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The Growing Importance of Family and Community: An Analysis of Changes in the Sibling Correlation in Earnings

Bhashkar Mazumder and David I. Levine

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BHASHKAR MAZUMDER
Federal Reserve Bank of Chicago

DAVID I. LEVINE
Haas School of Business
University of California

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Abstract: This study presents evidence that the correlation in brothers' earnings has risen in recent decades. We use two distinct cohorts of young men from the National Longitudinal Surveys and estimate that the correlation in earnings between brothers rose from 0.26 to 0.45. This suggests that family and community influences shared by siblings have become increasingly important in determining economic outcomes. We find that neither the correlation in years of schooling nor the rising return to schooling accounts for this increase. We also argue that the PSID is not an appropriate dataset for analyzing changes over time because of its sampling design, small sample of siblings, and high attrition rate.

1. Introduction

An individual's economic success in the U.S. labor market is strongly related to his or her family background. Recent studies suggest that the intergenerational elasticity in earnings is at least 0.4 and may be as high as 0.6 (Bowles and Gintis, 2002). These values imply that for a family living in poverty, it will take their descendents somewhere between 3 to 6 generations, on average, before they can expect their earnings to be within 5 percent of the national average.[1] Such persistence of disadvantage contradicts the widely held belief that the U.S. is a highly mobile society. But has this lack of mobility *always* been a characteristic of the U.S. economy, or is it a more recent phenomenon?

Given the well-documented rise in cross-sectional income inequality among men in recent decades (e.g. Levy and Murnane, 1992), this would appear to be an important question. Researchers have proposed many different explanations for the growth in inequality (e.g. rising returns to skill, international trade, "winner-take-all" markets) but have ignored the possible contribution of family background as either a direct or indirect influence. For example, there might be important characteristics (e.g. knowledge, skills, looks, social contacts) that are transmitted by parents through either "nature" or "nurture" that are increasingly rewarded by the labor market. If the intergenerational persistence in inequality is rising, current inequities might persist for far longer than we might expect irrespective of their underlying causes.

The investigation of changes over time in intergenerational mobility is also important for potentially understanding what underlying forces might be driving the strong association in income across generations. So far, researchers have only been able to account for a modest amount of this high persistence using standard variables such as parent education and IQ scores (Bowles and Gintis, 2002). Exploiting changes over time might be a fruitful avenue to gain insight into the underlying process by which income is transmitted across generations. For example, if educational attainment is the primary

[1] This example assumes a family of four with two children at the poverty threshold of just under $18,000 in 2001 (Proctor and Dalaker, 2002, p.5. The mean income for all families in the U.S. in 2001 was about $67,000. See

factor underlying this association, then we might expect to observe a sizable increase in the intergenerational transmission of income when comparing cohorts that entered the labor market in the 1970s versus the 1980s -- a period when the return to education grew markedly.[2]

For the most part, the literature on intergenerational mobility has ignored any *historical* dimension to the transmission of economic outcomes. The few studies that have tried to examine changes in the intergenerational transmission of income in the U.S. have found differing results ranging from an increase in mobility (Mayer and Lopoo, 2001; Fertig, 2001), to no change in mobility (Hauser, 1998) to a decrease in mobility (Levine and Mazumder, 2002). We believe that this mixture of findings has been primarily due to data limitations combined with a focus on only one descriptive parameter, the intergenerational income elasticity.

This paper contributes to the literature in several important ways. First, we use variance decomposition models to examine changes over time in the correlation in earnings between brothers. A brother correlation answers the following question: what percent of the observed variance in earnings among men is due to factors that are common to growing up in the same family? This correlation provides a broader measure of the overall importance of a wide variety of factors common to the family ranging from parental involvement to school and neighborhood quality – not just family income. Perhaps more importantly, the brother correlation avoids many of the data problems that are inherent in trying to create a large sample of families with accurate measures of income in *two* generations.

A second contribution of this study is that we use data that has not been previously used to measure the brother correlation in earnings in two different time periods. Specifically we use two different cohorts of men from the National Longitudinal Surveys (NLS). Unlike the Panel Study of Income Dynamics (PSID), the NLS datasets allow us to construct large samples of siblings that are nationally representative in *each* time period and are less susceptible to sample attrition.

Table FINC-01, Selected Characteristics of Families, by Total Money Income in 2001 at Census Bureau website, census.gov.

[2] That is, if the relationship between family income and children's educational attainment stayed roughly constant while the returns to education rose dramatically, the the intergenerational correlation of income will rise as well.

Using this approach we find that the brother correlation has increased sharply in the U.S. for cohorts of young men born between 1957 and 1965 compared to those born between 1944 and 1952. The correlation coefficient has risen from 0.26 to 0.45 and the change is statistically significant. The results are robust to a variety of sample selection rules. These results are striking: For the more recent cohorts, close to half of the variance in earnings can be explained by family and community influences!

The rising brother correlation offers strong evidence that family background and community influences have played an increasingly important role in determining young men's earnings in recent decades. However, we find no change in the correlation in years of schooling between these cohorts suggesting that this rise is not explained by a greater association between family influences and educational attainment. We also find that when we estimate the brother correlation in residual earnings that are purged of the effects of years of schooling, it explains little of the increase in the brother correlation. This suggests that the rising *return to education* also does not account for the rise in the brother correlation in earnings. However, it may still be the case that a rising return to other unobserved skills or other characteristics, which we cannot measure with our data, may still play an important role.

These results are bolstered by the findings of Levine and Mazumder (2002) who also use the NLS cohorts to investigate changes in the intergenerational elasticity in income. They find that the intergenerational elasticity exhibited a statistically significant rise from 0.22 for the cohorts in the early period to 0.41 in the later period.

