among men with high scores, nor among men with blue-collar as opposed to white-collar fathers. The tendency in the evidence is to suggest the opposite. Furthermore, with respect to the effects of measured ability on occupational status the evidence suggests that if there are differential effects of ability, they favor those with higher scores rather than those with lower scores. The findings suggest, at best, that the effects of test scores and schooling are similar for men regardless of initial standing, and at worst, that larger benefits accrue to men who are already advantaged. The results of this study provide little support for those who view compensatory education as a potent instrument for extending economic opportunity to the disadvantaged. (Author/AM)

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THE ECONOMIC EFFECTS OF COGNITIVE AND EDUCATIONAL DIFFERENCES AMONG LOW-ABILITY AND BLUE-COLLAR ORIGIN MEN: A COMPARATIVE ANALYSIS

POLICY RESEARCH REPORT

A Policy Research Report is an official document of the Educational Policy Research Center It presents results of work directed toward specific research objectives. The report is a comprehensive treatment of the objectives, scope, methodology, data, analyses, and conclusions, and presents the background, practical significance, and technical information required for a complete and full understanding of the research activity. The report is designed to be directly useful to educational policy makers

RESEARCH MEMORANDUM

A Research Memorandum is a working paper that presents the results of work in progress. The purpose of the Research Memorandum is to invite comment on research in progress. It is a comprehensive treatment of a single research area or of a facet of a research area within a larger field of study The Memorandum presents the background, objectives, scope, summary, and conclusions, as well as method and approach, in a condensed form. Since it presents views and conclusions drawn during the progress of research activity, it may be expanded or modified in the light of further research

RESEARCH NOTE

A Research Note is a working paper that presents the results of study related to a single phase or factor of a research problem It also may present preliminary exploration of an educational policy issue or an interim report which may later appear as a larger study The purpose of the Research Note is to instigate discussion and criticism It presents the concepts, findings, and/or conclusions of the author It may be altered, expanded, or withdrawn at any time

EXECUTIVE SUMMARY

Efforts to improve the school achievement of disadvantaged children have been based in part on the belief that adult economic success is strongly related to the level of an individual's cognitive skills and educational attainment. In 1972, Christopher Jencks and his colleagues published Inequality: A Reassessment of the Effect of Family and Schooling in America that reported evidence suggesting that little of the variation in income in the United States can be traced to variations in either cognitive skills or educational attainment. Jencks et al. argued that their findings refute popular conceptions about the connections between poverty and schooling, and that even if existing inequalities in cognitive abilities and education were eliminated completely, the remaining dispersion of income would be enormous, and the problem of poverty would be barely reduced.

As is often the case when popular beliefs are challenged, the findings in Inequality were attacked on several grounds. Among the criticisms was the suggestion that the findings reported were averages based on national samples that might not hold for all subgroups in the population. In particular, some critics hypothesized that increases in test scores or schooling might yield greater benefit in terms of later economic success for the disadvantaged population than for the population at large. Such a finding would have strong implications for educational policies by providing a rationale for emphasizing educational strategies as a means of improving the economic futures of disadvantaged children.

The aim of this paper is to test this hypothesis using a data base more extensive than that available when Inequality was researched and using analytic techniques sensitive to isolating differential effects of measured cognitive ability and schooling. The analyses employed five data sets, three of which were not formerly available. Each data set provided a measure of cognitive skills or ability and measures of years of schooling, occupational status, income, and various background characteristics. Three analysis strategies were followed, each providing a somewhat different picture of the relationships among test scores, education, and economic success.

The work was undertaken with the hope of finding evidence that increments in cognitive ability or schooling have larger economic effects among men with lower initial test scores or social status. Such evidence was not found. In fact, the findings suggest, at best, that the effects of test scores and schooling are similar for men regardless of initial standing, and, at worst, that larger benefits accrue to men who are already advantaged.

Although the samples are flawed, and the analyses limited by being descriptive of a static situation, the consistency of the results is striking. On the other hand, it is possible that the measures are sufficiently vague that the real questions of interest have not been addressed. For example, there may be a minimal level of literacy below which gainful employment is precluded. If the measures used in the samples do not adequately capture this "literacy," then the question has not been answered. It is also possible that a survey of a large sample of middle-aged, low-ability men whose employment has stabilized might provide a different picture. In the absence of adequate tests and the ideal sample, however, this must remain in the realm of speculation.

The results of these analyses provide little support for those who view compensatory education as a potent instrument for extending economic opportunity to the disadvantaged. The connections between measured cognitive skills, schooling, and economic success are simply too weak to suggest otherwise. Educators concerned with finding ways to help children learn should not be affected by these conclusions. Policymakers, for whom educational programs are but one alternative strategy, perhaps should seek remedies that are more directly relevant to the causes of persisting poverty and inequality than are the presumed cognitive deficiencies of the poor.

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TABLES

I INTRODUCTION

Efforts to improve the school achievement of disadvantaged children have been based, at least in part, on the belief that adult economic success is strongly affected by cognitive skills and educational attainment. Educational strategies, consequently, figure prominently in many of the programs operating under the "War on Poverty" rubric.*

The popular belief that improved schooling will significantly enhance the life chances of poor children and reduce poverty is indirectly supported by formal theories of income determination in human capital economics (see Becker, 1964; Mincer, 1974). However, occasional skepticism about the role of education in promoting social mobility or determining economic success can be found in sociological and economic literature that predates more recent and better-known attacks on the premises thought to underlie current policies.†

In 1972, Christopher Jencks and his colleagues published Inequality: A Reassessment of the Effect of Family and Schooling in America (New York: Basic Books), which reports evidence suggesting that little of the variation in income in the United States can be traced to variations in either cognitive skills or educational credentials. Jencks

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*For a detailed review of educational programs classified as part of the antipoverty effort see Levin (forthcoming). For statements illustrating the belief that poverty and low educational achievement are intimately connected see Ashline, Pezzulls, and Norris (1976); Silberman (1970), p. 64; Tyler (1974), pp. 167-169. For a critique of the view that antipoverty policy emphasized educational strategies, see Rivlin (1973).

†See Anderson (1961); Hansen, Weisbrod, and Scanlon (1970); Ribich (1968).

et al. argue that their findings refute popular conceptions about the connections between poverty and schooling, and that even if existing inequalities in cognitive ability and education were entirely eliminated, the remaining dispersion of income would be enormous, and the problem of poverty would barely be reduced.

The findings reported in Inequality rely largely on national samples, covering individuals from all segments of the population. The effects of cognitive skills or abilities, as measured by test scores,* and the effects of education that Jencks et al. report are averages presumed to hold for the general population. The analyses that estimate such effects assume that the world works similarly for individuals with varying backgrounds and abilities.+ But social policies are rarely aimed at the general population. Rather, they are aimed at specific groups. Therefore, if the world works differently for such groups than it does for the population at large, analyses of large-scale, so-called representative data may be, for the purposes of policy research, unrepresentative and misleading.

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*Cognitive skills and abilities are used interchangeably throughout the paper. Both refer to that which is measured by the tests used in the samples, and no attempt is made to distinguish between skills and ability.

+Jencks et al. do report testing for differential effects of education on income by socioeconomic background and test scores. They also report looking at differential income effects of test scores by background, and for nonlinearities in the effects of test scores. They do not report testing for such effects with respect to occupational status. In reply to critics, Jencks (1973) presented more information on the effects of nonlinearities and interactions in his data. Jencks' test for the importance of nonlinearities and interactions, however, was the increment in explained variance that they effected. This ignores the possibility that even in the absence of large increases in R^2, regression coefficients may vary significantly across subgroups. It is such variability that I am concerned with here. I am grateful to Christopher Jencks for calling my attention to his own sin of omission.

Although the economic effects of measured cognitive skills and educational attainment may be relatively small among individuals in general, the possibility remains that they are atypically large among individuals of low status origin or low initial ability. If this were to prove true, the rationale for emphasizing educational strategies to improve the economic life-chances of disadvantaged children would be strengthened.

To test this possibility, I investigated in five data sets the effects of measured cognitive skills on educational attainment, and the effects of cognitive skills and educational attainment on occupational status and earnings among men with low test scores, as compared to men with high test scores, and men with blue-collar fathers, as compared to men with white-collar fathers. Three of the five data sets were not available when Inequality was researched, and they significantly expand the data base on which conclusions about the interrelationships among background, ability, schooling, and economic success can be drawn. I also investigated the effects of measured ability and schooling on the earnings of a sample of low-ability men whose scores on the Armed Forces Qualifying Test disqualified them from military service.

In Section II, the data employed in the analyses are discussed. In Section III, the analytical methods used are discussed, and in Section IV, the results of the analyses are presented. In Section V, my results are compared to a study that asked similar questions, and the policy conclusions which my results support are presented.

II SAMPLE AND VARIABLE DESCRIPTIONS

A. The 1967-1974 Panel Study of Income Dynamics (PSID)

The University of Michigan Survey Research Center (SRC) surveyed several thousand families annually over a seven-year period to study the sources and stability of family income. The survey sampled only heads of households, and did not include other adults who lived in the household. The restriction results in a sample that is somewhat more advantaged than a random sample.

I analyzed 1971 data for 2,366 male heads of households aged 25 to 64, excluding students, military personnel, and, generally, those not institutionalized. Attrition due to missing data resulted in a final sample of 1,774 individuals.*

SRC administered a 13-item sentence completion test from the Lorge-Thorndike "intelligence test" to respondents. It is scored as the number of correct answers. The test requires respondents to complete cliché-type sentences of the sort, "It is better that ten guilty persons ______________________, than that one innocent suffer." Response choices to this example include "escape," "suffer," "capture," "starve," and "repent." Mueser reports that the correlations between the sentence completion test and other ability tests range from 0.20 to 0.60, and that its reliability is estimated at only 0.652. The rather low reported intercorrelations of the PSID test with other tests suggest that it is not a good measure of general ability. This will reduce estimates

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*See Mueser (forthcoming) for a more detailed description of this sample.

of the effects of adult "ability" from what they would be with more adequate measures. The test's relative unreliability will have a similar effect. If the reliability of the test is similar for all ability levels, however, this will not affect comparisons within the sample.

Use of the PSID test is complicated further by its timing. Respondents took the test at the same time they reported their occupations and incomes. Conceivably, their test scores reflect the impact of their economic positions rather than the other way around. Because adult ability is generally assumed to be stable, I doubt that this effect is large, but it nevertheless constrains causal interpretations from the PSID test data. The PSID sample is, however, the only large nationally representative sample with test score data for men 25 to 64. The ambiguities and limitations of the test must be weighed against this virtue.

SRC coded occupations into broad categories, rather than into detailed Census classifications. Mueser estimated Duncan Socioeconomic Index scores for these categories. Duncan scores, which I use to measure occupational status throughout this paper, are based on the education and income levels characteristic of specific occupations (see Duncan, 1961). The effects of grouping on a dependent variable are problematic and depend on the relationship between the criteria for grouping and the independent variables. SRC's grouping appears to have been related to education and, thus, compensates for the downward biases in coefficients and in R^2s, which might be expected from reduced variance.

My measure for income in the PSID is annual earnings, based on reported wages, salaries, or self-employment income, minus asset income.

The PSID education variable measures number of years of school completed. This is true of the education variables in all the samples which I have analyzed.

