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AUTHOR Ribich, Thomas; Murphy, James
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ABSTRACT

Using Project Talent and census data, estimates are made with a recursive model to explain scores on standardized tests, school continuation behavior, and lifetime income. Exogenous variables include socioeconomic backgrounds of students and their classmates, school spending, race, and region. The results suggest that: (1) school spending has its main impact on income through the channel of school continuation; (2) the time-discounted income gains associated with increased educational spending are smaller than the requisite spending increase; and (3) cross-sectional test score information can yield misleading results about the long-term effectiveness of schooling improvements. The results are reconciled with basic theoretical propositions in economics. (Author)

THE ECONOMIC RETURNS TO INCREASED EDUCATIONAL SPENDING

by

Thomas Ribich & James Murphy*

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* The authors are, respectively, Research Economist at the National Institute of Education and Professor of Economics at the University of North Carolina, Chapel Hill. The work for this paper was financed in part by a grant from the National Science Foundation and in part by the Institute for Research on Poverty at the University of Wisconsin-Madison through funds granted by the Office of Economic Opportunity pursuant to the provisions of the Economic Opportunity Act of 1964. The American Institutes for Research (Project Talent) was generous in its assistance and provision of data. Any views expressed in this paper are those of the authors alone.

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Much has been written in the last several years on the effectiveness, or ineffectiveness, of schools in producing learning and other beneficial outcomes. Much has also been written recently on the related topic of the multiple determinants of socioeconomic success. This paper attempts further consideration of these broad issues with the help of new data that are better in a number of respects than data used in prior work. The reward has been the corroboration of some prominent earlier work and the discovery of some relationships that previously were not clearly revealed.

The new and better data are the latest Project Talent follow-up observations on individuals who were in the ninth grade when Project Talent did their initial nation-wide survey.[1] With the follow-ups performed nine years after the original survey, and five years after the individual students should have graduated from high school, these data constitute the most extended longitudinal observations presently available that cover in detail the attributes of students and their schools as well as their early-adult socioeconomic history. Though this information has numerous uses, our concentration is on evaluating the outcomes of spending more money on public education; and the outcome that is given the closest attention is the effect on economic success, as measured by earnings, of those who experience more expensive education.

Our results show that, while extra educational spending does lead to greater lifetime income, the income gain (properly time-discounted) is less than the amount of the extra spending required to induce it. We do not view this as an especially surprising conclusion, though it is worth remarking that several recent studies have been very ambiguous on this quite important point.[2] We differ from recent studies in several other respects as well, such as: the specific linkages which connect more spending with higher earnings, the differential effects on subgroups of the population, the techniques used for stretching the longitudinal data into an even longer time-frame, and the nature of our normative and positive interpretations.

The paper proceeds in the customary sequence of outlining a model, describing the data, and presenting the results. The final section explores some implications.

I. THE HYPOTHESIZED MODEL

A lot of the theorizing and model building in the economics of education has progressed along two general lines -- the human-capital approach and the production-function approach.[3] The human capital approach usually employs income-maximizing models, where individuals undertake human investments in themselves up to the point where the rate of return in the last increment of investment is equal to the market rate of interest. The decision of individuals to continue or not to continue on to a higher level of education is the basic "policy" choice being considered. In the production-function approach, maximizing models have received less attention, largely because of a belief that schools fail to pursue any maximizing principle very industriously given the absence of market discipline. The emphasis, instead, has been on attempts to identify, with alternative single-equation statistical models, input variables that are consistently related to output variables -- test score results in particular. More recent models have involved networks of causation, dealing with more than one layer of inputs and outputs, combined in a recursive system.[4]

This last variant is the form of our theoretical approach, though aspects of human capital reasoning are blended in as well.

We begin with trying to explain the monetary earnings of individuals. Following various leads in earlier work, it seems reasonable to propose that earnings (Y) of an individual are in large part determined by his "ability" (A), the time he has spent in formal and informal education and training (E), the cost and quality of that schooling and training (C), the socioeconomic status of his parents (S_p), and the socioeconomic status of "others" he closely associates with during his "formative years" (S_o). Assuming these variables are linearly related to earnings, the following equation can be written:

$$Y = b_o + b_1A + b_2E + b_3C + b_4S_p + b_5S_o \ . \qquad (1)$$

All the variables in equation (1) can be considered "supply side" variables. We do not mean to assert by this that demand conditions are unimportant in determining earnings for various categories of workers, but only that a given cohort of workers are all assumed to face the same demand conditions. The meaning and tangibility of the variables in equation (1) should all be fairly evident, though some confusion might arise over the term "ability." "Ability" measures are certain to incorporate a mixture of innate and environmental influences. The variable A might therefore be better read as a combination of ability, aptitude, and achievement, with the exact mix determined by the nature of the testing instrument and the time of testing. The socioeconomic status of "others" involves similar complexities that also require pragmatic measures.[5]

While equation (1) might suffice as a reasonable equation for predicting earnings, the relative importance of the independent variables would likely be obscured by statistical tests that focused on this equation alone. For instance, the amount of education an individual eventually acquires (E) may be heavily influenced by the quality of early schooling and social background variables. Similar influences may also affect "ability." Hence, the total effect of school inputs and the socioeconomic variables on earnings should include not only the partial coefficients, holding "ability" and the time spend in school constant, but also the indirect effects that work through these "intermediate" variables. Equation (2) and (3) permit this consideration in our model.

$$Y = b_o + b_1A + b_2E + b_3C + b_4S_p + b_5S_o \quad (1)$$

$$E = c_o + c_1A + c_2C + c_3S_p + c_4S_o \quad (2)$$

$$A = d_o + d_1C + d_2S_p + d_3S_o \quad (3)$$

Note that Equation (3) does not include years of schooling as an explanatory variable. The presumption is that all individuals are tested at the same point in time and before the legal school-leaving age -- which is consistent with most data sources, including our own. Thus, the model can be recursive and estimated by ordinary least squares. From the independent equations (1) through (3), the total effect on earnings of varying schooling expenditures (C) is given by the combined coefficient $b_3 + c_2b_2 + d_1b_1 + d_1c_1b_2$. Similar combined coefficients can be defined for other independent variables appearing in the first two equations.

