358-76

INSTITUTE FOR RESEARCH ON POVERTY DISCUSSION PAPERS

THE EFFECTS OF EDUCATION ON OCCUPATIONAL STATUS AND EARNINGS

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August 1976

This paper was prepared as a draft of a chapter to be included in Who Gets Ahead?, edited by Christopher Jencks, Basic Books, forthcoming. This research has been supported by funds from the U.S. Department of Labor and the National Institute of Education thru the Center for the Study of Public Policy, Cambridge, Massachusetts (NIE-G-74-0077), and by the Institute for Research on Poverty, University of Wisconsin-Madison, under funds granted by the provisions of the Economic Opportunity Act of 1964 and administered by the U.S. Department of Health, Education, and Welfare. I am grateful to Susan Bartlett, David Bills, John Bishop, James Crouse, Robert Hauser, Christopher Jencks, Kent McClelland, and Peter Mueser for comments on an earlier draft. The opinions herein expressed are those of the author.

ABSTRACT

This paper investigates the effects of educational attainment on occupational status and earnings among men. Drawing on nine data sets, it attempts to assess the effects of schooling that persist after measured and unmeasured aspects of family background, and measured cognitive skill are controlled. It also examines nonlinearities in the effects of schooling, and differences in the effects of schooling among men of varying ages, races, cognitive levels, and parental occupational backgrounds.

Bias in the occupation-schooling relationship may be as high as 30 percent. Bias in the earnings-schooling relationship may be close to 50 percent. The effects of higher education are more robust and larger than the effects of elementary and secondary schooling. Interactions between education and race, measured ability, and father's occupational groups are inconsistent across samples, and usually insignificant.

Men with more schooling have higher-status jobs and earn more money than men with less schooling. Public policy recognizes this fact by according significant importance to educational programs in an effort to extend economic opportunity to the disadvantaged and thus reduce poverty.[1] Commonplaces like "Get an education" and "Stay in school" reflect the popular faith in the economic importance of schooling.

This paper is concerned primarily with the extent to which the apparent economic advantages of lengthier schooling are due to the characteristics of better-educated men which affect both educational attainment and economic success. If men who get a lot of schooling possess characteristics that would lead to economic advantage even in the absence of educational advantage, the apparent benefits of schooling are likely to exceed the actual benefits. If men who do not persist in their schooling were to acquire more schooling, they might well be disappointed in their expectations of realizing economic gain from their educational accomplishments.[2]

The secondary concern of this paper is the extent to which the economic advantages associated with lengthier schooling vary by level of schooling, race, social origin, age, and cognitive classifications. If public

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[1] See Henry Levin, "A Decade of Policy Development in Improving Education and Training for Low-Income Populations," in A Decade of Federal Antipoverty Policy: Achievements, Failure, and Lessons, ed. Robert Haveman (New York: Academic Press, forthcoming), for a discussion of the educational programs operating under the War on Poverty rubric.

[2] For technical treatments of the problem of bias due to omitted variables, see Arthur S. Goldberger and Otis D. Duncan, Structural Equation Models in the Social Sciences (New York: Seminar Press, 1973). For a discussion of the sources of the relationship between schooling and income, see Gary Becker, Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education (New York: Columbia University Press, 1964; Paul Taubman and Terrence Wales, Mental Ability and Higher Educational Attainment in the 20th Century (Berkeley: Carnegie Commission on Higher Education, 1974); Lester Thurow, "Education and Economic Equality," The Public Interest (Summer 1972); Lester Thurow, Generating Inequality (New York: Basic Books, 1975).

policy seeks to enhance economic opportunity by extending educational opportunity, it is important to know whether all increments in schooling promise the same benefits, or whether there are levels of schooling with effects that are unusually large or robust. Conversely, it is important to determine whether there are levels of schooling with effects that are small, or unusually biased by failure to control economically and educationally relevant characteristics.

Estimates of the effects of education that are true "on the average" may vary among subgroups. Policies based on relationships estimated over the general population may consequently be misguided if they are directed toward atypical target populations. Therefore, I have reported separately the relationships between economic outcomes and schooling for nonwhites and whites, sons of white-collar and blue-collar fathers, men with high, medium, and low test scores, and men from different age cohorts.

Throughout most of this paper I am concerned with the effects of years of schooling. Ideally, I would also measure quality of education as well as quantity, but with the exception of one data set, which included a measure of college quality, and a second set, which contained information on high school curriculum, I had no such data for this particular analysis. (A discussion of the effect of college quality and curriculum placement is included toward the end of this paper.) I first describe the data which I have analyzed. I next consider the effects of educational attainment on the status of the jobs men held early in their careers, then on their current occupations, and then on their current earnings or individual incomes.

Section 1: Data Sources and Variable Definitions

The results reported here draw upon regression analyses which I and others completed in connection with a two year project at the Center for the Study of Public Policy in Cambridge, Massachusetts. The project, under the direction of Christopher Jencks, has been engaged in investigating the determinants of economic success among men. It has involved reanalyses of several existing data sets, as well as analyses of some new data sets. Those sets upon which this study is based are described briefly in this section.

The 1970 Census 1/1000 Public Use Sample

In March 1970, the Census Bureau mailed a questionnaire to all occupied residences in the United States instructing the "householder" to complete the questionnaire. It is likely that many wives completed the questionnaire though the husband was considered the "householder", especially in those families who provided information only upon an enumerators follow-up. (Enumerators generally work during daytime hours.) This means that response errors in the data for male education and income may have been more severe than in data sets where all the information was gathered from the respondent.

The Census reported an overall response rate of 87.5 percent, though this varied by age and race. The present sample excludes men in institutions, the military, and in school. After taking into account item non-response, this sample includes 25,697 men aged 25 to 64 with positive earnings.

Occupation is coded using Duncan Socioeconomic Index scores, as is the case for all the data sets used. Earnings are 1969 pretax wages, salaries, tips, commissions and bonuses, plus income net of expenses from business, professional practice, and farms. The Census coded earnings to the midpoint of $100 intervals up to $50,000. Earnings over $50,000 were coded 70000. Experience is measured as Age -13 for men with less than eight years of education, and Age -Education -6 for men with eight or more years of schooling. Years of education is the exact number of years of schooling completed. Six or more years of college is coded 19. [See Bartlett and Jencks, forthcoming, for a detailed description of the Census sample.]

The 1962 Occupational Changes in a Generation Survey

In March 1962, the Census Bureau supplemented its regular Current Population Survey with a mail-back questionnaire for households that included men aged 20 to 64. The questionnaire surveyed respondents on their socioeconomic background, educational attainment, initial occupation, and marital status. The questionnaires, returned by 80 percent of the eligible respondents, were weighted to yield a sample of men aged 20 to 64, representative of the United States on age and race. The OCG sample has been the principal data set relied upon by recent sociologists investigating stratification in the entire United States and provided the basic data for Blau and Duncan's landmark, The American Occupational Structure [1967].

The present sample of OCG respondents includes 11,504 men aged 25 to 64 who have complete data on the items of interest, who were not in the military or an institution at the time of the survey, and who reported positive income. Part-time students who reported an occupation are included, but since the sample includes only men 25 years of age or older in 1962, this should not be a serious problem.

The OCG measure of income is for 1961 total personal income from all sources, and is coded to the midpoint of intervals of varying width. Men with incomes of $25,000 or more are coded 33000. Educational attainment is grouped into intervals: one to four years is coded 3; five to seven years is coded 6; eight years is coded 8; one to three years of high school is coded 10; four years of high school is coded 12; one to three years of college is coded 14; four years of college is coded 16; and five or more years of college is coded 18; Experience is measured as Age -Education -7 or Age -14, whichever is smaller. [See Jackson, forthcoming, for a detailed description of the 1962 OCG sample.]

The 1967-74 Panel Study of Income Dynamics

The University of Michigan Survey Research Center sampled several thousand families annually between 1967 and 1974 to study the sources and stability of family income. The survey sampled only heads of households, and does not include adult secondary individuals living in a household headed by another adult. This restriction results in a sample somewhat more advantaged than a random sample of similarly aged individuals. The initial response rate was 76 percent. In the fifth year, the SRC interviewed only 62 percent of the original respondents.

I have analyzed 1971 data for 1774 men aged 25 to 64 who were not students, or military personnel, and who were not institutionalized when first surveyed. All had positive 1971 earnings. (Due to an error, I conducted my analyses using N=1744 respondents.)

The SRC administered a thirteen-item sentence completion test from the Lorge-Thorndike "intelligence test." Mueser reports that the correlation between the sentence completion test and other cognitive ability tests range from 0.20 to 0.60, with a reliability estimated at only 0.652.

The SRC coded occupations into broad categories, rather than into detailed Census classifications and Mueser estimated Duncan SEI scores for each group. Earnings are 1971 wage and salary income, and an estimate of self-employment income derived after dividing self-employment income into labor and asset components. Earnings are coded to the exact dollar amount up to $99,999. Higher incomes are coded 99999.

Educational attainment from zero to five years is coded 3; from six to eight years is coded 7.5; nine to eleven years is coded 10; twelve years is coded 12; thirteen to fifteen years is coded 14; college degree is coded 16; and, advanced or professional degree is coded 18. Nonacademic training past high school is excluded.

Experience is coded as Age -Education -7, unless education is less than 7. In that case, experience is coded Age -14. [For a detailed description of the PSID sample, see Mueser, forthcoming.]

The 1965 Productive Americans Survey (PA)

In early 1965, the SRC interviewed 2214 heads of households 18 years old and over representing 84 percent of the original sample. For the purposes of this study, women, men under 25 years of age and over the age of 64, greater than half-time students, military personnel, and men without positive earnings were eliminated from the sample. After taking into account item nonresponse, the sample size is 1188.

Like the PSID, the PA occupation variable is recorded in broad categories, not in Census three-digit classifications. The earnings variable is self-reported 1964 wages, salary, bonuses, overtime, commissions, income from persons who room and/or board, income from professional practices, income from farming (less expenses), and take-home pay and restored-profit income from self-owned businesses. Income over $99,999 is coded 99999.

Again, as the PSID, the PA education variable excludes nonacademic training past high school. It is coded exactly as education in the PSID survey. The experience variable is also the same as experience in the PSID. [For a detailed description of the Productive Americans Survey, see McClelland, forthcoming (a).]

The 1966 National Longitudinal Survey

The National Longitudinal Surveys (Parnes data) are a joint project of the Census Bureau and the Ohio State University Center for Human Resources Research. The present sample is men aged 45 to 59 in 1966. The Census Bureau interviewed or mail-surveyed respondents six times between 1966 and 1971. The data for this analyses comes from 1966 and 1967 contacts, and pertains to 1966.

The Census Bureau originally interviewed 91 percent of a potential sample of 5518 men, 45 to 59 years old. This sample is drawn from that original but does not include individuals in institutions, military personnel, students, or zero and negative earners. Taking into account item nonresponse leaves a sample size of 2830 respondents.

The earnings data are for 1966 wage and salary income, plus net income from farms, businesses, professions, and partnerships. Earnings are coded to the exact dollar. Education is the exact number of years of "regular school" the respondent completed up to 18 years. Experience is coded the same as in the PSID. [Morgan, forthcoming, describes this sample in detail.]

The 1964 Veterans Survey

In October 1964, the Census Bureau conducted a special Current Population Survey of males aged 16 to 34. The National Opinion Research Center analyzed a subsample of veterans, 25 to 34 years of age for whom the Armed Forces Qualifying Test scores (AFQT) were available. The Veterans sample is not representative of its age cohort. Since the respondents aged 30 to 34 are somewhat more representative than the 25 to 29 year olds, I analyzed only the 30 to 34 year olds. Eliminating students, men without positive expected earnings, and men with missing data leaves 803 respondents in the sample.

In the original data, the AFQT scores were recorded in rather broad percentile categories. Assuming the distribution of "true" scores is normal, Jencks rescaled the mean percentile scores for categories to a mean of 100 with a standard deviation of 15.

The Veterans Survey included questions concerning expected 1964 annual and weekly earnings. The response rate for the weekly earnings question was much lower than the response rate for expected earnings, and the respondents who reported weekly earnings were atypically successful and homogeneous. I have used the expected annual earnings variables in the present analyses. The earnings data is grouped into categories of varying widths and coded at or close to interval midpoints. Men with earnings of $15,000 or more were coded 20000.

The Veterans Survey questioned respondents on the highest grade of "regular school" they had completed. Less than eighth grade is coded 6; eighth grade is coded 8; nineth, tenth, or eleventh grade is coded 10; high school graduate is coded 12; less than two years of college is coded 13; two or more years of college but no bachelor's degree is coded 14; Bachelor's degree is coded 16; and, graduate study beyond the bachelor's degree is coded 18.

Experience is coded Age -Education -6. [For a detailed description of the Veterans Survey see Jencks, forthcoming (c).]

The 1973 NORC Amalgam Survey (with brothers subsample)

The NORC Amalgam Surveys pool questions purchased by several clients. Our data come from Amalgam #4179, administered in December 1973 and January 1974 to 705 male respondents, representing noninstitutionalized men in the continental United States 18 years of age and over. Of the 705 respondents, 488 had at least one living brother. NORC conducted telephone interviews with the oldest living brother of 177 of these 488 respondents.

After restricting the sample to respondents 25 to 64 years of age whose brothers were also interviewed, eliminating students and persons not showing positive earnings for 1972, and taking into account item nonresponse, the sample consists of 150 pairs of brothers, or 300 individuals.

NORC asked several questions to determine educational attainment. Years of education is coded as the exact number of years of schooling completed up to twenty years, excluding nonacademic training.

1972 earnings were determined by asking respondents to check a category representing the interval that included the sum of their wages, salaries, and business and professional income. Intervals are of varying widths, and are coded to the midpoint. Earnings of $25,000 or more are coded 35000.

Experience is defined as Age -Education -6, except for men with less than eight years of school. For them, experience is defined as Age -14. [Eaglesfield, forthcoming (b), describes the NORC Amalgam Survey in detail.]

The Project Talent 11-Year Longitudinal Survey

In 1960, Project Talent administered a battery of sixty-five tests, and questionnaires on attitudes and personality to students in a 5 percent stratified random sample of American high schools. Talent followed up students one, five, and eleven years after high school graduation. The present sample is drawn from men who were in the eleventh grade in 1960.