B. The 1964 Veterans Survey

In October 1964, the Census Bureau conducted a special Current Population survey of men aged 16 to 34. The National Opinion Research Center analyzed a subsample of these men who were veterans, aged 25 to 34, for whom Armed Forces Qualifying Test scores were available.

I have analyzed data for 1,815 respondents who were not students and who expected positive earnings in 1964. After taking into account missing data, the sample size is 1438.*

The Veterans survey sample is not representative of its age cohort. During the period that the respondents were eligible for military service, the Army generally rejected men with AFQT scores below the 10th percentile and deferred men who were in school, or had children, or who were over 26. Higher nonresponse rates for unsuccessful individuals and greater nonavailability of AFQT scores for better-educated men who entered the military from ROTC further reduce the representativeness of the sample. The sample of veterans 30 to 34 is somewhat more representative than the sample of those 25 to 29 years old, and I emphasize results based on the former rather than on the combined sample.

The Armed Forces Qualifying Test, which the older veterans in this sample took, included sections on vocabulary, arithmetic reasoning, and spatial relations. Men who entered the military after 1953 took a test that also included a section on tool functions. Jencks reports that the correlations between the two versions of the AFQT with measures of economic success do not differ significantly.

The original data recorded AFQT scores in rather broad percentile categories. Assuming the distribution of "true" scores is normal, Jencks

*For a more detailed description of the sample, see Jencks (forthcoming).

rescaled the percentile scores to a mean of 100 and a standard deviation of 15. This makes the scores comparable to those which I use from the Project Talent and the Kalamazoo Brothers surveys, discussed later in this section.

Most of the Veterans survey respondents had completed their schooling before they took the AFQT.* This means that I cannot unambiguously measure the effects of ability on educational attainment in the Veterans sample. Nor can we be certain that the measured effects of AFQT on economic success are free from the effects of schooling on ability. For those interested in the effects of "initial," "early," or "prior" ability, this is a problem. For those who hope to increase ability levels by increasing schooling levels, it is not.

I have used the respondent's report of his expected 1964 earnings as the income variable. The Veterans survey included a measure of weekly earnings, but the response rate for that item is much lower than for expected earnings, and the subsample that responded to it is atypically successful and homogeneous.

The principal defects of the Veterans survey sample for my purposes are that it does not include men over 34 and that it generally excludes men with unusually low and unusually high test scores. The principal attraction of the sample is that it has a relatively reliable measure of cognitive skill, which can be related to measures of economic success.

C. The Project Talent Survey

In 1960, Project Talent administered a battery of 65 tests and questionnaires on attitudes and personality to students in a 5% stratified,

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*One-third of the respondents returned to school after taking the test. See Griliches and Mason (1972).

random sample of American high schools. Project Talent followed up students 1, 5, and 11 years after high school graduation.

I have analyzed 11-year follow-up data for 1,369 men who were in the eleventh grade in 1960. This means the respondents were around 28 years old when they were last surveyed. Eliminating respondents with missing data, left a sample size of 839.*

Project Talent produced several composites from its separate tests. Crouse reports that the Academic Composite best captures the effects of adolescent cognitive skills on educational attainment, occupational status, and earnings, and that adding additional tests to regression equations never raises R^2 by more than 0.013. My analyses use the Academic Composite, which is constructed from the weighted sum of scores on vocabulary, English, reading comprehension, creativity, abstract reasoning, and four math tests. The composite is standardized to a mean of 100 and a standard deviation of 15.

In Project Talent occupations were classified according to its own classification system, rather than to census categories. Marsha Brown estimated Duncan scores for the Project Talent categories.

Project Talent respondents were asked to report their current earnings at the time of the survey and to indicate whether it was an hourly, weekly, or monthly figure. Crouse used these reports to calculate an hourly earnings rate; and I used this variable. He did not calculate an annual earnings figure because Talent's question concerning weeks worked referred to the year prior to the survey, and, thus, possibly to a job different from the one to which a respondent's earnings pertain.

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*See Crouse (forthcoming$_b$) for a detailed description of this sample.

Because the Project Talent respondents were already in the eleventh grade when they were first surveyed, their level of eventual education is quite high. Ninety-seven percent have 12 or more years of schooling. This means that inferences from low scores in this sample to low scores in general are suspect. Low scorers with high educational attainment are likely to have compensating characteristics, which typical low scorers do not have. This will reduce the measured effects of test scores from what they would be in a more representative sample. The elimination of high school dropouts will also lower the variance in educational attainment for men with low scores. This will reduce the apparent effects of test scores on educational attainment and particularly may bias my comparisons between low and high scorers.

D. The Kalamazoo Brothers Survey

In 1973, I drew a sample of males who had been in the sixth grade in the Kalamazoo, Michigan, public schools from 1928 to 1950. Scores on standardized Terman or Otis group tests administered in the sixth grade were available for these individuals. I then used school records to determine siblingship and discarded individuals for whom I could find no brothers in the sample.

Beginning with a potential sample of close to 3,000 individuals from 1,200 families, I traced and interviewed 1,243 men during 1973 and 1974. The analyses that I report here are based on these 1,243 respondents and on various subsamples of them. Item nonresponse and failure to interview more than one brother in a pair introduces sample attrition.* Biases due to missing data fortunately appear to be small in this sample. Biases due to nonlocation or noninterviewing of over half the original sample could be more serious. However, the average test score of nonrespondents is only three points less than the average score of men I interviewed.

The Kalamazoo Brothers data are the only American data that include measures of early cognitive ability and adult economic success for a reasonably large sample of middle-aged men whose test scores span a representative range.† They are also the only data that permit analyses that control unmeasured as well as measured background factors when estimating the effects of ability and schooling for middle-aged men.‡

From 1928 to 1942, the Kalamazoo school system administered the Terman group test. After 1942, the system administered the Otis test. About a quarter of my respondents took the Otis rather than the Terman. Both tests emphasize verbal skills and are considered to be measures of "general brightness" and "general ability" (see Buros, 1975). The Otis

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*For detailed descriptions of this sample, see Olneck (1976, forthcoming$_b$). I am grateful to Dr. William Coates and Dr. David Bartz of the Kalamazoo Public School System for permission to utilize the Kalamazoo school records. I am grateful to Dr. Stanley Robin, Director of the Center for Sociological Research at Western Michigan University for extending the courtesies of the Center to me during the interviewing phase of the study.

†Daniel C. Rogers' sample of 343 white males who were in Connecticut eighth grades in 1935, and who were surveyed in 1965 most closely resembles mine on the timing of the test and the age at which respondents were followed up. His sample is unfortunately too small to permit useful disaggregated analyses. See Hause (1972) for attempts to utilize the Rogers sample.

‡The only other sample of brothers with test scores comes from the Project Talent file. At present, only 99 pairs of nontwins have been analyzed. Eventually, close to 2,000 pairs should be available. The Talent siblings, however, are still less than 30. See Crouse (forthcoming$_a$) and Olneck (forthcoming$_a$) for analyses of the Talent siblings.

test is scaled to a lower mean, but its variance and correlations with other variables are not generally significantly different from the Terman test. Therefore, after taking into account the secular trend toward higher parental socioeconomic background and the effects of background on test scores, I adjusted the scores for respondents who had taken the Otis test, and combined the groups (see Olneck, 1976, forthcoming$_b$).

I classified occupational reports according to the Census three-digit detailed classifications, and I assigned Duncan scores. I asked respondents to report their 1973 expected annual earnings, and recorded their responses in intervals of $1,000 up to $8,000, of $2,000 from $8,000 to $14,000, of $3,000 from $14,000 to $20,000, and of $5,000 from $20,000 to $25,000. Respondents reporting earnings over $25,000 were coded $34,000. I assigned the midpoint of each interval as each individual's earnings. Although these intervals are wider than might be desirable, they are nevertheless, more narrow than those usually employed by survey organizations such as NORC. My grouping procedures, especially at the top, and the absence of many respondents with earnings under $8,000 probably raises the correlations between earnings and its determinants over what they would otherwise be.

The Kalamazoo respondents are better educated and earn more money than similarly aged men in the nation as a whole. Their test scores, however, are only 3 points higher than nonrespondents. In the original sample, 11.2% of the potential respondents scored below 80. In the present sample, 8.4% have scores below 80.

The educational and earnings advantages of the Kalamazoo respondents are due in part to the characteristics of Kalamazoo. The city has had a public college (now a university) for some years and is an area of steady and skilled work. Still, I suspect there is some success bias in my sample. This means that although low scores are represented in close to the expected proportions, they may be atypical of low scorers in general. This would lead to downwardly biased estimates of the effects of test scores for low scorers. Nevertheless, the Kalamazoo data remain among the best available data with which to investigate the effects of measured early cognitive ability on adult success. This should, of course, suggest the rather severe limits to research in this area.

E. <u>The One-Third of a Nation Military Rejects Sample</u>

In 1963, the President's Task Force on Manpower Conservation interviewed close to 2,500 men, aged 17 to 25, who had been rejected for military service because they failed the AFQT under current standards of induction. The scores of the respondents are at or below the 30th percentile, which makes this a close to ideal sample with which to investigate the effects of ability increments in the low range of test scores. Unfortunately, the effects of cognitive ability on the earnings of workers under 30 are lower than the effects among older workers (see Fägerlind, 1975; Jencks forthcoming). This means that we cannot be certain that the results I report here would be replicated if the Rejects sample were resurveyed.*

Unlike the AFQT "score" I used in the Veterans sample, the score in the Rejects sample is simply the percentile in which the respondent placed. For scores in this range, percentile scores are approximately a linear transformation of raw scores. (See Hansen et al., 1970, p. 414).

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*For earlier analyses of this sample, see Hansen et al. (1970). I am grateful to William Maron for providing me with a tape of these data.

For a measure of income, I used the reported weekly earnings of the respondent's current or last job. I did this because I am skeptical of the measure of annual earnings that can be constructed by subtracting reported transfer income from reported total annual income.

III ANALYTICAL METHODS

I used three approaches to look for evidence that ability increments have larger effects among men with low scores than among men with average or high scores. I first conducted regression analyses of education, occupation, and earnings within each sample, adding a term defined as the square of Test Score, i.e., Test Score^2, after first including only Test Score. I next looked directly at the means and standard deviations of education, occupation, and earnings for men in different test score intervals. Finally, I looked at regression equations that estimate the average effects of differences in measured ability and educational attainment separately for men in different test score ranges. I also conducted separate regressions for men who had white-collar and blue-collar fathers. Each of my approaches is described briefly below.

Table 1 shows the results of regression analyses relating Education, Occupation, Earnings, and the natural logarithm of earnings (Ln Earnings) to Test Score, and to Test Score and Test Score^2 for different samples. If the effects of test score gains are generally larger at the lower end of the distribution and become smaller as test scores rise, the regression coefficient for Test Score^2 will be negative. If, on the other hand, there is generally a larger impact of ability differences among higher scores, the regression coefficient for Test Score^2 will be positive. If the effects of test score differences are not very different along the range of test scores, the regression coefficient for Test Score^2 will be small and statistically insignificant; that is, less than twice its standard error. If there are only a relatively small number of cases with scores whose effects depart from a linear pattern, the regression coefficient for Test Score^2 may be statistically significant, but including Test Score^2 will not add very much to the explained variance. In that case, the proportions of variance explained, R^2, shown in Table 1 will be quite similar for equations with and without the Test Score^2 term. This does not mean that nonlinearities are unimportant. It only means that nonlinearities are confined to particular segments of the distribution. Hence, looking at the coefficient of the Test Score^2 term is one indication of the extent to which relationships differ along the range of test scores.

