

## **Trends in Intergenerational Income Mobility**

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### Abstract

Previous studies of recent U.S. trends in intergenerational income mobility have produced widely varying results, partly because of large sampling errors. By making more efficient use of the available information in the Panel Study of Income Dynamics, we generate more reliable estimates of the recent time-series variation in intergenerational mobility. Our results, which pertain to the cohorts born between 1952 and 1975, do not reveal major changes in intergenerational mobility.

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# Trends in Intergenerational Income Mobility

## I. Introduction

Over the last two decades, a large body of new research has documented that the intergenerational transmission of economic status in the United States is much stronger than had been suggested by earlier sociological and economic analyses. More recently, several researchers have begun to investigate trends in intergenerational mobility. Researchers, public policy analysts, and journalists have been particularly interested in whether the large rise in income inequality that has occurred over the last thirty years has been accompanied by a decline in intergenerational mobility.<sup>1</sup>

Previous research on intergenerational mobility trends has produced wildly divergent estimates. This confusing array of evidence is partly an artifact of imprecise estimation, which in turn has stemmed from inefficient use of the available data. For example, Mayer and Lopoo (2005) use the Panel Study of Income Dynamics (PSID) to estimate the intergenerational income elasticity (i.e., the elasticity of offspring's adult income with respect to their parents' income) for each cohort of sons born between 1949 and 1965. For each cohort, the elasticity is estimated in the year that the cohort is 30 years old. Thus, the elasticity estimate for the year 1990 is based only on the cohort born in 1960 and ignores the 1990 data for all other cohorts. Looked at another way, the elasticity estimate for the 1960 cohort uses the cohort's earnings in only 1990 and ignores the 1960 cohort's earnings in all other years. The resulting point estimates show a large

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<sup>1</sup> See Solon (1999) for a survey of the intergenerational mobility literature and Chadwick and Solon (2002) and Mazumder (2005) for more recent studies. Popular interest in mobility trends is typified by Bernstein (2003), Wooldridge (2005), Wessel (2005), Scott and Leonhardt (2005), and Pew Charitable Trusts (2008).

downward trend in the intergenerational elasticity, but they are so imprecise that the estimated trend is statistically insignificant.<sup>2</sup>

Another example is the study by Levine and Mazumder (2002), which uses the National Longitudinal Surveys (NLS) and the General Social Survey (GSS) as well as the PSID. With the NLS and PSID, Levine and Mazumder estimate intergenerational elasticities for sons in the 28-36 age range in 1980 and in one year in the early 1990's, thus ignoring sons' earnings data in all other years. With the GSS, they pool the 1976-1984 data as the first period and the 1989-1994 data as the second. Levine and Mazumder's PSID results, like those of Mayer and Lopoo, show a large but statistically insignificant decline (from 0.45 to 0.29) in the estimated intergenerational elasticity. In contrast, their NLS estimates show a statistically significant increase from 0.22 to 0.41. This result, however, may be distorted by the differential reporting of parental income between the two cohorts. Levine and Mazumder use self-reported parental income for the later cohort and sons' reports of their parents' income for the earlier cohort. The GSS estimates also indicate an increase in the intergenerational elasticity, but the estimated increase is not statistically significant.

A common feature of these and other studies<sup>3</sup> is sparseness of data. Some studies focus on only a few years, in some instances only two. Some studies span more years,

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<sup>2</sup> The estimates imply that the elasticity declined from 0.41 for the 1949 cohort to 0.28 for the 1965 cohort. After estimating the time series of intergenerational elasticities for the 1949-1965 cohorts, Mayer and Lopoo reestimate the trend for only the 1953-1965 cohorts, for whom the negative trend estimate has an absolute *t*-statistic greater than 2. In a similar analysis for daughters, Mayer and Lopoo (2004) initially estimate an insignificantly *positive* trend, after which they restrict to the 1949-1960 cohorts to obtain a significantly positive estimate of the trend.

