Intergenerational Income Mobility in the United States

By Gary Solon*

Social scientists and policy analysts have long expressed concern about the extent of intergenerational income mobility in the United States, but remarkably little empirical evidence is available. The few existing estimates of the intergenerational correlation in income have been biased downward by measurement error, unrepresentative samples, or both. New estimates based on intergenerational data from the Panel Study of Income Dynamics imply that the intergenerational correlation in long-run income is at least 0.4, indicating dramatically less mobility than suggested by earlier research. (*JEL* D31, I32)

The degree to which income status is transmitted from one generation to the next has persistently interested social scientists and others concerned with social policy. This interest has stemmed largely from a belief that intergenerationally transmitted income inequality violates equal opportunity norms and warrants government intervention. Michael Harrington's influential book *The Other America*, for example, based its call for antipoverty efforts on just such a premise:

> ...the real explanation of why the poor are where they are is that they made the mistake of being born to the wrong parents, in the wrong section of the country, in the wrong industry, or in the wrong racial or ethnic group. Once that mistake has been made, they could have been paragons of will and morality, but most of them would never even have had a chance to get out of the other America. [1962 p. 21]

The recent literature on the "underclass" also has emphasized the extent to which income status, especially poverty, is passed from generation to generation. Ken Auletta (1982 p. 268), for instance, has written, "Today, perhaps for the first time, America has a sizable, and so far intractable, intergenerational underclass." In a similar vein, Martin Kilson (1981 p. 58) has argued that "those blacks who have come out of the 1960s and 1970s poverty ridden are more likely to pass on this awful plight to their offspring—offspring who, owing to inadequate schools, poor school performance, excessively high unemployment, low skills, and attendant social pathologies, have little opportunity to put the poverty of their parents behind them," Popular writings on the very wealthy likewise have stressed the intergenerational transmission of economic status (see e.g., Ferdinand Lundberg, 1968).

Given the widespread concern about intergenerational mobility, it is astonishing how few attempts have been made to measure the simple intergenerational correlation of income in the United States. The published estimates based on intergenerational income observations can be counted on one hand and have been generated by

*Department of Economics, Lorch Hall, The University of Michigan, Ann Arbor, MI 48109-1220. This research was supported by a grant to the Institute for Research on Poverty from the U.S. Department of Health and Human Services and by an Alfred P. Sloan Research Fellowship. The opinions and conclusions expressed in this paper are those of the author and do not necessarily reflect the opinions or policy of these organizations. The author thanks Deborah Laren for extraordinary assistance with the Panel Study of Income Dynamics data and Robert Wood for excellent research assistance. Valuable comments were received from Charles Brown, Paul Courant, Edward Gramlich, the referees, and seminar participants at the Institute for Fiscal Studies, the State University of New York at Stony Brook, Stanford University, and the Universities of Bristol, California at Los Angeles, Essex, and Wisconsin.

only two research teams.[1] Although several studies have been conducted in other countries, these, of course, are of no help for ascertaining the degree of intergenerational mobility in the United States.[2]

In stark contrast to the above quotations, which stress the importance of intergenerational transmission, the U.S. statistical studies have found strikingly small intergenerational income correlations. Jere Behrman and Paul Taubman (1985 p. 147) estimated the father–son correlation in the logarithm of earnings to be 0.2 or less and concluded, "The members of this sample come from a highly mobile society." William H. Sewell and Robert M. Hauser (1975 p. 72) estimated only a 0.18 correlation between sons' earnings and parents' income, and William T. Bielby and Hauser (1977 p. 267) estimated a 0.16 correlation between sons' log earnings and parents' income.[3] Based on a survey of European as well as U.S. studies, Gary S. Becker and Nigel Tomes (1986 p. 51) concluded, "Regression to the mean in earnings in rich countries appears to be rapid." Becker's presidential address to the American Economic Association (1988 p. 10) similarly concluded, "In all these countries, low earnings as well as high earnings are not strongly transmitted from fathers to sons...."

The obvious question is: are the policy-oriented writings that have emphasized intergenerational transmission unfounded, or is there something wrong with the statistical evidence? Section I of this paper demonstrates that the previous estimates of intergenerational income correlations have been biased downward by measurement error, unrepresentative samples, or both. Sections II, III, and IV describe a new analysis based on intergenerational data from the Panel Study of Income Dynamics. The results contain strong evidence that, in the United States, the father–son correlations in long-run earnings, hourly wages, and family income are about 0.4 or even higher. These results depict a much less mobile society than most previous studies have portrayed. Section V summarizes and discusses the findings.

I. Biases in Previous Studies

Previous estimates of intergenerational income mobility have been based on error-ridden data, unrepresentative samples, or both. To explore the likely effects of these

[1]Numerous studies, such as Otis Dudley Duncan et al. (1972) and Mary Corcoran and Christopher Jencks (1979), have estimated intergenerational correlations in measures of occupational prestige. Such estimates typically are larger than the existing ones for income. It has been unclear whether the estimates for occupational-status measures are higher because such measures are better indicators of long-run income than are the available income variables or because fathers and sons tend to be in similar occupational categories even when their long-run incomes are very different. Another study, by Donald J. Treiman and Robert M. Hauser (1977), imputed intergenerational income correlations in the absence of parental income data by imposing strong assumptions in an elaborate simultaneous-equations model of income, occupational prestige, and education. The imputed correlations range from 0.15 to 0.54. Treiman and Hauser repeatedly acknowledged the obvious desirability of obtaining parental income data to enable direct estimation of intergenerational income mobility. Still other studies have estimated the overall effects of family background by measuring sibling correlations in economic status (see Solon et al. [1991] for an example and a summary of the literature). While sibling studies are useful for assessing the combined effect of all background characteristics shared by siblings, they do not identify the portion of the effect related to parental income. For further discussion, see Corcoran et al. (1990). Finally, a great many studies have estimated regression relationships between income variables and large sets of background characteristics (see Corcoran et al. [1992] and the references therein). Such studies, however, do not directly address the simple intergenerational correlation in income.