A second way of looking for evidence of differential effects of cognitive ability on education, occupation, and income is to inspect means and standard deviations of these variables within test score intervals. Tables 2 to 6 show the means and standard deviations of Education, Occupation, Earnings, and Ln Earnings for men in different test score intervals in each sample. The best indication of the gains one might reasonably predict from raising test scores of low scorers is a comparison of the means of education, occupation, and income for the very lowest scorers to those in adjacent intervals. Adjacent intervals are of interest because educational interventions are not likely to result in dramatic increases in test scores. Comparison of the means also explicitly shows the pattern of increased advantage associated with test score gains at particular points along the entire distribution. These cannot be determined simply by examining the regression results such as those shown in Table 1.

The standard deviations in Tables 2-6 allow us to investigate the variability of outcomes for men in different ability levels. If men with very low test scores are concentrated in homogeneously low-paying and low-status jobs, and if men with higher test scores are distributed more widely, one might view higher test scores as an important advantage even if average earnings or status differentials were small across test score groups. Although ability advantages might, on the average, confer small benefits, they might, nevertheless, determine access to some higher-paying or higher-status jobs.*

The third analysis consists of regression carried out within test score intervals and separately for men in two status groups. Tables 7-16 give the results of regression analyses relating Education to Test Score and background measures, and Occupation, Earnings, and Ln Earnings to Test Score, Education, and various measures of family background, separately for men in different test score ranges, and for men with white-collar and blue-collar fathers. (Results from the Military Rejects Sample are shown separately in Tables 12 and 15.) To investigate whether the effects of test scores and schooling are similar for men from disadvantaged and advantaged background, I would have preferred to divide respondents according to parental income, rather than according to whether a respondent's father held a white-collar or blue-collar job. I did not have any data with which to do this, however.

In these analyses, I controlled a variety of background measures because the effects of ability and schooling are likely to be overestimated if they are omitted. This is because men with higher ability and more schooling tend to come from more favorable backgrounds, and would be economically more successful even in the absence of ability and educational advantages. Therefore, the effects of ability and schooling are artificially inflated by the effects of family background. For most of my samples, the only way I could control background was to include socioeconomic measures such as Father's Occupation. However, for some analyses I could control family background more fully in the

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*I am grateful to Marshall Smith for suggesting this line of reasoning to me.

Kalamazoo sample. I did this by defining variables as sibling differences. This eliminates the effects of all the confounding factors which vary between families. I was able to do this when looking at the effect of the Test Score^2 term (Table 1) and when I stratified the samples by Father's occupational group (Tables 8, 10, 13, 16). I could not do this when stratifying respondents by test score levels, because brothers could be in different ranges.

I looked at the effects of schooling as well as test scores on economic benefits for two reasons. One is that policymakers have a substantive interest in knowing the extent to which lengthier schooling is associated with greater economic success. The other is that I wanted to see the extent to which the economic benefits associated with higher test scores arise because men with higher scores also get more schooling. If improved cognitive skills affect occupational status and earnings largely by promoting educational attainment, policy interventions should take this into account.

The following section presents the results for each of the outcomes and samples.

IV RESULTS

The discussion of the results is organized first around the three outcomes: education, occupation, and earnings. For each outcome, I discuss the results and conclusions for all the samples in the following order. First, I report on the inclusion of the Test Score^2 term as shown in Table 1. Second, I discuss the means and standard deviations by test score intervals shown in Tables 2-6. Third and fourth are the results of the regressions stratified by test score level and father's occupational group, respectively, as presented in Tables 7-16. Fifth and finally, for each outcome I discuss the overall conclusions from all the analyses.

A. Education

First, I present the results of the analyses which look at the effects of measured cognitive ability on educational attainment. In the Veterans survey sample most respondents took the Armed Forces Qualifying Test after they finished school. In the PSID, all respondents were tested at the same time of the survey. If going to school longer raises test scores, it is inappropriate to study the effects of ability on educational attainment in the Veterans and PSID samples. In the Talent and Kalamazoo samples, however, respondents were tested before they completed their schooling. Therefore, I considered the effects of ability on educational attainment in those samples, but not in the others where lengthier education may have raised adult test scores.

1. Effect of Test Score^2

There is no evidence in Table 1 that increments in ability at the lower end of the test score distribution raise educational attainment more than they do at the higher end. Furthermore, there is evidence that the effects of measured cognitive ability on schooling may often be overestimated by studies that ignore the joint dependence of ability and schooling on family background.

The sign of Test Score^2 in Table 1 is positive for both the Talent and Kalamazoo samples. The coefficient is significant only in Talent, however. This is exactly what I would expect if the effects of test scores on educational attainment were really linear, but sample restrictions eliminated men with low attainment, as is the case in the Talent sample.

A comparison between Equations 3 and 5 suggests that the average effects of measured ability differences on education are substantially overestimated when the effects of family background are ignored. Among individuals in the Kalamazoo sample, a 10-point difference in test scores is associated with a 10 (0.1020) = 1.02 year difference in attainment. But a 10 point test score difference between brothers is associated with only a 10 (0.0584) = 0.58 year difference in attainment. This suggests that 1-0.58/1.02 = 43% of the relationship between test scores and schooling in the Kalamazoo sample arises because men with higher scores tend to come from families that somehow promote educational attainment independent of their sons' abilities. An alternative interpretation is that the abilities that vary across families and those that vary within families are different, but that a single ability measure obscures this fact. If the abilities that vary between families strongly affect education and those that vary within families do not, reduced coefficients for a single ability measure will result when family background is controlled. This does not mean that family background rather than ability causes higher educational attainment. It means that the effects of the two cannot be distinguished without direct measures of multiple abilities.

2. Differences in Means and Standard Deviation by Test Score Intervals

The results in Tables 2 and 3, which show the means and standard deviations of education by test score intervals for the Talent and Kalamazoo samples, suggest that although higher test scores are associated with higher attainment across the range of test scores, small increases in test scores for low scorers do not have dramatic effects nor do they significantly enlarge the range of educational choice open to low scorers. Increments in average attainment are greater across the higher intervals than across the lower intervals. Variation in attainment does not increase significantly for men whose scores are below the mean.

3. Regressions Stratified by Test Score Level

Table 7 presents the results of regressions of Education on Test Score and measured socioeconomic background separately for men in different test score intervals. Although the regression coefficients for low scorers may be biased downward because men with both low scores and low attainment are underrepresented, the evidence nevertheless gives no support for the contention that ability differences among low scorers have larger effects than among high scorers. What significant differences there are between coefficients suggest the opposite.

For example, in the Talent sample, a 10-point increment in test scores for men with scores less than 90 is associated with only a

10 (0.047) = 0.47 year increment in education. This is significantly lower than the effect of over 1 year associated with similar difference among men with scores over 90.

In the Kalamazoo sample, controlling measured background, test score differences are not significantly associated with educational differences for men with scores between 90 and 109. For men with test scores below 90, a 10-point test score increase is associated with a 10 (0.056) = 0.56 year increase in attainment. This is significantly lower than the analogous effect for men with scores above 110, i.e., 1.11 years.

4. Regressions Stratified by Father's Occupational Group

Table 8 presents the results of regressions of Education on Test Score and measures of socioeconomic background in the Talent sample, and of sibling educational differences on Test Score differences in the Kalamazoo sample separately for men with white-collar fathers and men with blue-collar fathers. The average effects (i.e., regression coefficients) of test scores on attainment do not differ significantly between white-collar and blue-collar sons in either sample. However, differences in $\bar{R}^2$s suggest that educational attainment is more random with respect to differences in test scores and family characteristics among blue-collar than white-collar origin men. This suggests that the process of educational attainment is more capricious and more sensitive to idiosyncratic events among men with blue-collar fathers than among men with white-collar fathers. If the process of attainment for individuals with poverty, and not merely blue-collar, origins were atypically random, then interventionist policies would have weaker results than even my data suggest.

5. Summary of Effects of Measured Ability on Educational Attainment

None of my analytical approaches detected evidence that ability differences have larger effects in educational attainment among men with low scores than among men with high scores, nor among men with blue-collar as opposed to white-collar fathers. The tendency in the evidence, although not compelling, is to suggest the opposite.

B. Occupation

The discussion below considers the effects of ability and schooling on current occupational status.

1. Effects of Test Score^2

Table 1 shows that the coefficient of Test Score^2 is positive in the four samples for which I analyzed occupational status. It is significant only in the PSID, however. The absence of any Test Score^2 term with a negative sign suggests that either the effects of cognitive ability differences on occupational status are linear or that they increase as test scores increase.

Equations 13 and 15 show that 1 - 0.465/0.696 = 33% of the relationship between test scores and occupational status in the Kalamazoo sample arises because both are related to family background. This suggests that studies that ignore the effects of family background are likely to overestimate the effects of cognitive skills on occupational status.

2. Differences in Means and Standard Deviations by Test Score Intervals

Tables 2 to 5 show the means and standard deviations of occupational status for men with different test scores in the Talent, Kalamazoo, PSID, and Veterans samples, respectively. These analyses confirm the findings in Table 1. They offer no evidence that increases in test scores at the lower end of the distribution have larger effects on occupational status than increases at the higher end. If anything, the opposite is more likely true. Differences in average occupational status tend to be larger across the higher test score intervals than across the lower, though the largest differences in the Kalamazoo sample are across the middle ranges of scores. There is also no evidence that men with only slightly higher scores than the lowest scorers have more variable statuses than the lowest scorers.

In the Talent sample (Table 2), the average occupational status of men with the lowest scores (i.e., less than 70) is close to the average for men with scores 71 to 85. There are large differences across the intervals 71 to 85, 86 to 100, and 101 to 115. Occupational differences across the higher intervals are less than those across the middle intervals. There is no marked tendency for the variation of occupational status to rise with test scores in the Talent sample.

In the Kalamazoo sample (Table 3), men with scores less than 70 are at a greater disadvantage relative to men with scores 71 to 85 than in the Talent sample, but there are only 18 Kalamazoo respondents with scores less than 70, so this result should not be taken too seriously. The largest occupational differences occur across the intervals 71 to 85, 86 to 100, and 101 to 115. Occupational differences are more modest across the higher intervals. The standard deviations of occupational status do not differ appreciably across test score intervals. The Kalamazoo results, then, are quite similar to those from the Talent sample.

In the PSID (Table 4), men who answered five or fewer questions correctly have occupational statuses quite close to those for men who answered six or seven questions correctly. The largest differences in occupational status are between men with scores of 10 and 11, and between men with scores of 12 and 13. There is a tendency in the PSID for the statuses of men with scores of 9 or less to be more homogeneous than those of men with higher scores.

The tendency for variation in status to rise with test scores is especially marked in the Veterans sample (Table 5). However, the widths of test score percentile intervals are also larger at the higher test score levels. Moreover, military selection procedures screened out most men with very low scores. Veterans with scores below the 10th percentile are no doubt atypically successful. For these reasons, I tend to discount the pattern of increasing variability in the Veterans sample.

Assuming some success bias in both the Kalamazoo and Talent samples, I suspect t e variability in occupational status for men with low scores in the populations covered by those surveys is even greater than the data suggest. Therefore, only the PSID offers any support for the contention that higher test scores significantly enlarge the range of occupational statuses men have. And in the PSID, the effect is pronounced only between the scores above 10 and below 10, and not within either of those ranges.

