University of Wisconsin-Madison

Institute for Research on Poverty

Discussion Papers

Michael R. Olneck
Ki-Seok Kim

THE RELATIONSHIP BETWEEN EDUCATION AND INCOME AMONG AMERICAN MEN: Some Revisions and Extensions

DP #871-88

Institute for Research on Poverty
Discussion Paper no. 871-88

The Relationship between Education and Income among American Men: Some Revisions and Extensions

Michael R. Olneck
School of Education,
Policy Studies, and
Department of Sociology
University of Wisconsin-Madison

and

Ki-Seok Kim
Department of Education
Seoul National University
Seoul, Republic of Korea

November 1988

We received and appreciated helpful comments on an earlier version of this paper from David Bills, James Crouse, Jock Evanson, Yitzak Haberfeld, Christopher Jencks, Burton Weisbrod, Philip Wexler, and two anonymous reviewers for Sociology of Education. Daedong Hahn provided much appreciated computer assistance.

The research reported in this paper was funded by the Wisconsin Center for Education Research under a grant from the National Institute of Education (NIE-G-81-0009). Any opinions expressed are solely those of the authors.

Abstract

Drawing on analyses of the 1973 and 1962 Occupational Changes in a Generation Surveys, this paper reports and attempts to explain empirically the apparent anomaly that between 1961 and 1972 the pecuniary effects of completing high school among men aged 25 to 34 years old rose appreciably.

Our finding is new because sociologists studying the relationship between educational attainment and income have generally heretofore omitted measures of a twelfth-grade "diploma" effect. Our finding is theoretically significant because, under conventional assumptions of human capital theory, we would have expected the effects of high school graduation to have fallen between 1961 and 1972 as the proportion of men who were high school graduates rose.

We are unable to explain the increase in the effects of high school completion in terms of widening human capital differences between dropouts and graduates, in terms of queuing processes, or in terms of demand factors associated with occupational composition. We are able to statistically, but not substantively, explain the increase as partly due to demand factors reflected in labor force participation differentials.

We conclude by suggesting that as high school graduation becomes increasingly common, the social definition of the high school dropout as an unqualified labor market pariah intensifies, and the economic disadvantages suffered by dropouts increase beyond those predicted by simple models of the education-income relationship.

I. INTRODUCTION

This paper reports and attempts to explain empirically the apparent anomaly that between 1961 and 1972 the pecuniary effects of completing high school among men aged 25 to 34 rose appreciably. The finding is new because sociologists studying the relationship between educational attainment and income have generally heretofore omitted measures of a twelfth-grade "diploma" effect. The finding is potentially anomalous and theoretically significant because the proportion of men who were high school graduates rose between 1961 and 1972. If income returns to schooling reflected returns to scholastically enhanced skills brought to a competitive labor market as traditional human capital theory assumes (Blaug, 1972, 1976), we would expect, all else constant, for the effects of high school graduation to have fallen between 1961 and 1972. That they did not invites our scrutiny.

We are not, of course, the first to notice that returns to schooling do not invariably fall when the supply of better-educated workers increases (see, for example, Becker, 1964: 127-135). Economists of the human capital school tend to assert that the expected fall in schooling returns under conditions of increased supply is blunted either by improvements in the quality of schooling which widen skill differentials between graduates and nongraduates, or by increased demand for educated labor arising from shifts in industrial composition or from changes in technology (see e.g., Tinney, 1972, cited in Welch, 1974; Levin, 1972; Featherman and Hauser, 1978). Conceivably, too, even in the absence of improvements in quality of schooling, widened skill differentials between graduates and nongraduates may have arisen if high school graduation became a finer "reverse filter."

An alternative explanation to accentuated differences in the attributes of those with more or less schooling, or to shifts in demand, would draw on those theories relating income differentials to educational attainment which emphasize the effects of position within the education distribution per se. Economists like Lester Thurow (1972, 1974, 1975) and sociologists like Aage Sorensen (1978), for example, have proposed models of "job competition" or "vacancy competition" in which schooling serves as ranking or queuing mechanism to allocate individuals to an exogenously determined distribution of jobs and income opportunities (see also Green, with Ericson and Seidman 1980, and Seidman, 1981, 1982). The choice of schooling to play this allocative function is explained by the presumed inverse correlation between educational attainment and training costs, or by reference to other economically valued traits for which schooling is a presumably valid screen, or by the normative appeal of schooling as a legitimate rationing device.

Under queuing models, increases in the proportions of well-educated workers can have the effects of <u>both</u> lowering the incomes of such workers relative to the average for all workers <u>and</u> widening the income gaps between well-educated and poorly educated workers. This happens because poorly educated workers are displaced into lower-paying jobs at rates such that they experience an even greater decline relative to the mean than do well-educated workers (Thurow, 1974; Seidmdan, 1982). In such a scenario, education becomes, in Thurow's words, a "defensive necessity," under which "[it] becomes a good investment not because it raises an individual's income above what it would have been if no one had increased his education, but because it raises his income above what it would be if others acquire an education and he does not" (1974: 416; see also Seidman, 1982: 272). The result is, of course, an inflationary spiral in educational credentials (Collins, 1979).

As is indicated below, our data are consistent with the possibility that an increase in the proportion of men who are high school graduates may be associated with both a decline in the income of graduates relative to the mean, and a widening differential between graduates and nongraduates. This finding in and of itself does not, however, establish that shifts in the relative position of high school graduates and dropouts in the education queue explain the widening gap between their incomes. For that to be the case, the competitive advantage provided by high school graduation would have to have increased relative to that provided by completion of earlier years of schooling. Alternatively, the impact of relative educational advantage on income would have to have risen.

In the analyses which follow, we attempt directly and indirectly to account, in terms of the alternative explanations just introduced, for the increase we observe in the effects of high school graduation. Our effort, as will be apparent, is not successful, and we are led, drawing on Meyer's (1977) observation that educational institutions define categories of qualified personnel, to suggest that as graduation rates increasingly approximate universality, societal perceptions of the high school dropout as unqualified become stronger and more widespread, with the result that relegation of dropouts to lower-paying niches increases in excess of changes in the differential attributes of dropouts and graduates, in excess of changes in the relative distribution of schooling, and in excess of changes in sectoral demand.

II. MEASURING EDUCATION

If increases in schooling increased job-related skills at a constant rate, and if other assumptions of human capital theory were met, educational attainment could be measured simply as years of school attended. If, on the other hand, the association between schooling and income arises because education screens individuals according to pre-existing attributes related to productivity, or because employers arbitrarily reward credentials, we should measure years of schooling completed and also allow for "sheepskin" or diploma effects. Finally, both because the economic relevance of educationally imparted skills or the severity of screening may vary by education level, and because shifts in demand may differentially affect the value of varying levels of schooling (Featherman and Hauser, 1978: 228), we should distinguish the effects of increases in attainment within differing ranges of schooling.

The surveys from which our data are drawn (see below) each asked respondents to report the highest grade of schooling they had completed (see Featherman and Hauser, 1978: 503). Following Featherman and Hauser (1978), we first measure schooling as years of graded schooling and years of higher education. Next, we specify educational attainment so as both to allow the effects of additional elementary and secondary schooling to differ form the effects of increments in higher education and to allow for diploma effects associated with both high school and college graduation. This last specification modifies Olneck's (1979) specification which omits a measure of high school graduation.[1]

Our measures of educational attainment to this point are variants of the natural metric in which education occurs. In order to test the applicability of queuing theories, we utilize several measures which formulate educational attainment as position within the distribution of schooling. First, following Sørensen (1978), we rescale educational attainment into a metric which expresses the competitive advantage associated with each year of schooling as an increasing function of the decreasing proportion of individuals whose years of schooling exceed any given level. In simple terms, our metric represents how close to the top of the educational distribution an individual stands. Increases in standing for each additional year of schooling are treated as greater the higher in the queue an individual is initially placed. Sorensen (1978: 41) defends this transformation as preferable to the simple use of percentiles on the grounds that the use of percentiles would imply that competitive advantage is distributed uniformly, a likelihood that he finds small.

On the premise that a queuing process might function not so much to distinguish individuals by how much closer they are to the top of the distribution, but by how much further they are from the bottom of the distribution, we next create a measure of "competitive nondisadvantage," calculated as a decreasing function of the increasing proportion of individuals whose educational attainment falls below any given level.

On the premise that competitive advantage might, in fact, be distributed uniformly, we also experiment with a percentile measure of educational attainment.

III. ANALYSIS STRATEGY

Utilizing Ordinary Least Squares Regression, we first estimate the separate effects of graded schooling and higher education on proportionate income (i.e., log-income). Then we test for the significance of diploma effects and test for the adequacy of our spline specification to accurately measure the effects of completing high school or college.