[2]See Gary S. Becker and Nigel Tomes (1986) for an international survey. The analysis by A. B. Atkinson et al. (1983) of intergenerational data from York, England, is methodologically the closest to the present study, and it produces similar estimates.

[3]According to Becker and Tomes (1986), applying a correction for response error to Shu-Ling Tsai's (1983) unpublished results yields a 0.28 estimate of the elasticity of sons' earnings with respect to parents' income. An unpublished printout circulated by Hauser contains a 0.24 estimate of the correlation between sons' earnings and parents' income. Both estimates are based on the same Wisconsin sample used by Sewell and Hauser (1975), the limitations of which are discussed in Section I.

problems, consider the following model. Let y_{1i} represent long-run economic status (e.g., the "permanent" component of log annual earnings) for a son in family i, and let y_{0i} be the same variable for his father. Let both variables be measured as deviations from generation means.[4] Let ρ denote the true population correlation between y_{0i} and y_{1i}, and assume for now that the population variance in y is the same, σ_y^2, in either generation. Then, if y_{0i} and y_{1i} were directly observed for a random sample of families, one could estimate ρ by applying least squares to the regression equation

$$(1) \qquad y_{1i} = \rho y_{0i} + \varepsilon_i .$$

The intergenerational correlation ρ could be consistently estimated by either $\hat{\rho}$, the estimated slope coefficient, or R, the square root of the R^2 statistic.[5]

This is essentially the estimation approach used in previous studies of intergenerational income mobility (both in the United States and in other countries), with two crucial exceptions. First, lacking direct measures of long-run status, these studies instead have used short-run proxies, sometimes only single-year measures of earnings or income. Second, they typically have used data from peculiarly homogeneous samples, rather than random samples. As discussed in detail in Solon (1989a), both factors generate downward biases in the estimated intergenerational correlations.

The first bias can be simply characterized by assuming that the short-run proxy for son's long-run status is his measured status in period t,

$$(2) \qquad y_{1it} = y_{1i} + v_{1it}$$

where v_{1it} is a transitory fluctuation around long-run status due to both actual transitory movement and random measurement error.[6] Similarly, the proxy for father's status is his measured status in period s,

$$(3) \qquad y_{0is} = y_{0i} + v_{0is} .$$

Let σ_{v0}^2 and σ_{v1}^2 denote the population variances of v for each generation, and assume that v_{0is} and v_{1it} are uncorrelated with each other and with y_{0i} and y_{1i}. Then, when previous studies have applied least squares to equation (1) with y_{0is} and y_{1it} in place of y_{0i} and y_{1i}, the resulting estimates have been subject to errors-in-variables biases. In particular, the probability limit of the estimated slope coefficient $\hat{\rho}$ is

$$(4) \qquad \operatorname{plim} \hat{\rho} = \rho\sigma_y^2 \big/ \left(\sigma_y^2 + \sigma_{v0}^2\right) < \rho$$

and the probability limit of R is

$$(5) \qquad \operatorname{plim} R = \rho\sigma_y^2 \Big/ \sqrt{\left(\sigma_y^2 + \sigma_{v0}^2\right)\left(\sigma_y^2 + \sigma_{v1}^2\right)} < \rho .$$

Whether this tendency to underestimate ρ is practically important depends on whether the variances of the transitory fluctuations are substantial relative to the variance in long-run status. Information on this point for the United States is available from several longitudinal studies of earnings and wage rates, which have decomposed the population variance in annual measures of these variables into permanent and transitory components. The results of these stud-

[4] The term "generation" is used loosely here. If the sons are from the same cohort, the fathers necessarily are not because they began fatherhood at different ages. Furthermore, the fathers' peers who never had sons are absent from the population of father–son pairs. These issues are discussed at length in Duncan (1966).

[5] If the variance in y differs between generations ($\sigma_{y0}^2 \neq \sigma_{y1}^2$), then the estimated slope coefficient estimates $\rho\sigma_{y1}/\sigma_{y0}$ rather than the correlation ρ itself. The empirical relevance of intergenerational change in the variance of long-run economic status is discussed in Section V.

[6] For simplicity, this formulation abstracts from life-cycle profiles in income variables. Such profiles are incorporated into the analysis in Section III. For evidence that the measurement-error aspect of v_{1it} is empirically important, see Greg J. Duncan and Daniel H. Hill (1985) and John Bound and Alan B. Krueger (1991).

ies suggest that, in an intergenerational analysis based on only single-year data, such as Behrman and Taubman (1985), errors-in-variables bias alone could be expected to depress estimates of ρ by more than 30 percent if the data came from a representative sample (see Lee A. Lillard and Robert J. Willis, 1978; Roger H. Gordon, 1984; Glenn M. MacDonald and Chris Robinson, 1985; Michael Baker, 1990; Solon et al., 1991).[7] In a study such as Sewell and Hauser (1975), which used status measures averaged over a few years, this bias would be reduced, though not eliminated. On the other hand, in a study such as Bielby and Hauser (1977), which measured parental income on the basis of the sons' recollections, the errors-in-variables bias probably would become even more extreme. Bielby and Hauser's estimation did incorporate a minor adjustment for response error, but the adjustment was based on the implausible assumption of zero correlation between the response errors in the sons' recollections at different times. Evidence presented in David L. Featherman (1980 pp. 166–7) and Michael P. Massagli and Hauser (1983 p. 426) suggests that the response error in sons' reports of parental income is much more severe than that accounted for by Bielby and Hauser's correction.