The justifications for the hypothesized relationships and the positive signs of the coefficients should be apparent, and they have been suggested earlier by others.[6] It should be noted, though, that there are few theoretical hints about the relative importance of the variables. Past empirical work, is more helpful than past theorizing in forming anticipations about what relationships are likely to be strong or weak.

Perhaps the most provocative finding from past work is the frequently observed failure of school inputs, when summarized by a per pupil expenditures variable, to have a statistically significant positive effect on measured learning and aptitude when one controls for socioeconomic variables. By testing all three equations outlined above, we wish to explore the possibility that this lack of influence on test score performance does not necessarily mean that increased expenditures yield no long-run benefits whatsoever.

It seems to us very likely that extra spending could have an inconsequential effect on the usual sorts of tests administered to students and yet succeed in encouraging (for instance) the development of personal attributes useful in the job market directly or conducive to greater interest in school continuation. Stated more strongly, it seems almost inconceivable that the striving exhibited by many communities to supply more abundant resources for their schools is based on mere illusion of long-run importance or a desire merely to provide a congenial ambience for school children. On the other hand -- and using a derivative of human-investment reasoning -- it seems unlikely that the earnings gain that does result from extra spending will be as great as the extra expenditure. Parents expect not only higher incomes for their children when highe. quality education is provided, but also returns to their children in the form of higher social status, a more intellectually enlightened life, and so on. It is therefore reasonable that increased spending often goes beyond the point of strict financial profitability where marginal spending equals marginal income gain.

II. DATA CHARACTERISTICS AND TRANSFORMATIONS

To test our model, we would ideally like to have complete lifetime histories of a large number of individuals, with abundant details on their backgrounds and achievements and with experimentally controlled interventions having occurred with respect to policy-relevant variables. In practice, what we have is a large number of individual files from Project Talent on ninth grade males attending public schools in 1959 including responses to questionnaires items concerning background and location, results from tests of many types, and information about the school attended as provided by the principal's responses to a questionnaire. In addition, we have follow-up data for the same individuals in 1968 which provide information on educational attainment, occupation, and income.[7]

For this study, we chose the ninth grade sample so as to include those who drop out of high school. We limited the analysis to public schools mainly for advantages of homogeneity; and we restricted the sample to males for purposes of comparability with other studies and because of the larger technical problems in dealing with the work experience of females. We excluded from our analysis those individuals who were in active military duty at the time of the follow-up survey since their reported annual earnings and occupation are probably inaccurate reflections of their potential in the civilian labor force. Our selection of cases was further restricted to those who answered the questions in the follow-up survey concerning education attained and current occupation. Those still in school must have answered questions related to their degree plan and course of study and their planned occupation. The overall sample size thus obtained from Project Talent was 9,527.[8]

The variables in the equations (1) to (3) are measured by the following statistics:

a) Ability, achievement, and aptitude (A) is measured by TAFQT, a test composite derived from the battery of tests administered by Project Talent. Many alternate test scores are available in the Project Talent data for measuring IQ, skills learned, job aptitudes, native reasoning, visual perception, etc. No one test or group of tests stands out as the perfect measure for the variable A. We experimented with several composite test scores of only academic, or only aptitude, or only nonacademic tests, but we finally decided to use a particular weighted combination of these, denoted by TAFQT, which is similar in composition to the Armed Forces Qualification Test (AFQT). This choice was influenced by the small variation in results using different test score measures and by the widespread understanding and usage in other studies of the AFQT.

b) Education attained (E) is measured by EDA, the number of years of education actually completed or an expected value for those still in school. Thus, dropouts may have completed 8 to 11 years of schooling depending on when they dropped out.[9] Other classifications include high school graduates with EDA = 12; those with a partial college education or junior college experience with EDA = 14; college graduates with EDA = 16; and those with some graduate work completed and now working full time with EDA = 17. The respondents who are still undergraduates were classified with EDA = 16 and those in graduate school with EDA = 18. The implicit assumptions are that those not in school in 1967 will never go back, those who are in college will achieve the bachelors degree, and those in advanced degree programs will average two years of post-college education.

c) School costs (C) is measured by CXPSY, the Costs reflected by the dollar eXpenditures per Pupil per year in the school SYstem, as recorded by the principal's response to question number 89 in the Project Talent school questionnaire. We used costs in the system rather than in the individual high school since the data were more complete and since all the dependent variables should be influenced by the quality of schools preceding high school as well as the quality of the high school itself.[10]

d) The socioeconomic status of the individual's family (S_p) and of his associates (S_o) is measured by the Project Talent socioeconomic index.[11] The index for the individual is denoted by SE while the index for his associates, that is the average socioeconomic status of all ninth graders in his school, is denoted by AVSE.

e) Earnings (Y) is measured by YLFA, final adjusted lifetime earnings, which is a discounted present value of a net benefit stream from age 16 to 65 based on current earnings and other information in the follow-up responses combined with census data. Some details of the construction and rationale for this measure merit attention.

