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The Inheritance of Economic Status - by John A. Brittain

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BOOK REVIEW

THE INHERITANCE OF ECONOMIC STATUS. By John A. Brittain. Washington, D.C.: The Brookings Institution, 1977. Pp. xiii, 185. \$9.95 cloth; \$3.95 paper.

*Reviewed by Michael R. Olneck**

In the mid-1960's and in the early 1970's, research results appeared that challenged conventional liberal beliefs about the causes and consequences of poverty. In 1966 the federal government published *Equality of Educational Opportunity*, a report prepared by James Coleman and his associates.¹ The data used in the report contained the startling result that, with some exceptions, within regions, the provision of educational resources was substantially uniform across racial and socioeconomic groups.² Moreover, the data showed that what measurable differences existed between the schools attended by disadvantaged and advantaged students did not account for the academic gap between the groups.³ Six years later, Frederick Mosteller and Daniel P. Moynihan published an exhaustive reanalysis of the Coleman data by several researchers that did little to amend the original report's principal conclusions.⁴ The net effect of these reports was to call into question the government's strategy to enhance the life chances of poor children by altering their educational environments.⁵

In 1972 Christopher Jencks and seven collaborators published *Inequality: A Reassessment of the Effect of Family and Schooling in America*.⁶ Jencks and his associates reiterated the earlier findings on the slight effects of school differences, but their principal contribution was a critical examination of the effects of individual characteristics that were presumed to determine success or failure in the

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1. J. COLEMAN, *EQUALITY OF EDUCATIONAL OPPORTUNITY* (1966).

2. *Id.* at 68, 73, 75-76, 78-79, 85, 95, 100-01, 103-04, 132-34. *See also* Jencks, *The Coleman Report and the Conventional Wisdom*, in F. MOSTELLER & D. MOYNIHAN, *ON EQUALITY OF EDUCATIONAL OPPORTUNITY* 73-81 (1972).

3. COLEMAN, *supra* note 1, at 290-324. *See also* Jencks, *supra* note 2, at 90-104.

4. *See* F. MOSTELLER & D. MOYNIHAN, *supra* note 2.

5. The government's strategy was also vulnerable to the discouraging findings of direct evaluations of compensatory education programs. *See* M. McLAUGHLIN, *EVALUATION: THE ELEMENTARY AND SECONDARY EDUCATION ACT OF 1965, TITLE I* (1975).

6. C. JENCKS, *INEQUALITY: A REASSESSMENT OF THE EFFECT OF FAMILY AND SCHOOLING IN AMERICA* (1972).

marketplace. Among their more dramatic conclusions was that differences in cognitive skills, length of schooling, and family background have little to do with differences in adult income.⁷ Governmental policy had assumed that today's poor were the children of yesterday's poor and that a "cycle of poverty" was perpetuated principally by low intellectual competence and the failure of poor children to persist in their schooling. Jencks and his colleagues argued that these results implied that mitigating the consequences of disadvantaged backgrounds by equalizing cognitive skills and educational credentials would do little to reduce the extent of poverty in the United States. They also argued that fully equalizing the life chances of children from different families would do little to reduce poverty. In short, they argued that inequality of opportunity in one generation was a trivial source of inequality in the next.⁸

The liberal community could be expected to react strongly to Jencks' arguments that enriched school environments for the disadvantaged would do little to equalize school achievement among social groups and that, even in the event school achievement and educational attainment were distributed equally and other consequences of differential family background were eliminated, the degree of economic inequality would remain substantially unchanged.⁹ Those arguments struck at both widespread ideals of fairness and conventional explanations for the incidence and locus of poverty. Not surprisingly, researchers disturbed by the Jencks and Coleman findings have tried to fault the data and analyses on which they are based and to undertake new studies that would restore scientific sanction to the old conventional wisdom. John Brittain's *The Inheritance of Economic Status* is in this tradition.

Brittain's principal contention is that the Jencks study seriously underestimated the importance of differences in social origins for determining individual adult success and the persistence of economic inequality. Brittain further contends that differences in educational attainment have strong effects on differences in socioeconomic success, suggesting the potential efficacy of reducing adult economic inequality through programs to equalize schooling. Neither Brittain's empirical results nor his interpretations of them are convincing.