In the Veterans sample men with scores below the 10th percentile have statuses higher than those of men in the 10th to 20th percentile. This may be due simply to sampling error, or to the atypicality of Veterans with very low scores. The largest increments in occupational status in the Veterans sample are across the higher test score intervals.

3. Regressions Stratified by Test Score Level

Table 9 presents the results of regressions of Occupation on Test Score, Education, and socioeconomic background separately for men with different test scores. The results suggest that the effects of ability differences are generally more important for men with higher test scores than for others. This is true both when comparing average effects (i.e., regression coefficients) and when considering the explanatory power of test scores (i.e., $\bar{R}^2$s). In general, the effects of ability arise because more able men--within any given test score interval--get more schooling than less able men. There are some exceptions to this pattern. They are not, however, systematic or consistent. Finally, when similar levels of schooling are compared, the effects of schooling do not seem to vary significantly between men with different ranges of test scores. Apparent interactions between schooling and ability are probably due to nonlinearities in the economic effects of education and differences in mean education between men with varying test scores.

In the PSID, the effect of a 1-point increase in test scores on occupational status is significantly and dramatically lower for men with low test scores than it is for men with high test scores. (The coefficients for Test Score in the PSID have been adjusted on the assumption of a standard deviation of 15 to make them comparable to the results from the other samples.) Among low scorers with equal amounts of education, measured ability differences have no significant effect on occupational status. Controlling ability differences and background, the effects of additional schooling among low scorers are significantly less than among high scorers. This probably does not mean, however, that a given year of schooling confers smaller occupational benefits for low scorers than others. In a representative sample like the PSID, the proportions of men with low scores who have some higher education is likely to be small. Because increments in higher education have larger effects on occupational status than increments in elementary and secondary education, the effects of measured schooling differences among low scorers are small (see Olneck, forthcoming$_a$). My sample sizes are, unfortunately, too small to confidently use nonlinear education terms in analyses that stratify samples.

The proportion of variance in occupational status that is explained by background, test scores, and schooling rises with test scores in the PSID. This is also true in the Veterans, Talent, and Kalamazoo samples. This suggests that variations in family background, ability, and education among low scorers play a smaller role in the attainment process than among individuals in general, and that unmeasured idiosyncratic factors play a correspondingly larger role. The likelihood of interventionist policies that affect cognitive skills and educational attainment having predictable and substantial results for this population consequently is reduced from what it might be if the process were more systematic and determinant.

The Veterans data in Table 9 pertain to veterans 30 to 34 years old. The results show that AFQT differences have significant effects only among men with scores of 103 or higher. Even among high scorers, the effect of AFQT is mediated by educational attainment. The effect of education is significantly less among men with scores less than 95 than among others, but this is probably due to nonlinearities in the effects of education.

In the Talent sample, the coefficient of Test Score is not significantly lower for low scorers than for others. This is what I would expect in a sample that effectively excludes high school dropouts. I assume that ability differences play a smaller role in the process of occupational differentiation for men who quit school than for men who continue.*

The effects of educational differences on occupation in the Talent sample do not vary significantly by test score intervals. This is, also, what I would expect in a sample where the overwhelming bulk of variation in attainment is at the level of higher education. This result supports my interpretation of the differential effects of education in the PSID and Veterans samples being due to nonlinear effects of education, rather than to a real test score-education interaction.

In the Kalamazoo sample, the effects of test score differences on occupational status are smallest among men with low scores. Indeed, they are statistically insignificant. The coefficients for Years of Education do not, however, differ significantly across test score groups.

Except for the Talent sample, then, my results suggest that ability differences are generally less important among men with low scores than among others. The Talent results are probably biased by sample restrictions. They certainly lend no support for the view that the effects of ability differences on occupation are unusually large at the low end of the ability distribution.

4. Regressions Stratified by Father's Occupational Group

Table 10 separates men with white-collar and blue-collar fathers. Although the results are mixed, they tend to suggest that ability advantages are more salient among men from white-collar backgrounds than among blue-collar backgrounds. They do not suggest that

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*In the Kalamazoo sample, the effect of a 1-point test score difference on occupational status for 182 high school dropouts is 0.283 points. Among men with 12 years of schooling (N = 371) it is 0.411 points.

ability differences have greater impact among men with blue-collar fathers.

In both the PSID and Veterans samples, the coefficient of Test Score, controlling measured background, is significantly larger for men with white-collar fathers. The coefficients are virtually identical in the Talent sample. The effect is larger among brothers from white-collar backgrounds than among brothers from blue-collar backgrounds in the Kalamazoo sample, but the difference is not statistically significant. My results, with the exception of Talent, also suggest that ability advantages that are not converted into educational advantages do not have significant effects on occupational status among blue-collar origin respondents. Ability continues to affect occupational status among white-collar origin respondents who have the same amount of education.

Finally, the effects of education on occupational status, controlling ability and family differences, do not differ significantly between white-collar and blue-collar origin respondents in any of my samples.

5. Summary of Effects of Measured Ability on Occupational Status

The results of these analyses are similar to the results with respect to education. The evidence is meager for systematic, consistent, and significant nonlinearities in the effects of measured ability on occupational status. The tendency in the evidence is to suggest that if there are differential effects of ability, they favor those with higher scores rather than those with lower scores.

C. Earnings

I looked at the effects of cognitive ability and schooling on both earnings and the natural logarithm of earnings. Analyzing earnings answers the natural question of "how much" additional ability or schooling is worth in dollars and cents. But my samples often measured earnings for different accounting periods, and the surveys were administered at varying times. Consequently, results differ in part because of inflation, changes in real income, and accounting differences. Moreover, equal increments in income probably do not have the same meaning for recipients at varying points in the income distribution. An additional $10 would seem to be "worth more" to a family making $50 a week than one making $150. The coefficients for variables predicting the natural logarithm of earnings (ln earnings) measure approximately the percentage increases in earnings associated with unit increases in independent variables. They, therefore, permit comparisons across samples, and assess the relative (proportionate) earnings advantages associated with increased schooling and test scores.

1. Effect of Test Score^2

Table 1 shows that the coefficient of Test Score^2 in equations predicting earnings is significant only in the PSID, Kalamazoo, and Rejects samples. It is positive in all three. This means that the dollar increments associated with test score increases are larger for men with higher test scores than for men with lower scores.

For example, the predicted earnings advantage of one brother with a score of 80 over another brother with a score of 70 in the Kalamazoo sample is $[(-296.43)(80-70) + 2.32(80^2-70^2)] = \$515.70$ (Equation 26). The predicted earnings advantage of a brother with a score of 110 over his brother with a score of 100 is $[(-296.43)(110-100) + 2.32(110^2-100^2)] = \$1907.70$. This means that a 10-point difference in test scores has substantially larger effects among higher scorers in the Kalamazoo samples than among lower scorers. Moreover, among individuals with no aspects of family background controlled, the analogous results are \$907 and \$1,875. These results suggest that for low scorers, the observed effects of ability differences on earnings are biased upward when family background is not controlled. However, the effects of ability differences among high scorers persist and at some level rise even when background is controlled. If this finding were to prove generally true, it would mean that observed differences in the effects of ability among lower and higher scorers understate true differences. Larger and more representative samples of brothers are necessary to test this hypothesis.

The coefficient of Test $Score^2$ in equations predicting the natural logarithm of earnings is not significant in any of my samples, indicating that the proportionate effects of ability differences on earnings are similar along the test score distribution.

2. Differences in Means and Standard Deviations by Test Score Intervals

Tables 2-6 show the means and standard deviations of earnings and ln earnings by test score intervals in five samples. The results are somewhat mixed, suggesting the vagaries of sampling error and sample biases. They do not generally suggest that test score differences among low scorers have larger effects than those among high scorers. In this they are consistent with the findings in Table 1, which is to be expected though it is not necessary. Nor do they convincingly suggest that the earnings of low scorers are unusually homogeneous, though some of the results are consistent with this hypothesis. The results do suggest that earnings vary widely among men with similar test scores, and that eliminating differences in earnings between men with very low and somewhat higher scores would do little to make earnings more equal.

In the Talent sample, there are modest differences in earnings across test score intervals, though the variation in earnings within intervals is large relative to the overall variation in earnings in the sample. The largest difference in earnings occurs between men with scores 70 and below, and those with scores 71 to 85. This result is one of the very few in my analyses that remotely suggests that increases in ability might have larger effects for low scorers. It is not confirmed by the Kalamazoo results, however. In the Kalamazoo sample, the smallest difference in mean earnings is between men with scores below 71 and men with scores 71 to 85. Because there are only 18 respondents with scores below 71, perhaps not too much should be made of this comparison.

In the Talent sample, men with scores below 71 have significantly more homogeneous earnings than men with scores 71 to 85. This is also true in the Kalamazoo sample, but similar results do not occur in the other data sets. I suspect that nonresponse by low scorers with very low earnings reduces the observed variance in earnings in the lowest intervals in the Kalamazoo and Talent samples. (Nonresponse of this sort would also cause us to underestimate mean differences in earnings between men with low scores and others.) Because of likely response bias in the Kalamazoo and Talent samples, and the absence of similar patterns in the PSID, Veterans, and Rejects samples, I would be hesitant to conclude that low scorers have systematically more uniform earnings than others.

In the PSID, the largest dollar differences are across the higher test score intervals, though the largest percentage increase is between men with scores of 1 to 5 and 6 to 7. In the Veterans sample, the earnings of men in the 0 to 9th percentiles are anomolously higher than the earnings of men in the 10th to 12th percentiles. The largest consistent differentials are across the three highest intervals.

The Rejects sample is the sample most suited to detecting minimum cognitive levels below which men are at extreme economic disadvantage. The data in Table 6, however, suggest that although improved ability is associated with higher earnings even within the low range of scores represented by this sample, the gains are not dramatic at any point. More importantly, the range of earnings for men with similar scores is very large relative to the range of earnings in general. Eta^2, or the proportion of explained variance, is less than 0.02 for both earnings and ln earnings. Analyses (not shown here) that do not group test score percentiles lead to the same conclusions. My grouping procedures do not obscure any important increments in earnings across levels of scores, nor do they obscure significant patterns of differing variation in earnings by test scores. The Rejects data clearly offer no support for the view that minimum cognitive ability limits can be identified for the purpose of predicting earnings among low-ability young workers. Earnings are virtually random with respect to AFQT in this sample. Perhaps I would find different results if I had a sample of older workers with test scores in this range. None of my other data sets, however, lead me to give this possibility much credence.

3. Regressions Stratified by Test Score Level

Tables 11 and 14 show regression results for earnings and ln earnings stratified by test score level for the PSID, Veterans, Talent, and Kalamazoo samples. Tables 12 and 15 show similar regressions for the Rejects sample.*

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*Because men of any given age with more schooling are likely to have less work experience than men with less schooling, and because experience affects earnings, economists often control experience when they measure the effects of education. The effects of Experience (defined as years of Schooling Minus Age Minus 6) in the PSID are nonlinear. Controlling Experience without controlling $Experience^2$ does not significantly affect the coefficients of other variables. The stratified samples from the PSID that were available to me did not include a measure of $Experience^2$, so I have ignored the issue in my analyses. Experience differentials have insignificant effects on

Looking at the equations without years of schooling included, the results of the analyses of the four samples with reasonably representative ranges of test scores--i.e., PSID, Veterans, Talent, and Kalamazoo--do not show a clear pattern of difference in the effects of ability on earnings according to level of test scores. However, neither do they offer any suggestion that the effects of ability differences are larger among low scorers than among others.