In order to maintain comparability with the previous literature, we also attempt to investigate changes in the brother correlation for the same cohorts using the PSID. The results here are inconclusive and are not robust to changes in sample selection rules. In any case, we do not believe that the PSID is ideally suited for this analysis for two major reasons. First, the survey has experienced substantial attrition over the relevant time period and requires using the *same* initial pool of families to study two different time periods. The sample used in the later period is necessarily a selected sample of families that remain in the survey more than a decade later —which likely leads to biased results due to sample attrition. Second, the sample sizes for the analysis cohorts in the PSID are also too small to produce reliable or precise results. In particular, with the PSID it is unfeasible to estimate a model using *only* data on siblings, which would be the ideal analysis sample to use.

Given these findings, we believe that there is reasonably strong evidence that intergenerational mobility has declined in recent decades. These results are consistent with the hypothesis that rising returns to various forms of skills may have resulted in a stronger intergenerational association in earnings in recent decades, though we do not find evidence of this with respect to years of schooling. At a minimum, we believe that studies that purport to show an increase in intergenerational income mobility using only one dataset should be interpreted with caution.

2. Background and Literature Review

Previous Studies on Changes in the Intergenerational Elasticity

In recent years a large and growing literature uses the coefficient from a regression of the log income or earnings of sons or daughters on the log income of their parents as a summary measure of intergenerational mobility. The intergenerational elasticity is useful for answering questions such as: "What percent of the difference in earnings between two families is expected to persist into the next generation?" Recent studies suggest that the intergenerational elasticity is at least 0.4 and may be as high as 0.6 (Mazumder, 2003). These results indicate a surprisingly high degree of persistence in income inequality from generation to generation in the U.S. As a point of comparison, recent studies have put the analogous figure at only about 0.2 in Canada and Finland, and 0.3 in Germany.[3]

Only a few studies have attempted to examine *changes* in the intergenerational elasticity in the U.S. over time to see whether this degree of immobility has characterized the U.S. over a long period of time. A few researchers using the PSID (e.g. Fertig, 2001; Mayer and Lopoo, 2001) have found suggestive evidence of a decline in the intergenerational elasticity, but the results are not always consistent over all groups examined or over all time spans. Hauser (1998) uses the GSS from 1972 and

1996, and finds no change in the correlation between fathers' economic status and sons' economic status.[4] Levine and Mazumder (2002) use the NLS cohorts, the PSID and the GSS and find evidence of a statistically significant decline in the intergenerational elasticity when using the NLS but inconclusive evidence in the other surveys.

We believe that this finding of ambiguous results is due to the lack of appropriate data to conclusively examine the change in the intergenerational elasticity. The PSID has relatively small samples for intergenerational analysis and has experienced considerable attrition since the panel began in 1968. This results in small samples when trying to examine more recent cohorts using the PSID. For example, Levine and Mazumder (2002) and Lopoo and Mayer (2001) have only about 300 families when examining recent cohorts. The attrition has also forced researchers to use a selected sample of families that have not attrited in order to measure the intergenerational elasticity for more recent cohorts. These families tend to have higher income and are less likely to be borrowing constrained. Several studies have presented empirical evidence (Mazumder, 2001; Gaviria, 1998) supporting the theoretical prediction of Becker and Tomes (1986) that families that are not borrowing constrained are more likely to invest optimally in their children's human capital, and therefore have a lower intergenerational elasticity in earnings.

The NLS cohorts can be used to construct vastly larger samples but suffer from other problems. For the 1966 cohort, data on parents' income is reported by the sons not the parents and is also categorical.[5] The GSS is a cross-sectional sample and relies on retrospective judgements about relative family income and parent occupation to approximate parental income. For these reasons it is not entirely surprising that the different datasets have yielded different conclusions.

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[3] Only studies of the U.K and South Africa have produced estimates greater than 0.4. See Solon (2002) for a review of studies outside of the U.S. and Solon (1999) for a review of U.S. studies.

[4] Hauser uses occupation to infer economic status by matching Census occupation codes to measures of income and education. Hauser also uses the same technique with the 1962 and 1973 Occupational Change in a Generation Surveys (OCG) and the 1986-1988 Survey of Income and Program Participation (SIPP).

[5] For a small subset of families, where the parents were also interviewed, better income data is available.

Sibling correlations: a summary measure of shared background[6]

An alternative approach to measuring intergenerational mobility has been to use the correlation between siblings in socioeconomic outcomes as a measure of the overall importance of family background. By using *contemporaneous* accounts of income between siblings, the data problems in using the NLS cohorts to examine changes in intergenerational mobility over time can be overcome.

Conceptually, the idea is to create a summary statistic that captures *all* of the possible effects of sharing a common family and thereby avoiding the types of problems just discussed. If the similarity in earnings between siblings is not much different compared to randomly chosen individuals, then we would expect a small correlation. If, however, a large part of the variance in earnings is due to factors common to growing up in the same family environment then the correlation might be sizable.

On the other hand, the correlation among siblings is not a precise measure of family background. It picks up *all* of the factors shared by siblings, not just having a common family. Thus the brother correlation captures factors such as the number of siblings, common neighborhoods and the quality of schools. Conversely, many aspects of family background will not be captured including genetic traits and parental behavior towards children that are *sibling-specific*. Overall, though, this measure is a useful way to characterize how important shared family and community characteristics are in explaining the overall variance in earnings.