Finding that diploma effects are, in a number of instances, significant, and that it is necessary that they be explicitly modeled, we attempt to account for the unexpected increase, between 1961 and 1972 among men aged 25 to 34 years old, in the effect of high school completion. Drawing on historical accounts of American education, on data pertaining to trends in achievement test scores, and on extant research testing for the interaction between educational attainment and quality of the educational process, we argue against the hypothesis that improvements in the quality of schooling which may have widened skill differentials between graduates and nongraduates can account for our puzzling finding. Drawing on extant research relating standardized test scores to income, and on evidence within our own data on the socioeconomic backgrounds of high school graduates and dropouts, we also reject the hypothesis that the attributes of high school dropouts in the later period were appreciably more distinct from those of graduates than they were in the earlier period.

We then investigate whether our positional measures of education accurately predict changes between 1961 and 1972 in the effects of high school completion. Finding that they do not, we turn to possible demand-related explanations for the change. While we might expect a general increase in the demand for labor, as occurred between 1962 and 1973,[2] to diminish the income differentials associated with variable worker characteristics (Thurow, 1975: 95), expansion of the "service" or "postindustrial" sector of the economy could, as Featherman and Hauser (1978) argue, raise the premium paid to better-educated workers. This could happen in one of three ways: educational differences could become more salient for occupational selection; income differences among occupations, holding constant education, could widen; and within those occupations characteristic of the service or postindustrial sectors, income differences among men with varying amounts of education could increase (Featherman and Hauser, 1978: 227-228). The expectation that increased effects of education on occupational attainment and earnings will be associated with an expanding service sector is usually expressed in reference to the effects of collegiate education. But if developments throughout the economy tended toward general increases in requirements for cognitive acuity or responsiveness to normative controls (Collins, 1974), increasing returns to secondary schooling might also be observed. We test for the consequences of these possibilities by introducing measures of occupation into models relating income and education, and by introducing a term for the interaction between schooling and occupation.

Last, in order to determine if changing differentials in labor force participation accounts for the increased impact of high school completion, we introduce terms representing annual weeks worked.

IV. DATA AND VARIABLES

Our analyses are conducted for the most part with the 1962 and 1973 Occupational Changes in a Generation survey samples. These surveys are supplements to the Census Bureau's 1962 and 1973 March Current Population Surveys of American households. The 1962 OCG-I survey sampled men aged 20 to 64 who were not living in institutions or barracks, and includes 20,700 respondents. The 1973 OCG-II survey sampled civilian, noninstitutionalized men aged 20 to 65. It includes 33,600 respondents. The data gathered by the surveys include measures of demographic and socioeconomic background characteristics such as age, race, region of birth, father's educational attainment and occupation, family composition and sibship size, and measures of each respondent's educational attainment, occupation, income, and labor force participation (for detailed sample description, see Featherman and Hauser, 1978: 3-9, 507-514). These data have figured centrally in the major sociological studies of status attainment and stratification published during the last twenty years (see especially Blau and Duncan, 1967; Duncan, Featherman, and Duncan, 1972; Jencks et al., 1972; Hauser and Featherman, 1977; Featherman and Hauser, 1978; and Jencks et al., 1979), and are the largest, nationally representative samples which include the kinds of information necessary for controlling socioeconomic background characteristics which may confound the relationship between educational attainment and income.

We limit our principal analysis samples to men aged 25 to 34, who were out of school and in the civilian labor force, and who reported positive incomes (for extended discussion of certain sample restrictions we employ, see Jencks, 1979a: 39-49). The most consequential of these limitations is the lower age restriction. The most important reason for this restriction is that it is hazardous to estimate the effects of schooling when large numbers of individuals have yet to complete their educational careers and enter the labor force. In a more inclusive population, the subset of individuals who are out of school and in the labor force are an unrepresentative base upon which to reach conclusions about the true consequences of educational differentials.

A side benefit to restricting our sample to men at least 25 years old is that above this age the direct effects of age per se are minimized (Jencks, 1979a: 47), so that the confounding of age effects with the effects of workforce experience and schooling is reduced.

Restricting our samples to men at least 25 years old means that if, as some evidence suggests, the economic plight of high school dropouts is especially severe in the years directly following school leaving (Hill, 1979; but contrast Blakemore and Low, 1984), we will underestimate the average effects of dropping out in the working population 34 years old and under. However, if the disadvantages young high school dropouts experience are temporary, and possibly due to youth itself rather than to schooling deficiencies, our results will be the more indicative of the long-term economic circumstances that dropouts encounter.

Zero and negative income recipients are excluded so as not to confound the determinants of the presence of positive income with the determinants of variation in received incomes.[3]

Our principal dependent variable is the natural logarithm of personal income (LNINC), measured for 1961 in the OCG-I survey, and for

1972 in the OCG-II survey. We chose for several reasons to analyze personal income, which includes wages and salaries, self-employment income, transfer payments, and "unearned" income (e.g., interest, dividends, rent) rather than earnings alone. The most important of these is that we wished to maintain comparability with the analyses of the 1962 OCG-I data reported in Olneck (1979). The second is that we could see no ready way to separate "labor" income from "asset" income among the self-employed. Finally, we reasoned that unless the object of interest is wage rates, the distinction between "earned" and "unearned" income is not entirely warranted. Unless unearned income derives from inheritance, it is in large measure a function of prior or current earnings. In the United States inheritance is of minor consequence for the distribution of income (Jencks et al., 1972: 214), and welfare eligibility, size of pensions, and availability of investment income are all influenced largely by how much an individual earns or has earned.

In retrospect, because the several theories upon which we draw pertain to the distribution of labor income, we believe our choice of income measure is somewhat problematic, but we did not reach this conclusion until our work was completed. As a check on our findings, we have confirmed that the changes which we report for men aged 25 to 34 in the effects of education between 1961 and 1972 are virtually identical whether we use our measure or a measure of labor income that includes wage and salary earnings, and farm and nonfarm income from self-employment (results not reported). In these cohorts, nonlabor income represents only 0.5 percent of personal income.

We have analyzed logarithmic income rather than dollar income to maintain comparability with earlier work and for several substantive reasons. Logarithmic variation is more sensitive to differences in the lower range of income than to differences in the upper range, and for reasons of public policy we are particularly interested in the potential of a high school education to reduce the likelihood of poverty. Moreover, increases in "utility" or "well-being" derived from any given additional income may well diminish as initial income rises. What may count most is proportionate increases, and the logarithmic coefficient is readily transformed into a percentage change.[4] Finally, for the previous reason, even when constant dollars are employed, when there could be temporal differences in income distributions, logarithmic coefficients are more readily comparable across time than are dollar coefficients.

As noted above, we employ several measures of educational attainment. Total years of education (YRSED) represents the highest grade of schooling attained, exclusive of business, vocational, or technical training, and is truncated at 17. Years of postsecondary schooling (YRSPSE) represents the number of years of schooling completed beyond grade 12. When used in conjunction with one another, the coefficient for total years of education measures the effect of an increase in elementary or secondary education, while the coefficient for years of postsecondary education measures the difference between the effect of an incremental year of higher education and an incremental year of lower education.

High school graduation (H.S.) is a dummy variable coded 1 for respondents with twelve or more years of schooling and 0 for those with fewer. College graduation (B.A.) is a dummy variable coded 1 for respondents with sixteen or more years of schooling and 0 for those with fewer. When used in conjunction with our measures of total years of schooling and years of postsecondary schooling, and with one another, the coefficient for high school graduation measures "diploma" effects arising from any difference between the incomes of men with exactly twelve years of schooling and the incomes of high school dropouts that is not predicted by a uniform effect of increments in lower education, while the coefficient for college graduation measures any difference between the incomes of college graduates, including men with seventeen or more years of schooling, and noncompleters that is not predicted by a uniform effect of increments in higher education. Our results therefore approximate, but do not measure exactly, the effects of the terminal years of high school and college.

Years of graded schooling (YRSGRADED) represents variation in educational attainment between 0 and 12 years. We use it in conjunction with years of postsecondary schooling (YRSPSE) in order to follow Featherman and Hauser (1978) in separately measuring the linear effects of lower and higher education.

Competitive educational advantage (EDSORN) follows Sorensen (1978: 16-19), and for each level of schooling is calculated as the negative of the natural logarithm of the proportion of individuals whose schooling is above the level in question. Because education is discrete, we interpolate to the midpoint of the values calculated for adjacent schooling levels. In the case of the highest attainment category, we divided the percentage of cases above the next to highest category by 2, and transformed the result into the new metric.

Competitive educational nondisadvantage (NONDISADV) is calculated as the natural logarithm of the proportion of individuals whose schooling is below the level in question. Because the proportion of individuals at each level of schooling is not uniform, competitive advantage and competitive nondisadvantage, while highly correlated, are not reciprocal.