Brittain's conclusions are based upon his reanalysis of a small number of men who were the surviving sons of a sample of decedents

7. *Id.* at 8, 226.

8. *Id.* at 7-8, 220.

9. See, e.g., Symposium, *Perspectives on Inequality: A Reassessment of the Effect of Family and Schooling in America*, 43 HARV. EDUC. REV. 37 (1973).

whose estates were filed in probate court in Cuyahoga County (Cleveland), Ohio. The sample was chosen between November 1964 and August 1965 for a study on inheritance. The original research team attempted to interview 537 sons associated with 659 estates represented in the sample. Only 324, or sixty percent, were interviewed.¹⁰ The largest subsample for which Brittain reports results involving income as a dependent variable is 263, and this analysis is confined to an appendix.¹¹ The text relies chiefly on his analysis of interviews with men whose brothers also were interviewed. Only 120 men who reported their incomes are included in this subsample.¹² Evidence within Brittain's book suggests that these men are not typical of the complete sample. Conclusions based upon interviews with them, therefore, may well be faulty.

The dispersion of incomes in Brittain's brothers subsample is considerably smaller than the dispersion of income among all men in his sample. In particular, men with low incomes are underrepresented in the brothers subsample.¹³ Brittain argues that the narrower range of variation in the brothers subsample may be attributable to homogeneity among brothers.¹⁴ But constrained variation is not a necessary statistical consequence of similarity among brothers, nor is it empirically confirmed in other data sets.¹⁵ The conse-

10. M. SUSSMAN, *THE FAMILY AND INHERITANCE* 51 (1970).

11. J. BRITTAİN, *THE INHERITANCE OF ECONOMIC STATUS* 176 (1977). Larger subsamples are analyzed when occupational status and residential quality are considered as dependent variables. Brittain also constructs composite measures of socioeconomic success, but I do not accept his characterization of them as "more complete measures of overall economic position." *Id.* at 87. The composites are based on principal component analysis, and to my mind, Brittain's analyses of them demonstrate only that much of the tendency for income, occupational status, and residential quality to vary together is related to the effects of family background, particularly to easily measured demographic aspects of background. Brittain's definition of variations in outcomes not related to his composite measures as "noise" is an error; consequently I do not consider his analyses of them in the course of this review. *Id.* at 53.

12. *Id.* at 48.

13. The standard deviation of the common logarithm of income in the brothers subsample is only 0.209. *Id.* at 55. For all male interviewees (N=263), it is 0.319. *Id.* at 89. In Brittain's sample of all married sons (N=237), the standard deviation of logarithmic income is given as 0.217. *Id.* at 91. This means that inclusion of only twenty-six unmarried men substantially alters the standard deviation. Perhaps they are quite young. Brittain does not comment on this pattern, except to say that single men are excluded because "the economic status of unmarried men . . . [is] generally lower and more erratic than that of married men. It thus seemed likely that the accuracy of the estimates could be enhanced by dropping the single sons." *Id.* at 174. Omitting men whose incomes are lower and tend to be less related to measured characteristics may improve goodness of fit and reduce standard errors, but I do not see how it improves the representativeness of estimates.

14. *Id.* at 41.

15. For example, the standard deviations of the logarithm of earnings in the 1973 NORC brothers sample and the NORC amalgam sample, from which the brothers sample was drawn, are similar. The same is true of the variations in the 1972 Project Talent follow-up

quences of this selectivity may be serious.

The usual consequence of such selectivity is to reduce the statistical relationship of a variable with its determinants. Consistent with this expectation, measured socioeconomic background more accurately predicts income in Brittain's larger sample than in his smaller brothers subsample. Overrepresenting men with high incomes in the brothers subsample, however, very likely has led Brittain to exaggerate the degree to which brothers' incomes are similar.¹⁶ Unfortunately, it is this supposed similarity upon which Brittain bases his conclusion that social origins are a powerful determinant of men's incomes and rejects the contrary conclusion of Jencks and his colleagues.