Previous studies on sibling correlations

An excellent review of economic studies on sibling correlations is found in Solon (1999). We briefly summarize that review. The estimates of brother correlations from studies that use only a *single year* of earnings for each sibling range from a low of .11 to a high of .44. The central tendency is about .25. Many of these studies use rather unique data sets that happen to track siblings from a particular community (e.g. Mormons in 19th century Utah) making it unclear how representative these findings are. A homogeneous sample is also more likely to lead to attenuation bias because it will tend to have less "signal" in the data without a commensurate decline in the "noise" (Solon et al, 1991). The studies also

[6] This section draws heavily from Solon, et al. (1991) and Solon (1999).

differ in the age at which they collect data on income, making comparisons such as changes over time, very difficult and possibly misleading.

Solon (1999) also argues that when estimates of the brother correlation based on single year earnings are corrected for measurement error and transitory shocks, they should be scaled up by a factor of somewhere between 1.4 and 2.0. This suggests that the brother correlation in "permanent" status should be close to 0.4.

There are only four studies we are aware of which produce estimates of the brother correlation in permanent status. Two of these studies use the PSID and two use the NLS. Solon et al. (1991) estimate the brother correlation in the permanent component of log annual earnings to be .34 when using the nationally representative portion of the PSID for the years covering 1975 to 1982. They estimate the brother correlation at .45 when they include the oversample of poor families in the PSID and use weights.[7] More recently Bjorkland et al (2002) have updated the PSID results of Solon et al (1991) in an attempt to compare the brother correlation in the U.S. to several Nordic countries. They use the nationally representative portion of the PSID over the time period from 1977 to 1993 and their estimates range between .42 and .45.

In combination, these PSID results appear to support Solon's conjecture that the brother correlation in permanent status in the U.S. is closer to 0.4 than 0.25. However, because the time periods of the two studies overlap they provide only a little guidance with respect to *changes over time*. Still if we compare the results that use the nationally representative portion of the PSID from Solon et al. with Bjorkland et al., there is suggestive evidence of an increase in the brother correlation; that is, the estimate for the 1977to 1993 period is larger than the 1975 to 1983 period. However, this might also be due to the fact that Bjorkland et al. use a slightly older sample than Solon et al, which might better capture permanent earnings.[8] As we discuss later, the fact that the PSID tracks only one set of families that are

[7] This latter estimate also accounts for serial correlation in transitory shocks.
[8] We can infer from their sample selection rules that Solon et al's (1991) sample is between the ages of 25 and 33, while Bjorkland et al's are between 25 and 42.

initially identified in 1968, coupled with the substantial attrition in the survey suggests that this dataset is not ideally suited for an analysis of changes over time.

With respect to the NLS, there are also two studies and both use only the original cohort of young men who are tracked from 1966 to 1981, roughly the same time period as covered by Solon et al. Altonji and Dunn (1991) estimate the brother correlation in the permanent component of log annual earnings using two different methodological approaches and their estimates are .32 and .37. Ashenfelter and Zimmerman (1997) use the same NLS cohort to study the return to education and in a table describing their sample they report a brother correlation of .31 in log annual wages averaged over 1978 and 1981. A reasonable reading of these results suggest that the brother correlation may be slightly more than 0.3 using the NLS data. These results are roughly in line with Solon et al's finding of 0.34 when using the nationally representative portion of the PSID.

It is important to note, however, that neither of the two studies using the NLS have *weighted* the sample, despite the large oversampling of black families. We will revisit the issue of weighting the NLS sample in the results we present in this paper. As far as we are aware no study to date, has used the National Longitudinal Survey of Youth (NLSY) cohort tracked since 1979 to measure the brother correlation for a more recent cohort.

Statistical Models and Estimation

Brother Correlations: Base Model

We now present the basic framework used in this analysis. We begin by discussing the method to estimate the brother correlation in earnings. First, we decompose the variance of earnings residuals obtained from the following regression:

$$(1)\quad y_{ijt} = \beta X_{ijt} + \varepsilon_{ijt}$$

The earnings for sibling *j*, in family *i* in year *t* are denoted as y_{ijt}. Here, the vector X_{ijt}, contains age and year dummies to account for lifecycle effects and year effects such as business cycle conditions. The residual, ε_{ijt}, which is purged of these effects is then decomposed as follows:

$$(2)\ \ \varepsilon_{ijt} = a_i + u_{ij} + v_{ijt}$$

The first term, a_i, is the permanent component that is common to all siblings in family *i*. The second term, u_{ij}, is the permanent component that is individual-specific. v_{ijt}, represents the transitory component that reflects noise due to either temporary shocks to earnings or measurement error in the survey.[9] As in previous studies we assume that these three components are "orthogonal by construction" in order to partition the variance into components that can be used to easily construct sibling correlations.[10] The variance of age-adjusted earnings, ε_{ijt}, then is simply:

$$(3)\ \ \sigma^2_{\varepsilon} = \sigma^2_a + \sigma^2_u + \sigma^2_v$$

and the correlation between brothers in *permanent* earnings, is

$$(4)\ \ \rho = \frac{\sigma_a^2}{\sigma_a^2 + \sigma_u^2},$$

or the fraction of the overall variance in permanent earnings that is common to siblings. The sibling correlation ρ is the focus of this analysis.

To estimate the model we use the analysis of variance (ANOVA) formulas presented in the Appendix of Solon et al (1991). These formulas adjust the classical analysis of variance estimators for differences in the number of years in earnings available for each individual, and for differences in the number of siblings within each family. We include survey weights for each year in all the estimation results. All standard errors are calculated by the bootstrap method.

A useful ancillary parameter that can be estimated is:

$$(5)\ \ \phi = \frac{\sigma_a^2 + \sigma_u^2}{\sigma_a^2 + \sigma_u^2 + \sigma_v^2}$$

Estimates of ϕ, provide a measure of the share of the overall earnings variance that is permanent. A separate literature in labor economics has used this decomposition to identify the extent to which the rise

[9] We will extend the model to the case where there is serial correlation in transitory shocks later in the section.