Finally, educational attainment percentile (EDPRCNTL) is calculated as the midpoint between the cumulative proportions of individuals at adjacent education levels.

Because better-educated individuals usually have less work experience than less well-educated individuals of the same age, and because work experience generally has a positive effect on income, especially early in the career, the true effect of schooling differentials at any one point is understated when experience is not controlled. Therefore, we have constructed and utilize a proxy measure of work experience (EXPR). Our measure is constructed as age - years of education - 7 for those with seven or more years of schooling, and age - 14 for those with less than seven years of schooling.[5]

To get accurate estimates of the causal effects of educational attainment on income we would ideally compare identical individuals who differ only in respect to their schooling. To the extent that prior characteristics affecting school continuation also affect income irrespective of educational attainment, the observed association between education and income will misstate the actual effects of lengthier schooling (see Olneck, 1979). One widely recognized source of such bias is socioeconomic and demographic background. Therefore, in our analyses, we have controlled measures of father's occupational status, father's educational attainment, number of siblings, and respondent's race.

A lengthier list of background variables is available in the OCG data, including measures of parental nativity, region of birth, family intactness, and farm origins, but inclusion of these variables in addition to those upon which we rely has only minor impact on the magnitudes of the education coefficients, and aggravates the problem of missing data.

Father's occupational status at respondent's age 16 (POPDUNC) is measured by the familiar Duncan Socioeconomic Index, which is a weighted function of the levels of education and income characteristic of Census-defined occupations and is often interpreted as a measure of the "goodness" or "desirability" of an occupation (Duncan, 1961; Featherman, Jones, and Hauser, 1975; Featherman and Hauser, 1976). Father's education (POPED) is analogous to respondent's total years of education. Number of siblings (SIBS) includes natural, adoptive and step-siblings. Race (RACE) is a dummy variable distinguishing whites from nonwhites.

Respondent's occupation is measured on the Duncan scale (DUNC), as well as by dummy variables representing ten major occupational categories. These are professional, technical and kindred; managers, officials, and proprietors; clerical; sales; craftsmen and foremen; operatives; laborers; service; farmers and farm managers; and farm laborers. In our analyses, the dummy for craftsmen and foremen is the omitted reference category.

Our measure of labor force participation is a categorical measure of annual weeks worked. We define 48 to 52 weeks as "high," 27 to 47 weeks as "medium," (MIDWKS) and 26 or fewer weeks as "low" (LOWWKS). The category "high" is the omitted reference category.[6]

All our analyses are conducted on subsamples with complete data on the variables employed.

V. RESULTS

Table 1 reports the results of regression equations utilizing our several measures of education. In each equation, socioeconomic and demographic background, as well as experience and experience-squared, are controlled. Table 2 reports the percentage differences in income expected under each model for particular comparisons of interest. These are eleventh grade attainment versus tenth grade, twelfth grade versus eleventh grade, twelfth versus tenth, three years of college versus two years, four years of college versus three, and four years of college versus high school graduation.[7]

In light of prior work (see especially, Olneck, 1979), our most striking finding is that in our 1973 sample of those 25 to 34 years old, the estimated value of completing the senior year of high school is more than <u>twice</u> that of each of the preceding years (see equation 2b).[8] On the basis of equation 2b, we would expect completing high school rather than leaving after the eleventh grade to raise men's incomes by 23.7 percent, whereas we would expect completion of the eleventh grade rather than the tenth to raise incomes by only 8.6 percent.[9] Had we relied on our spline specification, we would have predicted both the eleventh and twelfth grades to raise incomes by 11.1 percent (see equation 1b).

Our estimates, however, control only those influences common to income and schooling which are associated with measures of certain background characteristics. It is possible that "ability" and family-

Table 1

Metric Regression Coefficients of Effects of Selected Education Measures on Log-Income among Men 25 to 34 Years Old in the OCG-I and OCG-II Surveys[a]

Equation	1a	1b	2a	2b	3a	3b
Equation Variables	OCG-I	OCG-II	OCG-I	OCG-II	OCG-I	OCG-II
YRSGRADED	.1005 (.0092)	.1056 (.0073)				
YRSPSE	.0919 (.0112)	.0946 (.0064)	-.0984 (.0259)	[-.0163] (.0161)		
YRSED			.1038 (.0125)	.0826 (.0103)		
H.S.			[-.0036] (.0543)	.1303 (.0396)		
B.A.			.4047 (.0939)	.1242 (.0524)		
EDSORN					.2366 (.0228)	.2428 (.0143)
EDSORNSQ[b]						
NONDISADV						
NONDISADVSQ[c]						
EDPRCNTL						
$\bar{R}^2$	.198	.127	.204	.129	.169	.104
N	2308	5421	2308	5421	2308	5421

- Table, Continued -

Table 1, continued

Equation	4a	4b	5a	5b	6a	6b	7a	7b
Equation Variables	OCG-I	OCG-II	OCG-I	OCG-II	OCG-I	OCG-II	OCG-I	OCG-II
YRSGRADED								
YRSPSE								
YRSED								
H.S.								
B.A.								
EDSORN	.6372 (.0651)	.6263 (.0453)						
EDSORNSQ[b]	-.1125 (.0171)	-.1182 (.0133)						
NONDISADV			.2483 (.0194)	.2372 (.0120)	.3648 (.0438)	.2700 (.0040)		
NONDISADVSQ[c]					.0232 (.0078)	[.0075] (.0048)		
EDPRCNTL							.0101 (.0008)	.0092 (.0005)
$\bar{R}^2$	.184	.116	.188	.119	.191	.119	.186	.118
N	2308	5421	2308	5421	2308	5421	2308	5421

[a]Experience, experience-squared, father's education, father's occupation, number of siblings, and race controlled. See text for variable descriptions. Standard errors of coefficients in parentheses. Bracketed coefficients less than 1.96 times their standard errors.

[b]Square of EDSORN.

[c]Square of NONDISADV.

Table 2

Expected Percentage Differences in Income Associated with Selected Increments in Education among Men 25 to 34 Years Old in the OCG-I and OCG-II Surveys, and Change across Surveys in Expected Percentage Differences, across Selected Measures of Education[a]

Attainment Contrast	Model[b] 1	2	3	4	5	6	7
Panel A: OCG-I							
11 v. 10	10.6	10.9	2.1	5.0	0.8	0.9	6.4
12 v. 11	10.6	10.5	10.3	22.9	13.2	16.3	21.5
12 v. 10	22.2	22.6	12.6	29.1	14.1	17.4	29.3
15 v. 14	9.6	0.5	8.0	8.9	1.7	2.4	4.2
16 v. 15	9.6	50.7	10.2	7.7	1.3	1.9	6.8
16 v. 12	44.4	53.2	39.4	51.0	24.5	34.4	42.4
Panel B: OCG-II							
11 v. 10	11.1	8.6	1.4	3.3	9.7	9.7	4.3
12 v. 11	11.1	23.7	9.5	22.2	7.5	7.7	22.6
12 v. 10	23.5	23.5	11.0	26.2	17.9	18.1	27.8
15 v. 14	9.9	6.9	4.7	6.1	2.6	2.8	4.8
16 v. 15	9.9	21.0	11.7	11.2	1.7	1.9	7.2
16 v. 12	46.0	47.6	37.8	55.3	40.3	44.0	49.1
Panel C:[c] Change between 1961 and 1972 in Expected Differences in Income Associated with Selected Increments in Education (Panel B minus Panel A)							
11 v. 10	+0.5	-2.3	-0.7	-1.7	+8.9	+8.8	-2.1
12 v. 11	+0.5	+13.2	-0.8	-0.7	-5.7	-8.6	+1.1
12 v. 10	+1.3	+0.9	-1.6	-2.1	+3.8	+0.7	-1.5
15 v. 14	+0.3	+6.4	-3.3	-2.8	+0.9	+0.4	+0.6
16 v. 15	+0.3	-29.7	+1.5	+3.5	+0.4	0.0	+0.4
16 v. 12	+2.1	-5.6	-1.6	+4.3	+15.8	+9.6	+6.7

[a]Percentage differences calculated on the basis of coefficients in Table 1 (see footnote 4). For the values of EDSORN, NONDISADV, and EDPRCNTL associated with particular levels of educational attainment, see Table A.2.

[b]Models correspond to equations of same number in Table 1.