I have concentrated my analyses of the Talent sample on a subsample of 99 pairs of nontwin brothers, of which at least one from each pair was in the 1960 eleventh grade sample. The relationship between education and earnings differs for the 198 individuals comprising the sibling pairs and the 839 respondents comprising the project's complete data sample of Talent respondents (see Table 7). I concentrated on the sibling sample because I wanted to use the Talent data principally to assess the effects of controlling measured ability, and measured and unmeasured aspects of family background. Defining variables as sibling differences eliminates the effects of between family differences on schooling and economic outcomes.

Talent constructed several composites from its separate tests. Crouse [forthcoming (b)] 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 regressions never raises R^2 by more than 0.013. I used the Academic Composite to control "ability" in the present analyses.

Talent classified occupations according to its own classification system, rather than to Census categories. Marsha Brown estimated Duncan scores for the Talent categories [Crouse, forthcoming (a)]. Men who were in school more than half time are excluded from the present sample.

Education is coded 11 for those who did not finish high school; 12 for high school graduates; 13 for those with one year of college; 15 for those with at least two years of college, but no B.A.; 16 for those with a B.A.; 17 for those with graduate study and/or an M.A.; 18 for those with a six-year certificate of graduate study; and, 20 for those with a doctoral degree. It is unclear what categories professionals chose when reporting their educational attainments.

Talent asked respondents to report their current earnings at the time of the survey and to indicate whether the report was an hourly, weekly, or monthly figure. Crouse used these reports to calculate the hourly earnings rate, and I have used this variable in the present study.

There is no experience variable in the Talent Siblings tape. Experience is a direct measure of years of full-time work since June 1961 for the 839 regular Talent respondents. [For a detailed description of the Project Talent sample see Crouse, forthcoming (a).]

The Kalamazoo Brothers Sample

In 1973, I drew a sample of males who had been in the sixth grade in the Kalamazoo, Michigan public schools between 1928 and 1950 (inclusive). Scores on the 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 within the sample. Beginning with a potential sample of 2782 individuals from 1224 families, I traced and interviewed 1243 men during 1973 and 1974. Item nonresponse and failure to interview more than one brother in a pair introduced further sample attrition. The present analyses are based on 692 individuals, comprising 346 weighted pairs for whom complete data is available for both brothers.*

From 1928 to 1942, the Kalamazoo school system administered the Terman group test. After 1942, the system used the Otis group test. Close to a quarter of the respondents took the Otis rather than the Terman test.

* I am grateful to Dr. William Coates and Dr. David Bartz of the Kalamazoo Public School System for permission to use 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.

Both tests emphasize verbal skill. The Otis test is scaled to a lower mean, but its variance and correlations with other variables are generally not 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 of respondents who had taken the Otis test, and combined the two groups.

Respondents reported their 1973 expected pretax earnings from all jobs, businesses, and professions, and responses were recorded in intervals of varying widths, and coded to interval midpoints. Earnings over $25,000 are coded 34000.

Education is coded to exact number of years of schooling completed, and includes post-high school vocational, business or technical schooling. It does not include on-the-job training, or short-term or part-time programs.

Experience differences have insignificant effects in the Kalamazoo sample. This is because the age range of respondents is restricted. The men range from 35 to 59 years of age. In a subsample of 1962 OCG respondents aged 35 to 54, the effects of experience are also insignificant. I therefore have not included a measure of experience in the present analyses of the Kalamazoo data. [For a detailed description of the Kalamazoo Brothers Sample see Olneck, 1976; and Olneck, forthcoming.]

Note on the Specification of Education

To investigate the nonlinear effects of education, the project employed a spline function of the education variables. "Years of Education" is total years of schooling. "Years Higher Education" is zero for men with twelve or fewer years of schooling, and Years of Education -12 for men who went past high school. "BA" is zero for men with less than sixteen years of schooling, and one for others. With Years Higher Education and BA controlled, the coefficient of Years of Education measures the average effect of an extra year of elementary or secondary school. The coefficient of Years Higher Education measures the difference between the average effect of a year of higher education and the average effect of a year of elementary or secondary education. The coefficient of BA measures the additional advantage of completing the fourth year of college over and above the average effect of an extra year of higher education. However, if the effect of an extra year of college differs from an extra year of postcollege schooling, the BA effect under this specification will to some extent misestimate the strictly diploma effect involved in the advantage of a college graduate over a college dropout. [For a discussion of our choice of this specification see Jencks, forthcoming (b).]

Section 2. Initial Occupation

Three of our data sets include information on the first jobs respondents held after completing their education. The OCG item, however, is flawed, and I therefore ignored it throughout this paper.[3] Table 1

[3] See Otis D. Duncan, David Featherman, and Beverly Duncan, Socioeconomic Background and Achievement (New York: Seminar Press, 1972), pp. 210-212 for a discussion of this item.

shows the effects of education on initial occupational status for the Michigan Panel sample (PSID) and for the Kalamazoo Brothers Sample.

The average effect of an extra year of schooling on initial occupational status in the Kalamazoo sample is twice as large as the effect in the Michigan sample. This is due in part to the absence of men with less than eighth grade educations in the Kalamazoo sample. The effects of education on occupational status are nonlinear, rising with the schooling level. The difference is also due to the broad coding of the occupation variable in the PSID data. [Mueser, forthcoming.] Because the uncontrolled effects differ between the samples, it is necessary to discuss both absolute and proportionate biases.

There are educational advantages associated with both coming from more favorable home backgrounds and from displaying greater cognitive competence. There are also occupational advantages associated with variations in background and cognitive skill among men who have the same amount of schooling. If background and cognitive skill are ignored, the apparent effects of education on initial occupational status will be overestimated. The extent to which this is true, however, appears rather modest.

In the PSID sample, controlling test scores and measured family background reduces the effect of an extra year of schooling by 3.125 - 2.513 = 0.612 points or 0.612/3.125 = 19.6 percent. In the Kalamazoo sample, controlling test score differences among brothers and family background common to brothers, reduces the effect of education by 6.238 - 5.526 = 0.712 points or 0.712/6.238 = 11.4 percent. These results suggest that

Table 1

Effects of Education on Initial Occupational Status

(Bracketed coefficients less than 1.96 times their standard errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Michigan PSID (N=1744)	1.	3.125 (.129)			17.936	None
	2.	1.363 (.212)	2.669 (.614)	6.986 (2.405)	17.319	None
	3.	2.677 (.160)			17.733	Measured background[a]
	4.	.897 (.227)	2.457 (.620)	8.164 (2.396)	17.121	Measured background[a]
	5.	2.862 (.146)			17.866	Test score
	6.	1.014 (.224)	2.719 (.610)	7.211 (2.391)	17.218	Test score
	7.	2.513 (.150)			17.693	Measured background,[a] test score
	8.	.690 (.234)	2.493 (.618)	8.222 (2.388)	17.065	Measured background,[a] test score
Kalamazoo Brothers (N=692 or 346 pairs)	9.	6.238 (.232)			16.622	None
	10.	3.166 (.701)	[1.295] (1.016)	15.137 (3.264)	16.125	None
	11.	5.710 (.264)			16.377	Measured background[b]
	12.	2.389 (.718)	[1.710] (1.011)	14.274 (3.215)	15.861	Measured background[b]
	13.	5.997 (.283)			16.612	Test score
	14.	2.827 (.730)	[1.436] (1.019)	14.868 (3.264)	16.105	Test score
	15.	5.520 (.303)			16.366	Measured background,[b] test score

Table 1 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	16.	2.146 (.740)	[1.804] (1.013)	14.075 (3.217)	15.851	Measured background,[b] test score
	17.	5.578 (.454)			15.465	Family background[c]
	18.	[1.661] (1.210)	[2.614] (1.543)	13.787 (4.496)	15.025	Family background[c]
	19.	5.526 (.488)			15.490	Family background,[c] test score difference
	20.	[1.580] (1.232)	[2.644] (1.547)	13.744 (4.503)	15.044	Family background,[c] test score difference

NOTES: a. Race, father's education, father's occupation, father white collar, father foreign born, no male head, nonfarm origin, non-South origin, number of siblings.

b. Father's education, father's occupation, number of siblings.

c. Family background controlled by defining education, test score, and occupation variables as sibling differences.

when employers favor better-schooled young men they are either seeking characteristics that are relatively unrelated to cognitive ability and family background, or that they are bad judges of ability and background, and are forced to rely upon educational credentials as an imperfect guide. Analyses of a subsample in the Kalamazoo data for whom high school personality ratings are available, suggest that similar conclusions hold when personality characteristics such as initiative or industriousness are considered as possible sources of bias. Inclusion of the personality measures does not significantly change the education coefficient. [Olneck, 1976, Chapter 5.]

The extent to which increments in educational attainment are associated with higher occupational status in the early career and the sensitivity of the measured effects of schooling to the inclusion of background and ability measures vary by level of schooling.

Increments in schooling below the college level are associated with smaller early occupational advantages than increments at the college level, and they are reduced by a proportionately larger amount when test scores and background are controlled. In the PSID sample, the predicted advantage of a twelfth grade graduate over an eighth grade graduate with the same test score and measured background is only 4(0.690) = 2.760 points, or 2.760/5.452 or 50.6 percent of the uncontrolled effect. In the Kalamazoo sample, the analogous effect among respondents who came from the same homes and have equal test scores is 6.320 points, or 6.320/12.664 = 49.9 percent of the uncontrolled effect.

Four years of college, however, is associated with an extra 4(0.690 + 2.493) + 8.222 = 20.954 points among PSID respondents with equal test scores and similar backgrounds, and an extra 4(1.580 + 2.644) + 13.741 = 30.637 points among brothers with equal test scores in the Kalamazoo sample. These effects are 90.7 percent and 92.9 percent of the zero-order effects in the PSID and Kalamazoo samples, respectively.

The substantial relative bias in the effects of schooling below the college level indicates that men who complete high school get better jobs than men who drop out largely because the high school graduate is already advantaged. If this finding is accurate, and if it holds for young men today, programs aimed at discouraging high school students from dropping out of school will not likely be successful in increasing the prospective dropout's economic chances.

The robust effect of completing college suggests that either college augments employability for reasons unrelated to family background or cognitive skill, or that employers are less concerned with background and cognitive differences among college graduates. Since the economic impact of test scores increases during an individual's career, we cannot conclude that employers are indifferent to cognitive differences. But, since the impact of test scores on early occupational status is small after education is controlled, I conclude that college graduates benefit in job selection in large measure because employers treat them alike. At the same time, some employers refuse to hire men without degrees, even when they have test scores as high as or higher than typical college graduates.

Section 3. Current Occupation

Effects of Controlling Family Background

The occupational advantages associated with lengthier schooling vary across our samples. This is because researchers often sample different populations, have varying degrees of success in interviewing potential respondents, and code important variables differently. For example, Project Talent followed up men who had at least entered the eleventh grade—in fact, 97 percent of the Talent respondents completed the twelfth grade.[4] The effect of educational attainment in Talent, therefore, measures for the most part the effects of progress through college and graduate school, which is greater than the effects of progress through elementary and high school. The PSID and Productive Americans surveys relied upon broad categories of occupation, therefore reducing the variance in occupational status and reducing, to some extent, the measured effects of schooling.

Samples also differ in the effects of background and ability measures on education and economic outcomes. In some cases, this is because of differences in coding and missing-data procedures. In the PSID and the PA, missing values were assigned for father's education on the basis of reported literacy, and father's occupation was based on broad categories. In other cases, it is probably because of sampling error. The NORC Brothers intercorrelations among background variables are slightly higher

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[4] This figure applies to the Talent complete data sample described by James Crouse, "The Project Talent 11-14 Year Longitudinal Surveys," in Who Gets Ahead?, ed. Christopher Jencks, draft, Appendix H (New York: Basic Books, forthcoming). The Talent Siblings analyzed here average 0.36 years more schooling than the Talent complete data respondents.

than those among the background variables in the OCG. In still other cases, differences are due to atypical sample characteristics. The Talent and Veterans samples are selected in some measure on educational attainment, reducing the measured impact of background on education. The Kalamazoo sample may also be selected partly on current occupational status and earnings. In any case, the effects of father's occupation are certainly lower in that sample than in nationally representative data. [Olneck, 1976: 86-90.]

Since the uncontrolled effects of education are not the same across samples, and because the interrelations among measures of background, cognitive ability, schooling, and occupation vary, I cannot offer precise conclusions about the magnitude and sources of bias in the occupation-schooling relationship. I can, however, suggest the most important sources of bias, and the levels of schooling that are most sensitive to controls for omitted variables.

Higher-status families ensure their sons greater than average chances of attaining economic success mainly by promoting educational opportunity. However, measured family background is associated with occupational status, even among men with the same amount of education. Consequently, the occupation-schooling relationship is overestimated when the effects of measured background are ignored.