[10] The assumption that a_i and u_{ij} are uncorrelated is purely for analytical convenience and allows us conceptually, to divide the permanent component into a part that is perfectly correlated among siblings, and a part that is perfectly

in inequality in recent decades has been driven by factors related permanent differences among individuals (e.g, returns to skill) as opposed to growing earnings “instability”. The latter phenomenon is reflected in the rise in the transitory share that might be due to factors such as greater job displacement (e.g. Gottschalk and Moffitt, 1993, Baker, 1997, Haider, 2003). Measuring this parameter for two different cohorts might provide some additional insight into this important issue.

Brother Correlations: Model with Serially Correlated Transitory Component

We also consider the case where the transitory component is serially correlated and follows a first order autoregressive process:

$$(6)\quad v_{ijt} = \delta v_{ijt} + \xi_{ijt}$$

One might expect that a failure to allow for serially correlated transitory shocks might result in the analysis of variance procedure assigning more of the persistent part of earnings to the permanent component, σ^2_u instead of σ^2_v, thereby raising the denominator in (4) and resulting in a lower estimate of ρ. If the persistence of transitory fluctuations has changed over time, then this omission might affect the results when examining changes over time.

To address this we follow Solon et al’s methodology to uncover δ and then use the analysis of variance formulas on the “delta-differenced residuals”.[11] One problem with this approach is that it requires data on consecutive years of earnings in order to estimate δ and, as we discuss in the data section, this is not available in one of our NLS samples.

Brother Correlation in Education

We also investigate changes in the sibling correlation in years of schooling. Here, an error components model is unnecessary and a more straightforward approach is taken to measure the brother correlation. To estimate the correlation coefficient, we simply calculate the covariance in years of

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uncorrelated among siblings. For the assumption that a_i and v_{ijt} are uncorrelated we find (as did Solon et al, 1991) that there is little or no *cross-sectional* correlation in the transitory component.

schooling between all possible pairs of brothers, and calculate the variance in years of schooling for this sibling sample, and take the ratio of the two measures. This approach implicitly gives more weight to families with more siblings (Solon, Page and Duncan, 2000) but since we are interested in *changes over time* this poses less of an issue.

3. Data

NLS

To measure the correlation in earnings between brothers for two different cohorts we use two different samples from the National Longitudinal Surveys (NLS) sponsored by the Bureau of Labor Statistics. The first sample uses the Young Men Cohort, hereafter referred to as "NLS66", surveyed initially in 1966 and then nearly every year until 1981. The second sample uses men from the National Longitudinal Survey of Youth, (NLSY79) who were followed from 1979 through 2000. Both surveys oversampled blacks and so weights are used in all the analysis.

We begin by identifying men between the ages of 14 and 22 in the initial survey whose earnings are observed and are positive at least once when they are at least 26 years old.[12] For the NLS66 sample, all such years of positive earnings for the men are included in the estimation. This implies that earnings may potentially be observed in eight different years: 1970, 1971, 1973, 1975, 1976, 1978, 1980 and 1981. For the NLSY79 sample, many more years of earnings are potentially available, however, to maintain comparability with the NLS66, we use the years when the sample was closest in age to the sample of the NLS66.[13]

In the NLS66 this produces a sample of 13,861 person-year observations on 3481 men in 3079 families. For the NLSY79, there are a total of 16,995 observations on 5165 men in 4285 families. As a

[11] The residuals from (1) are first-differenced and the first difference is then regressed on its one period lag. The resulting coefficient is transformed to create an estimate of δ.
[12] Earnings include earnings from wage and salaries as well as business income.
[13] Our sample includes 1983, 1984, 1986, 1988, 1989, 1991, 1993 and 1995. The NLSY did not conduct a survey in 1995 to collect earnings data for the calendar year 1994 otherwise that would have been used to make the years perfectly parallel with the NLS66 sample.

point of comparison, Solon et al (1991) in their *largest* sample have only 2656 observations on 738 men in 583 families using the PSID. The sample characteristics are shown in Table 1.

In the main analysis the samples include siblings as well as non-siblings or "singletons". We do this to maintain comparability with Solon et al who had too small a sample of siblings to confine the analysis only to multiple sibling families. In the next section we conduct a wide range of robustness checks that include using a sample of only siblings. For the analysis that includes years of schooling, we measure the number years of school completed by age 26.

PSID

Although it is not well suited to study changes in the brother correlation over time, we also use the PSID to produce an analogous set of estimates using similar cohorts to maintain comparability with earlier studies and to illustrate the problems with the data. The PSID, begun in 1968, is a longitudinal study that contains both a representative sample of U.S. individuals and their families as well as an oversample of poorer households. We use both components of the survey and as with the NLS samples, we use weights in all the analysis. We select men between the ages of 14 and 22 in 1968 and 1979 to produce two samples that parallel the NLS cohorts. We use the same age and earnings restrictions as with the NLS but in order to produce samples as large as possible, we include *all* years of earnings within the time period covered by the NLS samples. For the earlier cohorts, hereafter referred to as "PSID68", we use all the years of earnings that meet the sample restrictions from 1972 through 1983. For the later cohort, (PSID79), we use the years from 1983 to 1992.[14] In the PSID, earnings are only observed for those who become a household head. The resulting sample for the PSID68 is 6461 observations for 979 men in 790 families. For the PSID79 there are 5444 observations for 1086 men in 901 families.