[c]Panel B minus Panel A.

to-family differences within socioeconomic strata are also common antecedents to schooling and income. Earlier work (Olneck, 1979) suggests that estimates of the effects of elementary and secondary schooling with unmeasured background and measured ability controlled, as well as measured background, are from 50 to 80 percent of the estimates with only measured background controlled.[10] Applying those limits to our results in equation 2b suggests that completing the last two years of high school increases income in our 1973 sample by 15.9 to 27.7 percent.[11]

For purposes of comparison with earlier work, we also ran equation 2 with an OCG-II sample aged 25 to 64 years. Taking omitted variable bias into account as we did above, the results from that exercise suggest that completing the last two years of high school raises incomes by 10.3 to 16.9 percent among men 25 to 64 years old.[12] Olneck's (1979) comparable estimate, based on analyses of several samples, is only 8 to 10 percent.

Our results also demonstrate the significance of "diploma" effects among college graduates, but because our specification of the effects of higher education parallels that in earlier work (Olneck, 1979), this does not occasion surprise.[13] Omission of a B.A. dummy does not, however, impart the same bias to estimates of the effects of acquiring a four-year college education that omission of a H.S. dummy does to estimates of the effects of completing the last two years of high school (see equation 1b). Olneck (1979) suggests an incremental bias of roughly 30 percent in the estimated effects of completing four years of college when unmeasured family factors and cognitive test scores are controlled in addition to measured socioeconomic characteristics. Taken in conjunction with our estimates of equation 2 for those 25 to 64 years old (see note 12), this suggests that in 1972 a four-year college education was worth a 44.3 percent increase in income.

The strong effect of high school completion in our 1973 sample of 25-to-34 year-olds is all the more striking by virtue of its contrast with the same effect in our 1962 sample. Whereas completion of the last two years of high school is expected to raise income by 22.6 percent in the 1962 sample, it is expected to raise income by 34.4 percent in the 1973 sample. While the coefficient for an average year of lower education falls insignificantly by 0.0212 (t = 1.31), the coefficient for the H.S. dummy variable increases significantly by 0.1339 (t = 1.99).[14]

As we discussed earlier, the fact that the proportionate income differentials between high school graduates and dropouts of equivalent work experience and social background widened between 1961 and 1972 is puzzling and theoretically significant. During that period, high school graduates became more common within the male labor force. In our OCG-I sample of men aged 25 to 34, 64 percent are high school graduates. The comparable figure in our OCG-II sample is 80 percent. Forty percent of the younger OCG-II respondents compared with 34 percent of the OCG-I respondents have completed exactly twelve years of schooling. If income returns to schooling reflected returns to enhanced skills brought to a competitive labor market, as traditional human capital theory assumes (Blaug, 1972, 1976), we would expect, all else constant, for the returns to high school graduation to have fallen between 1961 and 1972. That expectation is consistent with cross-sectional evidence that the earnings of high school graduates relative to elementary school graduates are higher in states with proportionately fewer high school graduates than in states where high school graduation is more typical (Welch, 1974: 196).

We devote the remainder of this paper to our attempt to explain this apparently anomalous finding.

VI. EXPLAINING THE INCREASED EFFECT OF HIGH SCHOOL COMPLETION

As we have noted earlier, adherents to human capital theory sometimes argue that the expected fall in schooling returns under conditions of increased supply may be blunted by improvements in the quality of schooling which widen skill differentials between graduates and nongraduates. It is plausible to assume that our OCG-II respondents graduated from high schools with more demanding curricula than did our OCG-I respondents. Men 25 to 34 years old in 1962 graduated from high school between approximately 1946 and 1955, while those the same age in 1973 graduated between approximately 1957 and 1966. Whereas the late 1940s and early 1950s were the heyday of education for "life adjustment," the late 1950s through early 1960s represented a period of intensified academic rigor in American high schools (Ravitch, 1983). Nevertheless, we are skeptical that shifts in school quality can explain our results.

First, the coefficients for education in earnings equations do not appear to be sensitive to the inclusion of such proxy measures of school quality as per student expenditures, as they would be if the quantity and quality of schooling interacted in affecting income (Johnson and Stafford, 1973; Link and Ratledge, 1975). Further, if we assume that white-collar youngsters attend "better" schools than blue-collar offspring, and if school quality affects the returns to schooling, we would expect to find that the returns to schooling were larger among men of white-collar origin than among men raised in blue-collar families. There is, however, no persuasive evidence for such an interaction (Olneck, 1979; Hauser, 1973). Finally, published Census data show that, despite continued increases in graduation rates, the effects of high school graduation among those 25 to 34 years old rose between 1969 and 1979 (U.S. Bureau of the Census, 1973: Table 1; 1984: Table 1). Men 25 to 34 years old in 1979 graduated from high school between approximately 1963 and 1972, a period not noted for high academic standards or high levels of scholastic achievement (National Commission on Excellence in Education, 1983; Congressional Budget Office, 1986).

Even if we reject the possibility that improved school quality after 1957 enhanced skill differentials between high school graduates and dropouts to a degree sufficient to account for our results, we might hypothesize that high school graduation became a finer "reverse filter," so that high school dropouts in the OCG-II sample are more distinct from graduates than are OCG-I dropouts. For example, if the apparent effects of educational differences derived from the effects of unmeasured cognitive differences, or if employers adjust their behavior to changes in the signaling value of particular levels of schooling, our results might be expected. Despite the appeal of this line of reasoning, we doubt its applicability to the case at hand.

We have already attempted to adjust our results for the effects of omitted test scores and family factors. Moreover, we have concluded, from admittedly crude calculations, that to explain the increase in the effect of high school completion evident between the two OCG surveys as spurious, achievement or ability differences between graduates (who go no further in school) and dropouts of similar background and experience levels would have to have increased by two-thirds to over two standard deviations in the years the OCG-II respondents attended high school.[15] Inasmuch as no data with which we are familiar indicate that gross differences between dropouts and graduates (who go no further) are at any one time larger than one-half of a standard deviation, we doubt that such an increase is plausible.[16] Certainly those correlates of ability which we are able to control evidenced no tendency of such magnitude. Increases in differences between dropouts and graduates on father's education, father's occupational status, and number of siblings range from only four one-hundredths to nine one-hundredths of a standard deviation. We cannot, however, rule out the possibility that employers' perceptions may exaggerate changes in the actual differences between graduates and dropouts.

The explanations we have just discussed emphasize the magnitudes of actual differences in the attributes of those with more or less schooling. An alternative class of explanations for income differentials related to educational attainment emphasizes the effects of position within the education distribution per se. Such "queuing" theories entertain, as we have noted above, the possibility that increases in the proportions of better-educated workers can have the effects both of lowering the incomes of better-educated workers relative to the mean and increasing income differentials between better-educated and less well-educated workers. Our data are consistent with this possibility. Results from Multiple Classification Analyses (results not shown) show that in the 1962 OCG-I sample, the income of men 25 to 34 years old with exactly 10 years of schooling, adjusted for experience, race, and socioeconomic background, is 85 percent of the average for the entire cohort. The adjusted income of men with exactly 12 years of schooling is 102 percent of the overall average. In the 1973 OCG-II sample, the analogous results are 72 and 95 percent. Men in both educational categories had lost ground relative to the mean, but high school dropouts had lost more ground, with the result that the income gap between dropouts after the tenth grade and graduates had widened.[17]

But, as we also noted earlier, this finding does not establish that shifts in the relative position of high school graduates and dropouts in the education queue explain the widening gap between their incomes. For that to be the case, the competitive advantage provided by high school graduation would have to have increased relative to that provided by completion of earlier years of schooling. Alternatively, the impact of relative educational advantage on income would have to have risen. We in fact find that the former is true for only one of our positional measures of education, and the latter is true for none of our positional measures. As Table A.2 shows, the increase in competitive advantage provided by high school graduation, as measured by EDSORN, relative to the increase in competitive advantage provided by completing grade 11, is trivially lower in our OCG-II sample. The increase in "nondisadvantage" provided by high school graduation, relative to that provided by completing grade 11, is appreciably lower in our OCG-II survey. Only when measured by percentiles does high school graduation evidence any gain in the competitive advantage it yields relative to the gain yielded by completion of grade 11.

As inspection of equations 3-7 in Table 1 indicates, in no case is there an appreciable or significant increase in the coefficients for any of our positional measures of education. Indeed, the coefficients are, for the most part, remarkably stable over the two samples, suggesting that to the extent queuing mechanisms are implicated in the education-income relationship, they are invariant with respect to shifts in the distribution of schooling.[18] In any event, as Table 2 clearly shows, when we substitute the values associated with particular levels of schooling into equations 3-7 in Table 1, we predict either declines in the effects of high school completion or, with EDPRCNT, an increase of only 1.1 percent, not the 13.2 percent increase which we predict from equation 2.