<sup>3</sup> See also Reville (1995), Corcoran (2001), Fertig (2003/2004), and Nam (2004). In research carried out concurrently with ours, Hertz (2007) also makes more complete use of the available PSID data. Despite numerous differences in sample selection criteria and model specification, he obtains results qualitatively similar to ours. Another new study, by Aaronson and Mazumder (forthcoming), uses the decennial censuses from 1940 through 2000 to estimate regressions of son's log earnings on the log of average

but use only one year's data per individual, discarding most of the available information on each individual. The resulting estimates are highly imprecise. Some of the studies estimate large increases in intergenerational mobility, some estimate large decreases, and most of the estimated changes are statistically insignificant.

Our study reanalyzes the PSID in a way that uses all available years for each sampled son and daughter. By making more efficient use of the available data, we are able to obtain more reliable estimates of changes in intergenerational mobility over time.

## II. New Evidence

### A. Data

The PSID, administered by the University of Michigan's Survey Research Center, is a longitudinal survey that began in 1968 with a national probability sample of almost 5,000 U.S. families. The PSID sample has been reinterviewed every year through 1997 and every other year since then. Each year's survey collects annual income data for the preceding calendar year. The PSID has been a great resource for intergenerational research because it has followed children from the original sample as they have grown into adulthood and formed their own households. It therefore is possible to relate the children's income status as adults to the status of their parents as reported by the parents themselves since the outset of the survey.

Our analysis pertains to sons and daughters born between 1952 and 1975.

Children born before 1952, who were older than 16 at the 1968 interview, are excluded to

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income of the parents' generation in the son's state of birth. As the authors acknowledge, the slope coefficient in this state-level regression is the same as the coefficient of the son's own parents' income only on the very strong assumption that the predictive power of state-of-birth income stems solely from its association with the income of the son's own parents.

avoid overrepresenting children who left home at late ages. Because we need the sons' and daughters' adult income observations to be at least somewhat informative about their longer-run income, we use income observed no earlier than age 25. Consequently, the first year in which we observe children's income is 1977 (as reported in the 1978 interview), the year when the 1952 cohort reached age 25. In 1978, we use income data on the 1952 cohort (then age 26) and the 1953 cohort (age 25). By 2000, the last year for which we have children's income data (as reported in the 2001 interview), the children's ages range from 25 for the 1975 cohort to 48 for the 1952 cohort.

Thus, our data on children's income in each year are observed over a range of ages, and the range changes over time. This raises the question of how to control for the children's age variation. Mayer and Lopoo (2004, 2005) do so by using observations at only age 30, but that approach throws out most of the available data. Our alternative approach is to retain the available data, but to include detailed age controls in our econometric specification, which is presented in the next subsection.

Like Mayer and Lopoo, we measure the children's adult income by the family income in the household in which they have become the head or head's spouse. In addition to facilitating comparisons with Mayer and Lopoo's results, our use of family income (instead of, say, individual labor earnings) provides a broader measure of economic status, which is especially important in a study including daughters. This point is discussed in detail by Chadwick and Solon (2002). We exclude income observations imputed by "major assignments," and we also exclude outlier observations for which income (in 1967 dollars as measured by the CPI-U) is less than \$100 or more than \$150,000. We use only the Survey Research Center component of the PSID. We

exclude the Survey of Economic Opportunity component (the so-called “poverty sample”) because the serious irregularities in that sample’s selection make it impossible to ascertain what population the sample represents.<sup>4</sup>

## B. Econometric specification

Most of the empirical research on intergenerational income mobility attempts to measure the intergenerational elasticity in long-run income, i.e., the slope coefficient in the regression of child’s log long-run income on parents’ log long-run income.<sup>5</sup> In practice, however, no intergenerational data set contains lifetime income data on both generations, so researchers use short-run income measures to proxy for long-run income, and they control for the life-cycle stage of both children and parents at the times their incomes are observed.