The results from the Rejects sample suggest that the effects of ability differences among military rejects may be smaller than the effects among veterans. Moreover, they suggest that the observed effects are biased upward substantially when family background is not controlled (compare Equations 4 and 5, Table 12).

Turning to the equations including years of schooling, the pattern of results with respect to the effects of education across the samples is consistent with the hypothesis that similar levels of education are associated with similar effects irrespective of ability levels. The apparent differences in the effects of schooling across ability levels are most likely due to nonlinearities in the effects of education.

In most cases, the effects of test scores persist in substantial measure even when education is included in the analyses. This is in contrast to the pattern of largely reduced effects of ability differences on occupation, when education is included. It suggests that men with greater cognitive skills will realize earnings gains even when

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earnings of men 35 to 60 years old, so I ignored experience in the Kalamazoo sample, I controlled Age rather than a constructed experience variable in the Rejects sample because I wanted my results to be comparable to earlier analyses of that sample.

they do not convert such skills into educational credentials. Detailed consideration of these results follows.

In the PSID, ability differences among low scorers have smaller dollar effects than among high scorers, but they have similar or higher percentage effects. Both the absolute and proportionate effects of educational differences among low scorers are smaller than the effects among high scorers. This is probably the result of nonlinearities in the effects of education.

In the Veterans sample, the effect of AFQT differences on both earnings and ln earnings is significant only for men with scores over 103. The effects of education are not, however, significantly lower among low scorers than among others. This may be because the Veterans sample underrepresents men with college educations.

Within test score intervals, differences in test scores have no statistically significant effects in the Talent sample. Differences in education among men with scores less than 90 also have small and insignificant effects. Schooling differences among higher scorers have higher and significant effects.

In the Kalamazoo sample, test score differences are significantly associated with earnings only among men with scores over 110. The effects of schooling, net of ability and background, are considerably higher for men with scores over 100 than for men with lower scores. With ln earnings as the dependent variable, test score differences still have insignificant effects for men with scores below 90, but the proportionate effects of schooling differences are similar for men with scores less than 90 and men with scores over 100.

Although $\bar{R}^2$ varies between samples, it does not vary appreciably between test score groups within any sample. Nor is it ever particularly large.

These results reinforce the conclusions that although there is no clearcut pattern that characterizes all four samples with reasonably representative ranges of test scores, there is also no suggestion that the effects of ability differences favor low scorers.

I first looked at the effects of AFQT and Education for 2,142 respondents in the Rejects sample. I then looked at a subsample of 1,173 men for which I could control a number of background measures. The subsample for which I had background data is not especially advantaged on earnings, nor is the variance in earnings any less than for the larger sample. I was, therefore, surprised to find that the weekly earnings advantage associated with a 1 percentile increase in AFQT is 49 cents in the larger sample, and only 26 cents in the smaller sample. The difference may be due simply to sampling error. It is, however, large enough to make me cautious in interpreting results from the subsample. Those results suggest that the earnings advantages of higher scorers within the 0 to 30th percentile range represented by the Rejects sample, are due largely to the effects of background factors that are correlated with test scores. Controlling measures of urban background, family size, family stability father's education, and race reduces the coefficient of AFQT in Equation 5 in Tables 12 and 15 below the level of statistical significance. The measured effect on earnings is only 0.044/0.257 = 17.1% of the zero-order effect. The measured effect on ln earnings is only 0.00206/0.00594 = 34.7% of the zero-order effect. These results, although surprising, are consistent with my finding in the Kalamazoo data that the effects of test scores at the low end of the distribution on earnings are more sensitive to controls for background than are the effects at the high end of the distribution (Table 1). If this result were to prove generally true, it would mean that policies that failed to identify and influence the background-related correlates of test scores would be unlikely to have even the effects that observed correlations predict.

Analysis of the Rejects subsample also indicate that the effects of additional schooling, net of ability and background, are small and insignificant. To check whether this was true for all levels of schooling, I constructed a dummy variable to represent the effects of graduating from high school. The results (not shown here) do not support the hypothesis of "sheepskin" effect. For men with equal scores and similar backgrounds, dropping out of high school after 11th grade conferred no earnings hardship larger than that predicted by quitting at an earlier point.*

The Rejects sample is the only sample that allows a comparison of whites and nonwhites. There are few nonwhites in the PSID, Veterans, and Talent samples. Those that there are, are very likely atypical. There are virtually no nonwhites in the Kalamazoo sample. The Military Rejects sample is, however, about 50% nonwhite. Although racial comparisons within that sample cannot say much about race differences in the general population, they can say something about the comparative worlds of whites and nonwhites who score low on ability tests and are rejected from military service. Results of separate regressions for whites and nonwhites shown in Tables 12 and 15 suggest that the effects of cognitive differences and schooling on earnings for low-ability men do not differ significantly by race. This conclusion could be sensitive

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*Hansen et al. (1970) report a similar finding for a larger subsample of the Rejects, using a measure of annual earnings. They caution that individuals with the same scores but different amounts of schooling may really be differentially able, and that this could obscure a diploma effect. Ideally, we would want a sample for whom the ability measure is unambiguously prior to the completion of schooling and for which there is significant variation in schooling below the twelfth grade level. The Kalamazoo data satisfy this criteria. There is no significant high school diploma effect in those data. See Olneck (forthcoming$_b$), Table 9B. This may, however, be due to sample response biases.

to controls for background that I omitted to preserve sample size. If the effects of ability or schooling were biased by different amounts, my comparisons would be misleading.

4. Regressions Stratified by Father's Occupational Grouping

Tables 13 and 16 present the results of regressions of earnings and ln earnings separately for men with blue-collar and white-collar fathers. The effects of ability differences do not differ significantly between such men in any of my samples. The results for education are, however, mixed. In some samples, the effects of schooling differ significantly between men with white-collar and blue-collar fathers. But the extent and pattern of difference in educational attainment between such men varies among samples, and this variation could well account for observed differences in the comparative effects of schooling between white-collar and blue-collar origin respondents. Indeed, I interpret my findings across the samples as indicating that the economic value of schooling depends on the level attained, and not on the individual characteristics of graduates.

In the PSID, an extra year of education, net of ability and background, raises earnings by 9.3% for white-collar origin respondents, but only by 5.5% for blue-collar origins. But in the Kalamazoo sample, a one-year difference in schooling, net of brothers' shared background and test scores, is associated with only a 0.8% difference in earnings for white-collar origin respondents, while the same schooling difference is associated with an earnings difference of 4.6% among blue-collar origin respondents. In the Talent and Veterans samples, the coefficients for education do not differ significantly by father's occupational group.

The Talent sample is selected on education, so it is not surprising that schooling differences do not differ in their effects for blue-collar and white-collar origin respondents. Variation in education in both segments of the Talent sample is variation in higher education. The Veterans sample excludes the extremes on education. Since men with unusually low and unusually high attainment are likely to differ on father's occupation, the Veterans sample could understate differences in the effects of education for men with white-collar and blue-collar fathers that derive not from a real interaction, but from nonlinearities in the effects of education.

The PSID results are consistent with the expectation that schooling differences among white-collar origin respondents are more likely to involve variation in higher education than differences among blue-collar origin respondents. Differences in higher education generally have larger effects and are less sensitive to controls for background and ability (see Olneck, forthcoming$_a$) than differences in elementary and secondary education.

The Kalamazoo results are difficult to evaluate. With no other variables controlled, the effect of a one-year difference in schooling on ln earnings is 0.05675 for white-collar origin respondents and 0.06831 for blue-collar origin respondents. As noted above, controlling background common to brothers and sibling test score differences reduces these effects to 0.00790 and 0.04622. Yet, the predicted earnings advantage of a college graduate over a high school graduate is quite similar for each group: 0.15970 for men with white-collar fathers and 0.18370 for men with blue-collar fathers.* This suggests that the difference between the PSID and Kalamazoo results are due to peculiarities in the distribution of education in the Kalamazoo sample and not to any unusual interaction between background and schooling.

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*Calculated from Equation 7, Table 4.13 in Olneck (1976).

5. Summary of Effects of Ability on Earnings and Ln Earnings

On the average, the effects of test score differences in dollar terms tend to be larger the higher the level of test scores. There is, however, no uniform pattern of increasing effects that is consistent across samples. The percentage effects of test score differences do not vary significantly between test score levels.

The observed effects of test score differences among low scorers appear to be unusually biased by the effects of family background on scores and earnings. Ignoring this will lead analysts to understate the differences in effects between low and high scorers.

There is no evidence of a definable minimum level of cognitive skill below which earnings fall dramatically.

V COMPARATIVE RESULTS AND CONCLUSIONS

Despite intense interest among both social scientists and policymakers in the economic benefits of academic achievement and educational attainment, our knowledge about either is quite limited. This is because we simply do not have adequate data bases with which to explore the complicated questions these issues raise. Our samples are often missing key variables, or are too small and unrepresentative to allow anything but cautious generalization. Strongly held opinions about these matters usually follow from theoretical or political predilections, or from understandable attachment to one's own data set.

I looked at my samples with the hope of finding evidence that increments in cognitive ability or schooling have larger economic effects among men with lower ability or lower social origins than men in general. I found no such evidence. Instead, I found that either the effects of ability and schooling are similar for men with varying characteristics, or that larger benefits accrue to men who are already advantaged. Although each of my samples is seriously flawed, the consistency of my results is striking.

My results are also consistent with previous research. Hauser (1973) divided respondents in the 1962 Occupational Changes in a Generation (see Blau and Duncan, 1967) according to their father's occupational status. He then ran separate regressions of respondent's occupation and ln income on background and schooling. He found no evidence of strong or consistent differences in the effects of schooling for men with high-status or low-status fathers. Hauser also looked for evidence of background-schooling interactions in a sample of 1957 Wisconsin high school seniors for whom 1964 occupational data and 1967 earnings data were available. He found no such evidence.

Despite the consistency of my results, we will not confidently know the measured effects of ability and schooling differences among low-ability individuals unless, and until, we adequately survey a large sample of such men who are old enough to have stable patterns of workforce participation and earnings. A follow-up of the Rejects sample whose respondents are now 30 to 38 years old might be recommended.

If, however, future research confirms my results, it will provide little support for those who view compensatory education as a potent instrument for extending economic opportunity to the disadvantaged. The connections between measured ability, schooling, and economic success, especially in the population of concern, are simply too weak to suggest otherwise. Educators concerned with finding ways to help children learn should not be concerned by these conclusions. Policymakers, for whom educational programs are but one alternative among several strategies, probably should seek remedies that are more directly relevant to the causes of persisting poverty and inequality than are the presumed intellectual deficiencies of the poor.