Data from the OCG and National Longitudinal (Parnes) studies suggest that close to 1.0 point of the apparent effect of education on occupational status is due to the joint association of education and occupation with measured background (see Table 2). The reductions in the education

Table 2

Effects of Education on Current Occupation
(Bracketed coefficients less than 1.96
times their standard errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census (N=25,697)	1.	4.337 (.034)			19.243	None
	2.	2.934 (.055)	2.465 (.164)	4.013 (.770)	18.835	None
1962 OCG (N=11,504)	3.	4.105 (.050)			19.731	None
	4.	2.701 (.073)	3.079 (.287)	5.163 (1.275)	19.138	None
	5.	3.354 (.058)			19.085	Measured background[a]
	6.	1.988 (.079)	2.928 (.284)	5.710 (1.234)	18.500	Measured background[a]
Michigan PSID (N=1744)	7.	3.910 (.119)			16.567	None
	8.	2.134 (.195)	2.951 (.564)	5.546 (2.210)	15.916	None
	9.	3.579 (.139)			16.443	Measured background[b]
	10.	1.684 (.209)	3.103 (.570)	6.001 (2.203)	15.743	Measured background[b]
	11.	3.664 (.135)			16.502	Test score
	12.	1.807 (.206)	2.997 (.560)	5.757 (2.197)	15.819	Test score
	13.	3.438 (.148)			16.410	Measured background,[b] test score
	14.	1.501 (.215)	3.136 (.569)	6.051 (2.197)	15.696	Measured background,[b] test score

Table 2 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	15.	1.377 (.211)	2.685 (.560)	4.565 (2.162)	15.394	Measured background,[b] test score, early occupation
Productive Americans (N=1188)	16.	3.509 (.139)			16.633	None
	17.	2.105 (.204)	3.861 (.764)	[.534] (3.066)	16.065	None
	18.	3.148 (.163)			16.398	Measured background[c]
	19.	1.669 (.221)	3.975 (.767)	[.778] (3.036)	15.800	Measured background[c]
Parnes Men aged 45 to 59 (N=2830)	20.	4.075 (.101)			19.745	None
	21.	2.896 (.143)	2.785 (.620)	5.490 (2.778)	19.268	None
	22.	3.352 (.117)			19.077	Measured background[d]
	23.	2.079 (.155)	3.220 (.604)	[4.227] (2.693)	18.563	Measured background[d]
NORC Vets aged 30 to 34 (N=803)	24.	5.070 (.242)			18.781	None
	25.	1.889 (.439)	4.816 (.933)	[4.843] (3.580)	17.945	None
	26.	4.677 (.258)			18.435	Measured background[e]
	27.	1.641 (.446)	4.472 (.929)	[5.438] (3.532)	17.663	Measured background[e]
	28.	4.385 (.287)			18.579	Test score
	29.	1.046 (.464)	4.851 (.919)	[5.511] (3.530)	17.679	Test score

Table 2 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	30.	4.131 (.296)			18.292	Measured background,[e] test score
	31.	.979 (.468)	4.466 (.919)	[6.069] (3.497)	17.475	Measured background,[e] test score
NORC Brothers (N=300 or 150 pairs)	32.	4.634 (.363)			19.480	None
	33.	3.260 (.684)	2.117 (1.541)	[1.593] (6.770)	19.360	None
	34.	4.321 (.401)			19.114	Measured background,[f1] age
	35.	2.676 (.747)	[2.871] (1.568)	[-2.008] (6.871)	18.952	Measured background,[f2] age, age^2
	36.	3.193 (.487)			17.967	Family background,[g]
	37.	[1.457] (1.112)	[3.778] (2.127)	[-.223] (9.008)	17.854	Family background,[g] age difference
Talent Siblings (N=198 or 99 pairs)	38.	8.324 (.525)			18.214	None
	39.	7.307 (.595)			17.988	Measured background[h]
	40.	6.912 (.678)			18.217	Test score
	41.	7.098 (.713)			18.021	Measured background,[h] test score
	42.	6.613 (1.091)			17.980	Family background,[g]
	43.	6.506 (1.206)			18.069	Family background,[g] Test score difference
Kalamazoo Brothers (N=692 or 346 pairs)	44.	5.012 (.261)			18.696	None
	45.	5.722 (.809)	-2.709 (1.172)	10.876 (3.766)	18.603	None

Table 2 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	46.	5.031 (.302)			18.723	Measured background[i]
	47.	5.654 (.843)	-2.576 (1.187)	10.866 (3.775)	18.624	Measured background[i]
	48.	4.192 (.314)			18.443	Test score
	49.	4.693 (.832)	[-2.283] (1.161)	10.058 (3.721)	18.359	Test score
	50.	4.264 (.355)			18.458	Measured background,[j] age, test score
	51.	4.668 (.890)	[-2.176] (1.201)	10.455 (3.788)	18.381	Measured background,[j] age, test score
	52.	2.659 (.416)				Measured background,[j] age, test score, early occupation
	53.	4.098 (1.011)	-2.746 (1.351)	[6.166] (3.738)	17.833	Measured background,[j] age, test score, early occupation
	54.	4.002 (.524)			17.836	Family background[g]
	55.	3.035 (1.426)	[-.092] (1.818)	13.700 (5.297)	17.702	Family background[g]
	56.	3.499 (.557)			17.702	Family background,[g] test score difference
	57.	[2.389] (1.439)	[-.689] (1.807)	13.338 (5.260)	17.570	Family background,[g] test score difference
	58.	2.150 (.639)			17.319	Family background,[g] test score difference, early occupation difference
	59.	[2.038] (1.418)	[-1.276] (1.784)	10.287 (5.241)	17.275	Family background,[g] test score difference, early occupation

Table 2 Continued

NOTES:

a. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, father's occupation by race, race, $siblings^2$.

b. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, father foreign born, race.

c. Father's education, nonfarm, non-South, siblings, father foreign born, race.

d. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, race.

e. Father's education, father's occupation, no male head, nonfarm, non-South, race.

f_1 Father's education, father's occupation, nonfarm, siblings, race.

f_2 Father's education, father's occupation, father white collar, no male head, nonfarm, siblings, race.

g. Variables defined as sibling differences.

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

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

j. Father's education, father's occupation, father white collar, no male head, siblings, mother's education, father foreign born, father foreign born by father's education.

coefficient controlling background are 0.751 points in the OCG and 0.723 points in the NLS. These reductions represent close to 18 percent of the bivariate relationship in each of these studies. Because of occupational coding differences, the reductions in the PSID and Productive Americans coefficients are lower than those in the OCG and NLS, 0.331 and 0.361, respectively.[5]

Family background is only imperfectly measured by socioeconomic variables [Olneck, 1976; Eaglesfield, forthcoming (a); Cocoran, Jencks, and Olneck, 1976]. If the unmeasured aspects of family background that affect education are related to the unmeasured aspects of background that affect occupational status, controlling measured socioeconomic background will not suffice to eliminate bias due to background. By analyzing the relationships among sibling differences on education and occupation in our three samples of brothers, I have attempted to estimate the bias in the schooling-occupation relationship due to the effects of overall family background, and to indicate the extent to which this estimate of bias differs from estimates based solely on controlling measured background. Unfortunately, the extent of bias introduced by measured background is substantially less in the surveys involving brothers than in our other samples. This may vitiate any generalizations concerning the relative importance of measured and unmeasured sources of bias. Evidence from the 1962 OCG suggests that this caution is warranted.

[5] The smaller reduction in the PA compared to the OCG is not due to the omission of a measure of father's occupation in the PA. Omitting father's occupation from the OCG background measures barely changes the estimated bias in the education coefficient.

In the NORC Brothers sample, controlling measured background (and age) reduces the effect of an extra year of schooling on occupation from 4.634 points to 4.321 points, or by 0.313/4.634 = 6.8 percent. Controlling all aspects of family background common to brothers, reduces the effect of education on occupation from 4.634 points to 3.161 points, or by 1.473/4.634 = 31.8 percent.

Among the Talent Siblings, controlling measured background reduces the simple coefficient by only 0.017 points, or 0.017/7.324 = 0.2 percent; but in the regression of sibling occupational differences on educational differences, the reduction from the simple coefficient is 0.711 points, or 0.711/7.324 = 9.7 percent.

In the Kalamazoo Brothers Sample, controlling measured background raises the estimated effect of education on occupation by an insignificant amount. But controlling common overall background reduces the effect by 1.010 points, or 1.010/5.012 = 20.2 percent.

The OCG Survey asked respondents to report their eldest brother's education. If brothers' characteristics do not directly affect one another and if the reliability of respondent's reports about their brother's educational attainments are nearly as reliable as self-reports, then the within-pair effects of education can be calculated for the OCG sample though the samples does not include full sibling data.[6]

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[6] For the tenability of these assumptions see Olneck, "Determinants of Educational Attainment," Chapter 4. I am grateful to Christopher Jencks for pointing out that these analyses could be conducted on the OCG data. Letting U denote respondent's education, U' denote brother's education, and Y denote respondent's occupation, the within-pair standardized coefficient (beta) is $\beta = \frac{r_{YU} - r_{YU'}}{1-r_{UU'}}$. For exposition of the model and equations underlying this result see Olneck, "Determinants of Educational Attainment," p. 160.

The simple correlation between education and occupational status among 5780 respondent's in Jackson's OCG sample for whom brother's reports of educational attainment are available is 0.585. The within-pair standardized coefficient is 0.453. This suggests that the bias in the education-occupation relationship in the OCG that is due to shared background among brothers, is 1 - 0.453/0.585 = 22.6 percent. This is only 4.3 percent more than the bias attributable to measured background in Jackson's complete data sample, and suggests that in a representative population, the family background factors common to education and occupation are for the most part factors measured by socioeconomic variables.

Evidence recently made available to me by Robert Hauser suggests otherwise, however. In a subsample of 6865 respondents aged 35 to 59 from the 1973 replication of the Occupational Changes in a Generation Survey, who reported their brothers' educations, the correlation between education and occupation is 0.611. The standardized regression coefficient controlling father's education, father's occupation, siblings, male headed family, race, and farm background is 0.520. The within-family standardized coefficient is 0.469. Thus, controlling measured background suggests a bias in the schooling-occupation relationship of 15 percent, while controlling all background factors common to brothers suggests a bias of 23 percent.

I have not systematically examined all the possible reasons that the contributions of measured and unmeasured background factors to bias in the schooling coefficient differ between my 1962 and 1973 OCG samples. I did perform similar calculations by age cohorts on the published correlations in Duncan, Featherman, and Duncan [1972]. They suggest a small bias for men aged 25 to 34, but a very large bias for men 55 to 64 years of age. Therefore, the exclusion of men aged 25 to 34 in the 1973 sample may be a factor in determining that difference. Reduced effects of race and farm background from 1962 to 1973 could also contribute to the difference. Note that the proportionate bias due to brothers' common background is virtually the same in both the 1962, and the 1973 samples, i.e., 23 percent. The absolute bias in the standardized coefficient is also quite similar across the two samples: 0.585 - 0.453 = 0.132 in the 1962 sample, and 0.611 - 0.469 = 0.142 in the 1972 sample. This suggests that the bias due to overall background is fairly constant, and insensitive to changes in the impact of measured background variables. However, this conclusion must remain tentative until other possible sources of difference in the results are examined. These include age composition, and also differences in the effects of measurement error across the two surveys.

Effects of Controlling Measured Ability

Measures of cognitive ability are related to educational attainment. They are also related to occupational status among men with equal amounts of schooling, though the extent to which this is true varies among our samples more than the strength of the schooling-test score relationship varies. Consequently, the estimate of the bias in the effect of schooling on occupational status that is due to the abilities measured by tests, varies across samples.

Once education is controlled, the effect of test score on occupation is trivial in the PSID Study. The same is true for the Talent respondents. Consequently, the reduction in the education coefficient when test scores are controlled is smaller in these two samples than it is in the Veterans and Kalamazoo samples, where the continuing effects of test scores are stronger. Most of the Veterans respondents, however, took the AFQT after completing their schooling. The PSID respondents were tested when they were surveyed. If lengthier schooling improves cognitive skills, controlling test scores in the Veterans and PSID samples will overestimate the bias in the schooling coefficient that is due to prior ability. The coefficient of schooling and controlling test scores in those samples should therefore be interpreted simply as the effect of schooling among men with equal test scores, not as the effect of schooling that is unbiased by ability.

The reductions in the bivariate coefficients when test scores are controlled are 0.246 points in the PSID, 0.412 among the Talent Siblings, 0.685 for the NORC Veterans, and 0.820 for the Kalamazoo Brothers. Because the PSID test is not very reliable and because the Talent Siblings are so few in number, I tend to put more faith in the Kalamazoo and Veterans results as estimates of the impact of including an ability measure when analyzing the occupation-schooling relationship.[7]

Effects of Controlling Both Ability and Family Background

Since both background and test scores affect schooling and occupation, we need to ask what the effects of schooling are among men who come from similar backgrounds and who also have similar cognitive ability. For two of our data sets, I can control measured background and test scores after school completion, and for two others I can control all background factors common to brothers, as well as sibling test score differences.

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[7] Jencks reports the reliability of the PSID test as only 0.652. See Peter Mueser, "The 1967-74 'Panel Study of Income Dynamics' Survey," in Who Gets Ahead?, ed. Christopher Jencks, draft, Appendix C (New York: Basic Books, forthcoming). Controlling test scores in the Wisconsin 1964 Follow Up reduces the occupation-schooling coefficient from 8.501 to 7.755 or by 0.746 points. This suggests that for this particular question, the youth of a sample are not especially important. See William H. Sewell and Robert M. Hauser, Education, Occupation and Earnings (New York: Academic Press, 1975).

In the PSID, controlling only measured background reduces the effect of schooling on occupation from 3.910 to 3.579. The effect of a one year difference in education on occupation controlling both measured background and test scores is 3.438, or 3.438/3.910 = 87.9 percent of the uncontrolled effect. In the Veterans sample, the effect of a one year difference in education on occupational status among men aged 30 to 34 who come from similar backgrounds and have the same test scores is 0.939/5.070 = 18.5 percent less than the bivariate coefficient.

Among the Talent Siblings, the effect of a one year difference in schooling between brothers who have the same test scores is 6.506 points, or 6.506/7.324 = 88.8 percent of the uncontrolled effect. Among the Kalamazoo Brothers the analogous results are 3.499 points and 3.499/5.012 = 69.8 percent of the uncontrolled effect.

Because the PSID test is questionable, because the AFQT was taken after most Veterans respondents had completed their schooling, and because the Talent Sibling sample is small, I suspect that the estimate of bias in the occupation-schooling relationship due to background and cognitive ability in the Kalamazoo data is closest to the truth. However, skepticism concerning the results from a relatively small, locally restricted sample is certainly warranted.

Family background and cognitive ability do not exhaust the potential sources of bias in the schooling-occupation relationship. Men with more drive, perseverance, initiative, and other personality characteristics generally thought to promote career success may well get more schooling than those with less favorable personality characteristics. Brothers are not fully alike on such characteristics, and so controlling common family background will not adequately control their effects.

Our best evidence on the bias imparted by the more favorable initial personality characteristics of the better-schooled and more successful is unfortunately weak. It comes from measures of personality characteristics rated by the homeroom teachers of the Kalamazoo respondents when they were in tenth grade.[8]

Controlling these measures after IQ and measured background are controlled, leaves the education coefficient virtually unchanged.[9] This result may mean: (1) that the personality characteristics of youths are a poor guide to adult characteristics; (2) that these ratings are unreliable; (3) that the characteristics teachers rate are not important to employers; or, (4) that the connection between personality characteristics and educational attainment is not as strong as employers who discriminate in favor of the better-educated think.