Close inspection of the discrepancies between our positional measures of education and our measure including nonlinear and diploma terms in the predicted effects of specific schooling differences indicates that competitive advantage (EDSORN) more often and more closely approximates the predictions from equation 2 in Table 1 then does "nondisadvantage" (NONDISADV), but that our percentile measure of education (EDPRCNT) is more consistent with equation 2 in its predictions than is either competitive advantage or nondisadvantage (see Table A.3). From this we conclude that to the extent that returns from secondary schooling and higher education derive from queuing mechanisms, those mechanisms operate more to select individuals from the head of the queue than to eliminate individuals from the bottom of it, and that incremental rankings in the educational queue are uniform in their effects.[19] Such mechanisms do not, however, as we have shown, appear to be implicated in the change we are attempting to explain.

Having exhausted our search for supply-related explanations for the increase in the effects of secondary school completion, we turn now to possible demand-related explanations. Table 3 reports the results of regression equations which first introduce controls for occupation into our preferred model of the income-education relationship (equation 2, Table 1), and which then introduce interaction terms between education and occupation, and, finally, which control a measure of annual weeks worked.

If the increase in the effects of high school completion on income arose either because high school completion more completely determined occupational destinations, or because the occupations which high school graduates tend to hold increased their economic advantage over the occupations held by dropouts, then changes across our two surveys in the coefficients for high school completion should be negligible once occupation is controlled. Equations 2a and 2b in Table 3 show that this is not the case. Even with both Duncan score and occupational category controlled, completion of the last year of high school is predicted to raise income by 19.3 percent for OCG-II respondents, but by only 7.0 percent among OCG-I respondents. The difference in coefficients for H.S. does, however, fall below conventional levels of statistical significance ($t = 1.83$). Nevertheless, the large remaining absolute difference between coefficients makes us reluctant to conclude that we have "explained" our earlier finding.

If the increase in the effect of high school completion derives from a shift in the proportion of workers employed in occupations which reward educational attainment especially highly, we would expect the difference in the main effects of high school completion to disappear

Table 3

Metric Regression Coefficients of Effects of Selected Education Measures on Log-Income among Men 25 to 34 Years Old, Omitting and Including Measures of Occupation and Annual Weeks Worked

	YRSED	H.S.	YRSPSE	B.A.	YRSED * DUNC[e]	H.S. * DUNC[f]	YRSPSE * DUNC[g]	B.A. * DUNC[h]	$\bar{R}^2$	Other Variables Controlled[a]
1a. OCG-I	.1038	[-.0036][c]	-.0984	.40474						
(N=2308)	(.0125)[b]	(.0543)	(.0259)	(.0939)					.204	
1b. OCG-II	.0826	.1303	[-.0163]	.1242						
(N=5421)	(.0103)	(.0396)	(.0161)	(.0524)					.129	
2a. OCG-I	.0735	[-.0060]	-.1016	.3555						
(N=2307)	(.0119)	(.0513)	(.0249)	(.0885)					.299	DUNC, OCCUP
2b. OCG-II	.0645	.1112	[-.0271]	.1107						
(N=5420)	(.0100)	(.0385)	(.0600)	(.0509)					.190	DUNC, OCCUP
3a. OCG-I	.1178	[-.0939]	-.1283	.4060	-.0019	[.0028]	[.0014]			
(N=2307)	(.0188)	(.0907)	(.0366)	(.0950)	(.0008)	(.0030)	(.0009)	d	.257	DUNC
3b. OCG-II	.0974	.1617	-.0992	[.1150]	-.0015	[-.0015]	.0210	[-.0008]		
(N=5420)	(.0160)	(.0694)	(.0324)	(.1337)	(.0007)	(.0024)	(.0008)	(.0023)	.166	DUNC
4a. OCG-I	.0669	[-.0392]	-.0759	.2731						DUNC, OCCUP,
(N=2181)	(.0114)	(.0484)	(.0234)	(.0831)					.388	WKSWRKD
4b. OCG-II	.0610	[.0568]	[-.0085]	[.0654]						DUNC, OCCUP,
(N=5419)	(.0091)	(.0352)	(.0146)	(.0464)					.324	WKSWRKD

[a]In addition to experience, experience-squared, father's education, father's occupation, number of siblings, race, which are controlled in each equation. See text for variable descriptions.

[b]Standard errors of coefficients in parentheses.

[c]Bracketed coefficients less than 1.96 times their standard errors.

[d]Tolerance too low for variable to enter.

[e]Multiplicative interaction between YRSED and DUNC.

[f]Multiplicative interaction between H.S. and DUNC.

[g]Multiplicative interaction between YRSPSE and DUNC.

[h]Multiplicative interaction between B.A. and DUNC.

when interactions between our schooling variable and Duncan scores are controlled.[20] Comparison of equations 3a and 3b in Table 3 shows that this is not the case. The difference across our OCG-I and OCG-II surveys between the coefficients for H.S. is 0.2556, and is statistically significant ($t = 2.24$). Moreover, we find that the interaction between lower education and occupation status is negative in both our surveys, though (insignificantly) less so in our 1973 sample, while the interaction between H.S. and Duncan score is also (insignificantly) negative in the OCG-II sample. From these results we would have predicted, as a result of the simple increase in mean Duncan score across the two samples (+2.48 points), a slight reduction in the observed effects of high school completion.

Finally, if demand-related factors explained the increase in the income differential between high school graduates and dropouts, we might expect to find evidence that differentials in labor force participation mediated the shift. In point of fact, there is a small increase across our surveys in the difference in the number of dropouts who worked less than 27 weeks in the previous year and the number of high school graduates with similarly low annual employment. Further, the direct impact of unemployment on income is higher in the recent survey than in 1962 (results not shown). Consequently, inclusion of measures of weeks worked in our equations does to some extent "explain" our finding that relative to dropouts, high school graduates improved their incomes between 1962 and 1973.

As equation 4b in Table 3 shows, addition of our measure of annual weeks worked to equation 2b, which includes our measures of occupation, reduces by half and renders statistically insignificant the coefficient for H.S. among OCG-II respondents. The difference across the OCG-I and OCG-II samples between the coefficients for H.S. in equations 4a and 4b is statistically insignificant (t = 1.60). However, the difference between the expected increase in log-income associated with completion of the last year of high school in the OCG-I and OCG-II samples remains absolutely large (0.0901) even with weeks worked controlled, and is 0.0901/0.1127 = 80 percent as large as the difference without weeks worked or occupation controlled (see equations 1a and 1b in Table 3). Moreover, our data do not let us explain shifts in employment differentials across schooling levels. Therefore, we remain reluctant to conclude that we have adequately accounted for the observed increase in the effects of high school completion. We would, however, conclude that factors associated with changes in demand appear more germane than factors associated with changes in supply, insofar as these refer to either differences between the actual attributes of graduates and nongraduates, or to the distribution of schooling per se.

VII. DISCUSSION AND CONCLUSION

Our inability to adequately explain the increase in the effects of high school completion between 1961 and 1972 in terms of enhanced human capital differentials, queuing mechanisms, or shifts in sectoral composition and demand does not, of course, invalidate the theories underlying these alternative explanations. The power of these theories to account for the education-income relationship at any one point in time, or for shifts in the value of a high school education during other periods, is not at issue here.

Nevertheless, our results do point to the complexity of the schooling-income relationship, and to the need to more directly examine the mechanisms which produce it. It is not sociologically satisfying to conclude that the shift in question may be attributed to unidentified and unanalyzed "demand" factors. We would like to know what those factors are and how they manifest themselves.

Data of the sort we have examined can, in principle, be utilized to more precisely locate the source of the shift. We can, for example, compare the income distributions of high school dropouts and of graduates, and ask whether the increase in the effects of high school completion is uniform across the distribution of income.[21] We can more precisely measure occupational and industrial location. We can conduct longitudinal research to discover whether the current disadvantages of dropouts may be traced to the long-term effects of disadvantages suffered upon labor market entry (e.g., low initial earnings, early unemployment, limited opportunities for on-the-job-training).

While we believe that such investigations are worthwhile to pursue, we believe that the special province of sociologists includes investigation of the shared understandings and social definitions which translate educational attainment levels into categories of qualified personnel (Meyer, 1977). David Bills (Bills, 1988a, 1988b) in an innovative study recently investigated the criteria by which employers in several Chicago firms and agencies hired and promoted individuals. Bills found that educational credentials played a significant, though subordinate, role in hiring and promotion processes, and that employers tended to view schooling as a source of general cognitive and noncognitive skills bearing on trainability, problem-solving ability, work habits, and interactional competence, rather than as a source of job-specific skills. In some cases, Bills found, employers attested to the value of well-educated employees for conveying a good image, suggesting that schooling is an aspect of a logic of confidence by which institutions establish their bona fides (see Meyer and Rowan, 1978).