The second source of bias is unrepresentatively homogeneous samples. Bielby and Hauser's (1977) study was based on a national probability sample, but the sample of fathers in Behrman and Taubman (1985) was drawn from a sample of white male twins born between 1917 and 1927. To remain in the sample, both twins had to have served in the armed forces, and both had to have survived until and cooperated with a succession of surveys. One would expect this sample to be more homogeneous than a random cohort sample, and some corroborating evidence appears in Behrman et al. (1980 [section 5.6.2]). They reported that "only about 12 percent of the twin respondents had incomes under $10,000 compared to over one-third of the subjects" (p. 137) in a general comparison group of white men. Relative to the comparison group, the twin respondents also contained a dramatically smaller fraction with no more than eight years of schooling. Most other intergenerational studies, both in the United States and abroad, also have relied on homogeneous samples. Sewell and Hauser's (1975) study, for example, was based on a sons sample of Wisconsin high school seniors who graduated in 1957 and were no longer in school in 1964.

To focus on the bias from homogeneity, assume for now that permanent status can be directly observed but that the fathers sample, as in the Behrman and Taubman (1985) study, is selected from a relatively homogeneous subpopulation where the variance in permanent status is $s_{y0}^2 < \sigma_y^2$. In that case, if one applies least squares to equation (1), the probability limit of R is[8]

$$\text{(6)} \quad \operatorname{plim} R = \rho \Big/ \sqrt{1+(1-\rho^2)\left[\left(\sigma_y^2/s_{y0}^2\right)-1\right]} < \rho.$$

The reason for the downward inconsistency is that the small sample dispersion in the regressor y_{0i} depresses the R^2 statistic. A similar result of downward inconsistency applies to the case in which R is based on a homogeneous sons sample, as in the Sewell-Hauser study.

Interestingly, for the case in which the sample selection is on fathers, the estimated regression coefficient $\hat{\rho}$, unlike R, would consistently estimate ρ if long-run status were directly observed.[9] In contrast, if the homogeneous selection is on sons, $\hat{\rho}$ too is generally (and probably downward) inconsistent (see Arthur S. Goldberger, 1981; Ching-Fan Chung and Goldberger, 1984; William H. Greene, 1990 Ch. 21). Even when the selection is on fathers, homogene-

[7]The calculations in Samuel Bowles's (1972) appendix yield a similar conclusion.

[8]See Solon (1989a) for the derivation.

[9]This point was recognized previously by Atkinson et al. (1983) and Paul L. Menchik (1979).

ity is no longer innocuous for $\hat{\rho}$ once short-run proxies are used in place of long-run status. In that case, sample homogeneity aggravates the errors-in-variables bias in $\hat{\rho}$ because, with a homogeneous fathers sample, the small sample dispersion in father's long-run status reduces the "signal-to-noise ratio" in father's measured status. In mathematical terms, the factor in equation (4) declines from $\sigma_y^2/(\sigma_y^2+\sigma_{v0}^2)$ to $s_{y0}^2/(s_{y0}^2+\sigma_{v0}^2)$.[10]

Because the crucial quantities are variances in permanent status, which is not directly observed, it is difficult to ascertain the severity of sample homogeneity in previous studies or its impact on their estimates of intergenerational mobility. Nevertheless, the sample-selection criteria in some cases appear to be strikingly prone to produce homogeneous samples, and it is quite conceivable that such samples, combined with substantial error in measuring long-run status, could produce extreme biases in the estimation of intergenerational income correlations. It therefore seems worthwhile to conduct a new analysis, designed to be less susceptible to the biases of earlier studies.

II. Data Description

The new analysis uses intergenerational data from the Panel Study of Income Dynamics (PSID), a nationally representative longitudinal survey of about 5,000 families that The University of Michigan's Survey Research Center has conducted annually since 1968.[11] Because the survey has followed children from the original PSID families as they have grown into adulthood and formed their own households, it is now possible to relate the children's income status as adults to the status of their parents, as annually reported by the parents themselves since the outset of the survey.[12] The PSID data are especially well suited for reducing the biases of earlier research. First, because the data come from a national probability sample, they avoid the homogeneity of the samples used in some previous studies. Second, the longitudinal nature of the data makes it possible to explore the empirical importance of using short-run versus long-run status measures.

This study focuses mainly on father–son correlations in earnings, hourly wage rates, and family income. The main sample comprises 348 father–son pairs from the Survey Research Center (SRC) component of the PSID. The Survey of Economic Opportunity (SEO) component, designed to overrepresent the low-income population, is excluded from the main analysis, although additional results for a combined SRC and SEO sample will be reported below. The families in the SEO component were selected into the PSID on the basis of their low 1966 incomes. Because the transitory term v_{0is} in parental income is serially correlated, including the SEO component would generate a nonrandom sample of v_{0is} in the 1967–1971 parental income data used in this study.

The sons in the sample are children from the original 1968 PSID households who, in the 1985 survey, reported positive annual earnings for 1984.[13] The sons sample is restricted to the cohort born between 1951 and 1959. Sons born before 1951, who were older than 17 at the 1968 interview, are excluded to avoid overrepresenting sons who left home at late ages. The 1959 restriction assures that the sons' 1984 status measures are observed at ages of at least 25. Earnings, wages, or income observed at younger

[10] This assumes that the sample is homogeneous with respect to permanent status but not with respect to transitory fluctuations in status. This seems a reasonable characterization of the Behrman and Taubman (1985) sample.

[11] See Survey Research Center (1988) for documentation. The PSID data used in this study come from the 1985 cross-year family–individual response–nonresponse file.

[12] Other recent efforts to exploit the intergenerational span of the PSID include Behrman and Taubman (1987), Martha S. Hill and Greg J. Duncan (1987), Joseph G. Altonji (1988), and Corcoran et al. (1992).

[13] Those with earnings imputed by "major assignments" are excluded from this sample.