The only income variable provided in the Project Talent follow-up is current annual earnings (1967) five years after scheduled high school graduation. While this has the advantage of being directly reported, it has the drawback of seriously misrepresenting the comparative long-term earnings prospects among those who have completed different levels of schooling and/or who in different occupations because age-earnings profiles differ markedly among educational and occupational categories.[12] Since information about the individual's occupation and education level completed were also available from the Project Talent follow-up, it was possible to use this in combination with reported income to construct a lifetime income measure (YLFA) which is not only technically superior to the current income figure but conceptually more convenient as well.[13]

Calculating a lifetime income estimate for each individual in our sample involved six laborious steps: (1) a calculation of a discounted lifetime earnings stream (adjusted for productivity growth, mortality, and morbidity) for each of the major occupational categories in the U.S. Census, for each of the educational attainment categories in the census, by race (white and non-white), and by region (South and Non-South);[14] (2) an estimate of the probability of remaining in an occupation and the probabilities of shifting into each of the other occupational categories, given one's occupation in early adulthood; (3) with the help of information from steps (1) and (2), the calculation of a "first approximation" expected lifetime income based on each individual's current occupation, educational attainment, race, and region; (4) a revision of this first approximation by taking into account the relationship of the individual's current earnings to the average current earnings of all individuals in the same occupation-education-region-race category; and (5) the netting out of costs for continued education.[15]

Data limitations forced us to make somewhat arbitrary assumptions about the timing of occupational shifts and the relation of current to lifetime income. For the former, it was assumed that no more than one shift takes place between major occupational categories and that it takes place (on average) at approximately age 30. The basis for this adjustment is a special Current Population Survey in 1962, providing information on the occupational category of "current" and "first" full-time employment for individuals grouped into 10-year age classifications.[16] For the second problem, it was assumed that an individual's relative lifetime income, compared to all those in the same occupation-education-race-region category, is the same as the ratio of the individual's current income to that of the average current income of all individuals in the Project Talent follow-up, with the same four-way classification. For example, if a white salesworker in the South with a high school education earns 20 percent more in 1967 than the average of all those with the same characteristics, then the discounted present value of his lifetime earnings is estimated to be 20 percent more than the expected lifetime income of all those with the same characteristics.

The estimated lifetime income (YLFA) for an individual of given occupation, education, region and race can be summarized (neglecting discounting terms) by the following equation:

$$YLFA = \sum_{i=15}^{D} \left[Y_{pi} - (Y_{fi} + E_i) \right] + \frac{Y_c}{\bar{Y}_{rkc}} \left[\sum_{i=(D+1)}^{29} \bar{Y}_{rki} + \sum_{j=1}^{10} P_{jk} \sum_{i=30}^{65} (Y_{ri})_j \right]$$

where the years of his added education span age 15 to the age of his departure from school; Y_{pi} is his part-time earnings while in school; Y_{fi} the full-time earnings he would have had if not in school; E_i the direct yearly costs of his education; Y_c his current earnings in 1967;[17] $\bar{Y}_{rkc}$ the average current earnings of all those in the sample with the same occupation, k, and same education, race and region r; $\bar{Y}_{rki}$ is the average earnings for all those in the census with the same occupation, education, etc., for the years following his departure from school until age 29; P_{jk} is the probability of being in the j^{th} occupation after age 29 given his current occupation; and $(\overline{Y}_{ri})$ is the average yearly earnings between age 30 and 65 in each of the ten major occupational categories tabulated in the census, for individuals of the same education, race, and region.

III. RESULTS

In order to facilitate interpretation of our regression results, the means, standard deviations, and the correlation matrix of all variables are reported in Table 1. Note that all the simple correlations are as expected. Expenditures per student (CXPSY) are positively related to all three "output measures:" test scores in the ninth grade (TAFQT), years of schooling completed (EDA), and lifetime income (YLFA). The same is true for the socioeconomic status of the individual (SE) and of his classmates (AVSE). Test scores are positively related to years of schooling completed and to lifetime income; being nonwhite and/or going to school in the South are negatively related to all input and output variables, and so on.[18]

Table 2 displays the results of linear-multiple regressions testing each of the three equations outlined in section I; with the data discussed in section II, for all the individuals in our sample.[19] Most of the signs of the simple correlations are retained and reflected in the signs of the regression coefficients, and most of the coefficients are of very high statistical significance. The exceptions are interesting. There is a clearcut reversal of sign on the relation of race and region to educational attainment. In other words, individuals who went to school in the South and/or who are nonwhite, manage to finish more years of schooling than white and non-South

Table 1

CORRELATION COEFFICIENTS, MEANS AND STANDARD DEVIATIONS

	TAFQT	CXPSY	SE	RACE	REGION	AVSE	YLFA	Mean	Standard Deviation	Units
EDA	.43	.10	.44	-.10	-.10	.31	.33	14.74	2.51	Years
TAFQT	-	.09	.39	-.25	-.16	.30	.20	187.02	49.29	Points
CXPSY	-	-	.12	-.05	-.50	.27	.07	399.27	138.40	Dollars
SE	-	-	-	-.17	-.16	.48	.21	99.27	10.o7	Standardized
RACE	-	-	-	-	.13	-.24	-.19	0.03	0.17	Nonwhite = 1 White = 0
REGION	-	-	-	-	-	-.31	-.09	0.17	0.38	South = 1 Non-South = 0
AVSE	-	-	-	-	-	-	.18	97.25	4.83	Standardized Index
YLFA	-	-	-	-	-	-	-	67893.81	29131.23	Dollars