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<sup>4</sup> See Brown (1996) for a summary of the numerous problems with that portion of the sample. The problem we find most disturbing is that, for reasons that remain unknown to this day, the computer consulting firm in Washington, DC that the Office of Economic Opportunity hired to select low-income households from the Census Bureau’s 1967 Survey of Economic Opportunity sample failed to include most of the eligible households in the lists it transmitted to the Survey Research Center. Worse yet, the omissions clearly were not random. Brown’s memo notes a racial pattern – the transmission rate was 55 percent for non-whites and 21 percent for whites. A passage he quotes from the Survey Research Center’s 1984 PSID *User Guide* also refers to “substantial” variation across geographic areas. That passage concludes, “By the time we realized that not all the addresses of the ‘signers’ had been forwarded, the Census personnel knowledgeable about the process had moved on to designing the 1970 Census, and OEO personnel were not able to provide us any information. Our repeated efforts to secure more information about the lost cases were not successful.”

<sup>5</sup> This elasticity provides an answer to questions like, if the parents’ long-run income is 50 percent above the average in their generation, what percentage above the average should we predict the son’s or daughter’s long-run income will be in the offspring’s generation? If the variances in logarithmic long-run income were the same in both generations, the intergenerational elasticity also would be the intergenerational correlation in log long-run income. More generally, the elasticity equals the correlation times the ratio of standard deviations between the offspring and parental generations. Both the elasticity and the correlation are of interest, but the elasticity has the virtue of expressing how *consequential* parental income is for offspring income. For example, if the intergenerational correlation stayed the same but income inequality were greater in the offspring’s generation, the intergenerational elasticity would increase, reflecting that percentage variation in parental income had come to be associated with greater percentage variation in offspring income.

Like many previous studies, ours uses a multi-year measure of parental income. In particular, we average log annual family income over the three years when the child was 15-17 years old. For example, for sons and daughters in the 1952 cohort, the parental income variable is for the years 1967-1969 (as reported in the 1968-1970 interviews).<sup>6</sup> We apply the same sample restrictions for parental income as described at the end of the previous subsection for children's income. Also, to account for the differences among the sample parents in how old they were when their children were 15-17, we control for a quartic in the average age of the household head over those years. As discussed by Solon (1989, 1992), Mazumder (2005), and Haider and Solon (2006) among others, using a short-run measure of parental income to proxy for the key regressor induces a downward errors-in-variables bias in the estimation of the intergenerational elasticity in long-run income. As long as this bias is approximately stable over time, however, it will not seriously distort our estimation of changes in intergenerational mobility.<sup>7</sup>

The dependent variable in the intergenerational regression equations we estimate is the son's or daughter's log annual family income in years ranging from 1977 to 2000 and at ages ranging from 25 to 48. As a proxy for the child's log long-run income, this variable also is subject to measurement error. Based on the classical errors-in-variables model discussed in almost every introductory econometrics textbook, researchers often assume that "left-side" measurement error causes no bias in least squares estimation of

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<sup>6</sup> Our choice of the 15-17 age range is dictated by data availability. For the 1952 cohort, we cannot observe parental income any earlier.

<sup>7</sup> Despite the rise in income inequality over our sample period, the noise-to-signal ratio in log current income as a proxy for log long-run income might have stayed about the same if the variances of the persistent and transitory components of log income increased in similar proportion. According to the estimates in Gottschalk and Moffitt (1994) and Haider (2001), that was indeed the case.

regression models. Grawe (2006) and Haider and Solon (2006), however, explain why the textbook model does not apply in the present context. Because of systematic heterogeneity across individuals in their rates of income growth over the life cycle, the measurement error in log current income as a proxy for log lifetime income is mean-reverting early in the life cycle and mean-departing later on. The source of this pattern is that individuals with high lifetime income tend to have steeper income growth trajectories. As a result, the current income gap early in the life cycle between those who will eventually have high lifetime income and those who will end up with low lifetime income understates the lifetime gap, and the current income gap later in the life cycle overstates the lifetime gap. This is why Reville (1995) and many other researchers have found that they estimate lower intergenerational elasticities when the sons' incomes are measured early in the life cycle. To account for this pattern in a sample in which we measure children's income over ages ranging from 25 to 48, we must control not only for the child's age, but also for the interaction of child's age with parental income.