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Table 1

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR SEPARATE REGRESSIONS OF EDUCATION, OCCUPATION, EARNINGS, AND Ln EARNINGS ON COGNITIVE TEST SCORES IN FIVE DATA SETS

Dependent Variable	Data Set	Equation No.	Test Score	Test Score^2	$\bar{R}^2$
Education	Talent 28-year-olds (n = 839)	1.	0.094*		0.314
			(0.005)		
		2.	-0.126*	0.0011*	0.330
			(0.049)	(0.0003)	
	Kalamazoo brothers (n = 730, or 365 pairs)	3.	0.1020*		0.330
			(0.0054)		
		4.	-0.0002	0.0005	0.333
			(0.0520)	(0.0003)	
		5.†	0.0584*		0.607‡
			(0.0080)		
		6.†	-0.0262	0.0004	0.608‡
			(0.0628)	(0.0003)	
Occupation	PSID (n = 1774)	7.	0.5031*§		0.128
			(0.0312)		
		8.	-0.3906*§	0.0496*§	0.140
			(0.1809)	(0.0099)	
	NORC veterans (n = 1438)	9.	0.6177*		0.136
			(0.0411)		
		10.	0.1205	0.0024	0.136
			(0.6021)	(0.0028)	
	Talent 28-year-olds (n = 839)	11.	0.771*		0.225
			(0.049)		
		12.	0.646	0.0006	0.225
			(0.516)	(0.0026)	
	Kalamazoo brothers (n = 730, or 365 pairs)	13.	0.6955*		0.209
			(0.0501)		
		14.	0.07455	0.00303	0.210
			(0.48439)	(0.00235)	
		15.†	0.46500*		0.338‡
			(0.08899)		
		16.	-0.21232	0.00333	0.338‡
			(0.69941)	(0.00341)	
Earnings	PSID (n = 1774; y = 1971 annual earnings)	17.	173.84*§		0.114
			(11.53)		
		18.	-67.83§	13.40*§	0.120
			(67.15)	(3.67)	
	NORC veterans (n = 1438; y = 1964 expected annual earnings)	19.	61.69*		0.0791
			(5.56)		
		20.	66.40	-0.02	0.0791
			(81.34)	(0.38)	

Table 1 (Concluded)

Dependent Variable	Data Set	Equation No.	Test Score	Test Score^2	R^2
	Talent 28-year-olds (n = 839; y = 1972 hourly earnings)	21.	0.029*		0.041
			(0.005)		
		22.	0.054	-0.0001	0.041
			(0.050)	(0.0003)	
	Kalamazoo brothers (n = 750, or 365 pairs; y = 1973 expected annual earnings)	23.	179.13*		0.129
			(17.24)		
		24.	-152.74	1.62*	0.133
			(166.31)	(0.81)	
		25.+	174.77*		0.295‡
			(30.12)		
		26.+	-296.43	2.32*	0.301‡
			(235.70)	(1.15)	
	Military rejects (n = 2099; y = 1963 weekly earnings)	27.	0.4806*		0.014
			(0.0874)		
		28.	-0.06866	0.02554*	0.017
			(0.25944)	(0.01136)	
Ln Earnings	PSID (n = 1774)	29.	0.01773*§		0.125
			(0.00112)		
		30.	0.02139*	-0.00020	0.125
			(0.00652)	(0.00036)	
	NORC veterans (n = 1438)	31.	0.0096*		0.0691
			(0.0009)		
			0.0260*	-0.0001	0.070
			(0.0136)	(0.0001)	
	Talent 28-year-olds (n = 839)	32.	0.006*		0.041
			(0.001)		
		33.	0.013	-0.00004	0.042
			(0.010)	(0.00005)	
	Kalamazoo brothers (n = 692)	34.	0.01105*		0.134
			(0.00105)		
		35.	-0.00210	0.00006	0.134
			(0.01010)	(0.00005)	
		36.+	0.01119*		0.272‡
			(0.00186)		
		37.+	-0.01357	0.00012	0.280‡
			(0.01456)	(0.00007)	
	Military rejects (n = 2099)	38.	0.01062*		0.016
			(0.00178)		
		39.	0.00535	0.00024	0.016
			(0.00530)	(0.00023)	

Key to Symbols

* $p < 0.05$.

+ These are regressions of sibling differences.

‡ Includes variance explained by family background common to brothers as well as by test scores.

§ PSID coefficients corrected for scale differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1.954) to 15 (0.13027).

Table 2

MEANS AND STANDARD DEVIATIONS OF EDUCATION, OCCUPATION, HOURLY EARNINGS, AND Ln HOURLY EARNINGS BY TEST SCORE INTERVALS IN THE TALENT 28-YEAR-OLD SAMPLE

		Education		Occupation		Hourly Earnings		Ln Hourly Earnings	
Test Score	N	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Less than 70	37	12.22	1.48	28.09	20.15	4.01	1.71	1.293	0.471
71-85	179	12.54	1.42	31.55	17.13	4.82	2.38	1.473	0.453
86-100	398	13.15	1.84	43.07	22.51	5.00	2.51	1.507	0.471
101-115	425	14.57	2.23	55.17	21.32	5.49	2.11	1.635	0.372
116-130	233	16.20	2.24	64.63	20.36	5.69	2.20	1.659	0.432
Greater than 130	12	18.00	2.37	69.17	22.76	5.32	2.48	1.558	0.526
Not available	85	13.79	2.38	48.13	20.86	4.96	1.86	1.520	0.440
Total	1,369	14.09	2.39	49.44	23.59	5.24	2.29	1.567	0.438
Eta^2		0.287		0.213		0.026		0.036	
Number reporting dependent variable		1,360		1,250		1,207		1,207	

Table 3

MEANS AND STANDARD DEVIATIONS OF EDUCATION, OCCUPATION, EARNINGS, AND Ln EARNINGS BY TEST SCORE INTERVALS IN THE KALAMAZOO BROTHERS SAMPLE

		Education		Occupation		Earnings		Ln Earnings	
Test Score	N	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Less than 70	18	9.89	1.68	28.12	17.55	11,382	3,421	9.29	0.314
71-85	204	11.22	1.74	35.94	21.38	12,986	6,181	9.36	0.532
86-100	424	12.33	2.06	45.20	22.11	15,304	7,035	9.53	0.454
101-115	401	13.53	2.51	55.33	21.11	17,944	7,672	9.71	0.398
116-130	152	14.99	2.85	61.53	20.46	20,079	8,094	9.83	0.386
Greater than 130	38	16.76	2.37	67.74	20.93	23,734	8,935	9.99	0.440
Not available	6	12.67	3.33	38.67	23.41	17,167	9,647	9.63	0.525
Total	1,243	12.96	2.65	49.38	23.19	16,592	7,723	9.61	0.469
Eta^2		0.260		0.154		0.111		0.123	
Number reporting dependent variable		1,237		1,220		1,115		1,115	

Table 4

MEANS AND STANDARD DEVIATIONS OF OCCUPATION, EARNINGS, AND Ln EARNINGS BY TEST SCORE INTERVALS IN THE PSID SAMPLE

Test Score	N	Occupation Mean	Occupation S.D.	Earnings Mean	Earnings S.D.	Ln Earnings Mean	Ln Earnings S.D.
1-5	74	26.75	16.31	5,664	3,989	8.175	1.362
6-7	145	28.40	15.56	7,148	3,869	8.722	0.603
8	208	32.26	16.75	8,315	4,483	8.807	0.802
9	305	34.05	18.26	9,790	5,172	9.012	0.724
10	493	38.52	20.51	10,920	5,855	9.130	0.725
11	461	45.87	21.48	12,906	7,064	9.305	0.633
12	311	47.82	20.49	14,589	9,875	9.414	0.611
13	126	58.10	19.70	16,365	13,475	9.478	0.709
Not available	244	37.09	20.83	11,483	9,031	9.097	0.754
Total	2,366	39.99	21.09	11,367	7,783	9.118	0.772
Eta^2		0.128		0.106		0.122	
Number reporting dependent variable		2,359		2,351		2,351	

Table 5

MEANS AND STANDARD DEVIATIONS OF OCCUPATION, EARNINGS, AND Ln EARNINGS BY AFQT PERCENTILE IN THE NORC VETERANS SAMPLE

AFQT Percentile	N	Occupation Mean	Occupation S.D.	Earnings Mean	Earnings S.D.	Ln Earnings Mean	Ln Earnings S.D.
0-9	21	24.45	14.79	6,040	3,559	8.589	0.476
10-12	63	21.92	14.91	4,613	2,048	8.290	0.643
13-20	157	27.97	17.91	5,364	2,827	8.415	0.675
21-30	233	29.65	17.81	5,697	2,376	8.552	0.489
31-49	450	34.51	20.67	6,118	2,734	8.629	0.444
50-64	271	37.55	21.12	6,342	3,148	8.647	0.481
65-92	472	46.24	24.61	7,248	2,933	8.795	0.481
93-100	148	55.59	22.50	8,190	3,710	8.911	0.474
Total	1,815	38.06	22.89	6,443	3,019	8.657	0.519
Eta^2		0.143		0.081		0.081	
Number reporting dependent variable		1,769		1,779		1,779	

Table 6

MEANS AND STANDARD DEVIATIONS OF EARNINGS AND Ln EARNINGS BY AFQT PERCENTILE IN THE MILITARY REJECTS SAMPLE

AFQT Percentile	N	Earnings Mean	Earnings S.D.	Ln Earnings Mean	Ln Earnings S.D.
0-4	708	52.30	28.73	3.808	0.596
5-9	647	52.88	25.48	3.850	0.526
10-14	482	54.75	25.71	3.875	0.562
15-19	376	59.08	24.98	3.970	0.524
20-24	115	64.54	31.91	4.051	0.498
25-30	48	66.90	33.19	4.072	0.563
32	1	38.00	0	3.638	0
Total	2,377	54.91	27.17	3.876	0.558
Eta^2		0.018		0.017	
Number reporting dependent variable		2,377		2,377	

Table 7

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF EDUCATION ON COGNITIVE TEST SCORES FOR MEN STRATIFIED BY TEST SCORE INTERVALS

Sample		Independent Variable: Test Score	$\bar{R}^{2*}$	Other Variables Controlled
Talent 11-year follow-up				
Less than 90 (N = 173)	1.	0.047† (0.015)	0.176	Measured background‡
90 to 110 (N = 395)	2.	0.118† (0.019)	0.093	Measured background‡
Over 110 (N = 271)	3.	0.121† (0.022)	0.238	Measured background‡
Kalamazoo brothers				
Less than 90 (N = 218)	4.	0.056† (0.017)	0.183	Measured background§
90 to 99 (N = 205)	5.	0.086 (0.047)	0.164	Measured background§
100 to 109 (N = 207)	6.	0.029 (0.053)	0.075	Measured background§
110 and over (N = 191)	7.	0.111† (0.022)	0.327	Measured background§

*Variance explained, adjusted for degrees of freedom.

†$p < 0.05$.

‡Race, father's education, father's occupation, broken family, siblings.

§Father's education, father's occupation, siblings.

Table 8

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF EDUCATION ON COGNITIVE TEST SCORES FOR MEN STRATIFIED BY FATHER'S OCCUPATIONAL GROUP

Sample		Independent Variable Test Score	$\bar{R}^{2*}$	Other Variables Controlled
Talent 11-year follow-up				
Father blue-collar (N = 448)	1.	0.075† (0.007)	0.232	Measured background‡
Father white-collar (N = 315)	2.	0.092† (0.009)	0.369	Measured background‡
Kalamazoo brothers				
Father blue-collar (N = 207 pairs)	3.	0.061† (0.011)	0.447	Shared family background§
Father white-collar (N = 139 pairs)	4.	0.056† (0.013)	0.635	Shared family background§

*Variance explained adjusted for degrees of freedom.

†$p < 0.05$.

‡Race, father's education, father's occupation, broken family, siblings.