Table 2

REGRESSION RESULTS: FULL SAMPLE, ALL VARIABLES INCLUDED[a]

Independent Variables	TAFQT	EDA	YLFA
	Dependent Variables		
CONSTANT	-45.98	.2826	-5581.95
EDA	-	-	3327.27 (24.11)**
TAFQT	-	.01480 (27.99)**	12.08 (1.73)*
CXPSY	-0.0C8 (1.87)*	.00066 (3.31)**	1.52 (.61)
SE	1.515 (27.25)**	.07441 (26.72)**	93.78 (2.59)**
RACE	-48.732 (17.46)**	.50978 (3.73)**	-23076.03 (13.19)**
REGION	-9.856 (6.33)**	.23496 (3.16)**	-2088.55 (2.26)*
AVSE	0.910 (7.38)**	.04099 (6.90)**	137.35 (1.85)*
R^2	.20	.28	.14

Note: Values in parentheses are t-ratios.

[a]The number of individual observations used in the regressions for the entire sample varied between 8,249 and 8,466. This is less than the total number in the available sample (8,902) because of missing values, and it varies because the regressions do not all require the same information.

*Statistically significant at .05 level.

**Statistically significant at .01 level.

individuals who are comparable in "all other" respects.[20] The only other switch in sign, from positive to negative, is the relation of expenditures per pupil to test scores. Unlike most other coefficients, however, the one relating expenditures to test scores is significant only at the 5 percent level of confidence. The implications of this result will be more apparent after some additional analysis, and hence will be returned to later (and more than once) in the discussion.

In the meantime, note that the coefficient directly relating school expenditures to lifetime income does not come close to a reasonable level of statistical significance. This result entails holding years of education constant, and therefore should <u>not</u> be interpreted as evidence that expenditures have no influence whatsoever on earnings. Expenditures do have a statistically significant effect on years of education attained, and years of education attained is an important determinant of lifetime income. This effect is at least partially counterbalanced, however, by the negative relationship of expenditures to test scores and the fact that lower test scores (according to the regressions) not only lead to lower income directly (holding years of education constant) but also to less educational attainment with a further diminution in income resulting from that.

Before estimating the net effect of expenditures on earnings, a look at some alternative calculations are in order. First note the complication arising due to the statistical association between expenditures and the socioeconomic status of classmates, and the related problem of ambiguous causal direction. As argued by critics of the Coleman report, increased expenditures may induce high status parents to move into the school district where taxpayers have decided to spend a relatively large amount on school quality.[21] Controlling statistically for the average socioeconomic status of classmates may therefore amount to an "over-control," and the net effect of spending more money may thus be seriously underestimated. In other words, an important "return" to increased spending may entail attracting high status children into the school district where they stimulate greater success by other pupils along one or more of the relevant output dimensions.

One easy way of testing the potential importance of this effect is simply to drop AVSE from the regression. The results of doing so are shown in Table 3. While the size and statistical significance of most of the regression coefficients are changed very little, the negative effect of expenditures on test scores drops well below acceptable significance levels and the coefficients relating spending to years of schooling attained rises by roughly one-third.[22] The regression coefficient relating spending to income directly (holding educational attainment constant) increases in size, but is still statistically insignificant.

It is tempting to conclude, on the basis of these results alone, that school spending increases lifetime income only (or mainly) by encouraging individuals to stay in school longer, and that the detracting influences of extra spending amount to little. The negative influence of increased spending on test scores never reaches the significance levels of most other variables and is practically nonexistent in the alternative specification of the model. A closer look at this conclusion is called for, however.

The next four tables, testing the same three basic equations, deal with selected subsamples of individuals. Table 4, including only whites who went

Table 3

REGRESSION RESULTS: FULL SAMPLE, ALL VARIABLES INCLUDED EXCEPT AVSE[a]

Independent Variables	Dependent Variables: TAFQT	EDA	YLFA
CONSTANT	23.296	3.398	4751.56
EDA	-	-	3347.88 (24.33)**
TAFQT	-	.01509 (28.57)**	12.88 (1.85)*
CXPSY	-0.0035 (.84)	.00086 (4.32)**	2.18 (.88)
SE	1.695 (33.85)**	.08203 (32.00)**	117.58 (3.47)**
RACE	-52.314 (18.97)**	.36380 (2.69)**	-23544.53 (13.60)**
REGION	-11.786 (7.72)**	.15144 (2.06)*	-2346.34 (2.57)**
R^2	.20	.20	.14

[a]See note to Table 1.

to school in the non-South, displays coefficients very close to those derived for the entire sample (see Table 2). Table 5, including only whites who went to school in the South, differs appreciably from earlier tables. Clearly, the results for the whole sample are dominated by the non-South whites who make up more than 80 percent of the whole sample. In contrast to the non-South subsample, the results for the South show a positive effect for expenditures on both test scores and educational attainment, but neither is statistically significant.