It is impossible to derive an accurate estimate of the similarity of brothers' incomes for the population represented by the Cleveland sample. In addition to the statistical problem referred to above, Brittain's small sample size simply precludes a precise estimate. It is also uncertain whether such an estimate would generalize to larger populations. An accurate estimate, however, would add to the growing body of estimates from which a consistent picture of the consequences of social origins for economic success might emerge.¹⁷

If Brittain's conclusion that family background explains forty percent of the variation in men's incomes is discounted as being to some extent artifactual and certainly imprecise, with what are we left? On the high side are Paul Taubman's 1976 estimate of thirty

and its brothers subsample. See Eaglesfield, *The 1973 NORC Amalgam Survey* and Crouse, *The 1960-1972 Project Talent Longitudinal Survey and The Project Talent Sibling Sample*, in C. JENCKS & L. RAINWATER, *EFFECTS OF FAMILY BACKGROUND, TEST SCORES, PERSONALITY TRAITS AND SCHOOLING ON ECONOMIC SUCCESS*, app. E, H, & K (1977).

16. I base this conclusion on my own analysis of the Kalamazoo brothers sample in which I found that excluding men with incomes in the upper open-ended interval (*i.e.*, incomes of \$25,000 and over) reduces the sibling logarithmic income correlation by one-half. See Olneck, *On the Use of Sibling Data to Estimate the Effects of Family Background, Cognitive Skills, and Schooling: Results from the Kalamazoo Brothers Study*, in *KINOMETRICS: DETERMINANTS OF SOCIOECONOMIC SUCCESS WITHIN AND BETWEEN FAMILIES* 125 (T. Tauberman ed. 1977). This is true even when I omit only the handful of pairs in which both brothers are in the upper interval. This suggests that in samples such as Brittain's and mine, the overrepresentation of high incomes, coupled with a single code for incomes above a chosen cut-off point, can substantially inflate estimates of the correlations between brothers' incomes. Even reweighting the Kalamazoo sample to reflect a nationally representative income distribution does not appreciably alter the sibling correlation. The results of reweighting are unconvincing, however, because the very few low-income respondents in the sample are unlikely to have been representative.

17. "Social origins" is defined here as all aspects of background that brothers share and includes both measured and unmeasured aspects of socioeconomic status, approximately one-half of their genetic endowments, and environmental influences that vary between families within the same socioeconomic stratum. Differences between families tend to place an upper bound on what most people would consider unequal opportunity.

percent from a large sample of middle-aged fraternal twins¹⁸ and Gary Chamberlain and Zvi Griliches' 1975 estimate of thirty-seven percent from a small and unsystematic survey of Indiana men conducted in 1927.¹⁹ Fraternal twins are likely to have more in common than ordinary brothers, and estimates of the influence of family background on twins therefore may not generalize.²⁰ The unsystematic Indiana sample contains the peculiar result that brothers' incomes are similar but their occupations are not. Because this is hardly credible, results from this sample can be ignored safely.

On the more modest side are estimates of twenty-one percent in a small national sample of twenty-eight year-olds and their brothers²¹ and twenty-two percent in a relatively large sample of men thirty-five years to fifty-nine years old who grew up in Kalamazoo, Michigan.²² The sample of twenty-eight year-olds is restricted to men who reached at least the eleventh grade and whose brothers were in the eleventh or twelfth grade when they were in the eleventh grade. The educational constraint and the age homogeneity in this sample may well inflate the estimate of sibling similarity. The youth of the men sampled may deflate it. The Kalamazoo sample most likely suffers from the same kind of selectivity bias as Brittain's sample and probably exaggerates the influence of background in the population it represents.

Finally, on the low side is the result that family background explains only thirteen percent of the variation in the earnings of 300 brothers in a 1973 national sample of men twenty-four years to sixty-three years old.²³ This sample is small, so the estimate it provides is subject to large sampling error. Nevertheless, the distribution of income in this sample is consistent with the distribution expected on the basis of census data.²⁴ Since the distribution of

18. Taubman, *The Determinants of Earnings: Genetics, Family and Other Environments, A Study of White Male Twins*, 66 AM. ECON. REV. 858-70 (1976).

19. Chamberlain & Griliches, *Unobservables with a Variance-Components Structure: Ability, Schooling, and the Economic Success of Brothers*, 16 INT'L ECON. REV. 422-49 (1975). The Indiana sample is described in D. GORSELINE, *THE EFFECT OF SCHOOLING UPON INCOME* (1932).