Thus, for sons and daughters separately, we apply ordinary least squares to the regression model

$$\begin{aligned}
 (1) \quad y_{ict} = & \alpha'D_t + \beta_t X_{ic} + \gamma_1 A_{ic} + \gamma_2 A_{ic}^2 + \gamma_3 A_{ic}^3 + \gamma_4 A_{ic}^4 + \delta_1(t-c-40) \\
 & + \delta_2(t-c-40)^2 + \delta_3(t-c-40)^3 + \delta_4(t-c-40)^4 + \theta_1 X_{ic}(t-c-40) \\
 & + \theta_2 X_{ic}(t-c-40)^2 + \theta_3 X_{ic}(t-c-40)^3 + \theta_4 X_{ic}(t-c-40)^4 + \varepsilon_{ict}
 \end{aligned}$$

where  $y_{ict}$  is log family income in year  $t$  for child  $i$  in birth cohort  $c=1952, 1953, \dots, 1975$ . With the PSID interviews switching to every other year near the end of our sample

period, the vector  $D_t$  contains year dummy variables for each  $t=1977, 1978, \dots, 1996, 1998, 2000$ . The key explanatory variable  $X_{ic}$  is the parental log income variable described above. Because our main purpose is to study change over time in intergenerational mobility, the coefficient  $\beta_t$  varies with year. As discussed above, the regression includes controls for a quartic in parental age  $A_{ic}$  at the time parental income is observed, a quartic in child's age  $t - c - 40$  at the time child's income is observed, and interactions of the child's age quartic with parental income. The child's age variable  $t - c - 40$  is normalized to equal zero at age 40. This simplifies interpretation of the  $\beta_t$  vector, which expresses how the intergenerational elasticity at child's age 40 evolves as successive cohorts pass through that age.<sup>8</sup> Later, when we plot how our estimates of  $\beta_t$  change over time, the normalization to age 40 will affect only the height of the plotted estimates, not the variation over time.

It is well known that time, age, and cohort effects are not separately defined without imposition of strong restrictions (see Rodgers, 1982). For example, when observing intergenerational income elasticities when the offspring are age 30, the comparison between the 1950 and 1951 birth cohorts is necessarily the same thing as the comparison between the years 1980 and 1981. In this paper, we make the arbitrary choice to label trends in terms of years, recognizing that, since we are controlling for age, the same trends might just as well be labeled in terms of cohorts. In a similar research project carried out concurrently with ours, Hertz (2007) makes the opposite arbitrary choice to label intergenerational mobility trends in terms of cohorts. In either case, the

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<sup>8</sup> The results in Haider and Solon (2006) and Bohlmark and Lindquist (2006) suggest that the errors-in-variables bias from non-classical "left-side" measurement error in child's log current income as a proxy for log long-run income is inconsequential if current income is measured at around age 40.

estimated trends should be interpreted as reflecting a combination of time and cohort trends. Not surprisingly, Hertz's conclusions are similar to ours.<sup>9</sup>

Our main specification shown as equation (1) assumes that the income/age trajectories of different cohorts have the same shape. The inclusion of year dummy variables allows the height of the trajectories to vary across cohorts (or, equivalently, across years), but otherwise the coefficients of all variables involving child's age are assumed to be common across cohorts.<sup>10</sup> Later, we will discuss the robustness of our results to relaxing that restriction.

Our estimation of equation (1) is based on unbalanced panel data in which we use repeated observations of the same individuals. Our sons sample contains 11,230 observations of 1,228 individuals, and our daughters sample contains 12,666 observations of 1,308 individuals. Thus, our samples average almost 10 observations per individual and about 50 individuals for each of the 24 cohorts born between 1952 and 1975.<sup>11</sup> The latter highlights why studies using only one cohort per year (or, equivalently, one year per cohort) obtain very imprecise estimates.