Dataset Comparison

Issues of Attrition

We have serious concerns about the suitability of using the PSID to study *changes* in the brother correlation using the methodology that we apply to the NLS cohorts. Sample attrition will lead to two important problems. The first problem is that when we select a sample of families for the *second* cohort group in the PSID in 1979, we are selecting a sample of families that has been subject to considerable attrition since the panel began in 1968. This is in contrast to the NLS cohorts, where the NLSY79 provides a second "fresh" sample of families that is representative of all families in 1979. The amount of attrition in the PSID was fairly significant as only 66 percent of the original 1968 sample remained in the sample by 1979.

Several studies (e.g. Becketti et al, 1988, Fitzgerald et al, 1998) have shown that attrition in the PSID tends to be concentrated among lower socioeconomic groups. If these groups tend to have a higher sibling correlation, then using the PSID79 sample may be biased toward finding a lower sibling correlation and a decrease in the sibling correlation over time. Siblings from lower income families may have a higher correlation in earnings due to borrowing constraints that prevent the parents from investing optimally in their children's human capital (Becker and Tomes, 1986). For example, high ability children from poor families may receive the same low level of education as a sibling with lower academic ability, compressing their earnings compared with similarly different siblings from a prosperous family. Thus, liquidity constraints could produce a greater degree of similarity in earnings among brothers from lower income households. Indeed there is growing evidence that families at the low end of the income distribution tend to have less intergenerational mobility (e.g. Gaviria, 1998; Mazumder, 2001; Hertz 2003). We will investigate this possibility in the next section.

To be clear, *this particular attrition problem*, as far as we are aware, has not been previously discussed or addressed by the validation studies that have been done on the PSID. To reiterate, this problem arises because the selection of our families in the *later* period using the PSID is based on a heavily attrited sample and is not representative.

The second problem with sample attrition is that *within* each cohort group there is attrition among the siblings when we try to observe their outcomes as adults. So in this case even if the sample of families is representative at the *beginning* of the panel, attrition among the siblings *over time* may create

[14] At the time of this study the PSID data was only fully processed up to 1992.

selection issues. For example, even though the PSID68 is a representative sample of families for the early cohorts, when we observe the earnings of the siblings in the 1970s and early 1980s we will tend to lose observations due to sample attrition of the siblings.

This second problem of attrition *within* a cohort group over time, is the type of attrition problem that has been more commonly examined by validation studies. While the studies of sample attrition in the PSID have generally found attrition bias to be relatively small for most types of economic analysis (e.g. regressions), this conclusion may be inappropriate for the *analysis of variance* approach undertaken in this study. Intuitively, one might expect attrition from the survey to be associated with individuals who experience large swings in earnings. In fact, Fitzgerald et al do find that the *variance* in log earnings is lower among non-attritors. So while the typical regression analysis including estimates of the intergenerational elasticity might be unaffected by this problem, this is a cause for concern for our analysis.

It is not immediately obvious how this bias affects each of the three components of the earnings variance and therefore, the sibling correlation. If an individual with a large transitory fluctuation in earnings is more likely to leave the sample, as Fitzgerald et al find, then the estimated transitory variance, σ^2_v, is likely to be reduced. Since, in principle this should not affect the sibling correlation in *permanent* earnings, this should not affect our results. However, if the individual is a member of a family with more than one sibling, then this would also tend to reduce the *within* family variance, σ^2_a, lowering the numerator in (5) and leading to a lower estimate of ρ. Therefore the effect of attrition might be to lower estimates of ρ.

This within cohort group attrition of course, will take place in both periods and is also likely to characterize the NLS samples, so therefore it might not affect our results concerning changes over time. For the early cohorts the attrition rate is slightly worse for the PSID than the NLS. From 1968 to 1983, about 40.2 percent of the original PSID sample is lost while from 1966 to 1981 about 35 percent of the NLS sample is lost. From 1979 to 1993 in the PSID an additional 21 percent of the original 1968 families is lost due to attrition. The analogous figure for the NLSY79, however, is only 7.9 percent. This strongly suggests that for both cohorts and especially for the later cohort, the NLS cohorts are less likely to be subject to attrition bias. In any case, the fact that the PSID79 sample *starts* with a potentially biased sample relative to the NLSY79 does present a potential problem.

Using Sibling Only Samples

If we weren't concerned about maintaining comparability with the previous literature (Solon et al, 1991; Bjorkland et al, 2002), the best way to determine the correlation among siblings would be to confine the analysis to only siblings. The sample size of siblings in the PSID, however, makes such an exercise unfeasible –the standard errors would simply be too large to make the estimates meaningful. Examining changes over time, of course, would be even less reliable with the PSID. That is another important reason why we prefer to use the NLS results where the samples can be confined to just siblings and where inferences about changes over time are still possible.

In particular, Solon et al (1991) speculate that including singletons in the analysis may lead to an underestimate of ρ if outliers tend to be more common among singletons than siblings. This is because while singletons earnings are not used to calculate σ^2_a, the numerator of ρ, they are included in $\sigma^2_{u,}$ which is in the denominator of ρ. This might result in enlarging the estimates of the variance of the permanent individual effect relative to the family effect thereby depressing the estimated sibling correlation. In any case, it is clear that it would be preferable to produce estimates using a sibling only sample.

Household Heads

Finally, another key difference between the samples is that in the PSID earnings information is only available for the men if they are classified as the head of the household. In contrast, in the NLS an attempt is made to collect information on earnings for all the men irrespective of their relation to the household head. In practice, this does not appear to make a big difference. For example, limiting the

NLS66 sample to those who were heads of households to be more comparable to the PSID lowers the sample of individuals from 3418 to 3226. We estimate the results with and without this restriction using the NLS samples to see if the results are robust to this difference.