20. In my own data, however, there is no difference between the correlations between brothers' incomes for pairs close in age (*i.e.*, 3 years or less) and pairs further apart in age. See Olneck, *supra* note 16, at 138-40.

21. Corcoran, Jencks, & Olneck, *The Effects of Family Background on Earnings*, 66 AM. ECON. REV. 430, 431 (1976).

22. Olneck, *supra* note 16, at 132-33.

23. Corcoran, Jencks, & Olneck, *supra* note 21, at 431.

24. Compare Eaglesfield in JENCKS & RAINWATER, *supra* note 15, Table 1 with Bartlett & Jencks, *The 1970 Census 1/1000 Sample*, in JENCKS & RAINWATER, *supra* note 15, Table 1, app. A. Brittain observes that the NORC brothers standard deviation is "suspiciously high," but this is true only in comparison to a variety of atypical samples with information on

income may critically affect estimates, the result of this particular sample is probably the most believable of those yet derived. Taken in conjunction with the Kalamazoo result, it implies that the "guesstimate" in Jenck's *Inequality* that brothers' incomes correlate between only 0.15 and 0.20 was accurate.

If family background explains a fifth of the variation in men's income, what can be said about inequality of opportunity as a source of income inequality? The answer depends upon the question asked. If, from the perspective of the overall distribution of income, we ask whether differences in social origin *produce* differences in income, the answer does not lie within the data. This answer is not found because individual characteristics associated with family background can function as selection mechanisms into a predetermined distribution of economic rewards. Brittain, like many economists, neglects this possibility and assumes that relationships in his data that persist when other variables are controlled are genuinely causal. He implies that eliminating inequality of opportunity in one generation would reduce the absolute level of inequality in the next. This reasoning is faulty. Equalizing the characteristics now associated with individual income differences might make those differences random and statistically inexplicable, but it does not necessarily reduce them.²⁵

If we ask what the distribution of male earnings would look like *if* differences now associated with family background were eliminated, the answer is that the distribution would not be very different. In 1970 the top fifth of male earners earned about 280 percent of the national average, while the bottom fifth earned about thirty-five percent of the national average. Eliminating differences associated with family background, on the assumption that background explains a fifth of the variation in earnings, would result in the top fifth earning 250 percent of the national average, compared to thirty-nine percent for the bottom fifth.²⁶ The incidence of very low incomes would be affected negligibly by eliminating the consequences of unequal opportunity.

If we next ask what the twenty percent variation result implies

brothers. BRITTAİN, *supra* note 11, at 54. It is not high compared to national samples that Brittain later cites. *Id.* at 89.

25. For example, inequality in educational attainment fell between 1950 and 1970. Inequality in income showed no parallel decline. See L. THURLOW, *GENERATING INEQUALITY* 61-66 (1975).

26. This result is calculated on the basis of a standard deviation of the natural logarithm of earnings of 0.743 for noninstitutional males twenty-five years to sixty-four years of age with positive earnings. See Bartlett and Jencks, in JENCKS & RAINWATER, *supra* note 15, Table 2.

for the life chances of individuals, the answer depends upon what statistics are emphasized. Jencks and his associates emphasized that brothers' incomes differed on the average by almost as much as random individuals' incomes. They calculated that in 1968 brothers differed by at least \$5600 a year in earnings, while random pairs of men differed by \$6200 a year, and concluded that "[w]hen people have had relatively equal opportunity, as brothers usually have, they still end up with very unequal incomes."²⁷ What Jencks and his colleagues did not emphasize—but what Brittain does emphasize—is that this does not mean that men with very unequal opportunities end up with equal incomes.

Even if family background explains only twenty percent of the variation in earnings in 1970, men raised in the most favorable fifth of homes would be expected to earn two and one-half times as much as men raised in the least favorable fifth of homes.²⁸ Brittain is right to stress results analogous to these. He is right also in extending them by calculating the probabilities that men reared in different levels of home background attain specific ranges of income. For example, Brittain finds the likelihood that a respondent born in the most favorable fifth of homes earned more than \$25,000 in 1976 was fifty-one percent, whereas for respondents born in the least favorable fifth of homes it was only 1.8 percent.²⁹ Because he is working with an unrealistically high correlation between brothers' incomes, Brittain exaggerates the differences in life chances for men from different backgrounds. But his exaggeration does not distort the basic picture.