Effects of Controlling Early Occupation

The occupational advantage that better-educated men have is due in part to their advantage in getting higher-status jobs early in their careers and in part to being promoted higher or engaging in more successful job changes than less-schooled men who begin their careers in similar jobs.

Controlling early occupational status among brothers in the Kalamazoo sample who have equal test scores, reduces the effect of education by

[8] See Michael Olneck, "The Determinants of Educational Attainment and Adult Status Among Brothers: The Kalamazoo Study," doctoral dissertation, Chapter 5, Harvard Graduate School of Education, 1976. The attitudinal variables in the PSID were measured at the same time as the outcome measures, thereby introducing causal ambiguity. I have, therefore, ignored them in this section. I have ignored the Talent personality measures because at this writing no analysis of their effects on education coefficients are available.

[9] I neglected to run regressions controlling only the personality ratings.

3.499 - 2.150 = 1.349 points, or 1.349/3.499 = 38.6 percent of the effect remaining after family background and test score differences are controlled. The effect of education when background, test scores, and early occupation are controlled is 2.150/5.012 = 42.9 percent of the uncontrolled effect. This result suggests that employers reward credentials per se when they promote or hire workers with at least some experience, or that better-educated men differ from less-educated men in ways that escape our measurement--possibly better-educated men are favored in training and on-the-job learning opportunities.[10]

Differential Effects According to Level of Schooling

The preceding discussion does not distinguish the effects of different kinds of schooling. But completing high school does not lead to occupational advantages as large as those advantages associated with completing college.[11]

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[10] This result may be particularly sensitive to measurement error. Measurement error corrections suggest that only 23 percent of the zero-order effect of education on occupation persists when family background, test scores, and initial occupation are controlled in the Kalamazoo data. See Olneck, "Determinants of Educational Attainment," Chapter 4. However, for contrary results suggesting a small impact of measurement error on the education-occupation relationship net of early occupation in the OCG 1973 replication, see William Bielby, Robert Hauser, and David Featherman, "Response Errors of Nonblack Males in Models of the Stratification Process," Institute for Research on Poverty Discussion Paper, 337-76 (Madison: Institute for Research on Poverty, 1976).

[11] I ignore the advantages associated with attending, but not completing college. This is because the meanings of our years higher education, and B.A. variables are ambiguous. If the effect of an extra year of graduate school is different from the effect of an extra year of college, the years higher education variable will be misleading as a guide to the effect of attending but not completing college. In that case, the B.A. variable captures the departure of the slope for the college years from the slope estimated by years post-secondary schooling, as well as capturing strictly "diploma" effects.

Moreover, the advantages associated with completing college are almost as large among men with similar backgrounds and test scores as among men in general, but the advantages associated with completing high school are substantially less among men with similar backgrounds and test scores.

In our four nationally representative samples, the predicted occupational advantage of a high school graduate over a grammar school graduate, when background characteristics are controlled, is between 6 to 8 points, or 70 to 80 percent of the observed difference, respectively. The predicted advantage of college graduates over high school graduates with background characteristics controlled is close to 25 points, or 90 to 96 percent of the observed advantage in all four samples.

Our less representative samples also indicate that the effects of completing college are larger and more robust than the effects of completing high school. For example, in the Kalamazoo Brothers Sample, controlling common family background and sibling test score differences reduces the advantages associated with completing four years of high school from 22.888 to 9.556 points, or by 13.332/22.888 = 58.2 percent. The analogous reduction in the effect of completing four years of college is only 2.790 points, or 2.790/22.928 = 12.2 percent of the uncontrolled effect. The proportionate reductions in the Veterans and NORC Brothers samples are similar.

College graduates are not uniformly bright.[12] Employers may be bad judges of ability, and are consequently forced to rely on diplomas as

[12] The standard deviations of test scores for men with four years of college are from 70 to 85 percent of the overall standard deviations of test scores in our samples with ability measures.

indicators of ability. This would seem to be a potentially expensive substitute for actually testing applicants, if high scorers really are better workers. Therefore, I conclude that college graduates differ systematically from high school graduates in economically favorable ways unrelated to background and test scores.

This could happen because schools and colleges actually generate such differences, or because they merely select on the basis of the same characteristics employers value. If education generated the traits employers value, I would expect schools and colleges to confer similar benefits. Since schools confer far less benefits than do colleges, I conclude that education does not produce economically favorable characteristics in students, but rather sorts and certifies students according to previously existing characteristics. An alternative conclusion is that colleges are more effective than high schools in augmenting students' productive capacities.

Age Differences in the Occupational Effects of Education

Men who differ in age also differ in cohort membership and in the point at which they stand in the life cycle. Consequently, observed differences in the effects of education across age groups may be due to historical trends, age differences, or both.

A recent replication of the Occupational Changes in a Generation Survey suggests, however, that the effects of educational attainment on occupational status are stable for most of an individual's career.[13] Therefore, I have interpreted the intercohort comparisons in the effects of education in our data as measurements of the historical trend in the relationship between schooling and occupational status (see Table 3).

The most reliable evidence we have for intercohort differences in the occupational effects of education comes from the 1970 Census and the 1962 OCG study. The numbers of respondents in individual cohorts in the other samples are too small to allow meaningful comparisons. The 1970 Census data suggest that the effect of an extra year of schooling below the college level is slightly larger among men aged 35 and over, than among younger men, though only the coefficient for 30 to 34 year olds differs significantly from the coefficients for older cohorts. Moreover, the OCG data, in which measured background is controlled, show no significant intercohort differences in the effects of elementary and secondary schooling. Since the effects of some measured background variables on education declined from 1962 to 1973 [Hauser and Featherman, 1976], I would expect that controlling measured background in the Census would reduce the schooling coefficient on occupation more for older men than for younger men, and would lead to results in accord with the 1962 OCG study.[14]

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[13] Within-cohort education coefficients controlling measured background show no significant differences between 1962 and 1973. See David Featherman and Robert Hauser, "Changes in the Socioeconomic Stratification of the Races, 1962-73," Institute for Research on Poverty Discussion Paper, 286-75 (Madison: Institute for Research on Poverty, 1975). The Kalamazoo data do suggest, however, that the effect of schooling on initial occupation is stronger than its effect on current occupation. Compare Table 14A2 with Table 14A4 in Michael Olneck, "The Kalamazoo Brothers Sample," in Who Gets Ahead?, ed. Christopher Jencks, draft, Appendix I (New York: Basic Books, forthcoming).

[14] The 1973 OCG data do suggest that the effects of education on occupation are systematically higher for younger individuals, but this result may reflect nonlinearities in the effects of education and rising mean attainment. At this writing, I do not have the data available to check this possib[illegible]y.

Table 3

Effects of Education on Current Occupational Status, Stratified by Age

(Bracketed coefficients less than 1.96 times their standard errors)

Sample	Equation No.	Years of Education	Years of Post Secondary Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
aged 25-29 (N=3748)	1.	2.722 (.318)	3.621 (.555)	4.700 (1.786)	18.248	Experience, experience^2
aged 30-34 (N=3375)	2.	2.285 (.284)	3.797 (.564)	5.563 (1.996)	18.678	Same as equation 1
aged 35-44 (N=6963)	3.	3.061 (.136)	2.213 (.332)	4.184 (1.423)	18.864	Same as equation 1
aged 45-54 (N=6834)	4.	3.082 (.127)	2.091 (.354)	3.989 (1.566)	19.136	Same as equation 1
aged 55-64 (N=4777)	5.	3.129 (.132)	2.895 (.457)	[1.072] (2.092)	18.503	Same as equation 1
OCG						
aged 25-34 (N=3166)	6.	2.385 (.191)	3.478 (.485)	8.246 (1.970)	17.586	Measured background,[a] experience
aged 35-44 (N=3443)	7.	2.366 (.165)	2.279 (.503)	8.063 (2.194)	18.302	Same as equation 6
aged 45-54 (N=2951)	8.	2.285 (.174)	2.344 (.580)	[3.267] (2.647)	18.414	Same as equation 6
aged 55-64 (N=1944)	9.	2.208 (.212)	3.453 (.853)	[-7.294] (3.845)	19.328	Same as equation 6
Michigan PSID						
aged 25-34 (N=545)	10.	2.561 (.656)	2.557 (1.177)	[4.161] (3.682)	16.090	Measured background,[b] vocational training, experience
aged 35-44 (N=528)	11.	1.832 (.489)	2.518 (1.085)	10.655 (4.147)	15.813	Same as equation 10
aged 45-54 (N=431)	12.	1.519 (.440)	4.275 (1.125)	[2.205] (4.438)	14.766	Same as equation 10

Table 3 Continued

Sample	Equation No.	Years of Education	Years of Post Secondary Education	BA	Standard Deviation of Residuals	Other Variables Controlled
aged 55-64 (N=270)	13.	[.751] (.537)	[2.909] (1.582)	[6.494] (6.661)	15.964	Same as equation 10
Productive Americans						
aged 25-34 (N=290)	14.	1.560 (.663)	6.790 (1.612)	[-1.492] (5.544)	15.161	Measured background,[c] vocational training, experience
aged 35-44 (N=338)	15.	1.572 (.555)	3.777 (1.508)	[-1.141] (5.375)	15.768	Same as equation 14
aged 45-54 (N=331)	16.	1.278 (.508)	3.869 (1.631)	[.716] (6.368)	16.093	Same as equation 14
aged 55-64 (N=229)	17.	2.132 (.553)	6.363 (2.018)	[-7.414] (8.554)	1 5.416	Same as equation 14
Kalamazoo Brothers						
aged 35-44 (N=279)	18.	5.589 (1.414)	[-2.561] (1.805)	(4.957) (5.513)	17.906	Measured background,[d] test score
aged 45-54 (N=413)	19.	4.403 (1.145)	[-2.355] (1.612)	14.571 (5.119)	18.699	Same as equation 13

NOTES: a. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, race.

b. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, race.

c. Father's education, nonfarm, non-South, siblings, race.

d. Father's education, father's occupation, no male head, siblings.

There is no evidence that the correlation between educational attainment and cognitive ability has fallen since the 1820s, but there is evidence that the standard deviation of education fell from 1920 to 1965, though the drop is not consistent across all cohorts. [Crouse, forthcoming (a); Bartlett and Jencks, forthcoming, Table 7; and Jackson, forthcoming, Table 7.] This means that a constant difference in educational attainment represents a larger relative difference among younger men than among older men, and, that younger high school dropouts must differ more in ability from graduates than do older dropouts. Since the occupational effects of schooling below the college level appear stable across cohorts, I conclude that the contribution of ability differences to the apparent impact of schooling at a single point in time is not a good guide to the sensitivity of the below-college schooling coefficient to changes in ability differentials between high school persisters and dropouts (see Table 2). This is probably because the educational position of high school graduates relative to the mean has fallen more precipitously than has the advantage of college graduates.[15]

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[15] In the 1970 Census, eighth graders 25 to 29 are 1.53 standard deviations below their cohort mean on education, while eighth graders 60 to 64 are only 0.51 standard deviations below their cohort mean. Twelfth grade graduates 25 to 29 are 0.14 standard deviations below their cohort mean, but high school graduates 60 to 64 are 0.55 standard deviations above their cohort mean on education. College graduates 25 to 29 are 1.25 standard deviations above their cohort mean, which is 1.25/1.61 = 77.6 percent of the relative advantage of college graduates 60 to 64.

While high school graduates today are more able compared to drop-outs, than were graduates in earlier decades, they are also more typical of the general population than earlier graduates.[16] College graduates, while more numerous, are, still highly advantaged relative to mean educational attainment. This may account for the tendency of younger college graduates in both the OCG and Census samples to have larger occupational advantages over high school graduates than do older graduates.

Racial Differences in the Occupational Effects of Education

It is commonly thought that the credentials held by nonwhites and whites are rewarded unequally. Our evidence suggests that while nonwhites of a given educational attainment may not have jobs equivalent in status to those held by whites with the same amount of schooling, the occupational advantage conferred by higher education may be larger among nonwhites as among whites (see Table 4). In all four of the data sets with substantial numbers of nonwhites, the predicted status advantage of a nonwhite college graduate over a nonwhite high school graduate is larger than the predicted advantage of a white college graduate over a white high school graduate. Rather than indicating any special advantage enjoyed by nonwhite college graduates, this result probably reflects the dismal treatment accorded nonwhites without college degrees.

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16 If this argument were correct, however, I would expect, with ability controlled, the educational advantage of high school graduates to be greatest among older workers. The PSID results are in the opposite direction than expected. The differences in coefficients in the PSID are too small to be statistically significant, but nonetheless they lend no support to my argument.

Table 4

Effects of Education on Current Occupational Status, Stratified by Race

(Bracketed coefficients less than 1.96 times their standard errors)

Sample	Equation No.	Years of Education	Years of Post Secondary Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
White (N=23,615	1.	3.217 (.065)	2.211 (.173)	4.165 (.790)	18.833	Experience, experience^2
Nonwhite (N=2082)	2.	1.481 (.134)	5.015 (.560)	[2.891] (2.910)	15.721	Same as equation 1
OCG						
White (N=10,395)	3.	2.708 (.094)	2.365 (.299)	5.221 (1.283)	18.715	Measured background,[a] experience, experience^2
Nonwhite (N=1110)	4.	.804 (.152)	3.509 (.866)	21.103 (4.012)	13.844	Same as equation 1
Michigan PSID						
White (N=1260)	5.	1.476 (.297)	3.379 (.703)	5.129 (2.552)	15.938	Measured background,[b] test score, vocational training, experience, experience^2
Nonwhite (N=514)	6.	1.473 (.273)	[1.116] (1.085)	25.166 (5.298)	13.081	Same as equation 4
Parnes aged 45-59						
White (N=2580)	7.	2.043 (.194)	3.112 (.643)	5.413 (2.747)	18.672	Measured background,[c] vocational training, experience, experience^2
Nonwhite (N=250)	8.	.671 (.322)	7.219 (2.256)	[-.857] (10.921)	13.249	Same as equation 5

NOTES: a, b. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings.

c. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South.

The evidence concerning the occupational benefits of elementary and secondary education is more consistent with the conventional wisdom concerning racial differences in the effects of schooling. In three of the four data sets in which I examined racial differences, the effect of an extra year of schooling below the college level is significantly higher for whites than nonwhites. In the PSID, the effects are virtually identical for whites and nonwhites. This is partially because controlling background and test scores reduces the coefficient of years of education more for whites than nonwhites. It may also be because nonwhite heads of households are less representative of nonwhites in general, than white heads of households are of whites in general.