Bills does not report on employers' perceptions of and beliefs about particular educational identities, such as "high school dropout" or "college graduate." If historical or retrospective investigation of this question were possible, we suspect that researchers would discover that as high school graduation has become increasingly universal, typification of the high school dropout as unqualified and irresponsible has become stronger. At the risk of even grosser speculation, we would suggest that heightened public campaigns against dropping out have the effect of increasingly defining the high school dropout as a social and labor market pariah. Such processes, we suspect, account for the increasing economic penalty for failure to acquire a high school diploma that we have been unable to otherwise explain. Our speculations are not, of course, a substitute for empirical research. We hope that they, and our work reported here, may add to the impetus for such research.

Table A.1

Metric Regression Coefficients of Effects of Selected Education Measures on Log-Income among Men 25 to 34 Years Old in the 1980 U.S. Census 1/10,000 Sample[a]
(N = 9946)

Equation	1	2	3	4	5	6	7
Equation Variables							
YRSGRADED	.1001 (.0059)						
YRSPSE	.1026 (.0041)	.0268 (.0117)					
YRSED		.0609 (.0085)					
H.S.		.2160 (.0337)					
B.A.		.0531 (.0342)					
EDSORN			.3077 (.0108)	.6578 (.0383)			
EDSORNSQ[b]				-.1363 (.0143)			
NONDISADV					.2172 (.0072)	.2386 (.0154)	
NONDISADVSQ[c]						[.0053] (.0034)	
EDPRCNTL							.0090 (.0003)
$\bar{R}^2$	.096	.099	.080	.088	.087	.088	.090

[a]Experience and experience-squared controlled. Standard errors of coefficients in parentheses. Bracketed coefficients less than 1.96 times their standard errors. See text for variable descriptions.

Table A.2

Values of Selected Measures of Education Corresponding to Particular Levels of Educational Attainment in the OCG-I and OCG-II Samples of Men 25 to 34 Years Old[a]

Education Measure	EDSORN		NONDISADV		EDPRCNTL	
	OCG-I	OCG-II	OCG-I	OCG-II	OCG-I	OCG-II
Attainment Level						
10	.3238	.1484	-1.6199	-2.3943	27.6	13.8
11	.4115	.2307	-1.5883	-2.0031	33.7	18.4
12	.8260	.5786	-1.0898	-1.6988	53.0	40.5
14	1.4956	1.2511	-0.3248	-0.4478	77.4	71.2
15	1.8195	1.4417	-0.2569	-0.3417	81.5	76.3
16	2.2288	1.8890	-0.2047	-0.2707	88.0	83.9

[a]See text for variable descriptions.

Table A.3

Differences between Selected Models of the Income-Education Relationship in Expected Percentage Differentials in Income Associated with Selected Increments in Education, and in Predicted Changes in Expected Differentials across OCG Surveys

Model Contrasts[a]	3 versus 2		4 versus 2		5 versus 2		6 versus 2		7 versus 2	
OCG Survey	I	II	I	II	I	II	I	II	I	II
A. Differences in Expected Percentage Differentials in Income[b]										
Attainment Contrast										
11 v. 10	-8.8	-7.2	-5.9	-5.3	-10.1	+1.1	-10.0	+1.1	-4.5	-4.3
12 v. 11	-0.2	-14.2	+12.4	-1.5	+2.7	-16.2	+5.8	-16.0	+11.0	-1.1
15 v. 14	+7.5	-2.2	+8.4	-0.8	+1.2	-4.3	+1.9	-4.1	+3.7	-2.1
16 v. 15	-40.5	-9.3	-43.0	-9.8	-49.4	-19.3	-48.8	-19.1	-43.9	-13.8
16 v. 12	-13.8	-9.8	-2.2	+7.7	-28.7	-7.3	-18.8	-3.6	-10.8	+1.5

B. Differences in Predicted Changes in Expected Differentials[c]

Attainment Contrast	3 versus 2	4 versus 2	5 versus 2	6 versus 2	7 versus 2
11 v. 10	+1.6	+0.6	+11.2	+11.1	+0.2
12 v. 11	-14.0	-13.9	-18.9	-21.8	-12.1
15 v. 14	-9.7	-9.2	-5.5	-6.0	-5.8
16 v. 15	+31.2	+33.2	+30.1	+29.7	+30.1
16 v. 12	+4.0	+9.9	+21.4	+15.2	+12.3

[a]Model 2 - YRSED, H.S. YRSPSE, B.A.
Model 3 - EDSORN
Model 4 - EDSORN, EDSORNSQ
Model 5 - NONDISADV
Model 6 - NONDISADV, NONDISADVSQ
Model 7 - EDPRCNTL
Models correspond to equations of same number in Table 1.

[b]Contrasts derived by subtracting expected differentials under Model 2 from differentials expected under other models. See Table 2.

[c]Calculated by subtracting entry in Panel A for OCG-I from entry for OCG-II.

NOTES

[1]Olneck (1979) rationalized his decision to omit a measure of high school graduation on the grounds of simplicity and of the apparent insignificance of such a measure in analyses of the 1970 U.S. Census data. Later analyses showed the measure to be significant when used in conjunction with a dummy variable measuring high school entrance (Olneck, 1979: 372). No nonlinear effect of high school entrance is evident in the data analyzed here (results not shown).

Featherman and Hauser (1978), utilizing the same data set we utilize, assess the effects of years of graded schooling and years of collegiate education on earnings, but do not represent diploma effects. They present graphic evidence for 25-to-29-year-olds, based on equations utilizing dummy variables representing single years of schooling, which reports the effects of education on log-income with background and occupation held constant. Their results indicate greater gains in the incomes of high school graduates than in the incomes of men with only some high school between 1962 and 1973. Nevertheless, on the grounds that the dummy variable specification explains only trivially larger amounts of variance in log-earnings than do their spline functions, Featherman and Hauser reject the need for dummy variable analyses (1978: 299-300). On similar grounds, they reject the need for dummies when analyzing the determinants of occupational status (1978: 265). We do not believe that an appreciable increment to R^2 is necessarily the best test for the appropriateness of a particular specification of a variable. Roughly equivalent R^2's may nevertheless reflect very different patterns of errors in prediction. Our interest here is in most accurately measuring the effects of completing high school and of acquiring a college education, and to accomplish this we believe it is important to represent diploma effects. Our results bear us out on this point.

Goodman (1979), however, concludes on the basis of analyses of 1970 U.S. Census data that high school diploma effects in log-earnings models are nonexistent when occupational prestige is controlled and, while statistically significant, are slight when occupational prestige is omitted. Goodman's sample, however, pertains to (not-in-school, non-military, employed) native-born white males of native-born parents, aged 15 to 65 years old, and his equations control age, not experience. Goodman's measure of earnings is adjusted to eliminate the effects of variations in weeks worked and in hours worked per week. In our OCG-II data, the effect of high school completion is lower among men 35 years old and older than among those 25 to 34 years old; utilization of age rather than of experience and experience-squared reduces the coefficient for H.S. by almost 40 percent (results not shown); and introduction of a measure of annual weeks worked reduces the coefficient for H.S. by one-half (see below). Therefore, we do not believe that Goodman's conclusion is preferable to our own.

[2]In 1973, the year of the OCG-II survey, unemployment among males aged 20 and above averaged 3.2 percent. In the three previous years it averaged 3.5 (1970), 4.4 (1971) and 4.0 (1972) percent, respectively. The unemployment rates for each of the years 1959-1962 were 4.7, 4.7, 5.7, and 4.6 percent, respectively. The average duration of unemployment was lower during 1970-1973 than in the period 1959-1962

(see Economic Report of the President, 1981: Tables B.29 and B.32). We conclude that demand for labor was higher during the period of the second survey than it was during the period of the first.

[3]One reviewer of an earlier draft of this paper worried that our conclusion might be sensitive to our treatment of low earnings. Following Featherman and Hauser (1978: 288-289), we reran equations 2a and 2b in Table 1 excluding respondents with incomes of less than $1,000. In the OCG-II subsample, the coefficient for high school completion rises appreciably, to 0.1831 (s.e. = 0.0316), the difference between the coefficients across surveys increases slightly, from 0.1339 to 0.1635, and the t-statistic for the difference increases from 1.99 to 3.29 (results not shown). We conclude that the increase in the effect of high school completion which we document is not an artifact of the inclusion of very low earners.

[4]To determine the percentage change in income implied by a logarithmic coefficient, we take the antilog of the coefficient (e^b), where b is the coefficient, and subtract 1.00. For example, if the coefficient for years of education were 0.0950, we would calculate that a one-year increase in schooling raises income by $e^{0.0950} - 1 = .0997$, or 9.97 percent.