Working with the more realistic assumption that brothers' earnings correlate no more than 0.20, I calculate that a man from the most favorable fifth of home backgrounds had a thirty-eight percent chance of earning more than \$15,000 in 1970, but that his disadvantaged counterpart had only a 4.4 percent chance of earning what was then a high income. The likelihood that a man from the top fifth of favorable backgrounds earned less than \$4000 in 1970 would have been only about 4.7 percent, while thirty-nine percent of the men from the least favorable fifth of backgrounds would have

27. See JENCKS, *supra* note 6, at 220.

28. The calculation is based upon a standard deviation of natural logarithms of income of 0.743 and a correlation between family background and \ln income of $[\.20]^{1/2} = .447$. The top and bottom fifths of a normal distribution average 2.8 standard deviations apart. $\text{Anti-}\ln 2.8(.447)(.743) = 2.534$.

Note that I use "favorable," not "favored." This is because the distribution of home backgrounds is defined here in terms of effects, not measurable early advantages, though these are involved in part.

29. BRITTAİN, *supra* note 11, at 58.

been in this low income bracket.³⁰ Thus Brittain is correct when he stresses the costs of unequal opportunity to individuals. He is wrong, however, to suggest that eliminating unequal opportunity would do much to reduce the incidence of low incomes. It merely would distribute them somewhat more randomly.

If Brittain is also right that we object most to inequalities associated with accidents of birth,³¹ then achieving complete randomization of poverty might well undermine any potential consensus to eliminate it. This realization no doubt motivates critics who fear the impact of the prior works, particularly that of Jencks. Much of the government's strategy in the War on Poverty rests on making individuals more like one another when they enter economic competition. But if individual differences are not a large source of income differences, then this strategy cannot work, and we must look elsewhere for an explanation of economic inequality and for the means to reduce it.

The role of education is important in debates about equal opportunity and strategies to reduce poverty. Consequently, researchers have devoted considerable attention to the extent to which family advantage is transmitted by the superior educational attainment that favored sons enjoy and the extent to which educational advantages translate into economic advantages. Brittain addresses these questions in detail, but his sample is too small and atypical, and the variables available to him are too limited to provide generalizable results.

For example, Brittain finds that the apparent income advantages enjoyed by men whose parents were well-educated and held good jobs, who came from smaller families, and whose families were affiliated with the more economically successful religious denominations are reduced by only about one-third when educational attainment is considered as an intervening influence.³² Large-scale representative national data show, however, that the sons of better educated fathers and men from smaller families earn more money almost entirely because they acquire more education than otherwise similar men.³³

Like others working with sibling data, Brittain often employs a broader definition of social origins than is indexed by explicit

30. The calculation is based upon the assumptions of a mean ln income of 8.946 and the statistics given in note 28 and of within-group normality.

31. BRITTAİN, *supra* note 11, at 1-3, 9-12, 28-33.

32. *Id.* at 126-34.

33. Corcoran, *The Effects of Family Background on Earnings*, in JENCKS & RAINWATER, *supra* note 15, ch. 3.

demographic variables. When family background is defined as all the factors that make brothers alike, the extent to which its influences on economic success are mediated by education is a matter of empirical analysis. Yet for income, Brittain simply assumes the result he obtains from his analysis of explicit background variables and applies it to his analysis of brothers.³⁴ Evidence within his book, however, suggests that only about twenty-five percent of the effect of overall family background on income is mediated by the similarity that brothers achieve in educational attainment.³⁵ This means that in Brittain's sample, brothers tend to earn similar incomes for reasons largely unrelated to education. If this result were generalizable, it would imply that unequal educational opportunity has little to do with unequal economic opportunity, at least insofar as opportunities varied across all families and not simply across conventionally defined socioeconomic strata.³⁶

A major problem that plagues efforts to assess the economic "payoff" to increased education is that men who complete more schooling are not exactly like men who quit school. Therefore, the observed differences in the earnings of men with more and less schooling almost certainly overstate the differences due to education alone.³⁷ Brittain tackles this problem by comparing the earnings differences associated with increments of schooling among individuals in his sample to those he finds among men who come from similar socioeconomic backgrounds and to those he finds among brothers.