If these results are correct, they suggest that nonwhites who pursue a college education will realize a substantial benefit, but those who quit high school before graduating will not suffer a substantial loss in occupational status relative to individuals who complete high school, but go no further. From the point of view of policies pertaining to school retention, however, these results should be viewed cautiously unless they are substantiated with data on current youths.

Ability Differences in the Occupational Effects of Education

If schooling enhances economic success because it augments relevant cognitive skills or knowledge, I would expect more able individuals to realize larger benefits from any given amount of schooling than less able individuals. This is because more able individuals presumably learn more in a given amount of time than do less able individuals. Our evidence suggests, however, that employers are either unaware of, or indifferent to productivity differences generated by the schooling experiences of individuals with differential ability.

A multiplicative education-test score interaction term does not have a significant effect on occupation in any of our data sets. Nor do the results in Table 5 offer any significant evidence that the occupational benefits of extra schooling are larger for men with high test scores than for men with low test scores (see Table 5). This suggests that employers reward credentials in large measure without regard to direct evidence of the abilities actually possessed by individuals. Alternatively, it suggests that the premise that high-scoring individuals gain more economically relevant skills and knowledge for a given amount of schooling is incorrect.

Differences by Father's Occupational Group in the Occupational Effects of Education

More and better schooling is frequently proposed to help increase the economic life chances of poor children. With this consideration it would be useful to adequately define poverty, and to focus on the experiences of men in our samples whose origins were poverty level. Unfortunately, none of our data sets include direct information on parental income. As a partial substitute for studying men stratified by parental income level, we have stratified our samples according to whether a respondent's father held a white-collar, blue-collar, or farm job. This should give us some indication of whether the effects of schooling are similar for men from both disadvantaged and advantaged homes.

Table 5

Effects of Education on Current Occupational Status, Stratified by Test Score

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Michigan PSID						
1-9 (N=764)	1.	1.589 (.270)	2.284 (1.044)	12.245 (4.607)	14.716	Measured background,[a] test score, vocational training, experience, experience^2
10-11 (N=707)	2.	1.489 (.427)	3.050 (.956)	8.482 (3.365)	16.240	Same as equation 2
12-13 (N=303)	3.	1.744 (.830)	[2.664] (1.402)	[2.699] (4.396)	15.534	Same as equation 2
NORC Veterans						
Below 31st percentile (N=236)	4.	[.557] (.612)	5.003 (2.219)	[18.451] (11.158)	16.504	Measured background,[b] test score
31st to 64th percentile (N=264)	5.	[.762] (.892)	5.845 (1.648)	[3.914] (6.396)	15.882	Same as equation 4
Above 64th percentile (N=303)	6.	[3.569] (1.868)	[.690] (2.357)	[6.467] (4.830)	19.059	Same as equation 4
Talent 28 year olds						
Less than 90 (N=173)	7.	5.698 (1.453)			17.212	Measured background,[c] test score, education^2, experience
90 to 110 (N=395)	8.	5.075 (.602)			18.777	Same as equation 7
Over 110 (N=271)	9.	5.220 (.708)			16.677	Same as equation 7
Kalamazoo Brothers						
Less than 90 (N=168)	10.	4.003 (1.401)	[3.057] (3.294)	[-6.157] (13.523)	19.364	Measured background,[d] test score

Table 5 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
90 to 110 (N=349)	11.	5.749 (1.482)	[-3.269] (2.003)	10.440 (5.854)	19.306	Measured background,[d] test score
Over 110 (N=175)	12.	[-.803] (3.696)	[-2.710] (3.913)	13.011 (4.659)	15.274	Measured background,[d] test score

NOTES: PSID test scores 1 to 9, Sample Mean = 9.958, Sample Standard Deviation = 1.954; AFQT scored in percentiles and rescaled, Sample Mean 103.411, Sample Standard Deviation = 13.685; Talent composite standardized to a population mean of 100 and standard deviation of 15; Kalamazoo Terman or adjusted Otis scores, Sample Mean = 100.893, Sample Standard Deviation = 15.326.

a. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, race.

b. Father's education, father's occupation, no male head, nonfarm, non-South, race.

c. Father's education, father's occupation, no male head, siblings, race.

d. Father's education, father's occupation, no male head, siblings.

The results in Table 6 indicate that the differences between the occupational effects of below-college schooling for sons with white-collar and blue-collar origins are statistically insignificant. In the OCG and Parnes data, the effects of elementary and secondary schooling are significantly lower for farm-born respondents than for others, but this is not true in the PSID or the Veterans data. It is possible that the Parnes sample, which covers men 45 to 59 years of age in 1966, and the OCG study, which was conducted in 1962, include larger proportions of high school graduates from farm backgrounds who remained in farming, than do the Veterans and PSID samples. If this were the case, high school graduation would confer smaller occupational benefits for men with farm backgrounds than for others.

Our evidence is mixed with respect to the occupational advantages gained from going to college by white-collar and blue-collar origin respondents. No consistently significant pattern is evident, and few of the individual coefficients are significantly different. On the other hand, there is a consistent pattern of a significantly larger advantage for graduating from college accruing to men with farm backgrounds than to others. This result suggests a conclusion similar to the conclusion I drew about racial differences in the effects of a college education. If white-collar and blue-collar sons who do not complete college have more favorable job opportunities than farm nongraduates, I would expect a smaller difference in occupational attainment between college and non-college men among them among them, than among men with farm backgrounds.

Table 6

Effects of Education on Current Occupational Status, Stratified by Father's Occupational Group

(Bracketed coefficients less than 1.96 times their standard errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1962 OCG						
Father white collar (N=2631)	1.	2.879 (.317)	1.635 (.571)	3.729 (1.871)	19.004	Measured background,[a] experience, experience^2
Father blue collar (N= 4915)	2.	2.604 (.136)	2.221 (.466)	10.991 (2.094)	18.647	Same as equation 1
Father farm (N=3288)	3.	1.943 (.128)	3.168 (.647)	10.185 (3.089)	17.197	Same as equation 1
Michigan PSID						
Father white collar (N=329)	4.	2.966 (.910)	[.397] (1.403)	[4.776] (3.811)	14.740	Measured background,[b] vocational training, test score, experience, experience^2
Father blue collar (N=862)	5.	1.248 (.339)	3.832 (.878)	[6.573] (3.422)	15.947	Same as equation 4
Father farm (N=583)	6.	1.285 (.339)	4.446 (1.089)	9.090 (4.484)	15.494	Same as equation 4
Parnes aged 45-59						
White collar (N=550)	7.	3.290 (.592)	[1.963] (1.183)	[6.417] (4.232)	18.299	Measured background,[c] vocational training, experience
Blue collar (N=1438)	8.	2.232 (.246)	4.204 (.893)	[-1.984] (4.179)	18.942	Same as equation 7
Farm (N=828)	9.	.965 (.268)	[1.334] (1.307)	24.201 (6.372)	16.756	Same as equation 7
NORC Veterans aged 30-34						
White collar (N=153)	10.	[2.169] (1.549)	[2.506] (2.184)	[7.474] (5.853)	17.147	Measured background,[d] test score, test score by education

Table 6 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Blue Collar (N=415)	11.	[1.244] (.671)	5.281 (1.320)	[.246] (5.164)	17.031	Same as equation 10
Farm (N=143)	12.	[-.615] (.930)	[3.029] (2.628)	28.173 (11.999)	18.042	Same as equation 10
Talent age 23						
White collar (N=315)	13.	4.532 (.700)			17.917	Measured background,[e] Education^2, experience
Blue collar (N=448)	14.	5.103 (.557)			17.982	Same as equation 13
Kalamazoo Brothers						
White collar (N=278 individuals or 139 pairs)	15.	4.412 (1.817)	[-2.631] (2.107)	12.026 (4.591)	16.890	Measured background,[f] test score
	16.	[3.151] (2.807)	[-3.054] (3.225)	20.126 (7.044)	16.792	Family background,[g] test score difference
Blue collar (N=414 individuals or 207 pairs)	17.	4.490 (1.035)	[-1.086] (1.605)	[8.581] (6.000)	19.422	Measured background,[g] test score
	18.	[2.208] (1.715)	[.966] (2.293)	[5.997] (7.845)	18.170	Family background,[g] test score difference

NOTES:
a. Father's education, father's occupation, no male head, non-South, siblings, race.

b. Father's education, father's occupation, no male head, non-South, siblings, race.

c. Father's education, father's occupation, no male head, non-South, race.

d. Father's education, father's occupation, no male head, non-South, race.

e. Father's education, father's occupation, no male head, non-South, race.

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

g. Variables defined as sibling differences.

Section 4. Earnings or Income

Occupational status is an important dimension upon which individuals are stratified. However, the scales with which we measure occupations are in some ways ambiguous and incomplete. The characteristics that define occupational scales are often characteristics of workers, not jobs.[17] Moreover, men with jobs that have the same Duncan score often have very different incomes,[18] and most economic theories on the effects of education are directed toward earnings or income, not occupational status. For these reasons, an analysis of the effects of education on occupational status does not give the full picture of the effects of schooling on economic success. This section extends my analysis of the effects of schooling to income and earnings.

Because income has risen over time and because of sampling differences, the distributions of income are not the same across our data sets. However, if the effects of education are proportional along the income distribution, a log transformation of income will yield similar results across samples from different years. I therefore used the natural logarithm of earnings or income as the dependent variable in my analyses. Sometimes I will speak of the effects of education in log dollars. This convention

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[17] For example Duncan scores are defined by the levels of educational attainment and earnings of men in Census three digit occupational classifications. This may not be a defect however, if the status of a job adheres to the characteristics of those who hold it rather than in what they do or if "important" jobs go to better educated men and pay higher. The Duncan scale was constructed on the second assumption, which is supported by analyses of NORC prestige ratings.

[18] The correlation between income and occupational status is only 0.481 in Jackson's OCG complete data sample. See Gregory Jackson, "The 1962 Survey of Occupational Changes in a Generation," in Who Gets Ahead?, ed. Christopher Jencks, draft, Appendix A (New York: Basic Books, forthcoming).

refers to the observed regression coefficients. Sometimes I will speak of the effects of education in terms of percentage changes. This convention refers to results based upon the antilogs of the observed coefficients.[19]

The regression results shown in Table 7 include equations that control experience, and equations that do not. If men who get more schooling extend their working lives to compensate for the years spent in school, ignoring experience will bias downward the estimates of education averaged over a working life. However, if men with more schooling retire at the same age as those men who quit school earlier, the effects of schooling are best estimated with experience excluded.

Mincer [1974] reports the average working life for men with twelve or fewer years of schooling is forty-five years, and the duration is forty-seven years for men with thirteen or more years of schooling. This means that an extra year of schooling is generally accompanied by an extra year of work. The exception is that men who continue through college generally do not extend their working lives to compensate completely for their additional years of schooling in comparison to high school graduates. This raises the question of whether the effects of schooling are best estimated by ignoring or including experience differences.

Fortunately, the omission or inclusion of experience does not usually affect the estimated amount by which the schooling coefficient is biased because of the exclusion of background and ability measures. In young samples (e.g., Talent and NORC Veterans), excluding experience does

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[19] See Christopher Jencks, "Statistical Methods," in Who Gets Ahead? ed. Christopher Jencks, draft, Chapter 3. (New York: Basic Books, forthcoming), for a discussion of our variable definitions and statistical methods.

Table 7

Effects of Education on Natural Logarithm of Earnings or Income

(Bracketed coefficients less than 1.96 times their standard errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census (N=25,697)	1.	.0785 (.0012)			.661	None
	2.	.0818 (.0019)	-.0255 (.0058)	.1110 (.0270)	.661	None
	3.	.0867 (.0013)			.650	Experience, experience,^{2}
	4.	.0849 (.0020)	-.0166 (.0057)	.1256 (.0266)	.650	Experience, experience^2
1962 OCG (N=11,504)	5.	.0898 (.0019)			.749	None
	6.	.1057 (.0029)	-.0924 (.0113)	.2743 (.0498)	.747	None
	7.	.1005 (.0021)			.741	Experience, experience ^{2}
	8.	.1128 (.0031)	-.0837 (.0112)	.2857 (.0493)	.740	Experience, experience ^{2}
	9.	.0656 (.0022)			.721	Measured background[a]
	10.	.0778 (.0030)	-.0822 (.0110)	.2716 (.0480)	.720	Measured background[a]
	11.	.0732 (.0024)			.714	Measured background, experience, experience ^{2}
	12.	.0814 (.0032)	-.0721 (.0109)	.2840 (.0475)	.713	Measured background, experience, experience ^{2}
Michigan PSID (N=1744)	13.	.1001 (.0048)			.675	None

Table 7 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	14.	.1042 (.0082)	[-.0494] (.0237)	.2314 (.0930)	.675	None
	15.	.0931 (.0053)			.655	Experience, experience ^{2}
	16.	.0836 (.0087)	[-.0110] (.0235)	.1765 (.0909)	.654	Experience, experience ^{2}
	17.	.0874 (.0056)			.666	Measured background[b]
	18.	.0868 (.0088)	[-.0444] (.0241)	.2517 (.0930)	.664	Measured background[b]
	19.	.0804 (.0054)			.664	Test score
	20.	.0813 (.0086)	[-.0441] (.0235)	.2389 (.0921)	.663	Test score
	21.	.0747 (.0059)			.658	Measured background,[b] test score
	22.	.0726 (.0090)	[-.0419] (.0238)	.2556 (.0920)	.657	Measured background,[b] test score
	23.	.0654 (.0062)			.637	Measured background,[b] test score, experience, experience ^{2}
	24.	.0512 (.0093)	[-.0086] .0233	.2113 (.0891)	.636	Measured background, test score, experience, experience ^{2}
Productive Americans (N=1188)	25.	.0995 (.005)			.618	None
	26.	.1036 (.008)	[-.0171] (.029)	[.0295] (.118)	.618	None
	27.	.1080 (.0059)			.615	Experience
	28.	.1136 (.0085)	[-.0229] (.0293)	[.0419] (.1176)	.616	Experience