The results for Southern whites could be read as evidence that things simply work differently in the South, with expenditures being less reliably effective in producing long-run benefits. What is perhaps more likely is that the relatively small sample of Southerners and the associated problem of larger sample bias are at the root of the difficulty. All the equations for the South, as compared to the North, are generally weaker in terms of the statistical significance of the independent variables. And when similar analyses are performed on nonwhites in the South and non-South -- where samples are even smaller and sampling problems likely more serious -- practically nothing is statistically significant except for the relation between educational attainment and lifetime income.

Tables 6 and 7 indicate, among other things, that the weak results for nonwhites and Southerners are not due simply to the low average socioeconomic status of these two groups. Table 6 includes only non-South whites whose socioeconomic status in ninth grade was among the bottom 40 percent of all individuals surveyed by Project Talent, and Table 7 includes all non-South whites among the top 40 percent. The results are generally quite similar for

Table 4

REGRESSION RESULTS: NON-SOUTH WHITES[a]

Independent Variables	Dependent Variables TAFQT	EDA	YLFA
CONSTANT	-34.22	-.7705	-8747.71
EDA	-	-	3261.51 (20.54)**
TAFQT	-	.01429 (24.44)**	9.15 (1.14)
CXPSY	0.008 (1.81)*	.00077 (3.78)**	1.61 (.60)
SE	1.581 (25.44)**	.07550 (23.99)**	69.07 (1.60)
AVSE	.726 (5.39)**	.05112 (7.83)**	209.47 (2.43)**
R^2	.13	.26	.09

[a]Sample size for all regressions were 6,850. See note to Table 1.

Table 5

REGRESSION RESULTS: SOUTH WHITES[a]

Independent Variables	Dependent Variables TAFQT	EDA	YLFA
CONSTANT	-138.5	3.382	5378.29
EDA	-	-	4336.17 (14.11)**
TAFQT	-	.01823 (14.29)**	25.65 (1.67)*
CXPSY	.018 (1.03)	.00029 (.35)	-4.50 (.47)
SE	1.291 (10.38)**	.07008 (11.57)**	141.48 (1.97)*
AVSE	1.952 (6.66)**	.00774 (.70)	-207.97 (1.31)
R^2	.17	.31	.21

[a]Sample size for all regressions were 1,350. See note to Table 1.

Table 6

REGRESSION RESULTS: LOW STATUS, NON-SOUTH WHITES[a]

Independent Variables	Dependent Variables: TAFQT	EDA	YLFA
CONSTANT	-77.130	1.9264	-50756.07
	-	-	3291.45 (16.06)**
TAFQT	-	.01530 (17.11)**	17.18 (1.59)
CXPSY	-0.008 (1.29)	.00067 (2.09)**	2.19 (.59)
SE	1.973 (14.49)**	.06653 (9.36)**	164.62 (1.97)**
AVSE	.799 (3.85)**	.02980 (2.83)**	532.88 (4.36)**
R^2	.08	.15	.11

[a]Sample size for all regressions were 3,200. See note to Table 1.

Table 7

REGRESSION RESULTS: HIGH STATUS, NON-SOUTH WHITES[a]

Independent Variables	Dependent Variables: TAFQT	EDA	YLFA
CONSTANT	15.177	-.31231	23397.41
EDA	-	-	3227.52 (12.43)**
TAFQT	-	.01259 (15.89)**	-4.57 (.37)
CXPSY	-0.008 (1.38)	.00086 (3.12)**	-0.01 (.011)
SE	1.089 (7.33)**	.05998 (8.74)**	-17.89 (.17)
AVSE	.765 (4.16)**	.06691 (7.93)**	21.11 (.17)
R^2	.03	.15	.05

[a]Sample size for all regressions were 3,350. See note to Table 1.

the two groups. For high and low status individuals alike, expenditures have a statistically significant effect on years of schooling completed and a statistically insignificant effect both on learning and on earnings when years of schooling are held constant. Higher status individuals enjoy a somewhat larger increase in educational attainment when expenditures are increased, but low status individuals profit somewhat more in terms of yearly income for each additional year of schooling completed. Sharper contrasts appear in the coefficients for the socioeconomic variables, especially in relation to YLFA. Family and classmates' status apparently have a larger effect on the future income of low status children that on that of high status children.

Additional statistical analysis might seem desirable to fill out the details on differential effects and to probe deeper into the reasons for the results reported above, and much additional statistical analysis was indeed performed. Log and semi-log functions were fitted; alternative test scores and income measures were used; interaction terms were entered into the equations; physical inputs rather than expenditures per pupil were experimented with; and various additional stratifications were imposed on the data, including stratifications by community size, by level of education, and by high-spending and low-spending school districts. The existence and character of response bias was also explored by analyzing separately those individuals who responded to the follow-up questionnaire and the smaller (but presumably more representative) sample of individuals who were tracked down and interviewed personally.

None of the alternative functional forms or alternative variable definitions gave results clearly superior to those reported above, and most alternatives yielded very much the same impressions.[23] Attempts to pin down the effect of response bias on the overall results were generally unsuccessful; and the additional stratifications yielded little insight, with two notable exceptions. First, when the non-South whites were stratified into districts that spent more than the median and those that spent less than the median, the effect of school spending on years of schooling became statistically insignificant. What significance we are detecting seems, then, to be mainly the result of the extremes of the expenditure spectrum; and the apparent weak effect of spending in the South, reported earlier, may be at least partially attributable to the relatively small variance in school expenditures in the South.[24] Second, when years of education completed were treated as alternative dummy dependent variables describing the probability of continuing on to an advanced level of education, the greatest effect of school expenditure was at higher education levels. Coefficients were positive for all levels, but statistically significant only for the probability of entering college, graduating from college, and attending graduate school.[25]

IV. IMPLICATIONS

Taking the above results at face value, some interesting conclusions follow.[26] Most important, the main hypothesis put forward in our theoretical section is generally supported. Spending more on education apparently does give rise to increased earnings mainly through the indirect effect of increased educational attainment, except in those subgroups where sample size and the range of spending variability is small. But the regression coefficient relating spending and attainment is not very large, and the implied net gain in lifetime income is less than the amount of the related spending.