Brittain's education variable is not scaled in actual years of education, and he nowhere indicates the standard deviation of the variable for his brothers' subsample. Consequently, it is impossible to adjust his results to permit direct comparison to those reported

34. BRITTAİN, *supra* note 11, at 133. An explicit analysis is reported for occupational status, but the model solved is overidentified. *Id.* at 138. Brittain's error here is that he ignores the possibility that the family-related factors affecting education need not correlate perfectly with those affecting occupation and that he does not test for this by utilizing the correlation between a respondent's educational attainment and his brother's occupational status.

35. The standardized regression coefficient of education for log income controlling background in Brittain's sample is 0.265. *Id.* at 147. The correlation between brother's educational attainments is 0.383. *Id.* at 48. The sibling correlation on log income predicted solely on the basis of brothers' similarity on education is $(.265)(.383)(.265) = .027$, which implies a correlation between log income and family background of $[\.027]^{1/2} = .164$. The actual correlation between log income and family background in Brittain's sample is 0.63. *Id.* at 133. Thus $.164/.636 = .258$, or approximately 25 %.

36. The Kalamazoo data imply that half the effect of family background on income is mediated by education.

37. Downward biases may be present because of inaccuracies in reported education, nonrandom entrance into occupations, and negative correlations between unmeasured determinants of education and income.

elsewhere. In Brittain's sample the apparent effect of an additional year of education on income is reduced by almost one-third among men who come from similar backgrounds. This is true whether background is defined narrowly or broadly. In this respect, Brittain's results are consistent with those found in national data.³⁸

Men who come from the very same family, however, are quite likely to differ in a variety of respects that affect both educational attainment and earnings. Thus, holding family background constant only partially controls out the spurious fraction of the education-income relationship. Brittain is aware of this problem, and he notes, for example, that he has no direct measure of intellectual ability. But on the basis of an uncritical review of earlier studies and of the presumed similarity of brothers in ability, he reaches the tentative view that omission of an ability measure is not serious. Evidence elsewhere suggests that this is not the case. In the Kalamazoo brothers sample, an additional year of education appears to raise annual earnings by almost seven percent. Among brothers, the effect is five percent, which is consistent with Brittain's findings. On the other hand, when sixth grade standardized aptitude test scores are also controlled, the effect falls to only three percent. This suggests that, on the average, over half of the observed monetary benefits of additional education are spurious, arising because men with more schooling are more likely to earn higher incomes irrespective of their educational credentials.

The average effect of an additional year of education is not, however, necessarily indicative of the effects of particular levels of education. Brittain ignores this problem and calculates the effects of additional schooling as if they were uniform across all levels.³⁹ My own best estimate, based upon analyses of several large national data sets and several smaller data sets with information on brothers or test scores, is that among similar men, four years of high school are worth only a fifteen to twenty percent increase in earnings, while four years of college are worth between thirty-three and forty-one percent.⁴⁰ The difference arises mainly because receiving a high

38. For example, controlling family background in a manner analogous to Brittain's for 5780 respondents in the classic 1962 "Occupational Changes in a Generation Survey" reduces the apparent monetary benefits of an additional year of schooling by 29 percent. Only data on brothers' educations and one respondent's income are required for this calculation. See Olneck, *The Effects of Education*, in JENCKS & RAINWATER, *supra* note 15, at 342.

39. Table 4.5 appears to present results for separate levels of schooling, but the calculations are based upon the assumption that equal increments in schooling produce constant percentage gains, *i.e.*, that the ratios of incomes for successive levels of schooling are constant. BRITTAİN, *supra* note 11, at 143.