TABLE 1—SAMPLE CHARACTERISTICS

Variable	Mean	Standard deviation	Minimum	Maximum
Son's age in 1984	29.6	2.4	25.0	33.0
Son's earnings in 1984	22,479	15,019	19	147,656
Son's log earnings in 1984	9.75	0.94	2.94	11.90
Father's age in 1967	42.0	7.7	27.0	68.0
Father's earnings in 1967[a]	29,304	20,015	405	202,215
Father's log earnings in 1967[a]	10.10	0.69	6.00	12.22

[a]The sample statistics for father's 1967 earnings are in 1984 dollars and pertain to the sample of 322 fathers analyzed in the first row and column of Table 2.

ages would be particularly noisy measures of long-run status. By the same token, where more than one son from the same family meets all the above restrictions, only the oldest is retained in the main sample, because his 1984 status is likely to be a more accurate indicator of his long-run status. Again, however, additional results will be reported for a sample of 428 sons that includes multiple sons from the same families.

The "fathers" in the sample are the male heads of the households the sons inhabited in 1968. In some cases, these "fathers" are not the sons' natural fathers. Such cases are retained in the sample because the object of this study is not to measure genetic transmission, but to measure the correlation between economic status as an adult and the status of the household in which one grew up. Some additional results will be reported for a sample incorporating sons from mother-headed families.

Table 1 presents some summary statistics on the age and annual earnings of the main sample's fathers and sons. Despite the main sample's preference for older sons, the sample mean age for sons in 1984 is still slightly less than 30, while the sample mean for fathers in 1967 is 42. Because the sons are observed at an earlier stage of the life cycle, their mean earnings are lower, and the standard deviation of the natural logarithm of their earnings is higher.[14]

[14]The sons are at the left side of the well-known U-shaped pattern of log-earnings variance over the life cycle. See Gordon (1984) for detailed longitudinal evidence on the relationship between σ_t^2 and age.

It is important to recognize that use of the PSID does not altogether eliminate the issue of sample homogeneity. Although the PSID started as a national probability sample, its representativeness undoubtedly has been affected by attrition. In a general analysis of attrition in the PSID, Sean Becketti et al. (1988 p. 483) did not find extreme departures from representativeness, but they did note, "Low-income and high-income individuals are more likely to leave than those in the middle-income categories." This general tendency toward income homogeneity in the remaining sample evidently applies to this study's sons sample as well. The 428 members of the multiple-sons sample, who reported positive 1984 earnings in the 1985 survey, are survivors of a cohort that numbered 726 in 1968. Of the 298 lost sons, 272 had disappeared from the survey by 1985 (because of death, refusal to cooperate, or inability of the Survey Research Center to locate them), 12 remained in the survey but their 1984 earnings were missing or imputed by "major assignment," and 14 reported zero earnings. It is impossible, of course, to compare the 1984 earnings of the lost individuals to those of the individuals in the 1985 survey, but there are signs that the remaining sample of 428 underrepresents the low end of the earnings distribution. Only 6 percent of the 428 are black, compared to 16 percent of the 298; 39 percent of the 428 have fathers with less than a 12th-grade education, compared to 51 percent of the 298. For the 428, the fathers' 1967 earnings (in 1984 dollars) average \$29,437 with standard deviation \$20,379; for the 298, they average \$27,391 with standard deviation \$22,156.

As discussed in the previous section, if the remaining sample were homogeneous with respect to father's status only, this would cause inconsistency in the estimated regression coefficient $\hat{\rho}$ only insofar as the homogeneity aggravated the errors-in-variables problem. Most likely, however, the observed homogeneity in father's status arises at least partly from a negative effect of *son's* low income on the probability of staying in the sample. If so, this study suffers from a weaker version of the selection on sons that afflicted Sewell and Hauser's (1975) study and, as a result, the estimates in the next section probably are subject to some downward inconsistency.[15]

III. Econometric Models

The models estimated in this study extend the model in Section I to incorporate age profiles in earnings, wages, and income. For any of these status variables measured in year t, the model for son's status in equation (2) is extended to

$$(7) \quad y_{1it} = y_{1i} + \alpha_1 + \beta_1 A_{1it} + \gamma_1 A_{1it}^2 + v_{1it}$$

where A_{1it} is the age of the son from family i in year t. Similarly, the model in equation (3) for father's status in year s is extended to

$$(8) \quad y_{0is} = y_{0i} + \alpha_0 + \beta_0 A_{0is} + \gamma_0 A_{0is}^2 + v_{0is}$$

where A_{0is} is the father's age in year s.[16] The quadratic form of the age profiles is less restrictive than it might seem at first, because a different quadratic is allowed for the son's and father's generations, which are observed over different age ranges. Solving equations (7) and (8) for y_{0i} and y_{1i} and substituting the results into equation (1) yields

$$(9) \quad y_{1it} = (\alpha_1 - \rho\alpha_0) + \rho y_{0is} + \beta_1 A_{1it} + \gamma_1 A_{1it}^2 - \rho\beta_0 A_{0is} - \rho\gamma_0 A_{0is}^2 + \varepsilon_i + v_{1it} - \rho v_{0is}.$$

Equation (9) expresses son's observed status in year t as a regression function of father's observed status in year s and age controls for both father and son. If equation (9) is estimated by least squares, the resulting $\hat{\rho}$ is subject to an errors-in-variables bias because of the correlation between v_{0is} and y_{0is}. In fact, if, in addition to the assumptions in Section I, the age variables are assumed to be uncorrelated with long-run status and the v's, $\hat{\rho}$ continues to be downward inconsistent by a factor of $\sigma_y^2/(\sigma_y^2 + \sigma_{v0}^2)$. This inconsistency should be less severe with the PSID data than with more homogeneous samples but still could be quite substantial.

[15]One conceivable reason to suspect *upward* inconsistency is that a son whose economic success differs greatly from that of his father might be especially difficult to locate or unwilling to cooperate. In that case, the remaining sample would exhibit greater correlation between fathers and sons than would the population at large. It seems improbable, though, that this tendency is strong enough to outweigh the tendencies toward downward inconsistency.