4. Results

Brother Correlations: Base Model

The estimates for the variance decomposition are shown in Table 2. We first present the results that do not account for serially correlated transitory shocks. Starting first with the NLS cohorts, we find that σ^2_a, the variance of permanent component shared by siblings, rose sharply from 0.07 to 0.22 and is statistically significant. The estimates for the individual-specific permanent component, σ^2_u also rose from 0.21 to 0.27 but the change was not statistically significant. The transitory variance essentially stayed constant. The implication of these changes is a sharp rise in ρ, the brother correlation in earnings. We find that ρ was just 0.26 for the NLS66 but was greater than 0.45 for the NLSY79 and that the change is statistically significant at the 9 percent level.

Compared to the two other studies using the NLS66 our brother correlation in earnings are a little bit smaller. This is due to the fact that we weight the sample to account for the oversample of blacks. In fact if we calculate ρ, unweighted, our estimate for the NLS66 is 0.33 -- almost identical to the estimates of Altonji and Dunn's (1991), 0.32, and of Ashenfelter and Zimmerman's (1997), 0.31.[15] Given the evidence that there is a higher intergenerational transmission of earnings among blacks (Borjas, 1992, Mazumder 2001, Hertz, 2003), it is important to weight the samples. As far as we are aware, 0.45, is the first estimate of the brother correlation in earnings using the NLSY79.

The results using the PSID are a bit different. Both permanent components rise as is the case with the NLS cohorts. However, the increase in the family component is small and insignificant while the

[15] Altonji and Dunn (1991) produce an estimate of 0.37 when they use a method of moments estimator and make a much stronger assumption that the transitory component of earnings are uncorrelated when they are two years apart.

increase in the individuals-specific component is fairly large rising from 0.16 to 0.25. This results in a small and statistically insignificant *decrease* in the estimate of ρ from 0.51 to 0.45.

The estimate of a brother correlation of around 0.5 for the older cohorts in the PSID, covering a similar period as Solon, et al. (1991) is somewhat surprising given their lower estimates (.34 with the nationally representative sample and .45 with the entire sample weighted and accounting for autocorrelated errors). To reconcile our results with theirs, we attempt to replicate the preliminary analysis in Solon et al that does not account for serially correlated transitory shocks and estimates ρ to be 0.34. We are roughly able to reproduce their results, finding an estimate of 0.31, suggesting that our formulas are probably consistent.[16]

A key difference between our sample and Solon et al's that explains much of the discrepancy is the age restriction. Solon et al, quite sensibly, restrict their age range to those between the ages of 10 and 17 in 1968 in order to avoid over-representing men who lived with their parents at a late age. Since our NLS cohorts have a minimum age of 14 we cannot use this rule and maintain consistency across datasets. However, if we restrict our age range to those between 14 and 18 (instead of 14 to 22) in 1968, our PSID results are now much closer to Solon et al's. We present these results along with other sensitivity checks in Table 3. We now estimate the brother correlation in earnings to be 0.39, which is reasonably close to Solon et al's estimate of 0.34. We now find that the brother correlation increases over time in *both* samples by exactly the same amount, 0.18, although the point estimates are significantly higher in both periods using the PSID (row 2). Since the samples are considerably smaller using this age restriction, the increase is now significant at only the 16 percent level with the NLS.

We also check whether or not the results are sensitive to the inclusion of singletons. As we discussed earlier, if outliers tend to be more common among singletons, the result might be to depress the estimated sibling correlation. We actually find the opposite result. In all four estimates using both datasets, the estimates are lower when singletons are excluded (row 3 compared to row 1). It is

[16] We produce a sample of 1936 observations on 458 men in 352 families compared to their sample of 1854 observations on 433 men from 342 families.

interesting to note that with this sample, the estimate of the increase in the brother correlation with the NLS cohorts is now significant at the 2 percent level. We interpret this as fairly strong evidence of a rise in the brother correlation over time, since we suspect that many researchers would find a sample of only siblings to be the most natural sample to use for this analysis.

As a further check we investigate the possibility that outliers might influence the results, particularly given the low estimate for the NLS66. In row 4, we restrict earnings to be at least $100 in 1970 dollars and require that the individuals not be enrolled in school in the previous year. This appears to have little effect for the later period in either dataset, but has a strong effect on the early period. In the case of the PSID, the early period estimate of the brother correlation is now a stunning 0.66 and suggests a possible *significant decrease* in the brother correlation over time. The change in the NLS estimates show a smaller but still sizable increase (0.11) over time that is significant at the 16 percent level.

Finally, we combine all of the restrictions from rows 2, 3 and 4 in row 5. Here, the age restriction and the exclusion of singletons sharply reduces the number of families used in the analysis. In the case of the PSID samples, there are now less than 100 families. Even with all of these restrictions, the results in the NLS hold up extremely well. The brother correlation rises from 0.22 to 0.42 and is significant at the 8 percent level. The results from Table 3 taken as a whole, suggest that the results from the NLS cohorts appear to be very robust to changes in sample selection rules while the PSID results fluctuate dramatically. The estimate of the *change* over time in ρ ranges from an increase of 0.11 to an increase of 0.22 with the NLS, but ranges from a decrease of 0.21 to an increase of 0.18 when using the PSID.

As we point out in the last section, we are concerned that the PSID results in the later period might be flawed due to the selective nature of sample attrition that has been well established in previous studies (Becketti et al 1988; Fitzgerald et al, 1998). As a rough check on the potential effects of selecting higher income families on the brother correlation, we divided the PSID samples by the median income level in each year and estimate the brother correlation for each subgroup. The results are shown in Table 4. As we anticipated the brother correlation is significantly higher for families with income below the median. While the raw differences are striking, these estimates are based on fairly small PSID samples and should be taken with some caution. Overall, we believe that the findings from the base model provide fairly strong evidence of a rise over time in the brother correlation and casts considerable doubt on the use of the PSID as a viable dataset for answering this question.