§Variables defined as sibling differences for computational purposes. $\bar{R}^2$ is a proportion of variance explained for individual values controlling background factors common to brothers and test score differences within families. The observed standard error of residuals for sibling differences is first multiplied by 0.5 to equate it to the standard deviation of the deviations of individual scores from pair means, and then multiplied by 1.414 to correct for degrees of freedom. The calculated error variance divided by the observed total variance of individual values is $\bar{R}^2$.

Table 9

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF CURRENT OCCUPATIONAL STATUS ON COGNITIVE TEST SCORES AND SCHOOLING FOR MEN STRATIFIED BY TEST SCORE INTERVALS

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2*}$	Other Variables Controlled
Michigan panel study					
1 to 9 (N = 764)	1.	0.118†‡ (0.055)		0.083	Measured background§
	2.	0-.065†‡ (0.403)	2.576† (0.204)	0.242	Measured background§
10 to 11 (N = 707)	3.	0.618†‡ (0.195)		0.139	Measured background§
	4.	0.580†‡ (0.169)	3.763† (0.244)	0.357	Measured background§
12 to 13 (N = 303)	5.	1.358†‡ (0.321)		0.145	Measured background§
	6.	0.878†‡ (0.046)	4.160* (0.355)	0.417	Measured background§
NORC veterans 30 to 34					
Less than 95 (N = 236)	7.	0.417 (0.304)		0.028	Measured background**
	8.	0.023 (0.317)	1.904† (0.535)	0.076	Measured background**
96 to 102 (N = 264)	9.	0.569 (0.358)		0.137	Measured background**
	10.	0.182 (0.308)	4.865† (0.493)	0.373	Measured background**
103 and over (N = 303)	11.	0.701† (0.198)		0.159	Measured background**
	12.	0.178 (0.178)	5.167† (0.507)	0.376	Measured background**

Table 9 (Concluded)

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^2$*	Other Variables Controlled
Talent 11-year follow-up					
Less than 90 (N = 173)	13.	0.753† (0.202)		0.092	Measured background††
	14.	0.501† (0.192)	5.383† (0.987)	0.226	Measured background††
90 to 110 (N = 395)	15.	1.078† (0.203)		0.096	Measured background††
	16.	0.549† (0.183)	5.244* (0.469)	0.315	Measured background††
Over 110 (N = 271)	17.	0.816† (0.228)		0.141	Measured background††
	18.	0.137 (0.200)	5.619† (0.521)	0.402	Measured background††
Kalamazoo brothers					
Less than 90 (N = 218)	19.	0.409 (0.228)		0.056	Measured background‡‡
	20.	0.164 (0.222)	4.399† (0.877)	0.152	Measured background‡‡
90 to 99 (N = 205)	21.	1.135† (0.552)		0.051	Measured background‡‡
	22.	0.791 (0.528)	3.989† (0.782)	0.157	Measured background‡‡
100 to 109 (N = 207)	23.	1.438† (0.534)		0.067	Measured background‡‡
	24.	1.309† (0.481)	4.388† (0.634)	0.243	Measured background‡‡
110 and over (N = 191)	25.	0.554† (0.152)		0.109	Measured background‡‡
	26.	0.145 (0.138)	3.689† (0.438)	0.353	Measured background‡‡

*Variance explained adjusted for degrees of freedom.

†$p < 0.05$.

‡PSID coefficients corrected for scale differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1-954) to 15 (0.13027).

§Race, father's education, father's occupation, father foreign born, broken family, siblings, nonfarm background, non-South background.

**Race, father's education, father's occupation, broken family, nonfarm background, Non-South background.

††Race, father's education, father's occupation, broken family, siblings.

‡‡Father's education, father's occupation, siblings.

Table 10

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF CURRENT OCCUPATIONAL STATUS ON COGNITIVE TEST SCORES AND SCHOOLING FOR MEN STRATIFIED BY FATHER'S OCCUPATIONAL GROUP

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2}$*	Other Variables Controlled
Michigan panel study					
Father blue-collar (N = 862)	1.	0.323†‡ (0.048)		0.171	Measured background§
	2.	0.080‡ (0.046)	3.379† (0.227)	0.341	Measured background§
Father white-collar (N = 329)	3.	0.523†‡ (0.079)		0.146	Measured background§
	4.	0.236†‡ (0.070)	4.121† (0.347)	0.404	Measured background§
NORC veterans					
Father blue-collar (N = 415)	5.	0.522† (0.077)		0.181	Measured background**
	6.	0.150 (0.077)	4.309† (0.415)	0.352	Measured background**
Father white-collar (N = 153)	7.	0.848† (0.130)		0.234	Measured background**
	8.	0.330† (0.128)	5.189† (0.665)	0.458	Measured background**
Talent 11-year follow-up					
Father blue-collar (N = 448)	9.	0.673† (0.074)		0.174	Measured background††
	10.	0.292† (0.073)	5.093† (0.449)	0.360	Measured background††
Father white-collar (N = 315)	11.	0.672† (0.093)		0.202	Measured background††
	12.	0.190† (0.094)	5.230† (0.518)	0.399	Measured background††
Kalamazoo brothers					
Father blue-collar (N = 207 pairs)	13.	0.389† (0.117)		0.331	Shared family background‡‡
	14.	0.165 (0.119)	3.685† (0.731)	0.402	Shared family background‡‡
Father white-collar (N = 139 pairs)	15.	0.513† (0.141)		0.297	Shared family background‡‡
	16.	0.331† (0.144)	3.213† (0.867)	0.357	Shared family background‡‡

*Variance explained, adjusted for degrees of freedom.

†$p < 0.05$.

‡PSID coefficients corrected for scale differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1.954) to 15 (0.13027).

§Race, father's education, father's occupation, father foreign born, broken family, siblings, nonfarm background, non-South background.

**Race, father's education, father's occupation, broken family, nonfarm background, non-South background.

††Race, father's education, father's occupation, broken family, siblings.

‡‡Variables defined as sibling differences (see Note §, Table 8).

Table 11

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF EARNINGS ON COGNITIVE TEST SCORES AND SCHOOLING FOR MEN STRATIFIED BY TEST SCORE INTERVALS

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2*}$	Other Variables Controlled
Michigan panel study					
1 to 9 (N = 764)	1.	84.76†‡ (14.93)		0.131	Measured background§
	2.	51.08†‡ (14.31)	274.92† (58.04)	0.201	Measured background§
10 to 11 (N = 707)	3.	170.01†‡ (62.76)		0.079	Measured background§
	4.	162.17†‡ (59.45)	774.03† (86.07)	0.174	Measured background§
12 to 13 (N = 303)	5.	200.91‡ (183.16)		0.052	Measured background§
	6.	41.24‡ (175.28)	1384.17† (232.37)	0.152	Measured background§
NORC veterans					
Less than 95 (N = 236)	7.	36.75 (39.72)		0.066	Measured background**
	8.	24.97 (42.43)	56.90 (71.73)	0.065	Measured background**
	9.	34.90 (41.52)	473.08† (138.09)	0.109	Measured background, experience
96 to 102 (N = 264)	10.	93.97 (48.89)		0.120	Measured background**
	11.	73.20 (48.32)	261.02† (77.48)	0.154	Measured background**
	12.	73.66 (48.23)	74.03 (152.19)	0.157	Measured background, experience**
103 and over (N = 303)	13.	114.60† (29.25)		0.087	Measured background**
	14.	97.16† (30.40)	172.39† (86.75)	0.096	Measured background**
	15.	89.35† (30.47)	467.16† (166.95)	0.106	Measured background,** experience

Table 11 (Continued)

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2*}$	Other Variables Controlled
Project talent 11-year follow-up (Dependent variable is hourly earnings)					
Less than 90 (N = 173)	16.	0.014 (0.019)		0.006	Measured background‡‡
	17.	0.008 (0.020)	0.124 (0.100)	0.009	Measured background‡‡
	18.	0.004 (0.020)	0.046 (0.114)	0.011	Measured background,‡‡ grades, experience
90 to 110 (N = 395)	19.	0.036 (0.018)		0.022	Measured background‡‡
	20.	0.020 (0.019)	0.155† (0.048)	0.045	Measured background‡‡
	21.	0.020 (0.019)	0.254† (0.058)	0.066	Measured background,‡‡ grades, experience
Over 110 (N = 271)	22.	-0.010 (0.026)		0.024	Measured background‡‡
	23.	-0.027 (0.027)	0.145† (0.071)	0.036	Measured background‡‡
	24.	-0.029 (0.028)	0.305† (0.096)	0.062	Measured background,‡‡ grades, experience
Kalamazoo Brothers					
Less than 90 (N = 218)	25.	95.00 (61.42)		0.028	Measured background‡‡
	26.	63.92 (62.38)	556.69† (246.82)	0.046	Measured background‡‡
90 to 99 (N = 205)	27.	89.57 (188.49)		0.019	Measured background‡‡
	28.	37.75 (188.34)	601.30† (297.18)	0.037	Measured background‡‡
100 to 109 (N = 207)	29.	333.59 (171.46)		0.041	Measured background‡‡
	30.	304.58 (163.72)	285.40† (215.59)	0.127	Measured background‡‡

Table 11 (Concluded)

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^2$*	Other Variables Controlled
110 and over (N = 191)	31.	166.56† (63.02)		0.050	Measured background‡‡
	32.	58.25 (63.53)	978.46† (201.22)	0.153	Measured background‡

*Variance explained, adjusted for degrees of freedom.

†$p < 0.05$.

‡PSID coefficients corrected for scate differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1.954) to 15 (0.13027).

§Race, father's education, father's occupation, father foreign born, broken family, siblings, nonfarm background, non-South background.

**Race, father's education, father's occupation, broken family, nonfarm background, non-South background.

††Race, father's education, father's occupation, broken family, siblings.

‡‡Father's education, father's occupation, siblings.

Table 12

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF WEEKLY EARNINGS ON AFQT AND EDUCATION IN THE MILITARY REJECT SAMPLE

Subsample		Independent Variables		$\bar{R}^{2*}$	Other Variables Controlled
		AFQT	Education		
2142 men with data on AFQT, age, education, and earnings	1.	0.4912† (0.0871)		0.01418	None
	2.	0.51315† (0.09353)	-0.17549 (0.27232)	0.01391	None
	3.	0.61260 (0.09173)	-0.15498 (0.26565)	0.06167	Age
1173 men with data on AFQT, age, education, earnings, and background	4.	0.25697+ (0.10586)		0.00416	None
	5.	0.04397 (0.10563)		0.07592	Background‡
	6.	0.02165 (0.10895)	-0.36537 (0.40142)	0.07579	Background‡
	7.	0.10924 (0.10634)	-0.33534 (0.39143)	0.12129	Background, age‡
1150 nonwhites	8.	0.51113+ (0.10632)		0.01888	None
	9.	0.44640+ (0.11669)	0.42991 (0.31991)	0.01957	None
	10.	0.52298† (0.11493)	0.36106 (0.31377)	0.05784	Age
991 whites	11.	0.31177+ (0.13327)		0.00450	None
	12.	0.20819 (0.14394)	0.86937 (0.46005)	0.00708	None
	13.	0.34971† (0.14229)	0.84721 (0.44989)	0.05051	Age

*Variance explained adjusted for degrees of freedom.

+ $p < 0.05$.

‡Urban background, siblings, parents not divorced, father's education and race.