[16]These models, which account for life-cycle stage with individual-invariant age coefficients, assume that different individuals do not have systematically different age profiles for earnings, wages, or income. This assumption is seemingly supported by John M. Abowd and David Card's (1989) analysis of short panels of first-differenced earnings data. Baker (1990), however, has found evidence of substantial heterogeneity in individual-specific earnings growth rates in a new analysis of earnings over a 20-year span. Baker's results also cast doubt on Abowd and Card's conclusion that the process for v_{it} is characterized by a unit root. A clearer understanding of earnings dynamics will require further research. If the process governing earnings dynamics were known, that knowledge could be exploited to achieve consistent estimation of the intergenerational correlation in long-run earnings. Such an approach has been attempted by David J. Zimmerman (1992), who assumes that v_{it} follows a first-order autoregressive process, and by Altonji and Thomas A. Dunn (1991), who assume at different points that v_{it} is white noise or that it follows a second-order moving-average process. Because considerable uncertainty still clouds the current understanding of earnings dynamics and because the data set used in the present study could not possibly resolve the issues, the present study settles for using inconsistent estimators and discussing the likely direction of inconsistency.

The analysis in Section IV pursues two strategies for treating the errors-in-variables bias. One approach is to average father's status in equation (8) over T years, so that equation (9) is modified to

$$(10)\quad y_{1it} = (\alpha_1 - \rho\alpha_0) + \rho\bar{y}_{0i} + \beta_1 A_{1it} + \gamma_1 A_{1it}^2 - \rho\beta_0\bar{A}_{0i} - \rho\gamma_0\overline{A_{0i}^2} + \varepsilon_i + v_{1it} - \rho\bar{v}_{0i}$$

where, for any variable z_{0is},

$$\bar{z}_{0i} = \sum_{j=s}^{s+T} z_{0ij}/T.$$

If equation (10) is estimated by least squares, the resulting $\hat{\rho}$ is still downward inconsistent, but the magnitude of the inconsistency is reduced, because the averaging across years decreases the variance of the "noise" relative to the "signal."[17]

The second approach is to apply instrumental-variable estimation to equation (9), with father's years of education as the instrument for father's single-year status. This is a somewhat odd context for instrumental-variable estimation, because father's education would not necessarily be excluded from a structural model for son's economic status. As will be discussed later, however, if certain plausible assumptions apply, the inconsistency of this instrumental-variable estimator of ρ is in an upward direction. If so, the probability limits of the two proposed estimators bracket the true value of ρ.

IV. Empirical Results

The first part of this section presents estimates of ρ based on ordinary least-squares (OLS) estimation of equations (9) and (10). The second part presents instrumental-variable (IV) estimates. The third part discusses the implications of the estimates for the degree of intergenerational income mobility in the United States.

A. *OLS Results*

Tables 2 and 3 display estimates of ρ from OLS estimation of equations (9) and (10), where y_{1it} is the natural logarithm of the son's annual earnings in 1984 and y_{0is} is the natural logarithm of the father's annual earnings in year s. Results are reported for each of $s = 1967, 1968, \ldots, 1971$. All earnings variables, as well as the wage and family income variables considered later, are in 1984 dollars as measured by the consumer price index. The results in Table 2 are based on different sample sizes (shown in brackets) because the number of missing observations varies with s. In particular, father's earnings might be missing in year s because of the father's attrition from the sample, because his earnings were not reported, or, in a few instances, because he had zero earnings.[18]

The estimates of ρ in the first column of Table 2 come from OLS estimation of equation (9), that is, from regressions involving single-year measures of father's log earnings. These estimates, which are expected to suffer from substantial errors-in-variables bias, range from 0.25 when father's 1971 log earnings are the regressor to 0.39 when his 1967 earnings are used. These estimates differ because of both the change in regressors and the change in sample composition. To hold the latter constant, equation (9) is reestimated for the 290 cases in which the father's earnings are available for all the years from 1967 to 1971. The resulting estimates of ρ, shown in the first column of Table 3, range from 0.28 for $s = 1971$ to 0.41 for $s = 1969$. Once sample composition is

[17] For example, if v_{0is} is white noise, then σ_{v0}^2 is replaced by σ_{v0}^2/T in the formula for the factor of inconsistency. If v_{0is} follows a stationary first-order moving-average process and θ denotes the first-order autocorrelation, then σ_{v0}^2 is replaced by $(\sigma_{v0}^2/T)\times[1+2\theta(T-1)/T]$. If v_{0is} follows a stationary first-order autoregressive process, then σ_{v0}^2 is replaced by $(\sigma_{v0}^2/T)\{1+2\theta[T-(1-\theta^T)/(1-\theta)]/[T(1-\theta)]\}$. See Solon (1984) for the derivations.

[18] In addition, for comparability with the later IV estimates, two observations are excluded because father's years of education were not reported. Retaining those observations has virtually no effect on the OLS results.

TABLE 2—OLS ESTIMATES OF ρ FROM LOG EARNINGS DATA

Year of father's log earnings	Measure of father's log earnings				
	Single-year measure	Two-year average	Three-year average	Four-year average	Five-year average
1967	0.386				
	(0.079)				
	[322]	0.425			
		(0.090)			
1968	0.271	[313]	0.408		
	(0.074)		(0.087)		
	[326]	0.365	[309]	0.413	
		(0.081)		(0.088)	
1969	0.326	[317]	0.369	[301]	0.413
	(0.073)		(0.083)		(0.093)
	[320]	0.342	[309]	0.357	[290]
		(0.078)		(0.088)	
1970	0.285	[312]	0.336	[298]	
	(0.073)		(0.084)		
	[318]	0.290	[301]		
		(0.082)			
1971	0.247	[303]			
	(0.073)				
	[307]				

Notes: Standard-error estimates are in parentheses, and sample sizes are in brackets.