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<sup>9</sup>Although the choice between labeling trends in terms of time versus cohorts is conceptually arbitrary, the results are not numerically identical. For example, our analysis pertains to 24 birth-year cohorts born between 1952 and 1975, but, because the PSID was not administered in the years 1998 and 2000, the 24-year time span of our analysis pertains to only 22 years. If Hertz's analysis and ours had produced surprisingly different results, such a discrepancy would require an investigation into why seemingly minor specification differences lead to qualitatively different conclusions. Hertz's conclusion (p. 23), however, is, "There has been no long-run linear trend in the IGE [intergenerational elasticity] over this period or, at least, no trend steep enough to be statistically significant given achievable limits of precision." Because we reach a similar conclusion, we view Hertz's study and ours as complementary paths to the same destination.

<sup>10</sup> Clearly, the previous studies that have restricted their observation of offspring's income to certain ages have similarly relied on an assumption of common life-cycle trajectories across cohorts.

<sup>11</sup> For sons, the average is 58 individuals per cohort born between 1952 and 1963, and the average is 44 for the 1964-1975 cohorts. The corresponding averages for daughters are 63 and 46. Clearly, like all intergenerational elasticity estimates based on the PSID, ours are vulnerable to bias from non-random attrition. As discussed in detail in Solon (1992, pp. 398-9), the bias for the intergenerational elasticity coefficient probably is towards zero. It is less clear how trend estimates are biased. Hertz (2007) adjusts for attrition bias with a correction suggested by Fitzgerald, Gottschalk, and Moffitt (1998) and finds that the correction makes little difference for his estimation of intergenerational elasticity trends.

Positive autocorrelation of the error term  $\varepsilon_{ict}$  over different years for the same individual complicates the proper estimation of our standard errors. Unlike previous authors who discarded all observations except one for each individual, we use all the available information, but apply standard methods of robust covariance matrix estimation to correct our standard error estimates. In particular, we use the robust covariance matrix estimator described on page 330 of Wooldridge (2002). Although Wooldridge's discussion on that page is motivated by the dependence among error terms for different individuals within the same geographic cluster, the same approach applies to the serial correlation among error terms in different time periods for the same individual. The robust covariance matrix estimator corrects not only for serial correlation, but for heteroskedasticity as well.

### C. Results

The second column of table 1 shows our estimates of the intergenerational income elasticity  $\beta_t$  for sons in each available year from 1977 through 2000. As explained above, although these estimates are based on multiple cohorts, they are normalized in terms of the elasticity for 40-year-olds in each year. As shown by the standard error estimates in parentheses, the elasticity estimates for the early years are relatively imprecise because they are based on small samples of sons still in their twenties. Over time, as more cohorts age into our sample, the elasticity estimates become more precise.<sup>12</sup>

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<sup>12</sup> Had we parameterized trends in terms of cohorts instead of years, the same pattern would have appeared as relatively imprecise estimates for later cohorts, which are observed for fewer years. Either way, the same phenomenon is similarly manifested once one contrasts the elasticity estimates either between earlier and later years or between earlier and later cohorts.

The simple average of the 22 year-specific estimates is 0.44, a fairly typical estimate relative to the relevant literature surveyed in Solon (1999). Every one of the year-specific estimates is within 0.10 of that average. The thick line connecting the circle-shaped data points in figure 1 plots the time series of the year-specific estimates. Visual inspection of the plot suggests no clear-cut trend. The last row in table 1 displays the Wald statistic (based on the robust covariance matrix estimate) for testing the null hypothesis that all 22 of the year-specific elasticities are the same. As one might guess from viewing figure 1, the test does not reject that hypothesis at conventional significance levels.

The third column of the table displays the corresponding estimates for daughters. The simple average of the year-specific estimates is 0.43. In the case of daughters, the relatively imprecise estimates for the early years are way below that average. Once more cohorts have joined the sample by 1983, all the subsequent estimates lie between 0.43 and 0.56. Visual inspection of the thick line connecting the circle-shaped data points in figure 2 suggests no clear-cut trend from 1983 on. The Wald statistic in the bottom row of table 2 rejects the null hypothesis of equal elasticities at the 0.05 significance level, mainly because of the lower estimates in the early years. We are reluctant, however, to conclude that the elasticity for daughters truly increased from 1977 to 1983. Not only are our estimates for that period based on small samples, but they also rely especially heavily on our method for extrapolating elasticities for daughters in their twenties to elasticities at other ages.<sup>13</sup>