[5]The choice of seven years to represent the probable age of school entrance was an oversight. The convention is to use age 6 and to use grade 8 as the education level at or above which this age adjustment is made (Jencks, 1979: 22-23), though Mueser (1977) uses our construction

in his analysis of the Michigan Panel Study data. The conventional construction assigns the same amount of experience to men of the same age with seven or eight years of education, while our construction assigns men with seven years of education one more year of experience than men who finished eighth grade. Otherwise the two constructions yield the same differences in experience for men of the same age with varying educational attainments.

[6]The original measure of annual weeks worked from which ours is derived is also categorical. We collapsed categories 1-3, representing 0 to 26 weeks, categories 4-5, representing 27 to 47 weeks, and categories 6-7, representing 48 to 52 weeks. The percentage LOWWKS are 6.7 and 5.8, and MIDWKS, 12.9 and 10.8 in the OCG-I and OCG-II subsamples of those 25 to 34 years old, respectively.

[7]Appendix Table A.1 reports comparable equations from supplementary analyses using the 1980 1/10,000 U.S. Census sample. Because no measures of socioeconomic background are available in the Census data, our analyses of them control only experience and experienced squared. Comparison of our 1980 Census results with results from analyses of our OCG-II subsamples which omit controls for background suggest that the effects of high school completion continued to rise beyond 1971, even as graduation rates continued to rise (results not shown). Differences in coefficients in our OCG-II subsample and our 1980 Census subsample do not, however, attain statistical significance. Our results, nevertheless, are consistent with trends evident in published Census data (see below).

[8]The estimated coefficient for an average year of elementary and secondary schooling is the coefficient for YRSED, 0.0826. The coefficient for the senior year is estimated as 0.0826 + 0.1303 (the coefficient for H.S.) = 0.2129.

[9]$e^{0.0826} = 1.0861$. $e^{0.2129} = 1.2373$.

[10]However, for skepticism that omitted family factors impart significant bias to schooling coefficients, see Hauser and Sewell (1986).

[11]0.5(0.0826 + 0.2129) = 0.1478; $e^{0.1478} = 1.1592$. 0.8(0.0826 + 0.2129) = 0.2364; $e^{0.2364} = 1.2667$. We focus on completion of the last two years of high school, because completion of the first two years is close to universal.

[12]The results for equation 2 in an OCG-II sample 25 to 64 years old are: $b_{LNINC,YRSED} = 0.0653$(s.e. = 0.0045), $b_{LNINC,H.S.} = 0.0645$(s.e. = 0.0212), $b_{LNINC,YRSPSE} = -0.0061$(s.e. = 0.0095), $b_{LNINC,B.A.} = 0.1303$(s.e. = 0.0361), $R^2 = .158$. To derive our parentage estimates, we have $0.5(0.0653 \cdot 2 + 0.0645) = 0.0976$, $e^{0.0976} = 1.1025$; and $0.8(0.0653 \cdot 2 + 0.0645) = 0.1561$, $e^{0.1561} = 1.1689$.

[13]Because education is truncated at 17 in the OCG surveys, we cannot directly test whether the coefficient for the B.A. variable is an artifact of uncontrolled variation in number of years of postcollegiate schooling. We believe, however, that that is unlikely. Because the

truncation is at 17, not at 16, the coefficient for years of postsecondary schooling does subsume the graduate school-B.A.-only contrast. Moreover, in the 1970 Census data for those 25 to 64 years old, the effects of years of graduate school are smaller than are the effects of years of college, and the coefficient for B.A. is positive and significant (Bartlett and Jencks, 1977: A25). We are troubled, however, by the absence of a significant B.A. effect in the 1972 Project TALENT sample of 28 and 29-year-olds (Olneck, 1979: 179). On the other hand, Hauser and Daymont (1977: 203-204) do report a possible B.A. effect among 1957 Wisconsin high school graduates at approximately age 32.

[14]We calculate the significance of the difference between two coefficients as $t = (B_1 - B_2)/(s^2_{B1} + s^2_{B2})^{1/2}$, where B is a coefficient and s_B is its standard error (Jencks, 1979: 39). Our confidence in the trend toward declining effects of pregraduation years of secondary schooling and increasing effects of graduation is strengthened by our findings form the 1980 Census data (see Table A.1), though as noted above (see note 7) the differences in coefficients between the 1972 OCG-II data and the 1980 Census data do not attain significance.

[15]Utilizing upper- and lower-level estimates for the coefficient for tested cognitive ability in income equations controlling measured background and education, drawn from Crouse (1977: H73), Hauser and Daymont (1977: 198), and Olneck (1977: I90), we worked through the implications for our results of the formula for omitted variables bias presented by Griliches and Mason (1972).

[16]Among respondents in Olneck's (1977) Kalamazoo Brothers sample, who would have graduated from high school between 1934 and 1956, men who graduated from high school but went no further outscored men who quit school before graduating on sixth grade standardized tests of academic aptitude by 0.46 standard deviations (calculated from Olneck, 1977: I62). Among 1960 Project TALENT ninth-grade respondents, who would have graduated from high school in 1964, men with high school diplomas but no further schooling scored 0.43 standard deviations higher than high school dropouts on a ninth-grade academic composite test (calculated from information provided by L. Stale in personal communication; for Project TALENT, see Wise, 1977). Alexander, Natriello and Pallas (1985) find that completing the last two years of high school raises standardized test scores by only 0.10 standard deviations.

[17]Among men who dropped out after the eleventh grade, adjusted income is 103 percent of the average among OCG-I respondents and 78 percent of the average among the OCG-II respondents. Again, dropouts suffered a larger reduction in relation to the mean than did high school graduates. Note the anomaly that in the OCG-I sample, dropouts after the eleventh grade have adjusted incomes somewhat higher than those of high school graduates. The observed income of graduates is, however, 6 percent higher than that for men who dropped out after the eleventh grade.

In order to ensure that our conclusion respecting an increase in the effect of high school completion between 1961 and 1972 is not an artifact of this anomaly, we reran equations 2a and 2b in Table 1 including a dummy variable representing completion of the junior year (results not shown). The difference across the surveys between the coefficients representing the high school diploma effect increases, as does the significance of the differences. In comparisons excluding respondents with incomes below $1,000 (see note 3), the difference between the coefficients increases slightly when the junior year dummy is included, while the significance of the difference falls slightly.

[18]This conclusion is strengthened by our results utilizing the 1980 U.S. Census sample (see Table A.1).

The significant negative coefficients for the square terms in Equation 4a and 4b of Table 1 suggest that the competitive advantage associated with additional schooling, does not, as Sorensen (1978) assumes, increase as a function of schooling level.

[19]Our conclusions would differ if we focused attention on the effects of grades lower than high school. There, nondisadvantage more closely predicts effects consistent with equation 2 in Table 1. Note that for equations utilizing our positional variables, R^2 is marginally higher in these utilizing our measures of nondisadvantage.

[20]The Duncan S.E.I. score may be expected to bear a reasonable correspondence to the location of an occupation in reference to the service or postindustrial sector, higher-status occupations being those most likely to demand and reward more highly educated workers. For example, in our OCG-II sample, the correlation between Duncan score and our categorical measure of professional and technical occupation is 0.598. This line of inquiry is rendered moot, however, by the fact that

the proportions of men in each of our 25-to-34-year-old samples that are in the professional and technical category are virtually identical (0.192 and 0.194, respectively).

[21]In ancillary analyses reported elsewhere (Olneck and Kim, 1985), we demonstrate that the increase in the effects of high school completion across the OCG-I and OCG-II subsamples of those 25 to 34 years old tends to fall monotomically with income quintile within education-specific income distributions. For example, while the ratio of incomes for high school graduates at the tenth percentile of income (among graduates) to incomes for dropouts (with nine or ten years of schooling) at the tenth percentile (among dropouts) rises by 21 percent across the surveys, the analogous ratio at the ninetieth percentile rises by only 4 percent (see Olneck and Kim, 1985: 60). This finding, we believe, is consistent with our suggestion of an increasing aversion to high school dropouts, as distinct from an increasing demand for the attributes possessed by the average graduate.

REFERENCES

Alexander, Karl, Gary Natriello and Aaron Pallas. 1985. "For Whom the Bell Tolls: The Impact of Dropping Out on Cognitive Performance." American Sociological Review 50: 409-420.

Bartlett, Susan and Christopher Jencks. 1977. "The 1970 Census 1/1000 Sample." Appendix A in Christopher Jencks and Lee Rainwater (principal investigators), The Effects of Family Background, Test Scores, Personality Traits, and Schooling on Economic Success. Cambridge, Massachusetts: Center for the Study of Public Policy, National Institute of Education Grant NIE-G-74-007.

Becker, Gary. 1964. Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education. New York: Columbia University Press.