Assuming that spending influences earnings "reliably" only through the conduit of increased educational attainment, a \$100 increase in spending for each of the first nine years of education is associated with a net lifetime earnings gain in a range between \$230 and \$300. This outcome follows from the regression estimate that a \$100 spending increase stimulates less than one-tenth of an extra year of educational attainment, and an extra year of educational attainment is associated with a net lifetime income gain of about \$3,300.[27] The \$230-\$300 amount is substantially less than the discounted value (back to age six) of spending \$100 more in each of the first nine years of education, which is approximately \$750.

If one takes all coefficients into account regardless of statistical significance levels, the increased-earnings value of extra spending rises appreciably. Combining coefficients as suggested in the discussion of our model, and using the values in Table 3 to calculate the total effect of a \$100 spending increase, results in the following:

$$(218) + (.086)\ (3,348) + (-.35)\ (12.9) + (-.35)\ (.015)\ (3,348) = \$484,$$

which is 65 percent of the \$750 in total spending required to generate the estimated earnings gain.[28] It is apparent that the introduction of the negative influence on earnings by way of estimated lower test scores associated with increased spending is more than counterbalanced by the direct positive effect of spending on earnings.[29] Calculations with the other regression results yield a similar net gain in the earnings-increase estimate when all direct and indirect effects of spending are taken into account.

As anticipated, the increase in earnings is not as great as the related spending increase. This may be taken as evidence that non-monetary returns operate in the way suggested earlier, though other explanations of this result are surely possible. In terms of normative economics, the results are somewhat more ambiguous. _If_ earnings gains had turned out to be greater than the related spending increase, this would have constituted a strong prima facie case that educational spending should be generally increased. That differences in school quality may not explain much of the total variance in earnings would not have altered this conclusion in the least. Since net earnings gains are less than the associated higher spending, those who would urge spending increases must instead assert that the unmeasured and non-monetary gains to education are enough to make up the gap between increased spending and the resulting increased income. Such an assertion would not seem unreasonable. Well over half the cost of extra spending may be recouped by the associated gain in earnings. Moreover, a $100 increase in yearly educational spending can be said to encourage approximately one individual in ten to undertake an additional year of schooling that he otherwise would not have undertaken. If these estimates are close to the truth, it is difficult to accuse high-spending districts of behaving foolishly. Any parent intensely interested in the future overall status of his children and respectful of the intrinsic values of continued education might find these figures encouraging enough to justify increased school spending.

For both high status and low status whites in the non-South the relation between spending and earnings is about the same as calculated for the sample as a whole. If only the effect through encouraged continued schooling is counted, low status individuals appear to gain relatively less income than high status individuals for a given spending increase; but taking all coefficients into account regardless of statistical significance results in a relatively larger earnings gain for low status individuals.[30] A reallocation of funds from more affluent to less affluent children therefore has no strong efficiency justification according to our results, but neither is there support here for the argument that under-privileged children are especially resistent to benefiting from increased educational spending.

Also of policy interest is the larger apparent earnings change for low status, as compared to high status, individuals that results from changing the socioeconomic mix of other children in the school. Combining coefficients as we did above to assess the total effect of school spending, a 5-point (one standard deviation) increase in the average socioeconomic index for others in the school is associated with a lifetime income gain of roughly $680 for low status individuals and only $265 for high status individuals. Similar to the analysis in the Coleman Report, this suggests that rearrangements of student populations leading to a more equal socioeconomic mix of students among districts results in a net benefit, if one ignores the cost (e.g., busing) of achieving the required student shifts. Different from the Coleman Report, our standard is a modified lifetime income measure rather than a test score measure. Further, only a very small part of the estimated income change resulting from a change in classmates' socioeconomic status works through the test score link.

While recognizing our results are not definitive, we do believe they add information useful to both analytical and policy issue debates. On perhaps the most general level, our results suggest that the emphasis in past studies on test scores in the evaluation of schooling changes is inappropriate. Even when the test score changes associated with a schooling change are negligible or negative, the measured long-term effects on important tangibles such as school continuance and on lifetime income may be sufficiently great to justify undertaking the change anyway.

FOOTNOTES

[1] A detailed description of the follow-up can be found in J. C. Flanagan et. al., Five Years After High School, Project Talent, American Institute for Research and University of Pittsburgh, 1971.

[2] Many studies have lacked the data to make a link between school quality and income, and those attempting the connection often focus on percent of variance in income explained rather than working with the relevant regression coefficients. A prominent example of the latter is Christopher Jencks et. al., Inequality, (Basic Books, 1972).