Table 7 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	29.	.0782 (.0059)			.595	Measured background[c]
	30.	.0783 (.0083)	[-.0099] (.0289)	[.0513] (.1145)	.596	Measured background[c]
	31.	.0849 (.0066)			.594	Measured background,[c] experience
	32.	.0862 (.0090)	[-.0152] (.0290)	[.617] (.1144)	.595	Measured background,[c] experience
Parnes Men aged 45-59 (N=2830)	33.	.1051 (.0041)			.794	None
	34.	.1069 (.0059)	[-.0198] (.0256)	[.0814] (.1145)	.794	None
	35.	.0824 (.0048)			.777	Measured background[d]
	36.	.0792 (.0065)	[.0010] (.0253)	[.0525] (.1127)	.777	Measured background[d]
	37.	.0686 (.0058)			.774	Measured background,[d] experience
	38.	.0665 (.0072)	[-.0027] (.0252)	[.0564) (.1124)	.775	Measured background,[d] experience
NORC Veterans aged 30-34 (N=803)	39.	.0565 (.0061)			.473	None
	40.	.0532 (.0116)	[-.0012] (.0246)	[.0433] (.0945)	.473	None
	41.	.0964 (.0140)			.470	Experience
	42.	.0952 (.0178)	[-.0055] (.0245)	.0466 (.0940)	.471	Experience
	43.	.0425 (.0064)			.459	Measured background[e]
	44.	.0381 (.0115)	[-.005] (.0239)	[.0500] (.0911)	.455	Measured background[e]

Table 7 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	45.	.0308 (.0071)			.461	Test score
	46.	.0244 (.0121)	[.0000] (.0240)	[.0661] (.0921)	.461	Test score
	47.	.0244 (.0073)			.448	Measured background,[e] test score
	48.	[.0181] (.0120)	[-.006] (.0236)	[.0690] (.0898)	.449	Measured background,[e] test score
	49.	.0557 (.0143)			.447	Measured background,[e] test score, experience
	50.	.0509 (.0179)	[-.0045] (.0236)	[.0714] (.0895)	.447	Measured background,[e] test score, experience
NORC Brothers (N=300 individuals or 150 pairs)	51.	.0997 (.0152)			.814	None
	52.	.1506 (.0286)	[-.1110] (.0645)	[.1375] (.2834)	.810	None
	53.	.0963 (.0172)			.820	Measured background,[f] age
	54.	.157 (.032)	[-.124] (.067)	[.184] (.294)	.810	Same as equation 53
	55.	.1097 (.0211)			.778	Family background[g]
	56.	.156 (.048)	[-.109] (.092)	[-.085] (.394)	.774	Same as equation 53
Talent age 28 (N=839)	57.	.0364 (.0055)			.387	None
	58.	.0567 (.0077)			.384	Experience
	59.	.0299 (.0061)			.386	Measured background[h]

Table 7 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	60.	.0260 (.0066)			.385	Test score
	61.	.0221 (.0069)			.385	Measured background,[h] test score
	62.	.0429 (.0085)			.381	Measured background,[h] test score, experience
Talent Siblings (N=198 individuals or 99 pairs)	63.	.0604 (.0110)			.380	None
	64.	.0707 (.0124)			.376	Measured background[i]
	65.	.0388 (.0140)			.375	Test score
	66.	.0494 (.0146)			.370	Measured background,[i] test score
	67.	.0566 (.0214)			.352	Family background[g]
	68.	[.0420] (.0233]			.349	Family background,[g] test score difference
Kalamazoo Brothers (N=692 individuals or 346 pairs)	69.	.0671 (.0057)			.411	None
	70.	.0792 (.0177)	[-.0265] (.0257)	[.0645] (.0825)	.407	None
	71.	.0642 (.0066)			.412	Measured background[j]
	72.	.0742 (.0185)	[-.0224] (.0260)	[.0582] (.0826)	.408	Measured background[j]
	73.	.0492 (.0069)			.406	Test score

Table 7 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
	74.	.0558 (.0182)	[-.0167] (.0254)	[.0459] (.0814)	.402	Test score
	75.	.0480 (.0075)			.406	Measured background,[j] test score
	76.	.0535 (.0188)	[-.0144] (.0257)	[.0413] (.0816)	.402	Measured background,[j] test score
	77.	.0499 (.0113)			.384	Family background[g]
	78.	[.0474] (.0310)	[-.0237] (.0395)	[.1772] (.1150)	.384	Family background[g]
	79.	.0310 (.0118)			.374	Family background,[g] test score difference
	80.	[.0229] (.0306)	[-.0148] (.0385)	[.1635] (.1120)	.374	Family background,[g] test score difference

NOTES:

a. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, $siblings^2$, race, father's occupation by race.

b. Father's education, father's occupation, father white collar, father foreign born, no male head, nonfarm, non-South, siblings, race.

c. Father's education, father foreign born, nonfarm, non-South, siblings, race.

d. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, race.

e. Father's education, father's occupation, no male head, nonfarm, non-South, race.

f. Father's education, father's occupation, father white collar, no male head, nonfarm, siblings, race.

g. Variables defined as sibling differences.

h. Father's education, father's occupation, siblings, no male head, race.

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

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

result in a larger proportionate bias due to test scores and socioeconomic background. In other samples, the proportionate bias attributable to ability and background is somewhat, though not substantially, smaller when experience is excluded. To simplify the following discussion, I generally consider the effects of education with experience controlled for those samples where experience differences have significant effects.

The Census, PSID, and PA surveys suggest that an extra year of schooling is associated with an approximate 9 to 11 percent increment in annual earnings for men aged 25 to 64 with the same amount of experience. Taking into account biases due to lower reliability of the education measure in the Census data, and coding and sample peculiarities in the PSID and PA studies, suggest [the bivariate effect of schooling on earnings is close to 10 percent. [McClelland, forthcoming (b).]

The OCG study measured annual income, and suggests that an extra year of schooling is associated with an 11 percent increase in annual income for men with equal experience. McClelland's work with the PSID indicates that substituting income for earnings does not significantly change the estimate of the bivariate effect of schooling (personal communication), so results from the OCG will be discussed concurrently with results from other surveys, with no dintinction made between earnings and income.

Effects of Controlling Family Background

In the OCG, PSID, and PA surveys, an additional year of schooling among men from similar socioeconomic backgrounds with the same amount of experience is associated with a 7.6 to 8.9 percent increase in earnings. This means that the observed relationship between schooling and ln earnings overestimates the actual effects because men from favored backgrounds enjoy earnings advantages that are independent of their higher-than-average educational attainments. Our results suggest that for men between the ages of 25 and 64 with equal experience, from 20 to 25 percent of the apparent relationship between schooling and earnings arises for this reason. The Parnes data suggest a similar bias for men 45 to 59 years old.

It is possible that unmeasured aspects of family background impart biases to the income-schooling relationship, which are not removed when only measures of socioeconomic status are controlled. In the NORC Brothers survey, however, the regression coefficient for schooling differences between brothers, when age differences are controlled, is only trivially different from the coefficient when socioeconomic background and age differences among individuals are controlled (0.09439 vs. 0.09632). Moreover, when age differences are ignored, the within-pair coefficient is slightly higher than the simple bivariate coefficient (0.10972 vs. 0.0997).

In the Talent Sibling sample, controlling measured background raises the schooling coefficient by 0.0104. Controlling family background common to brothers reduces it, but only by 0.0038. The NORC Brothers and Talent Siblings data, therefore, suggest that unmeasured family background is a minor source of bias in the income-schooling relationship.

The Kalamazoo Brothers data suggest the opposite conclusion. The regression coefficient of sibling differences in ln earnings on differences in years of schooling is 0.0499. That is 0.0172 or 0.0172/0.0671 = 25.6 percent less than the simple bivariate coefficient. It is 0.0143 less than the coefficient when measured background is controlled. Because the Kalamazoo sample is considerably larger than our other two brothers sample, our confidence is greater in the stability, if not in the generality of its results.

Even if the Kalamazoo results accurately indicate the differences between the simple regression of income on schooling and the regression of sibling difference in income on sibling differences in educational attainment, the relative importance of unmeasured versus measured background for the size of the bias in the simple coefficient remains problematic. This is because the effects of measured background on occupation and income are substantially lower in the Kalamazoo data than in nationally representative samples. If the unmeasured aspects of family background that affect education and income in the general population are weakly related to one another, sibling data would not give results much different from results found when only measured background is controlled. The 1962 OCG data suggest that this may well be the case.

Controlling measured background in Jackson's OCG complete data sample reduces the bivariate coefficient of schooling for ln income from 0.0898 to 0.0656, or by 0.0242/0.0898 = 27 percent. Among 5780 OCG respondents who reported their eldest brother's education, the correlation between ln income and education is 0.385. The within-pair standardized coefficient is 0.273, which suggests a bias due to siblings's common background of [(0.385 - 0.273)/0.385] = 29.1 percent.[20] This result suggests that the

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[20] $$\beta \text{ within} = \frac{r_{UY} - r_{U'Y}}{1 - r_{UU'}} = \frac{0.385 - 0.277}{1 - 0.605} = .273$$

U = respondent's education
U' = brother's education
Y = respondent's income

(See Footnote 6.)

family background factors common to education and earnings are, like those factors common to education and occupation in the 1962 OCG data, for the most part measured by socioeconomic variables.[21]

Effects of Controlling Cognitive Ability

Men who get more schooling are often perceived as more able than men who quit school. Indeed, this is presumably one reason employers favor men with more schooling. If men who are initially more able in an economic sense, persist in school longer than those who are less able, ignoring ability will lead to an overestimate of the effects of educational attainment on economic success.

Our measures of ability are admittedly imprecise. Cognitive tests measure only a subset of abilities. Getting through school and succeeding at work may require many abilities that are not measured by such tests. The extent to which the unmeasured abilities that affect educational and economic success are the same, or are related to one another, is unknown, and, it is therefore impossible to determine whether controlling the test scores from our data removed a large or small part of the "ability" bias in the income-schooling relationship.

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21 Again, evidence from the 1973 OCG replication suggests otherwise. Among the 6865 respondents aged 35 to 59, reporting their brothers' educations, the correlation between education and ln income is 0.396. With measured background controlled, the standardized coefficient of education is 0.318. Controlling brothers' common background, the standardized coefficient is only 0.252. The bias in the income-schooling relationship due to background appears on the order of 36 percent, rather than the 20 percent suggested by controlling only measured socioeconomic variables. The results for ln income are similar, though not as dramatic as for income.

Efforts to measure the ability bias are further limited by the fact that the Veterans and PSID tests were administered to respondents after most of them completed their schooling. If increased schooling raises test scores, we will overestimate the biases due to ability in those samples. Therefore, it is more correct to speak of the effects of schooling that are independent of test scores, than to speak of the unbiased (or less biased) effects of schooling in those data sets.

The effects of schooling on ln earnings are significantly attenuated among men with the same test scores. Controlling test scores reduces the coefficient of education by 0.0197 in the PSID, by 0.0257 in the Veterans sample, by 0.0216 among the Talent Siblings, and by 0.0179 among the Kalamazoo Brothers. These reductions represent 19.7, 45.5, 35.8, and 26.7 percent of the simple bivariate coefficient in each of those samples, respectively.

Cumulative Reductions in the Effects of Education Due to Background and Ability

The effects of schooling are even lower when men have both the same test scores and come from similar backgrounds. The coefficients of schooling, controlling measured background and test scores, are 0.0254/0.1001 = 25.4 percent and 0.0321/0.0565 = 56.8 percent less than the simple bivariate coefficients in the Michigan and Veterans samples, respectively. Controlling brothers' common background and sibling test score differences reduces the uncontrolled effect by 1 - (.0420/.0604) = 30.5 percent in the Talent Sibling sample, and by 1 - (.0310/.0671) = 53.8 percent in the Kalamazoo Brothers Sample. There are several reasons to place more confidence in the generalizability of results from the Veterans and Kalamazoo data than in the results from the PSID and Talent data. The correlation between test scores and schooling is unusually low in the PSID data, and the Talent siblings are both young and almost all at least high school graduates.[22] The Veterans and Kalamazoo results suggest that at least one-half of the observed effect of schooling on ln earnings disappears when family background and cognitive ability are controlled. With experience controlled in the Veterans sample, the estimate of proportionate bias in the effect of schooling (net of experience) is 42 percent.

If we could take into account additional differences between men with more and less schooling, it is likely that we would find that the effects of schooling on income would be further reduced. Among 389 respondents in the Kalamazoo sample for whom measured background, test scores, teacher personality ratings, and follow-up data are available, adding a rating of "executive ability" in tenth grade to an earnings equation already including socioeconomic background and test scores, reduces the effect of education by an additional ninety-seven dollars, or by 97/1119 = 8.7 percent of the effect controlling only background and test scores [Olneck, 1976, Chapter 5]. Unfortunately, our data are disappointingly inadequate for extensive exploration into biasing effects of noncognitive characteristics.

[22] The correlation between test scores and education is only 0.473 in the PSID. It is 0.554 in the Veterans sample, 0.606 among the Talent siblings, and 0.576 for the Kalamazoo brothers.

Differential Effects According to Level of Schooling

An average year of higher education is associated with a smaller percentage increase in earnings than an average year of education below the college level. But graduating from college confers substantial economic benefits, so that in most of our data sets, four years of college is associated with percentage increases in earnings that are greater than or close to the percentage increases associated with completing four years of high school. The difference between the percentage earnings increase associated with four years of high school and four years of college is greater than 10 percent only in the PSID.

Most of our data sets suggest that when background or ability are controlled, the estimates for the effects of four years of high school fall more than the estimates for the effects of four years of college. Consequently, the PSID, Parnes, Veterans, and Kalamazoo data suggest that for men who are initially similar, four years of college raises earnings by a larger percentage than four years of high school. Since the earnings of men who go to college are greater than the earnings of men who stop their schooling with high school, even in those data sets where the percentage increases associated with four years of college are the same as those associated with four years of college net of background (i.e., OCG, PA), the dollar increases associated with completing college are greater than those associated with finishing high school.