TABLE 3—OLS ESTIMATES OF ρ FROM LOG EARNINGS DATA FOR "BALANCED" SAMPLE ($N = 290$)

Year of father's log earnings	Measure of father's log earnings				
	Single-year measure	Two-year average	Three-year average	Four-year average	Five-year average
1967	0.369				
	(0.094)				
		0.409			
1968	0.396	(0.093)	0.431		
	(0.087)		(0.093)		
		0.422		0.420	
1969	0.406	(0.088)	0.405	(0.094)	0.413
	(0.085)		(0.090)		(0.093)
		0.382		0.397	
1970	0.309	(0.089)	0.374	(0.090)	
	(0.087)		(0.088)		
		0.324			
1971	0.285	(0.086)			
	(0.078)				

Note: Standard-error estimates are in parentheses.

held constant, the estimates for 1967–1969 are fairly similar, but those for 1970–1971 are noticeably smaller. Part of the explanation, especially for 1971, seems to be that the increased variance in father's log annual earnings in recession years worsens the errors-in-variables bias.

To explore further the robustness of the results, several sets of variants of the Table 2 regression for $s = 1967$ are estimated. The first set involves exclusion of outlier observations. Reestimation excluding sons and fathers with annual earnings less than \$1,000 reduces the sample size to 311 and gives a $\hat{\rho}$

of 0.358 (with estimated standard error 0.064). Excluding fathers whose age in 1967 was less than 30 or greater than 59 leads to $\hat{\rho} = 0.412$ (SE = 0.085) with sample size 308. Imposing both restrictions simultaneously leads to $\hat{\rho} = 0.374$ (SE = 0.066) with sample size 298. A second experiment involves including multiple sons from the same family. Doing so expands the sample to 428 and produces $\hat{\rho} = 0.348$ (SE = 0.066).[19] In a third experiment, restricting the sample to non-Hispanic whites leads to $\hat{\rho} = 0.366$ (SE = 0.085) with sample size 298. In a fourth, adding control variables for major region of both fathers and sons leads to $\hat{\rho} = 0.356$ (SE = 0.083).[20]

Despite the variation in results, *all* the estimates are distinctly above 0.2, the value described by Behrman and Taubman (1985) as an upper bound on the intergenerational correlation in log earnings. Even though the present estimates are biased downward by the use of single-year measures of father's earnings, they apparently are less biased than previous estimates based on samples more homogeneous than the PSID. A simple exercise to illustrate the importance of the homogeneity issue is to imitate Sewell and Hauser's (1975) exclusion of sons who are not high school graduates. When the analyses reported in the first column of Table 2 are repeated with the sons samples restricted to those with at least 12 years of education, the estimated ρ for $s = 1967$ falls from 0.39 to 0.26 (SE = 0.08) with sample size 285. Similarly, $\hat{\rho}$ declines from 0.27 to 0.20 (SE = 0.07) for $s = 1968$, from 0.33 to 0.22 (SE = 0.08) for $s = 1969$, from 0.29 to 0.17 (SE = 0.08) for $s = 1970$, and from 0.25 to 0.18 (SE = 0.08) for $s = 1971$.

Next, to reduce the errors-in-variables bias, OLS is applied to equation (10), that is, to regressions in which father's log earnings are averaged over multiple years. The results are displayed in the remaining columns of Tables 2 and 3. For example, the entries in the second column of Table 2 indicate that $\hat{\rho}$ equals 0.425 when the regressor is father's log earnings averaged over 1967 and 1968, 0.365 when the average is over 1968 and 1969, and so forth. The entries in the third column indicate that $\hat{\rho}$ equals 0.408 when father's log earnings are averaged over the three years 1967–1969, 0.369 when the average is over 1968–1970, and so forth. Table 3 gives the corresponding results for the "balanced" sample of 290 cases in which father's earnings are available for all years. As expected, the general pattern in both tables is that $\hat{\rho}$ tends to get larger as father's log earnings are averaged over more years. Most of the estimates based on at least three years are in the neighborhood of 0.4 and are much larger than the estimates in previous studies. Furthermore, even these estimates presumably are subject to at least minor downward biases from both measurement error and attrition-induced homogeneity in the PSID.

To supplement the results on intergenerational earnings correlations, Table 4 presents results in which the economic status measures for fathers and sons are the logarithms of their hourly wage rates, their family incomes, and their family incomes relative to the official federal poverty threshold. The hourly wage is measured as the ratio of annual earnings to annual hours of work. Division of family income by the relevant poverty standard is a crude effort to adjust family income for family size and composition.

The first column of Table 4 reports OLS estimates of ρ based on single-year measures of father's status with $s = 1967$. The estimated ρ of 0.39 for log earnings is copied from Table 2, while ρ is estimated at 0.29 for the log wage and at 0.48 for both family income variables. That the smallest estimate appears for the hourly wage is not surprising given Duncan and Hill's (1985) finding that measurement error in both earnings and hours of work causes the ratio of the two to be especially noisy. Even though all

[19]The reported standard-error estimate ignores the correlation of error terms across sons from the same family. An analysis of residuals, however, estimates this correlation to be only 0.11.

[20]The results in Corcoran et al. (1992) show that the estimated coefficients of parental-income variables remain substantial even after controlling for a large set of family and community background variables besides race and location.

TABLE 4—OLS AND IV ESTIMATES OF ρ FOR VARIOUS SINGLE-YEAR INCOME MEASURES IN 1967

Income measure	OLS	IV	Sample size
Log earnings	0.386 (0.079)	0.526 (0.135)	322
Log wage	0.294 (0.052)	0.449 (0.095)	316
Log family income	0.483 (0.069)	0.530 (0.123)	313
Log (family income/poverty line)	0.476 (0.060)	0.563 (0.103)	313

Note: Standard-error estimates are in parentheses.

these OLS estimates are biased downward by their reliance on single-year measures, they are strikingly large relative to previous studies' estimates of intergenerational correlations.