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<sup>13</sup> As noted above, our main specification shown in equation (1) assumes that the shape of the income/age trajectory is the same for every cohort. We have checked the sensitivity of our results to that assumption by expanding the vector of explanatory variables to include interactions of the year dummies with a linear age term. Somewhat surprisingly, in the analyses for both sons and daughters, a Wald test of the null

Notwithstanding our concern about that extrapolation, we still believe that the efficiency payoff from using all available data is considerable. To highlight that payoff, in our lengthier working paper (Lee and Solon, 2006), we have imitated Mayer and Lopoo's (2004, 2005) practice of using observations only at age 30 and discarding the rest of the data. Here we summarize the results by including the resulting time series of estimated intergenerational elasticities as the thin lines connecting the triangle-shaped data points in figures 1 and 2. As these plots show, the estimates based on only 30-year-olds are wildly volatile, ranging from  $-0.39$  to  $0.75$  (in adjacent years!) for sons and from  $0.14$  to  $1.02$  for daughters. Our working paper also presents elasticity estimates based on only 29-year-olds or only 28-year-olds, and again the estimates are subject to large standard errors and are wildly volatile. Obviously, the large fluctuations are driven mainly by sampling error, not by actual changes in the population.

Compared to previous studies, our approach of using data for all available ages improves the precision of the estimation of intergenerational elasticities over time, but it does so at a cost: the additional complexity associated with our model's elaborate controls for the offspring's stage of life cycle makes it harder to understand what patterns in the data drive our results. To gain a more direct view of the basis for our results, for each overlapping five-year birth cohort (1952-1956, 1953-1957, ..., 1971-1975) we have estimated separate regressions of son's or daughter's log income in each available year from 1981 through 2000 on parental log income and parental age controls. Table 2 provides a partial summary of the results.

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hypothesis of zero coefficients for all the additional explanatory variables fails to reject that null hypothesis at any conventional significance level. More importantly, the inclusion of the additional explanatory variables produces very little change in our estimated time series of the intergenerational elasticities for either gender.

For example, the first entry in table 2, the 0.31 figure for sons aged 25-29 in 1981-1985, is the simple average of the five estimated elasticities for the 1952-1956 birth cohort in 1981, the 1953-1957 cohort in 1982, ..., the 1956-1960 cohort in 1985. Similarly, the 0.44 entry for ages 30-34 in 1986-1990 is the simple average of the five estimated elasticities for the 1952-1956 cohort in 1986 through the 1956-1960 cohort in 1990. The purpose of the averaging across five adjacent birth cohorts and five adjacent years is to average out some of the noise from sampling error.

The advantage of table 2 is that it enables a straightforward eyeballing of patterns across time periods, cohorts, and ages. Reading each panel in the table diagonally from northwest to southeast follows the evolution of a cohort's elasticity estimates over time (and over the life cycle). Reading a column vertically compares elasticity estimates of younger and older cohorts within a time period. Reading a row horizontally holds age constant and views the evolution of that age group's elasticity estimates as different cohorts pass through that age interval over time.

In accordance with the previous literature's finding that intergenerational income elasticity estimates come out lower when son's income is measured early in the life cycle, viewing the sons' panel in table 2 diagonally shows a clear tendency for cohorts' estimated elasticities to rise as they age from their twenties to thirties. Viewing the panel vertically shows the same life-cycle pattern for younger versus older cohorts within time period. These patterns are less clear-cut for daughters. In both panels, however, a horizontal reading of estimates across time holding age constant reveals no consistent trend. This is the source of our model-based results in table 1 and figures 1 and 2 that display no obvious time (or cohort) trend in intergenerational mobility.