Brother Correlations: Model with Serially Correlated Transitory Component

To ensure that the results are not sensitive to the omission of serially correlated transitory shocks, we estimate the model taking into account this possibility. First we use the methodology employed by Solon et al to first estimate the autocorrelation parameter and then apply the analysis of variance procedure on the adjusted residuals. Unfortunately because of the ad hoc years that are available, this approach cannot be used for the NLS66 sample. The results for the other three samples are shown in Table 5. The first row repeats the baseline estimates from Table 2. The results for the brother correlation in row 2 for the NLSY79 sample barely changes from 0.452 to 0.450. The estimate for δ, the autocorrelation parameter is 0.29. For the PSID, the estimates show a slight increase going from 0.48 to 0.55. The estimates of δ decrease from 0.36 to 0.27. Again, while the NLS results don't appear to be sensitive to accounting for serial correlation, the PSID results are.

How Much Is Explained By Education?

While these results present evidence that brother correlation in earnings has been rising, the natural question is what explains this phenomenon? From the point of view of economic theory, the critical determinant of earnings is human capital. Therefore, it is natural to examine the extent to which schooling and other forms of skills might have influenced the sibling correlation in earnings in each period. Unfortunately due to data limitations such as having common variables available for both cohorts, we can only use years of schooling as a proxy for human capital. Using the two NLS cohorts we find that correlation in years of education was unchanged over the period with estimates of 0.62 for the NLS66 and 0.59 for the NLSY79 (see Table 6).

These results appear to be a bit higher than previous estimates. Ashenfelter and Zimmerman (1997) also use the NLS66 and report a correlation in years of schooling of 0.51. However, because of different sample selection rules, their sample consists of just 143 brother pairs while we use 341.[17] We also make use of sample weights. Solon, Page and Duncan (2000) provide four estimates ranging from 0.51 to 0.57 using the PSID for a sample similar to our PSID68 cohort. Our methodology, which weights each brother pair equally regardless of how many siblings in the family, is most similar to that which produces Solon, Page and Duncan's highest estimate of 0.57. In any case, since we use the same technique in both periods, methodological considerations should not affect our conclusions concerning changes *over time*.

While the correlation in the *quantity* of completed schooling appears to have stayed the same over time, it might be the case that the well documented rise in the *returns to schooling* since the 1970s could account for the rise in the brother correlation in earnings. In other words, even if the association between family and neighborhood characteristics and schooling was constant over time, if the payoff to schooling in the labor market increased substantially, this might account for the increase in sibling earnings correlation. To test for this possibility, we ran our model in both time periods using earnings residuals that were *purged of the effects of education*. Specifically, we estimate (1) including years of education attained by age 26 in addition to age and year dummies. This has the effect of eliminating both the years of schooling *and* the returns to schooling from the calculation of the residual earnings correlation. If we were to find that after removing the effects of education on earnings that there was little change in the brother correlation in the residual earnings, then this might imply that that the rise in returns to schooling accounted for the large increase in the brother earnings correlation.

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[17] The main difference is that Ashenfelter and Zimmerman (1997) require positive hourly wages in 1978 and/or 1981.

In fact we do not find this to be the case. As Table 6 shows, the brother correlation in earnings is now estimated at 0.20 for the NLS66 and 0.35 for the NLSY79. The estimates in both years are reduced by about 20 percent after removing the effects of education from the earnings residuals. While the change in the brother correlation over time falls slightly (0.19 to 0.15), in percentage terms, the rise is actually higher (72% to 76%). Overall, it is clear that rise in the brother correlation is still substantial even if we remove the effects of schooling.

So what factors contribute to this correlation in residual earnings and which are responsible for the increase in the correlation over time? To begin with, it could still be the case that we are not adequately measuring human capital. There are other aspects such as quality of schools, job experience, exposure to knowledge from parents, and other common skills shared by brothers that are not captured simply by looking at years of schooling. It could be that there are certain other unobserved traits such as perseverance, confidence, height, looks or intelligence that could be shared by siblings for genetic or environmental reasons. If the labor market rewards to these characteristics have risen then this might explain a rising brother correlation in earnings. Finally it could be common peer effects or neighborhood effects that account for the growing similarity between brothers in earnings. The fact that we find a sharply higher brother correlation in the lower half of the parents' income distribution provides some evidence consistent with the borrowing contraints hypothesis discussed earlier (Becker and Tomes, 1986) but results from a more convincing research design are required.

5. Conclusion

We present new evidence that the importance of family background and community influences has been rising in recent decades. The correlation in earnings between brothers rose from 0.26 for men entering the labor force during the 1970s, to 0.45 for men entering the labor force in the late 1980s. This rise is significant at the 10 percent level. This result is robust to various changes in sample selection rules and methodological approaches. However, we would like to verify this result using other data sources. Unfortunately, the PSID which has been used previously to estimate the brother correlation, has an insufficient sample of siblings and is vulnerable to attrition problems when attempting to analyze changes in the brother correlation over time.

We do not find any change in the brother correlation in years of schooling, suggesting that the relationship between family background and years of schooling has remained stable. We also find no evidence that accounting for the rise in the *return to schooling* can explain our finding of an increase in the brother correlation. The fact that the brother correlation appears to be much higher at the low end of the income distribution is consistent with the argument that borrowing constraints may be at work but the evidence is only sketchy. Understanding the underlying causes for the high similarity in earnings for brothers in recent years remains an important area for future research.