These findings suggest that (1) college graduates initially differ more from nongraduates on characteristics that we have not measured, than do high school graduates, (2) college augments productivity more than high school does, or (3) employers irrationally over-reward college credentials. Because the coefficient for holding a B.A. is especially insensitive to controls for background and ability in the OCG and PSID data, I think it is improbable that measures of other kinds of characteristics would reduce the apparent effect of having completed college. If college augmented productivity more than high school, I would expect the effect of an average year of higher education to be larger than the effect of an average year of secondary school. Since this is not the case, and since I cannot conceive of unique effects of the senior year that enhance an individual's productivity, I conclude that employers favor college graduates even when they are quite similar to nongraduates. This may, of course, only be irrational in specific instances. On the average, college graduates may be sufficiently superior workers to economically warrant the favorable treatment which they are accorded.

Age Differences in the Effects of Schooling on Ln Earnings

Our evidence on the effects of education for men of varying ages is difficult to interpret (see Table 8). This is because observed intercohort differences in the effects of schooling may arise because of age differences, cohort differences, differences associated with cohorts at particular ages, and sampling error. Bartlett's analysis of Census data for 1939 thru 1949 suggests that most of the observed differences between the effects of schooling among men of varying ages at any one point in time time are due to changes in coefficients that are related to age, rather than to differences between cohorts [Bartlett, forthcoming].

Table 8

Effects of Education on Natural Logarithm of Earnings or Income, Stratified by Age

(Bracketed Coefficients Less than 1.96 Times their Standard Errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
aged 25-29 (N=3748)	1.	.0951 (.0109)	[-.0241] (.0191)	[.0308] (.0613)	.626	Experience, experience^2
aged 30-34 (N=3375)	2.	.0841 (.0088)	[-.0146] (.0174)	[.0554] (.0615)	.575	Same as equation 1
aged 35-44 (N=6963)	3.	.0884 (.0044)	[-.0229] (.0122)	.1907 (.0463)	.614	Same as equation 1
aged 45-54 (N=6834)	4.	.0893 (.0044)	-.0286 (.0122)	.2062 (.0538)	.658	Same as equation 1
aged 55-64 (N=4777)	5.	.0602 (.0053)	[.0123] (.0183)	.1112 (.0835)	.739	Same as equation 1
OCG						
aged 25-34 (N=3166)	6.	.1004 (.0071)	-.1120 (.0179)	.4173 (.0727)	.649	Measured background, experience
aged 35-44 (N=3443)	7.	.0862 (.0058)	-.0758 (.0177)	.3197 (.0770)	.642	Same as equation 6
aged 45-54 (N=2951)	8.	.0735 (.0074)	[-.0352] (.0247)	[.0783] (.1125)	.782	Same as equation 6
aged 55-64 (N=1944)	9.	.0951 (.0088)	[-.0481] (.0353)	[.1024] (.1590)	.800	Same as equation 6
Michigan PSID						
aged 25-34 (N=545)	10.	.1223 (.0213)	[-.0393] (.0382)	.0695 (.1195)	.522	Measured background,[b] vocational, training, test score, experience
aged 35-44 (N=[illegible]28)	11.	.0708 (.0177)	[.0574] (.0392)	[-.0337] (.1500)	.572	Same as equation 10
aged 45-54 (N=431)	12.	.0582 (.0157)	[-.0403] (.0402)	.4376 (.1585)	.527	Same as equation 10
aged 55-64 (N=270)	13.	[.0130] (.0324)	[.0045] (.0954)	[.4181] (.4014)	.962	Same as equation 10

Table 8 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variable Controlled
Productive Americans						
aged 25-34 (N=290)	14.	.0914 (.019)	[.0176] (.047)	[-.0936] (.161)	.532	Measured background,[c] vocational training, experience
aged 35-44 (N=338)	15.	.0768 (.017)	[-.0004] (.046)	[.0811] (.163)	.605	Same as equation 14
aged 45-54 (N=331)	16.	.0498 (.021)	[.0107] (.067)	[.0788] (.260)	.775	Same as equation 14
aged 55-64 (N=229)	17.	.1079 (.028)	[.0414] (.101)	[-.1946] (.429)	.883	Same as equation 14
Kalamazoo Brothers						
Under 45 (N=279)	18.	.0728 (.0346)	[-.0408] (.0441)	[-.0169] (.1348)	.438	Measured background,[d] test score
45 and over (N=413)	19.	[.0448] (.0231)	[.0011] (.0325)	[.0783] (.1032)	.377	Same as equation 18

NOTES: a. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, race.

b. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, race.

c. Father's education, nonfarm, non-South, siblings, race.

d. Father's education, father's occupation, no male head, siblings.

Nevertheless, for some levels of schooling and experience there appear to be cohort differences in her data.[23]

Since the effects of a high school education appear to be reduced more than the effects of a college education when test scores are controlled, and since ability differences seem to have larger effects among men over the age of 30 than among younger men, I would prefer to rely on the PSID results rather than on our other national samples for intercohort comparisons. But as Table 8 shows, the results in the PSID are particularly sensitive to sampling error. Moreover, for men under age 35 and for men over 55, the relationships between our measures of education and ln earnings, with no other variables controlled, are quite different in the PSID from the relationships in the 1970 Census. These discrepancies preclude the use of the PSID to make general inferences about the effects of controlling ability or background on the schooling coefficients for men of varying ages.

The OCG data are also discrepant with the Census data in that the former suggest that the proportionate effects of a college education are lower for men over 45 than for men younger than 45 years of age. The PSID data and the Census data confirm that the effects of a college education are smallest among men under age 35.

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[23] For example, I calculated the predicted percentage income advantage of a high school graduate over an eighth grade graduate with no prior work experience as 21.3 percent in 1969, compared to 35.9 percent in 1949 and 38.6 percent in 1959. The predicted advantage of a college graduate over a high school graduate with 40 years of experience is 27.5 percent for 1949, compared to 41.5 percent for 1959 and 46.6 percent for 1969. Calculated from Tables 4 and 8 in Susan Bartlett, "Time Trends in the Effects of Education and Experience," in Who Gets Ahead?, ed. Christopher Jencks, draft, Chapter 14 (New York: Academic Press, forthcoming).

Since differences in the reliability of measures of education, differences in population coverage and differences in coding procedures, as well as sampling error, no doubt contribute to the varied results in the sampling analyses, I consider it fruitless to draw conclusions from Table 8.

Racial Differences in the Effects of Education on Ln Earnings

Table 9 presents the effects of education on ln earnings separately for whites and nonwhites. The results are inconsistent across samples. Most of the observed differences between coefficients for nonwhites and whites in any one sample are statistically insignificant. Thus, the inconsistencies in racial differences in coefficients between samples can be attributed to sampling error. This is particularly unsatisfying since the question of differential returns to schooling by race has concerned researchers and policymakers for some time. I would have hoped that our data would contribute toward a reasonably precise answer to the question. While they do not, neither do they support the conventional wisdom that education confers smaller economic advantages on nonwhites than it does on whites. The coefficient for elementary and secondary schooling differs significantly between racial groups only in the OCG data and the effect is larger for nonwhites than for whites. The only significant difference in the effects of higher education is also in the OCG data, and also favors nonwhites. The discrepancy between these results and the conventional wisdom is due at least in part to our choice of ln earnings as the dependent variable. Other researchers who have looked at the effects of education on ln earnings have similarly concluded that percentage effects

Table 9

Effects of Education on Natural Logarithm of Earnings, Stratified by Race

(Bracketed Coefficients Less than 1.96 Times their Standard Errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
1970 Census						
White (N=23,615)	1.	.0772 (.0021)	[-.0101] (.0058)	.1270 (.0266)	.633	Experience, experience^2
Nonwhite (N=2082)	2.	.0809 (.0064)	[.0065] (.0268)	[-.0628] (.1389)	.750	Same as equation 1
1962 OCG						
White (N=10,395)	3.	.0941 (.0033)	-.0599 (.0112)	.2436 (.0485)	.708	Experience, experience^2
	4.	.0771 (.0035)	-.0615 (.0111)	.2522 (.0477)	.696	Measured background,[a] experience, experience^2
Nonwhite (N=1110)	5.	.1128 (.0087)	-.1795 (.0533)	.7722 (.2473)	.855	Experience, experience^2
	6.	.1020 (.0093)	-.1776 (.0530)	.8221 (.2456)	.848	Measured background,[a] experience, experience^2
PSID						
White (N=1260)	7.	.0785 (.0106)	[-.0138] (.0261)	.1778 (.0969)	.609	Experience, experience^2
	8.	.0598 (.0112)	[-.0170] (.0260)	.2045 (.0960)	.601	Measured background,[b] test score, experience, experience^2
Nonwhite (N=514)	9.	.0360 (.0169)	[.0642] (.0710)	[.4138] (.3510)	.906	Experience, experience^2
	10.	.0370 (.0180)	[.0396] (.0745)	[.4430] (.3622)	.894	Measured background,[b] test score, experience, experience^2

NOTE: a,b. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings.

of schooling are not lower for blacks than for whites [Weiss and Williamson, 1971]. However, since whites earn more on the average than nonwhites, similar percentage returns to education do imply a larger dollar return for whites than nonwhites.

Ability Differences in the Effects of Education on Earnings

If more able men learn more and faster during a given educational experience than less able men, and if the economic benefits of educational attainment depend on learning, I would expect the measured effects of schooling to be greater for men with high test scores than for men with low scores. I would also expect more able men to compound their initial advantages as they continued in school. Ability differences would then have greater effects among better-educated men than among less-educated men. Our data do not support these expectations.[24]

Table 10 shows that there are few significant differences between schooling coefficients across ability groups in any of our samples.

[24] Nor do other data. The effects of measured ability on ln earnings show inconsistent and largely insignificant differences across schooling levels in the NBER-TH, Rogers, Talent 5-Year Follow up, and Husen samples analyzed by John C. Hause, "Earnings Profile: Ability and Schooling," Journal of Political Economy 80 (May/June 1972). Hause interpreted his findings as demonstrating an ability-schooling interaction, but I do believe the data he reports sustain his conclusions.

Weisbrod called attention to the possible omission of measures correlated with both ability and schooling in Hause's analysis, e.g., motivation. This would not in itself bear on the question of an ability-education interaction. However, if an omitted variable bore a different relationship to ability across several levels of education, it could account for an apparent ability-education interaction. For example, if motivational differences between ability levels are greater among better educated men than among less educated men, and if as Weisbrod suggests, motivation and ability are negatively correlated within educational levels, then the differences between the actual ability coefficients across educational levels would be larger than present data suggest. Burton Weisbrod, "Comment on Hause's 'Earnings Profile: Ability and Schooling'," Journal of Political 80 (May/June 1972).

Table 10

Effects of Education on Natural Logarithm of Earnings, Stratified by Test Score

(Bracketed coefficients Less than 1.96 Times their Standard Errors)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
PSID						
1-9 (N=764)	1.	.0416 (.0107)			.735	Measured background,[a] test score, experience
	2.	.0438 (.0132)	[-.0927] (.0507)	.5771 (.2294)	.732	Same as equation 1
10-11 (N=707)	3.	.0772 (.0093)			.606	Same as equation 1
	4.	.0868 (.0156)	-.0689 (.0347)	.2759 (.1247)	.605	Same as equation 1
12-13 (N=303)	5.	.1020 (.0142)			.594	Same as equation 1
	6.	.0966 (.0315)	[-.0094] (.0527)	[.0873] (.1667)	.596	Same as equation 1
Kalamazoo Brothers						
Less than 90 (N=168)	7.	.0753 (.0178)			.370	Measured background,[b] test score
	8.	.0881 (.0268)	[-.0655] (.0631)	[.2682] (.2590)	.371	Same as equation 7
90-110 (N=349)	9.	.0356 (.0115)			.434	Same as equation 7
	10.	[.0370] (.0334)	[.0273] (.0451)	[-.1701] (.1318)	.435	Same as equation 7
Over 110 (N=175)	11.	.0483 (.0117)			.362	Same as equation 7
	12.	[.0355] (.0870)	[-.0215] (.0921)	.2155 (.1097)	.360	Same as equation 7

Table 10 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Talent age 28						
Less than 90 (N=173)	13.	[.0151] (.0247)			.380	Measured background test score, experience
90-110 (N=395)	14.	.0540 (.0109)			..362	Same as equation 13
Over 110 (N=271)	15.	.0484 (.0173)			.405	Same as equation 19
NORC Veterans						
Less than 96 (N=236)	16.	.1015 (.0299)			.487	Measured background,[d] AFQT, experience
	17.	.1064 (.0313)	[-.0761] (.0662)	[.2499] (.3335)	.487	Same as equation 16
96-103 (N=264)	18.	[.0124] (.0239)			.413	Same as equation 16
	19.	[-.0016] (.0318)	[.0221] (.0431)	[-.0068] (.1670)	.414	Same as equation 16
Over 103 (N=303)	20.	.0516 (.0219)			.426	Same as equation 16
	21.	[.0497] (.0460)	[-.0071] (.0528)	.0534 (.1084)	.427	Same as equation 16

NOTES: PSID test scored 1 to 13, Sample Mean = 9.958, Sample Standard Deviation = 1.954, AFQT scored in percentiles and rescaled, Sample Mean = 103.411; Sample Standard Deviation = 13.685; Talent composite standardized to a population mean of 100 and standard deviation of 15; Kalamazoo Terman or adjusted Otis scores, Sample Mean = 100.893, Sample Standard Deviation = 15.326.

a. Father's education, father's occupation, father white collar, no male head, nonfarm, non-South, siblings, race.

b. Father's education, father's occupation, no male head, siblings.

c. Father's education, father's occupation, no male head, siblings, race.

d. Father's education, father's occupation, no male head, nonfarm, non-South, race.

Moreover, the patterns of observed differences among ability groups are not consistent across samples.

Jencks looked at ability effects within educational levels for the NORC Veterans sample, and I did so for the Kalamazoo sample. We found no consistent and few significant differences in ability coefficients across educational levels.

Ad hoc explanations can be conceived to explain away our negative findings. The plausibility of an ability-schooling interaction to explain the greater educational investments of more-able individuals is theoretically appealing. But ad hoc explanations cannot substitute for positive evidence. Evidence such as that reported here, as well as other research, does not sustain the hypothesis of a systematic or significant education-ability interaction with respect to ln earnings. Because high ability men earn more on the average, the absence of a negative ability-schooling interaction with respect to ln earnings does indicate that the dollar returns to increased schooling may be significantly higher among high scores than among low scores.