### III. Summary and Discussion

Previous estimates of trends in intergenerational mobility have yielded widely divergent results, partly because of the imprecision resulting from sparse samples. By making more efficient use of the available data from the Panel Study of Income Dynamics, we are able to obtain more reliable evidence. Our estimates are still too imprecise to rule out modest trends in either direction. For the most part, though, our results for the cohorts born between 1952 and 1975 suggest that intergenerational income elasticities in the United States have not changed dramatically over the last two decades.<sup>14</sup>

This finding may seem surprising. As mentioned in our introduction, popular interest in intergenerational mobility trends has stemmed largely from a concern that mobility might have declined during the recent era of rising income inequality. Indeed, the theoretical analysis in Solon (2004) shows that, other things equal, a rise in earnings inequality associated with increased returns to human capital would bring with it an increased intergenerational earnings elasticity as well. But, although many economists agree that rising returns to human capital have played an important role in rising income inequality (Acemoglu, 2002), other things have not been equal. For example, Mayer and Lopoo (2004, 2005) note that, at the same time that rising returns to human capital have been a force towards stronger intergenerational transmission, the increased progressivity of public investment in children's education, health, and nutrition since the initiation of the War on Poverty has been a force in the opposite direction.<sup>15</sup> The theoretical results in

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<sup>14</sup> Given that income inequality has increased over this period, this finding on intergenerational elasticities suggests further that intergenerational income *correlations* may have decreased.

<sup>15</sup> Similarly, in his Ely Lecture to the American Economic Association, Welch (1999, p. 16) said, "... in the last three decades of increasing wage dispersion, if we had seen reduced intergenerational mobility and

Solon (2004) indicate that, when equalizing and disequalizing forces are simultaneously at work, they may indeed have differential net effects on cross-sectional inequality and intergenerational mobility.<sup>16</sup> In any case, the question of why intergenerational income associations are as high and as low as they are at different times and in different countries remains an important topic for further research.

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increased differentiation in economic status between Americans of African and European ancestry and between men and women, I suspect that most of us would not be sanguine about the accomplishments of the period. The evidence is not available regarding intergenerational mobility, but when it is, I believe we will find that it has increased.”

<sup>16</sup> For instance, inspection of equations (2.14) and (2.16) in Solon (2004) makes clear that, if an increase in the return-to-human-capital parameter  $p$  were offset by a proportionally equal decrease in  $1 - \gamma$  (reflecting more progressive public investment in children’s human capital), the steady-state intergenerational elasticity would stay the same, but steady-state cross-sectional inequality would increase.

Table 1. Estimated Intergenerational Income Elasticities by Year and Gender

Year	Estimates for Sons	Estimates for Daughters
1977	0.34 (0.20)	0.05 (0.17)
1978	0.54 (0.13)	0.19 (0.13)
1979	0.50 (0.15)	0.20 (0.12)
1980	0.48 (0.13)	0.27 (0.11)
1981	0.42 (0.14)	0.37 (0.11)
1982	0.52 (0.12)	0.37 (0.11)
1983	0.46 (0.11)	0.45 (0.11)
1984	0.39 (0.11)	0.49 (0.10)
1985	0.41 (0.12)	0.53 (0.10)
1986	0.47 (0.10)	0.49 (0.10)
1987	0.41 (0.12)	0.50 (0.09)
1988	0.38 (0.09)	0.54 (0.09)
1989	0.42 (0.09)	0.56 (0.08)
1990	0.36 (0.08)	0.52 (0.07)
1991	0.43 (0.08)	0.53 (0.07)
1992	0.45 (0.08)	0.49 (0.07)
1993	0.49 (0.08)	0.50 (0.07)
1994	0.43 (0.07)	0.48 (0.07)
1995	0.40 (0.07)	0.48 (0.06)
1996	0.43 (0.07)	0.43 (0.06)
1998	0.47 (0.06)	0.45 (0.05)
2000	0.49 (0.06)	0.46 (0.06)
Number of observations	11,230	12,666
Wald statistic for testing equality of elasticities across years	26.75 (p=0.18)	33.34 (p=0.04)

Note: The parenthesized numbers next to the elasticity estimates are standard error estimates. As explained in the text, the standard error estimates are robust to the serial correlation resulting from repeated observations of the same individuals.