40. Olneck, *The Effects of Education*, in C. JENCKS, WHO GETS AHEAD? (forthcoming).

school diploma does not confer the extra sheepskin bonus that receiving a baccalaureate degree confers and because the measured effects of additional secondary schooling are more sensitive to controls for family background and ability than are the effects of higher education. These results suggest that making schooling more equal by making high school graduation universal will not prove an effective antipoverty device. Although distributing higher education more equally might have larger effects, it would be more likely to reduce the incidence of high incomes than to reduce the incidence of low incomes.⁴¹

Brittain's work is marred not only by unconvincing results and interpretation but also by an inadequate sample description and erroneous assertions about the principal secondary source to which *The Inheritance of Economic Status* is a response. While a great deal of detail is provided about the Cleveland sample, nowhere is there a compact presentation of the sample statistics (*i.e.*, correlations, means, standard deviations, frequency distributions) underlying Brittain's analyses. Some crucial statistics are not presented at all. Consequently, evaluating Brittain's result in the light of other work is difficult.

Brittain indicates that Jencks and his colleagues worked with no sibling data whatsoever, leaving the impression that their results lack credibility. This is not true. Jencks and his associates had available estimates of the correlations between brothers' educations and brothers' occupational statuses.⁴² It is true that they lacked direct information on brothers' incomes and that they may have erred in assuming that unmeasured aspects of family background are of negligible importance in the determination of individual income. But if I am correct that Brittain's result is exaggerated, the magnitude of their error would appear small in the light of other subsequent research.

Brittain also contends that Jencks and his associates failed to

41. The actual trend is toward a more unequal distribution of higher education. D. Featherman and R. Hauser (unpublished analyses of 1973 Occupational Changes in a Generation Survey replication) (copy on file with the *Vanderbilt Law Review*). Eliminating all differences in earnings associated with differences in length of schooling would have reduced earnings inequality in 1970 by only about fifteen percent. Susan Bartlett has demonstrated that changes in the distribution of earnings between 1939 and 1969 were determined mainly by factors unrelated to schooling and that those changes that were related to schooling had to do with changes in the returns to education, not with the distribution of education. Bartlett, *Changes in the Effects of Education and Experience: 1939-1969*, in JENCKS & RAINWATER, *supra* note 15. These findings suggest that Brittain's optimism regarding the efficacy of equalizing educational attainment in order to equalize incomes is misplaced.

42. Their estimate for occupational status was probably somewhat low, but it was not inferred. See JENCKS, *supra* note 6, at 329, 337, 343.

treat correctly their statistical estimates of the correlations between measures of brothers' economic success, and he includes an extensive, but confused and confusing, appendix devoted to the issue. Like other readers, Brittain may have been misled by imprecise language in the Jencks study. He erroneously concludes that Jencks and his collaborators failed to introduce appropriate statistical corrections.⁴³ This is not at all the case. The source of any difference between Jencks' and Brittain's results lies principally in differences in data bases.

Brittain's work no doubt will receive favorable attention because it is associated with an important research institution and because its conclusions accord with longstanding preconceptions about the causes of poverty. Policymakers who find the results and arguments in the Jencks study to be unpalatable now will be able to invoke a standard reference in defense of earlier premises. Unfortunately, those premises are no more credible now than they ever were, and social policy will not be made more effective by pretending otherwise.⁴⁴

43. Jencks concluded that "[e]ven if family background explains 20 percent of the income variance, the within-family standard deviation will be $[1-0.20]^{1/2} = 90$ percent of the population standard deviation." *Id.* at 239-40. Brittain misinterprets the 0.20 figure as a conventional measure of explained variance that was uncorrected for the number of members per family underlying the result. BRITAIN, *supra* note 11, at 59, 170. Consequently, he misinterprets the use of "within-family standard deviation" to refer to the *observed* within-pair standard deviation. The 0.20 figure in fact refers to an adjusted percentage of explained variance and is equivalent to the sibling correlations with which Brittain himself worked. The so-called within-family standard deviation refers to the error standard deviation, net of the effects of family background, and does not refer to an observed sample statistic, as Brittain implies. Consequently, the methodological debate Brittain pursues is devoted to a nonissue.

44. This is not, of course, to question the potential of policies implementing direct income redistribution. By neglecting the extent of more direct measures, critics may tend to exaggerate the role of the so-called human capital approach to reducing poverty. For criticism of Jencks and his colleagues along this line, see A. Rivlin, *Forensic Social Science*, 43 *HARV. EDUC. REV.* 61 (1973). For an overview of antipoverty programs, see R. HAVEMAN, *A DECADE OF FEDERAL ANTIPOVERTY PROGRAMS* (1977).