Although this study focuses mainly on father–son correlations, it is reasonable to ask how the results would be affected by inclusion of sons from mother-headed families. Doing so expands the sample size for the family income analyses from 313 to 340 and decreases $\hat{\rho}$ from 0.48 to 0.44 (SE = 0.06) for both family income variables. Again, despite the errors-in-variables and attrition biases, these estimates are dramatically larger than those from previous studies.

B. *IV Results*

An alternative strategy for treating the errors-in-variables problem is to apply IV estimation to equation (9) with father's years of education as the instrument for y_{0is}. Because the PSID's 1968 information on education is in interval form, the instrument actually used is set at the midpoint of the reported interval except that fathers in the highest education category are assigned 18 years of education. Although this procedure inescapably produces measurement error in father's years of education, as long as the measurement error is uncorrelated with the error term in equation (9), the IV estimator remains consistent.

A more subtle issue is whether the father's education can be a valid instrument when it might belong as a regressor in a structural model for son's income status. As detailed in the Appendix, this problem may cause inconsistency in the IV estimator, but under plausible assumptions, the inconsistency is in an upward direction. If so, the probability limits of the OLS and IV estimators bracket the true ρ. If not, even the IV estimator may tend to underestimate ρ.

The second column of Table 4 presents IV estimates of ρ for $s = 1967$. As expected, the IV estimates are larger than the OLS estimates, ranging from 0.45 for the log of the wage to 0.56 for the log of family income relative to the poverty line. While these estimates may be upward-biased, in combination with the downward-biased OLS estimates, they strongly suggest that the intergenerational income correlation in the United States is around 0.4, or possibly higher.

C. *Implications for Intergenerational Mobility*

Contrary to previous studies' conclusion that the intergenerational income correlation in the United States is less than 0.2, this study's results suggest that the correlation is at least 0.4, and the family-income results cluster around 0.5. What do these different correlation estimates imply about the extent of intergenerational income mobility in the United States? One approach for obtaining suggestive results is to assume that long-run status (e.g., the permanent component of log earnings) is normally distributed in each generation and then to calculate the probability that a son's long-run status lies in various intervals of the population distribution as a function of the percentile of his father's status. The results of that exercise suggest that a ρ of 0.4 or 0.5 implies a very different degree of intergenerational mobility than a ρ of 0.2. For example, if $\rho = 0.2$, a son whose father's status is at the fifth percentile has a 0.30 chance of remaining in the bottom quintile, a 0.37 chance of rising above the median, and a 0.12 chance of reaching the top quintile. However, if $\rho = 0.4$, he has a 0.42 chance of remaining in the bottom quintile, only a 0.24 chance of rising above the median, and only a 0.05 chance of reaching the top quintile. Further, if $\rho = 0.5$, he has a 0.49 chance of remaining in the bottom quintile, only a 0.17 chance of rising above the median, and a mere 0.03 chance of reaching the top quintile.[21] Evidently, unless the assumption of bivariate normality has produced gross distortions, the higher intergenerational correlations estimated in this study imply a dramatically less mobile society.

One drawback of the bivariate-normality assumption is that it imposes linearity on the relationship between fathers' and sons' incomes. This overlooks the possibility that the strength of intergenerational transmission may be greater at one end of the income distribution than at the other. For example, in an analysis of English data, Atkinson et al. (1983 p. 114) found, "The proportion of upwardly mobile sons from the bottom 20 percent appears to be considerably higher and the proportion of downwardly mobile sons from the top 20 percent appears to be lower" than would be consistent with bivariate normality, although they emphasized that small sample size rendered this a tentative conclusion. A straightforward way of probing this issue in the present study is to generalize the Table 2 regression for $s = 1967$ by adding the square of father's log earnings as another explanatory variable. This results in coefficient estimates of -0.108 (with estimated standard error 0.786) for father's log earnings and 0.0258 (SE = 0.0408) for its square. The implied elasticity of son's earnings with respect to father's earnings is 0.41 if the father's log earnings are at the sample mean, 0.34 if the father's log earnings are two standard deviations below the mean, and 0.48 if they are two standard deviations above the mean. Like the results of Atkinson et al., these results suggest that "riches to rags" may occur less frequently than "rags to riches," but small sample size prevents precise estimation of the quadratic, and the t ratio for the estimated coefficient of the squared term is quite insignificant.

The sample size can be increased, at the risk of producing an unrepresentative sample of v_{0is}, by adding in the SEO sample. Reestimating the quadratic regression for the combined sample, while minimizing the unrepresentativeness problem by weighting each observation by the son's inverse probability of selection into the sample, leads to coefficient estimates of -0.402 (SE = 0.336) for father's log earnings and 0.0374 (SE = 0.0191) for its square. Because the weighting produces heteroscedasticity, the standard errors are estimated by Halbert White's (1980) method. Evaluated at the same values of father's log earnings that implied elasticities of 0.41, 0.34, and 0.48 with the SRC estimates, the results for the combined SRC and SEO data imply respective elasticities of 0.35, 0.25, and 0.46. Again the results suggest a greater prevalence of "rags to riches" than "riches to rags," and, as indicated by the larger t ratio for the estimated coefficient of the squared term, the evidence for nonlinearity in the relationship between son's and father's log earnings has become somewhat stronger.

V. Summary and Discussion

Measurement error and homogeneous samples have caused some previous studies to exaggerate the extent of intergenerational income mobility in the United States. This paper's analysis, based on intergenerational data from the Panel Study of Income Dynamics, is designed to be less susceptible to the biases in earlier studies. The results, which indicate that the intergenerational in-

[21] For a much wider set of results, see table 5 in Solon (1989b).

come correlation in the United States is at least 0.4 and possibly higher, portray a much less mobile society than has been described in earlier research.