Table 2. Estimated Intergenerational Income Elasticities by Gender and Age Group  
Averaged over Five-Year Periods

Gender and Age Range	1981-1985	1986-1990	1991-1995	1996, 1998, 2000
<b>Sons</b>				
25-29	0.31	0.20	0.35	0.32
30-34		0.44	0.38	0.45
35-39			0.48	0.45
40-44				0.40
<b>Daughters</b>				
25-29	0.40	0.52	0.40	0.36
30-34		0.47	0.48	0.36
35-39			0.46	0.51
40-44				0.43

Figure 1. Estimated Intergenerational Income Elasticities for Sons, 1977-2000

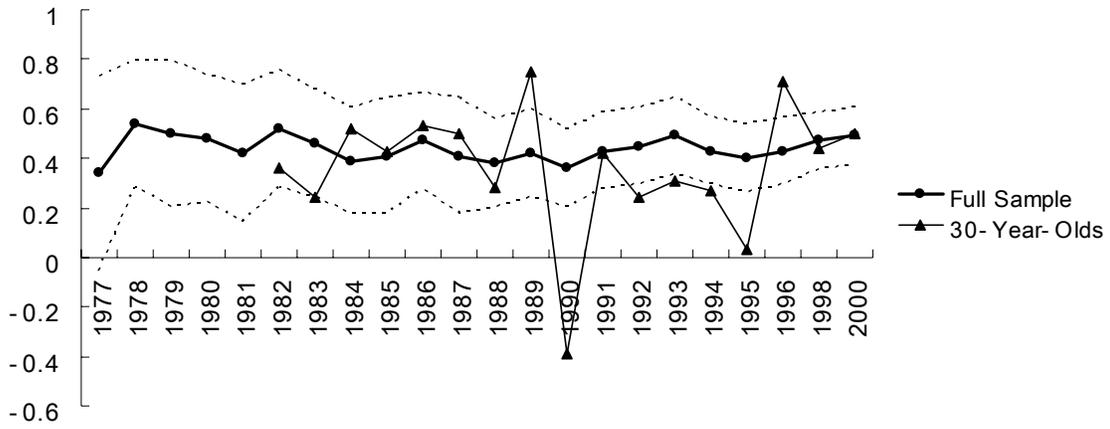
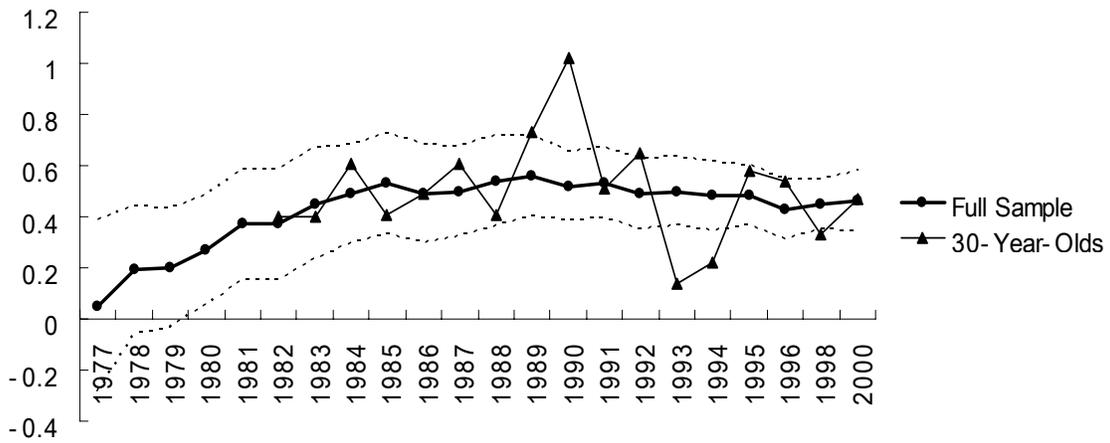


Figure 2. Estimated Intergenerational Income Elasticities for Daughters, 1977-2000



Note: The dotted lines show the 95% confidence bands for the estimated elasticities based on the full samples.

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