[3] Mark Blaug, among others, has noted this dichotomy of approach. See the introduction to his Readings in the Economics of Education Volume 1, (Penguin, 1969). An important milestone in human capital work is Gary Becker, Human Capital, (National Bureau of Economic Research, 1964). Good examples of production-function studies are Herbert Kiesling, "Measuring a Local Government Service: A Study of School Districts in New York State," Review of Economics and Statistics (August 1967), pp. 356-367; and Henry Levin, "A Cost-Effectiveness Analysis of Teacher Selection," Journal of Human Resources (Winter 1970), pp. 24-33. Though less concerned with a formal production-function specification, the "Coleman Report" belongs in the same general category. See James Coleman et. al., Equality of Educational Opportunity (Washington, D.C.: U.S. Government Printing Office, 1966).

[4] Advanced model building and testing along these lines can be found in Zvi Griliches and James Mason, "Education, Income, and Ability," Journal of Political Economy (May/June 1972, Part II), pp. S74-S103, and William Sewell and Robert Hauser, "Causes and Consequences of Higher Education: Models of the Status Attainment Process," American Journal of Agricultural Economics (forthcoming). However, we have not seen any model of this type tested which has used longitudinal data including information on both school quality and later-life income.

[5] Close relatives, neighborhood friends, and those who sit in the same classroom may all be relevant, but measurement will often involve a range extimate of school-wide or community-wide status. The term "socio-economic status" also has ambiguities, and the best definition for predicting a given dependent variable may not be the best for predicting another.

[6] See, for instance, James Guthr'e et. al., Schools and Inequality (the MIT Press 1971).

[7] Our data on individuals and schools came directly from Project Talent tapes. For details on all aspects of the survey and for basic tabulations, see J.C. Flanagan et. al., Studies of the American High School, Cooperative Research Project No. 226, (Project Talent, Pittsburgh, 1962), pp. A1-A7;

The American High School Student, Cooperative Research Project No. 635, (Project Talent, Pittsburgh, 1964), Chapter 5; and Five Years After High School, (Project Talent, American Institute for Research and University of Pittsburgh, 1971).

[8] All measures for all cases were scanned to delete obvious outlines, contradictory cases, and reporting errors. 625 cases were so eliminated leaving a workable sample of 8902.

[9] Only six in our sample dropped out in ninth grade after completing the Project Talent questionnaire and were assigned a value of 8 for EDA.

[10] This assumes that the earlier schooling of individuals took place in the same school district as the high schools they attended when surveyed. Since over 80 percent of the individuals in the ninth-grade survey reported they have always lived in the same school district, and most of the remainder have had most of their previous schooling in the district, the assumption is not unreasonable.

[11] This index is Project Talent item P*801 created from nine items in the Student Information Blank including family income, parent's education, occupation of household head, number of siblings, etc. See J.C. Flanagan, et. al., Project Talent One-Year Follow-up Studies, Cooperative Research Project No. 2333, (Project Talent, Pittsburgh, 1966), page E-10, E-11.

[12] See Jacob Mincer, "On the Job Training; Costs, Returns, and Some Implications," Journal of Political Economy (Supplement, October 1962), pp. 50-79.

[13] For those individuals not in school, the sample correlation coefficient between current income in 1967 and YLFA is .39. Nevertheless, the use of current income as a dependent variable often gave results that differed markedly from those when lifetime income was used, and the current income version of the same equations were more often misleading and/or difficult to interpret. A major reason for this was that college graduates, at the time of the follow-up survey, often had less than a year of labor market experience. Even without this problem, discounted lifetime income has the advantage of being more meaningfully compared with school expenditure.

[14] Project Talent 3-digit occupations codes were translated into eleven major occupational categories in the Census. Five education levels were used, from high school dropouts to graduate study. The South was classified as the twelve South-Eastern and South-Central states, extending from Louisiana to Virginia, with the non-South including all other states plus the District of Columbia. Earnings streams for each of 220 relevant categories (11 occupations by 5 education levels by 2 regions by 2 races) were calculated from U.S. Census of Population, 1960, Final Report. PC (2)-7R, Earnings by Occupation and Education, U.S. Department of Commerce, Tables

2, 3, 5, and 6. The five Census earnings values by broad age cohort were adjusted upwards to reflect an assumed earnings growth of 1.25 percent per annum. (See Gary Becker, Human Capital (NBER, 1965), pp. 74, 123.) Linear interpolation between the midpoints from pairwise convex combinations of successive adjusted cohort values, smoothed by fitting an eleven period weighted moving average representing a second degree polynomial equation, produced expected annual earnings for each year of working life. Mortality and morbidity adjustments came from U.S. Department of Health, Education, and Welfare, Vital Statistics of the U.S. (1967), Vol. II. A 5 percent discount rate was used, with discounting beginning at the time of the assumed first-increment to the investment stream: i.e., age six.

[15] Due to the mixing in census data of earnings for teen-agers in and out of school, indirect methods were used for estimating the foregone earnings costs of attending high school. What would have been earned by high school graduates (from ages 16 through 18), if they had dropped out of high school, was estimated by backwards extrapolation of the calculated earnings streams of high school dropouts. (See footnote 14.) Part-time earnings of high school students were estimated as one-fourth the earnings of dropouts. Foregone earnings were calculated as the difference between these two amounts. (For an alternative approach to the same problem, see T.W. Schultz, "Capital Formation by Education," Journal of Political Economy, December 1960, pp. 571-83.) Part-time earnings of former undergraduate college students and graduate students were estimated from the average reported part-time earnings (by race and region) of those in the Project Talent follow-up who were still in school. The reported part-time earnings of those still in college or graduate school were used directly for these individuals. What would have been earned if the individual had not attended college or graduate school was estimated from the reported earnings of individuals in the follow-up with the relevant lower levels of educational attainment. The direct costs of schooling were assumed to be: for high school -- 1.25 times the individuals' associated EXPSY per year; for college -- $2,000 and $1,800 per year in the non-South and South respectively; and for graduate school -- $5,000 and $4,500 per year in the non-South and South respectively. The high school adjustment is based on U.S. Department of Health, Education and Welfare, Current Expenditures Per-Pupil in Public School Systems (1958-59 (1961) Table 6. Estimates for college education are from Fred Hines, Luther Tweeten, and Martin Redfern, "Social and Private Rates of Return to Investment in Schooling by Race-Sex Groups and Regions." Journal of Human Resources (Summer 1970), pp. 318-340. Since YLFA is net of total resource costs of education, it is not a lifetime income measure in the "usual" sense. It is, however, suitable for estimating the net economic consequence of varying the costs of education in public schools.