Differences by Father's Occupational Group in the Effects of Education on Earnings

Our evidence on the differential effects of schooling for men from varying social backgrounds is also in accord with previous work. It shows no consistent differences among men from white-collar, blue-collar, and farm backgrounds (see Table 11).[25]

Caveat on Measurement Error

This paper emphasized omitted variables as a source of upward bias in the observed effects of schooling on occupational status and earnings. There is a well-known source of downward bias that I have ignored--measurement error. If education or background variables are measured inaccurately, there is some likelihood that the effects of education will be underestimated when cognitive skills and family background are controlled. The extent of the remaining downward bias depends on the relationships among errors in measurement, and among errors and the true values of variables, as well as on the effects of still omitted variables affecting both schooling and income.

I ignored the effects of measurement error because I generally did not have the data needed to correct for it. Accuracy of measurement varies from survey to survey, so reliabilities or estimates of error variance from one sample may not apply to others. Few of our data sets have multiple measures of variables that are essential to estimating reliabilities for correlations, and none include information that permit confident estimates of the relationships between errors in measurement and true values, which are necessary for estimating true variances.

[25] Hauser divided OCG and 1957 Wisconsin High School Senior respondents by farm background, and father's Duncan score for nonfarm men. He found no convincing nor consistent differences in the effects of schooling on ln income or ln earnings in either sample. See Hauser, "Earnings Profile."

Table 11

Effects of Education on Natural Logarithm of Earnings or Income, Stratified by Father's Occupational Group

(Bracketed coefficients less than 1.96 Times their Standard Error)

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
OCG						
White collar (N=2631)	1.	.0502 (.0115)	[-.0230] (.0206)	.2512 (.0676)	.686	Measured background,[a] experience, experience^2
Blue collar (N=4915)	2.	.0762 (.0046)	-.0750 (.0157)	.2600 (.0706)	.628	Same as equation 1
Farm (N=3288)	3.	.0863 (.0062)	-.0825 (.0313)	.3821 (.1493)	.831	Same as equation 1
Michigan PSID						
White collar (N=329)	4.	.1377 (.0355)	[-.0578] (.0547)	[.0392] (.1486)	.575	Measured background,[b] test score, vocational training, experience, experience^2
Blue collar (N=862)	5.	.0320 (.0140)	[.0075] (.0364)	[.2555] (.1417)	.661	Same as equation 4
Farm (N=583)	6.	.0595 (.0141)	[-.0137] (.0453)	[.3343] (.1867)	.645	Same as equation 4
Talent 28 Year Olds						
White collar (N=448)	7.	.038 (.011)			.355	Measured background,[c] test score, experience^2, experience
Blue collar (N=315)	8.	.060 (.016)			.402	Same as equation 7
Kalamazoo Brothers						
White collar (N=278 individuals	9.	[.0909] (.0494)	[-.0933] (.0573)	[.1541] (.1249)		Measured background,[d] test score
or 139 pairs)	10.	[.0412] (.0701)	[-.0954] (.0805)	.3765 (.1759)		Family background,[e] test score difference

Table 11 Continued

Sample	Equation No.	Years of Education	Years Higher Education	BA	Standard Deviation of Residuals	Other Variables Controlled
Blue collar (N=414	11.	.0456 (.0190)	[.0298] (.0294)	[-.1044] (.1099)		Measured background,[d] test score
individuals or 207 pairs)	12.	[.0196] (.0318)	[.0434] (.0426)	[-.0683] (.1457)		Family background,[e] test score

NOTES: a, b. Father's education, father's occupation, no male head, nonfarm, non-South, siblings, race.

c. Father's education, father's occupation, no male head, siblings.

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

e. Variables defined as sibling differences.

Our evidence, along with other recent work assessing the consequences of measurement error, suggests that my estimates of the effects of education are not seriously underestimated by ignoring the problem. Bielby, Hauser, and Featherman [1976] indicate that errors in measuring parental socioeconomic status and education in the 1973 OCG, impart a 10 percent downward bias to the schooling coefficient in their equation predicting occupational status. The difference between the corrected and uncorrected coefficients is only 4.91 - 4.39 = 0.52 points [Bielby, Hauser, and Featherman, 1976, Tables 7 and 8].

Corrections for measurement error affecting correlations in the Kalamazoo data suggest that the true standardized effect of education on dollar earnings, controlling sibling test score differences and family background common to brothers is 0.226. The effect without correcting for measurement error is 0.220 [Olneck, 1976: 196].

Bishop [1976] has noted that the use of sibling data can exacerbate the problem of measurement error, and has argued that the within-pair unstandardized effect of schooling on earnings is, at a maximum, only 83 percent of the true effect. However, the accuracy of educational reports in the Kalamazoo data appears to be slightly higher than in the CPS data Bishop analyzed.[26] My results would indicate that if there were no

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[26] Bishop estimated the correlation between reported and true values as 0.90, assuming that errors in separate reports of education are correlated at 0.40. See John Bishop, "Reporting Errors and the True Return to Schooling," unpublished paper (Madison: University of Wisconsin, 1976), p. 5. I estimated the correlation between true and reported values of education in the Kalamazoo data as 0.964. See Olneck, "Determinants of Educational Attainment," pp. 172-178.

other omitted variables, the observed within-pair coefficient of education for earnings is 89 percent of the true coefficient.[27]

However, the Kalamazoo sample also includes an ability measure. The remaining bias in the within-pair education coefficient due to measurement error depends on the relative degree of error in the schooling and ability variables and on the sibling correlations for these variables. Since the ratio of error variance to the variance of sibling differences in education appears to be smaller than the analogous ratio for test scores, adding test score differences reduces the remaining downward bias in the within-pair education coefficient.[28] Therefore, the observed coefficient of 0.0310 for ln earnings in the Kalamazoo data is probably at least 90 percent of the true effect, unless there are important remaining omitted variables. These calculations do not suggest that my conclusions regarding

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[27] I calculated the error variance of schooling as $(2.73)^2\ (1-.964^2) = 0.5270$. Bishop gives the ratio of the observed to the true coefficient as

$$b_t/\beta = 1/\alpha\ [1 - \frac{2V(u_i)}{V(\Delta P)}],$$

where β = true coefficient

b_t = observed coefficient

α = correction for floor and ceiling effects producing a correlation between the errors in measurement and true values.

$V(u_i)$ = error variance in education

$V(\Delta P)$ = variance of sibling differences in education.

Adopting Bishop's values of $\alpha = 0.95$, I have $bt/\beta = [1 - 2\ (.527)/6.720] \div .95 = .888$.

[28] Assuming random errors and a reliability of 0.9293, the error variance in schooling is $(2.73)^2(1-0.9293) = 0.5270$. The ratio of error variance to the variance of sibling differences is 0.5279/6.7288 = 0.07832. If errors in test scores are random, assuming a reliability of 0.900 yields an error variance of $(15.32)^2(1-0.900) = 23.3292$. The ratio of error variance in test scores to the variance of sibling differences is 23.3292/249.5294 = 0.0935. (See Bishop, "Reporting Errors.")

the effects of education would be substantially altered by corrections for measurement error. Since such corrections are problematic and arbitrary, ignoring them seems reasonable.

Effects of Educational Quality

Individuals often try to go to a good school because they believe that going to a good school leads to higher economic benefits. But individuals who go to good schools are usually also the "right kind of ma-material." Sorting out the effects of school resources, characteristics of classmates, and individual characteristics is difficult, and our evidence on the effects of college quality is plagued by the confounding of these factors.

The Productive Americans Survey rated the colleges respondents attended by a selectivity index that is divided into unaccredited, non-selective, selective, highly selective, and very highly selective categories. [See McClelland, forthcoming(a), for a description of the index.] The index is based on the ratio of acceptances to applicants, freshman test scores, freshman high school rankings, and similar data. It therefore does not separate student characteristics from institutional resources.

For men with similar backgrounds in the PA, the differences in college selectivity bear no significant relationship to occupational attainment. [McClelland, forthcoming(a), Tables 14a and 16a.] Indeed, men from non-selective colleges have a slight occupational advantage over men from more selective colleges. The earnings of men with similar backgrounds and occupations, who worked the same amount of weeks, from selective and very highly selective colleges are about 41.9 percent higher than the earnings of men from non-selective colleges. The earnings of men from highly selective colleges are about 18.5 percent higher than the earnings of men from non-selective colleges, but the effect is statistically insignificant.

I suspect that if we could control individual ability, our estimates of the earnings effects of college selectivity would fall substantially, and would perhaps even be negative. In a subsample of 1957 Wisconsin high school seniors who attended college, only one-twentieth of the variance in 1967 earnings lay between twelve categories of college type. Controlling socioeconomic background and tenth grade aptitude test scores reduced the amount of between-college type earnings variance to one-fortieth. Moreover, increased college prestige bore no consistently positive relationship to earnings at age 27. [Sewell and Hauser, 1975].

The likelihood that apparent differences in the economic benefits of differential educational experiences are due to prior differences between individuals is supported by analyses of the effects of high school track assignment in the Veterans data. Taken alone, assignment to a college track is associated with large and significant advantages on both occupational status and earnings. However, once socioeconomic background and

AFQT are controlled, the effects of track assignment on both outcomes are small and insignificant.[29]

Section 5. Conclusions

The effects of schooling on economic success are not uniform. When cognitive ability and family background are controlled, only one-half of the advantage of high school graduates over grammar school graduates on early occupational status persists, but the advantage of college graduates over high school graduates is more than 80 percent as large as it is among men in general.

Among men who are similar, the advantage in current occupational status associated with completing four years of high school is less than one-half of the advantage associated with completing four years of college. One-half of the apparent effect of a high school education on occupation is due to the joint association of schooling and occupational status with family background and ability. Only 10 percent of the apparent effect of completing college is similarly spurious.

Nonwhites and sons of farmers gain the most occupational advantage from completing college, but the occupational effects of completing high school do not consistently favor any subgroup.

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[29] Since most respondents took the AFQT after completing their schooling, a skeptic could argue that track assignment affects test scores, and that controlling AFQT is consequently illegitimate. However, analyses of Project Talent high school data suggest that changes in test scores from nineth to twelfth grade that are related to track placement are quite small. Christopher Jencks et al., Inequality: A Reassessment of the Effect of Family and Schooling in America (New York: Basic Books, 1972), p. 108.

The bias in the simple education-ln earnings relationship due to family background and cognitive ability may be over 50 percent. Corrections for measurement error would probably reduce this estimate to not less than 45 percent; introducing reliable measures of relevant non-cognitive characteristics might increase it. The percentage effects of a college education on earnings are larger and more robust than the effects of a high school education. Whites do not receive larger proportionate benefits from increments in schooling than do nonwhites. Nor is there significant evidence suggesting that cognitive ability or socioeconomic background interact with education.

APPENDIX:

Sample Standard Deviations of Current Occupation and Ln Earnings

Sample	N	Current Occupation	Ln Earnings
1970 Census			
Total	25697	24.543	0.716
Aged 25-29	3748	24.166	0.648
Aged 30-34	3375	24.748	0.616
Aged 35-44	6963	24.926	0.682
Aged 45-54	6834	24.336	0.733
Aged 55-64	4777	24.034	0.814
White	23615	24.466	0.695
Nonwhite	2082	20.544	0.802
1962 OCG			
Total	11504	24.873	0.819
Aged 25-34	3166	25.608	0.738
Aged 35-44	3443	25.176	0.758
Aged 45-54	2951	23.832	0.887
Aged 55-64	1944	24.463	0.912
White	10395	24.742	0.769
Nonwhite	1110	18.162	0.937
Father White Collar	2631	23.912	0.745
Father Blue Collar	4915	23.541	0.693
Father Farm	3288	20.721	0.915

Sample	N	Current Occupation	Ln Earnings
Michigan PSID			
Total	1774	21.067	0.753
Aged 25-34	545	21.076	0.582
Aged 35-44	528	21.727	0.680
Aged 45-54	431	20.301	0.642
Aged 55-64	270	20.150	1.097
White	1260	20.879	0.700
Nonwhite	514	18.933	1.001
Test Score 1-9	764	17.433	0.810
Test Score 10-11	707	21.140	0.667
Test Score 12-13	303	20.537	0.652
Father White Collar	329	19.132	0.660
Father Blue Collar	862	20.611	0.752
Father Farm	583	20.128	0.778
Productive Americans			
Total	1188	20.610	0.707
Aged 25-34	290	21.920	0.532
Aged 35-44	338	20.526	0.605
Aged 45-54	331	20.064	0.775
Aged 55-64	229	19.511	0.883
Parnes 45-59 Year Olds			
Total	2830	24.794	0.883
White	2580	24.761	0.854
Nonwhite	250	17.789	0.997

Sample	N	Current Occupation	Ln Earnings
Father White Collar	550	24.015	0.649
Father Blue Collar	1438	23.587	0.712
Father Farm	825	20.484	1.113
NORC Veterans 30-34 Year Olds			
Total	803	23.368	0.498
AFQT Below 31st Percentile	236	17.814	0.516
AFQT Between 31st and 64th Percentile	264	21.105	0.444
AFQT Above 64th Percentile	303	24.127	0.459
Father White Collar	153	23.583	Not analysed
Father Blue Collar	415	21.914	Not analysed
Father Farm	143	21.527	Not analysed
NORC Brothers			
Total	300	24.194	0.870
Talent 28 Year Olds			
Total	839	Not analysed	0.396
Test Score Less than 90	173	19.409	0.382
Test Score 90 to 110	395	22.671	0.377
Test Score Over 110	271	21.454	0.412

Sample	N	Current Occupation	Ln Earnings
Father White Collar	315	23.076	0.422
Father Blue Collar	448	22.393	0.362
Talent Siblings			
Total	198	25.643	0.407
Kalamazoo Brothers			
Total	692	23.157	0.446
Under 45	279	23.829	0.482
45 and over	413	22.572	0.419
Test Score Less than 90	168	20.957	0.387
Test Score 90 to 110	349	22.461	0.449
Test Score Over 110	175	18.782	0.385
Father White Collar	242	20.973	0.502
Father Blue Collar	450	23.409	0.396

NOTES: OCG item is ln income, not ln earnings.

S.D. of initial occupation is 20.732 in the PSID, and 23.787 in the Kalamazoo sample.

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