Bills, David. 1988a. "Credentials and Capacities: Employers' Perceptions of the Acquisition of Skills." Sociological Quarterly 29: 439-449.

______. 1988b. "Educational Credentials and Promotions: Does Schooling Do More Than Get You in the Door?" Sociology of Education 61: 52-60.

Blakemore, Arthur E. and Stuart A. Low. 1984. "The High School Dropout Decision and Its Wage Consequences." Economics of Education Review 3: 111-119.

Blau, Peter, and Otis Dudley Duncan. 1967. The American Occupational Structure. New York: John Wiley and Sons.

Blaug, Mark. 1972. "The Correlation between Education and Earnings: What Does It Signify?" Higher Education 1: 53-76.

______. 1976. "The Empirical Status of Human Capital Theory: A Slightly Jaundiced Survey." Journal of Economic Literature 14: 827-855.

Collins, Randall. 1974. "Where Are Educational Requirements for Employment Highest?" Sociology of Education 74: 419-442.

______. 1979. The Credential Society: An Historical Sociology of Education and Stratification. New York: Academic Press.

Congressional Budget Office. 1986. Trends in Educational Achievement. Washington, D.C.: U.S. Government Printing Office.

Crouse, James. 1977. "The 1960-1972 Project Talent Longitudinal Survey." Appendix H in Christopher Jencks and Lee Rainwater (principal investigators), The Effects of Family Background, Test Scores, Personality Traits, and Schooling on Economic Success. Cambridge, Massachusetts: Center for the Study of Public Policy, National Institute of Education Grant NIE-G-74-007.

Duncan, Otis Dudley. 1961. "A Socioeconomic Index for All Occupations." Pp. 109-138 in A. J. Reiss, Jr. (ed.), Occupations and Social Status. New York: Free Press.

Duncan, Otis Dudley, David Featherman, and Beverly Duncan. 1972. Socioeconomic Background and Achievement. New York: Seminar Press.

Economic Report of the President. 1981. Washington, D.C.: United States Government Printing Office.

Featherman, David L. and Robert M. Hauser. 1976. "Prestige or Socioeconomic Scales in the Study of Occupational Achievement." Sociological Methods and Research 4: 403-422.

Featherman, David L. and Robert M. Hauser. 1978. Opportunity and Change. New York: Academic Press.

Featherman, David L., F. Lancaster Jones, and Robert M. Hauser. 1975. "Assumptions of Social Mobility Research in the United States: The Case of Occupational Status." Social Science Research 4: 329-360.

Goodman, Jerry D. 1979. "The Economic Returns of Education: An Assessment of Alternative Models." Social Science Quarterly 60: 269-283.

Green, Thomas F., with David P. Ericson and Robert H. Seidman. 1980. Predicting the Behavior of the Educational System. Syracuse: Syracuse University Press.

Griliches, Zvi and William M. Mason. 1972. "Education, Income, and Ability." Journal of Political Economy 80: S74-S103.

Hauser, Robert M. 1973. "Socioeconomic Background and Differential Returns to Education." Pp. 129-145 in Paul Taubman and Lewis C. Solomon (eds.), Does College Matter? New York: Academic Press.

Hauser, Robert M. and Thomas N. Daymont. 1977. "Schooling, Ability, and Earnings: Cross-Sectional Findings Eight to Fourteen Years after High School Graduation." Sociology of Education 50: 182-206.

Hauser, Robert M. and David L. Featherman. 1977. The Process of Stratification: Trends and Analyses. New York: Academic Press.

Hauser, Robert M. and William H. Sewell. 1986. "Family Effects in Simple Models of Education, Occupational Status, and Earnings: Findings from the Wisconsin and Kalamazoo Studies." Journal of Labor Economics 4: S83-S115.

Hill, C. Russell. 1979. "Capacities, Opportunities and Educational Investments: The Case of High School Dropout." Review of Economics and Statistics 61: 9-20.

Jencks, Christopher S. 1979. "Methods." Pp. 17-49 in Christopher S. Jencks et al., Who Gets Ahead?: The Determinants of Economic Success in America. New York: Basic Books.

Jencks, Christopher S., Marshall Smith, Henry Acland, Mary Jo Bane, David Cohen, Herbert Gintis, Barbara Heyns, and Stephan Michelson. 1972. Inequality: A Reassessment of the Effects of Family and Schooling in America. New York: Basic Books.

Jencks, Christopher S., Susan Bartlett, Mary Corcoran, James Crouse, David Eaglesfield, Gregory Jackson, Kent McClelland, Peter Muesen, Michael Olneck, Joseph Schwartz, Sherry Ward, and Jill Williams. 1979. Who Gets Ahead?: The Determinants of Economic Success in America. New York: Basic Books.

Johnson, George F. and Frank P. Stafford. 1973. "Social Returns to Quantity and Quality of Schooling." Journal of Human Resources 8: 139-155.

Levin, Henry M. 1972. The Costs to the Nation of Inadequate Education: A Report Prepared for the Select Committee on Equal Educational Opportunity of the U.S. Senate. Washington, D.C.: U.S. Government Printing Office.

Link, Charles R. and Edward C. Ratledge. 1975. "Social Returns to Quantity and Quality of Education: A Further Statement." Journal of Human Resources 10: 78-89.

Meyer, John. 1977. "The Effects of Education as an Institution." American Journal of Sociology 83: 55-77.

Meyer, John and Brian Rowan. 1978. "The Structure of Educational Organizations." Pp. 78-109 in M. W. Meyer and Associates (eds.), Environments and Organizations. San Francisco: Jossey-Bass.

Mueser, Peter. 1977. "The 1967-74 Panel Study of Income Dynamics." Appendix D in Christopher Jencks and Lee Rainwater (principal investigators), The Effects of Family Background, Test Scores, Personality Traits, and Schooling on Economic Success. Cambridge, Massachusetts: Center for the Study of Public Policy, National Institute of Education Grant NIE-G-74-007.

National Commission on Excellence in Education. 1983. A Nation at Risk. Washington, D.C.: U.S. Government Printing Office.

Olneck, Michael. 1977. "The 1974 Kalamazoo Brothers Sample." Appendix I in Christopher Jencks and Lee Rainwater (principal investigators), The Effects of Family Background, Test Scores, Personality Traits, and Schooling on Economic Success. Cambridge, Massachusetts: Center for the Study of Public Policy, National Institute of Education Grant NIE-G-74-007.

______. 1979. "The Effects of Education." Pp. 159-190 in Christopher S. Jencks et al., Who Gets Ahead?: The Determinants of Economic Success in America. New York: Basic Books.

Olneck, Michael, and Ki-Seok Kim. 1985. "The Relationship between Education and Income among American Men: Some Revisions and Extensions." Wisconsin Center for Education Research, University of Wisconsin-Madison.

Ravitch, Diane. 1983. The Troubled Crusade: American Education, 1945-1980. New York: Basic Books.

Seidman, Robert H. 1981. "The Explanatory Power of Two Idealized Models of Educational and Social Attainment." Paper presented at the meetings of the American Educational Research Center, Los Angeles, California.

______. 1982. "The Logic and Behavioral Principles of Educational Systems: Social Independence or Dependence?" Pp. 267-292 in Margaret Archer (ed.), The Sociology of Educational Expansion. London: Sage Publications.

Sørensen, Aage B. 1978. "A Model and a Metric for the Analysis of Status Attainment Processes." Madison, Wisconsin: Institute for Research on Poverty Discussion Paper No. 492-78.

Steele, Laurie. 1985. Personal Communication.

Thurow, Lester. 1972. "Education and Economic Equality." The Public Interest 28: 66-81.

______. 1974. "Measuring the Economic Benefits of Education." Pp. 373-418 in Margaret S. Gordon (ed.), Higher Education and the Labor Market. New York: McGraw-Hill.

______. 1975. Generating Inequality. New York: Basic Books.

Tinney, R. 1972. "Effects of Change in Industrial Composition of Output." Washington, D.C.: Urban Institute; cited in Welch, 1974.

U.S. Bureau of the Census. 1973. U.S. Census of Population: 1970. Subject Reports, PC(2)-8B, Earnings by Occupation and Education. Washington, D.C.: U.S. Government Printing Office.

______. 1984. U.S. Census of Population: 1980. Subject Reports, PC80-2-8B, Earnings by Occupation and Education. Washington, D.C.: U.S. Government Printing Office.

Welch, Finis. 1974. "Relationships between Income and Schooling." Pp. 179-201 in Fred Kerlinger (ed.), Review of Research in Education. Itasca, Illinois: Peacock Publishers.

Wise, Lauress L. 1977. The American Citizen: Eleven Years after High School, Volume 2. Palo Alto, California: American Institutes for Research.