[16] The CPS information indicated that the probability of workers age 25 to 34 holding a job in the same occupational category as the full-time job they held when first they entered the labor force was approximately the same as that for the older groups. Occupational composition also tended to remain fairly stable for the age cohorts from the 25-34 group on up, and was sub-

stantially different from occupation of first jobs. This all suggested that most shifts among major occupational groupings takes place before age 35. See "Lifetime Occupational Mobility of Adult Males, March 1962," Current Population Reports, Technical Studies. Series P-23, number 11, Tables 2 and 5.

[17]Y_c is assumed to be equal to Y_{rkc} for those individuals still in school. That is, no adjustment on average lifetime is made since current full-time earnings is not a meaningful concept in these cases.

[18]From the mean values, means, and their comparisons with information from government surveys, some things can be said about the representativeness of the sample. The mean educational attainment of over 14 years is somewhat higher than expected for 23 year olds -- median educational attainment for 20-24 years olds in the U.S. was roughly 13 years in 1968. Per pupil expenditures for the schools of the respondents is very close to the average for the U.S. in 1959-60. The socio-economic status (in ninth grade) of those who responded to the questionnaire is nearly identical to the average for all individuals initially surveyed by Project Talent (99 vs. 100). The only clear shortcoming of the sample is the disproportionately small numbers of non-whites and Southerners. As indicated by the means of the dummy variables, non-whites constituted only 3 percent of total respondents to the questionnaire and respondents (of all races) who went to school in the South only 17 percent. National figures suggest the percentages should have been more like 9 percent and 27 percent, respectively. Our analysis of non-whites and Southerners was consequently handicapped by small samples and larger worries of sample and response bias. Information for the U.S. as a whole comes from the United States Statistical Abstracts, 1960-70, Washington, D.C.

[19]Ordinary least squares estimation is reported throughout since the model is recursive and the number of observations is very large. Disturbance terms in each equation are assumed to be normally distributed with mean zero and with a scalar variance-covariance matrix; and disturbance terms in different equations are assumed to be independent. The validity of this assumption is suggested since the use of two stage least squares estimation on the three equation system or of Zellner efficient estimation on each seemingly unrelated equation provided nearly identical results to each other and to ordinary least squares.

[20]One explanation for this result lies with the fact that Southerners and non-whites compete for jobs largely within their respective demographic categories.

[21]See Samuel Bowles and Henry Levin, "The Determinants of Scholastic Achievement -- An Appraisal of Some Recent Evidence," Journal of Human Resources (Winter 1968), pp. 3-24; Glen Cain and Harold Watts, "Problems in Making Inferences From the Coleman Report," American Sociological Review (April 1970), pp. 228-242.

[22]Note, however, that a $100 increase in expenditures per year only

increases average years of education attained by less than one-tenth of one year.

[23]Log and semi-log functions did not produce higher R^2's. Very little shifting in significance levels and signs took place. The interaction terms did not yield patterns with ready explanations, and their contribution to variance explained was minimal.

[24]The standard deviation of CXPSY in the South was $67 as compared to $128 in the non-South.

[25]Linear regressions were tested with the same independent variables as in the EDA equations above and with alternative dummy variables -- 0 = high school dropouts, 1 = high school graduates; or 0 = high school graduate only, 1 = some college, for all high school graduates, etc. Probit analysis of the same relations gave similar results with the expected but unproved convergence of probit coefficients to least squares coefficients in large sample sizes, 4186 to 8160.

[26]The suitability of the above regression results as a basis for drawing conclusions about the real world have the usual sorts of limitations. The most serious worry, perhaps, is the absence of any experimentally controlled intervention. With that missing, observed statistical associations always run the risk of giving misleading clues on causal connections. For the results under consideration, such risk would seem no larger than usual.

[27]A year of schooling is estimated to be worth over $4,300 for Southern whites, but the regression coefficient relating spending and years of schooling is unusually small and statistically insignificant.

[28]The statistically "insignificant" coefficients used in this calculation are $d_1 = 0.35$ and $b_3 = 2.18$ which would pass a 20 percent significance test.

[29]Eliminating the terms made negative by the test score coefficient would raise the income change estimate by only $21. While the observed negative relation of spending to test scores turns out to be quantitatively trivial, the reasons for its appearance at all may be a worthy topic for future research. Not only TAFQT, but also the other test score composities from Project Talent that we experimented with, tend to show a negative (though weak) relation to spending.

[30]The earnings gain associated with a $100 cost increase is a little less than $280 for high status individuals in both versions of the calculation. For low status individuals, the comparable figures are $217 and $445. The latter value uses a coefficient with a t-statistic of only .59.