Table 13

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF EARNINGS ON COGNITIVE TEST SCORES AND SCHOOLING FOR MEN STRATIFIED BY FATHER'S OCCUPATIONAL GROUP

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2*}$	Other Variables Controlled
Michigan panel study					
Father blue-collar (N = 862)	1.	110.45‡ (13.94)		0.172	Measured background§
	2.	63.12‡ (14.22)	656.92† (70.41)	0.248	Measured background§
Father white-collar (N = 329)	3.	162.13‡ (38.43)		0.109	Measured background§
	4.	79.24‡ (38.72)	1187.76† (191.19)	0.203	Measured background§
NORC veterans					
Father blue-collar (N = 415)	5.	64.49† (9.29)		0.182	Measured background**
	6.	59.31† (10.49)	60.04 (56.46)	0.182	Measured background**
	7.	58.43† (10.46)	251.41† (109.91)	0.188	Measured background, experience**
Father white-collar (N = 153)	8.	86.14† (22.03)		0.124	Measured background**
	9.	57.46† (25.45)	286.96† (132.22)	0.146	Measured background**
	10.	58.26† (25.63)	213.68 (247.67)	0.140	Measured background, experience**
Talent 11-year follow-up (Dependant variable is hourly earnings)					
Father blue-Collar (N = 448)	11.	0.017† (0.007)		0.021	Measured background††
	12.	0.007 (0.008)	0.127† (0.047)	0.035	Measured background††
	13.	0.007 (0.008)	0.175* (0.056)	0.038	Measured background, grades, experience††

Table 13 (Concluded)

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^2$	Other Variables Controlled
Father white-collar (N = 315)	14.	0.028† (0.010)		0.040	Measured background††
	15.	0.014 (0.012)	0.152† (0.064)	0.054	Measured background††
	16.	0.018 (0.012)	0.321† (0.085)	0.076	Measured background,†† grades, experience
Kalamazoo brothers					
Father blue-collar (N = 207 pairs)	17.	150.27* (37.06)		0.154	Shared family background‡‡
	18.	94.35† (38.64)	918.72† (236.70)	0.209	Shared family background‡‡
Father white-collar (N = 139 pairs)	19.	202.24† (54.23)		0.343	Shared family background‡‡
	20.	194.31 (57.84)	140.59 (348.83)	0.339	Shared family background‡‡

*Variance explained adjusted for degrees of freedom.

†$p < 0.05$.

‡PSID coefficients corrected for scale differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1.954) to 15 (0.13027).

§Race, father's education, father's occupation, father foreign born, broken family, siblings, nonform background, non-South background.

**Race, father's education, father's occupation, broken family, nonfarm background, non-South background.

††Race, father's education, father's occupation, broken family, siblings.

Variables defined as sibling differences. See Note †, Table 8.

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Table 14

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF THE Ln EARNINGS ON COGNITIVE TEST SCORES AND SCHOOLING FOR MEN STRATIFIED BY TEST SCORE INTERVALS

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2*}$	Other Variables Controlled
Michigan panel study					
1 to 9 (N = 764)	1.	0.01414†‡ (0.00202)		0.105	Measured background§
	2.	0.00764†‡ (0.00258)	0.06355† (0.01006)	0.149	Measured background§
10 to 11 (N = 707)	3.	0.01416†‡ (0.00634)		0.089	Measured background§
	4.	0.01340†‡ (0.00603)	0.07498† (0.00873)	0.175	Measured background§
12 to 13 (N = 303)	5.	0.00418 (0.01085)		0.031	Measured background§
	6.	-0.00680 (0.01016)	0.09514† (0.01347)	0.170	Measured background§
NORC veterans					
Less than 95 (N = 236)	7.	0.00711 (0.00862)		0.069	Measured background**
	8.	0.00545 (0.00922)	0.00803 (0.01558)	0.066	Measured background**
	9.	0.00768 (0.00900)	0.10150† (0.02994)	0.114	Measured background, experience**
96 to 102 (N = 264)	10.	0.01176 (0.00775)		0.087	Measured background**
	11.	0.00802 (0.00761)	0.04701† (0.01220)	0.134	Measured background**
	12.	0.00811 (0.00758)	0.01242 (0.02393)	0.140	Measured background, experience**
103 and over (N = 303)	13.	0.01366† (0.00332)		0.131	Measured background**
	14.	0.01205† (0.00398)	0.01591 (0.01137)	0.134	Measured background**
	15.	0.01111† (0.00400)	0.05155† (0.0219)	0.142	Measured background, experience**
Talent 11-year follow-up					
Less than 90 (N = 173)	16.	0.00296 (0.00415)		0.009	Measured background††
	17.	0.00156 (0.00427)	0.02989 (0.02194)	0.015	Measured background††
	18.	0.00077 (0.00431)	0.01609 (0.02493)	0.014	Measured background, grades, experience††

Table 14 (Concluded)

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^2$*	Other Variables Controlled
90 to 110 (N = 395)	19.	0.00851† (0.00351)		0.024	Measured background††
	20.	0.00541 (0.00359)	0.03078* (0.00918)	0.049	Measured background††
	21.	0.00536 (0.00355)	0.05292† (0.01095)	0.082	Measured background†† grades, experience
Over 110 (N = 271)	22.	-0.00195) (0.00469)		0.008	Measured background††
	23.	-0.00344 (0.00495)	0.01241† (0.01288)	0.007	Measured background††
	24.	-0.00277 (0.00510)	0.04653† (0.01743)	0.037	Measured background†† grades, experience
Kalamazoo brothers					
Less than 90 (N = 218)	25.	0.00627 (0.00462)		0.021	Measured background‡‡
	26.	0.00379 (0.00468)	0.04436† (0.01853)	0.042	Measured background‡‡
90 to 99 (N = 205)	27.	0.00002 (0.01227)		0.005	Measured background‡‡
	28.	-0.00190 (0.01236)	0.02233 (0.01832)	0.007	Measured background‡‡
100 to 109 (N = 207)	29.	0.02185† (0.00941)		0.047	Measured background‡‡
	30.	0.02030† (0.00901)	0.05269† (0.01187)	0.128	Measured background‡‡
110 and over (N = 191)	31.	0.00775† (0.00307)		0.047	Measured background‡‡
	32.	0.00239 (0.00309)	0.04841† (0.00978)	0.154	Measured background‡‡

*Variance explained adjusted for degrees of freedom.

†$p < 0.05$.

‡PSID coefficients corrected for scale differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1.954) to 15 (0.13027).

§Race, father's education, father's occupation, father foreign born, broken family, siblings, nonfarm background, non-South background.

**Race, father's education, father's occupation, broken family, nonfarm background, non-South background.

††Race, father's education, father's occupation, broken family, siblings.

‡‡Father's education, father's occupation, siblings.

Table 15

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF THE Ln WEEKLY EARNINGS ON AFQT AND EDUCATION IN THE MILITARY REJECTS SAMPLE

Subsample		Independent Variables: AFQT	Independent Variables: Education	$\bar{R}^{2*}$	Other Variables Controlled
2142 men with data on AFQT, age, education, and earnings	1.	0.01103† (0.00177)		0.01738	None
	2.	0.01070† (0.0019)	0.00262 (0.00553)	0.01702	None
	3.	0.01272† (0.00186)	0.07746† (0.00738)	0.06470	Age
1173 men with data on AFQT, age, education, earnings, and background	4.	0.00594† (0.00200)		0.00662	None
	5.	0.00206 (0.00201)		0.0666	Background‡
	6.	0.00150 (0.00206)	0.00911 (0.00763)	0.06694	Background‡
	7.	0.00316 (0.00202)	0.00854 (0.00744)	0.11238	Background, age‡
1150 nonwhites	8.	0.01079† (0.00245)		0.01576	None
	9.	0.00873† (0.00269)	0.01363 (0.00736)	0.01784	None
	10.	0.01070† (0.00263)	0.01186 (0.00719)	0.06572	Age
991 whites	11.	0.00863† (0.00252)		0.01076	None
	12.	0.00636† (0.00272)	0.01912† (0.00868)	0.01460	None
	13.	0.00876† (0.00270)	0.01874† (0.00853)	0.04934	Age

† $p < 0.05$.

* Variance explained adjusted for degrees of freedom.

‡ Urban background, siblings, parents not divorced, father's education, race.

Table 16

UNSTANDARDIZED COEFFICIENTS, THEIR STANDARD ERRORS AND $\bar{R}^2$ FOR REGRESSIONS OF THE Ln EARNINGS ON COGNITIVE TEST SCORES AND SCHOOLING FOR MEN STRATIFIED BY FATHER'S OCCUPATIONAL GROUP

Sample		Independent Variables: Test Score	Independent Variables: Years of Schooling	$\bar{R}^{2*}$	Other Variables Controlled
Michigan panel study					
Father blue-collar (N = 862)	1.	0.01129•‡ (0.00177)		0.161	Measured background§
	2.	0.00731•‡ (0.00178)	0.05518• (0.00919)	0.194	Measured background§
Father white-collar (N = 329)	3.	0.01053•‡ (0.00284)			Measured background§
	4.	0.00401‡ (0 00284)	0.09346• (0.01400)	0.188	Measured background§
NORC veterans					
Father blue-collar (N = 415)	5	0.00890• (0.00157)		0.182	Measured background**
	6.	0.00776• (0.00178)	0.01325 (0.00956)	0.1[illegible]	Measured background**
	7.	0.00768• (0.00178)	0.03032 (0.01868)	0.184	Measured background**
Father white-collar (N = 153)	8.	0.01132• (0.00232)		0 196	Measured background**
	9.	0.00757• (0.00266)	0.03761• (0.01383)	0.230	Measured background**
	10.	0 00756• (0.00268)	0.03827 (0.02591)	0.225	Measured background, experience**
Talent 11-year follow-up					
Father blue-collar (N = 448)	11.	0.00377• (0.00130)		0 021	Measured background••
	12.	0 00201 (0.00145)	0.02354• (0.00892)	0.034	Measured background••
	13.	0 00183 (0 00149)	0.03644• (0.01056)	0 043	Measured background, experience, grades••
Father white-collar (N = 315)	14.	0.00401• (0.00187)		0.042	Measured background••
	15.	0.00243 (0.00217)	0.01714 (0.01195)	0.046	Measured background••
	16.	0.0033 (0.00228)	0.05325• (0.01588)	0.076	Measured background, experience, grades••
Kalamazoo brothers					
Father blue-collar (N = 207 pairs)	17.	0.00879• (0.00211)		0.235	Shared family background‡‡
	18.	0.00598• (0.00221)	0 04622• (0.01355)	0.272	Shared family background‡‡
Father white-collar (N = 139 pairs)	19.	0.01321• (0.00332)		0.266	Shared family background‡‡
	20	0.01276• (0.00354)	0.00790 (0.02137)	0 262	Shared family background‡‡

* Variance explained, adjusted for degree of freedom.

• $p < 0.05$.

‡ PSID coefficients corrected for scale differences by multiplying observed coefficients by the ratio of the PSID standard deviation (1.954) to 15 (0.13027).

§ Race, father's education, father's occupation, father foreign born, broken family, siblings, nonfarm background, non-South background.

** Race, father's education, father's occupation, broken family, nonfarm background, non-South background.

•• Race, father's education, father's occupation, broken family, siblings.

‡‡ Variable defined as sibling differences. See Note ‡, Table 8.