One obvious limitation of the present study is its reliance on a single data set. Further research with other data would be very worthwhile. Indeed, new research by Altonji and Dunn (1991) and Zimmerman (1992) has used intergenerational data from the National Longitudinal Surveys of labor-market experience to produce results similar to those reported here. Another limitation of the present study is that most of its analyses characterize the association between fathers' and sons' incomes in terms of a linear relationship. A more thorough investigation of nonlinearities than has been attempted here will require larger sample sizes than have been available thus far.

Finally, all of this study's estimates have been based on the simplifying assumption that the variance in long-run status is the same in both generations ($\sigma_{y0}^2 = \sigma_{y1}^2$). The numerous studies that have found increasing inequality in annual earnings over recent years (e.g., Martin D. Dooley and Peter Gottschalk, 1984; W. Norton Grubb and Robert H. Wilson, 1989) call this assumption into question, though whether inequality in *long-run* status has grown remains unclear. However, even if the variance in long-run status grew by as much as 20 percent from the fathers' generation to the sons' generation ($\sigma_{y1}^2/\sigma_{y0}^2 = 1.2$), the estimates in this study would need to be divided through by only $\sqrt{1.2}$ (see footnote 5). Thus, for example, an estimated intergenerational income correlation of 0.40 would be revised to 0.37. Clearly, even extreme adjustments for intergenerational change in inequality would leave intact this study's main finding that intergenerational income mobility in the United States is much weaker than some previous estimates have suggested.

APPENDIX

Suppose that son's long-run income status y_{1i} is determined by

$$\text{(A1)} \qquad y_{1i} = \beta_1 y_{0i} + \beta_2 E_i + u_i$$

where y_{0i} is the father's long-run income status, E_i is the father's level of education (in years), and all variables are expressed as deviations from means. Equation (A1) differs from equation (1) in Section I in that it distinguishes separate effects of father's income and education. The object of this paper, however, is not to estimate β_1 and β_2, but to estimate ρ, the projection of y_{1i} on y_{0i} alone. The relationship between ρ and β_1 and β_2 follows the familiar omitted-variable formula:

$$\text{(A2)} \qquad \rho = \beta_1 + \beta_2 \operatorname{Cov}(E_i, y_{0i})/\sigma_y^2$$
$$= \beta_1 + \beta_2 \lambda \sigma_E/\sigma_y$$

where λ is the correlation between E_i and y_{0i} and σ_E^2 is the variance of E_i.

The difficulty for consistent estimation of ρ is that neither y_{1i} nor y_{0i} is directly observed. Instead, they are proxied by the short-run measures $y_{1it} = y_{1i} + v_{1it}$ and $y_{0is} = y_{0i} + v_{0is}$. Under the assumptions described in Section I, if OLS is applied to the regression of y_{1it} on y_{0is}, the probability limit of the estimated coefficient is

$$\text{(A3)} \qquad \operatorname{plim} \hat{\rho}_{\text{OLS}} = \left[\sigma_y^2/\left(\sigma_y^2 + \sigma_{v0}^2\right)\right]\rho$$

so that $\hat{\rho}_{\text{OLS}}$ is downward-inconsistent.

An alternative strategy is to estimate the regression of y_{1it} on y_{01s} by IV with father's education E_i as the instrument. Assuming that E_i is uncorrelated with v_{1it} and v_{0is}, the probability limit of the IV estimator is

$$\text{(A4)} \qquad \operatorname{plim} \hat{\rho}_{\text{IV}} = \operatorname{Cov}(E_i, y_{1it})/\operatorname{Cov}(E_i, y_{0is})$$
$$= \operatorname{Cov}(E_i, \beta_1 y_{0is} + \beta_2 E_i + u_i + v_{1it} - \beta_1 v_{0is})/\operatorname{Cov}(E_i, y_{0is})$$
$$= \beta_1 + \beta_2 \sigma_E^2/(\lambda \sigma_E \sigma_y)$$
$$= \beta_1 + \beta_2 \sigma_E/(\lambda \sigma_y)$$
$$= (\beta_1 + \beta_2 \lambda \sigma_E/\sigma_y) + \beta_2[(\sigma_E/\lambda\sigma_y) - (\lambda\sigma_E/\sigma_y)]$$
$$= \rho + \beta_2 \sigma_E (1 - \lambda^2)/(\lambda \sigma_y).$$

Therefore, $\hat{\rho}_{\mathrm{IV}}$ consistently estimates ρ only if $\beta_2 = 0$ (father's education does not influence son's status) or $|\lambda| = 1$ (father's education and income are perfectly correlated). Assuming that $0 < \lambda < 1$ (father's education and income are positively but imperfectly correlated), $\hat{\rho}_{\mathrm{IV}}$ is upward-inconsistent, consistent, or downward-inconsistent as $\beta_2 \gtreqless 0$. The possibility that $\beta_2 \simeq 0$ is not out of the question: Sewell and Hauser (1975) and Corcoran et al. (1992) have found that, once parental-income variables averaged over several years are controlled for, the estimated effect of parental education on son's earnings is indistinguishable from zero. However, if β_2 is nonzero, the more plausible case seems to be that β_2 is positive (e.g., the son of a highly educated clergyman with a moderate income tends to earn somewhat more than the son of a less-educated moderate-income father). If so, $\hat{\rho}_{\mathrm{IV}}$ is upward-inconsistent, and the probability limits of $\hat{\rho}_{\mathrm{OLS}}$ and $\hat{\rho}_{\mathrm{IV}}$ bracket the true ρ. If instead $\beta_2 < 0$, then both estimators are downward-inconsistent. It may be worth noting that these results are unaffected if the education instrument actually used is not the true E_i, but a proxy subject to classical measurement error.

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