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Table 1: Sample Characteristics

Samples used to estimate brother correlation in earnings in Table 2 (unweighted)

NLS				
	NLS66		***NLSY79***	
	Mean	S.D.	Mean	S.D.
Log annual earnings (1970 dollars)	8.83	0.80	8.63	0.94
Age	29.67	2.85	29.08	2.45
Black	0.21	0.41	0.26	0.44
Years of education by age 26	13.14	2.72	12.67	2.49
Log avg. family income in 1966 or 1978 (1970$)*	9.05	0.80	9.92	0.89
Families	3079		4285	
Individuals	3418		5165	
Singletons	2770		3545	
Person-Years	13861		16995	
PSID				
	PSID68		***PSID79***	
Log annual earnings (1970 dollars)	8.73	0.79	8.62	0.96
Age	29.64	2.80	29.12	2.44
Black	0.31	0.46	0.36	0.48
Log avg. family income in 1968 or 1978 (1970$)	8.92	0.74	8.88	1.02
Families	790		901	
Individuals	979		1086	
Singletons	638		741	
Years	6461		5444	

Table 2: Changes in the Brother Correlation in Earnings

	NLS 66		***NLSY 79***		***Change***		
	estimate	s.e.	estimate	s.e.	diff.	t-stat	p-value*
s^2_a	0.074	(0.04)	0.222	(0.02)	0.148	3.483	0.000
s^2_u	0.208	(0.04)	0.269	(0.03)	0.061	1.263	0.103
s^2_v	0.335	(0.03)	0.330	(0.03)	-0.005	-0.116	0.454
r	**0.263**	(0.13)	**0.452**	(0.05)	**0.189**	1.358	0.087
l	0.458	(0.03)	0.598	(0.02)	0.140	4.447	0.000
observations		13861		16995			

	PSID 68		***PSID 79***		***Change***		
	estimate	s.e.	estimate	s.e.	diff.	t-stat	p-value*
s^2_a	0.164	(0.04)	0.206	(0.05)	0.042	0.626	0.266
s^2_u	0.158	(0.04)	0.248	(0.05)	0.090	1.424	0.077
s^2_v	0.237	(0.02)	0.293	(0.02)	0.056	1.670	0.047
r	**0.509**	(0.12)	**0.454**	(0.11)	**-0.055**	-0.339	0.367
l	0.576	(0.03)	0.608	(0.03)	0.032	0.804	0.211
observations		6461		5444			

Note: All estimates are weighted. Standard errors are bootstrapped using 200 iterations.
*Using a one tailed test that the change in absolute value is significantly different from zero

Table 3: Sensitivity Analysis of Changes in the Brother Correlation in Earnings

Estimates of the brother correlation Standard errors in parentheses Number of families	*Early Cohorts*	*Later Cohorts*	*diff.*	*Change t-stat*	*p-value**
1. Results from Table 2					
NLS	0.263	0.452	0.189	1.358	0.087
	(0.13)	(0.05)	(0.14)		
	3079	4285			
PSID	0.509	0.454	-0.055	-0.339	0.367
	(0.12)	(0.11)	(0.16)		
	790	901			
2. Aged 14 to 18 in base year					
NLS	0.204	0.384	0.181	1.009	0.157
	(0.15)	(0.10)	(0.18)		
	2071	2648			
PSID	0.391	0.572	0.181	0.621	0.267
	(0.24)	(0.16)	(0.29)		
	493	463			
3. Sibling-Only sample					
NLS	0.215	0.438	0.224	2.103	0.018
	(0.09)	(0.05)	(0.11)		
	309	740			
PSID	0.485	0.443	-0.042	-0.302	0.381
	(0.09)	(0.11)	(0.14)		
	152	160			
4. Earnings and enrollment restriction					
NLS	0.358	0.470	0.111	0.994	0.160
	(0.10)	(0.06)	(0.11)		
	3030	4216			
PSID	0.662	0.450	-0.212	-1.674	0.047
	(0.07)	(0.10)	(0.13)		
	786	898			
5. Combine rows 2, 3 and 4					
NLS	0.221	0.418	0.197	1.408	0.080
	(0.12)	(0.07)	(0.14)		
	198	405			
PSID	0.515	0.497	-0.017	-0.095	0.462
	(0.13)	(0.12)	(0.18)		
	61	84			

Note: All estimates are weighted. Standard errors are bootstrapped using 200 iterations.
*Using a one tailed test that the change in absolute value is significantly different from zero

Table 4: Brother Correlation by Income Level in the PSID

Estimates of the brother correlation (unweighted)
Standard errors in parentheses
Number of families
Number of Individuals

			Difference, Low- High		
	Low Income	*High Income*	*diff.*	*t-stat*	*p-value**
PSID68	0.668	0.340	0.327	4.615	0.000
	(0.02)	(0.07)	(0.07)		
	408	382			
	489	490			
PSID79	0.516	0.085	0.431	7.074	0.000
	(0.02)	(0.06)	(0.06)		
	458	443			
	542	544			

Estimates of the brother correlation (weighted)

			Difference, Low- High		
	Low Income	*High Income*	*diff.*	*t-stat*	*p-value**
PSID68	0.826	0.236	0.590	3.116	0.001
	(0.06)	(0.18)	(0.19)		
	408	382			
	489	490			
PSID79	0.552	0.217	0.335	1.453	0.073
	(0.13)	(0.19)	(0.23)		
	458	443			
	542	544			

Table 5: Model with Serially Correlated Transitory Shocks

Estimates of the brother correlation
Standard errors in parentheses
Number of families

	Early Cohorts	*Later Cohorts*	*diff.*	*Change* *t-stat*	*p-value**
1. Results from Table 2					
NLS	0.263	0.452	0.189	1.358	0.087
	(0.13)	(0.05)	(0.14)		
	3079	4285			
PSID	0.509	0.454	-0.055	-0.339	0.367
	(0.12)	(0.11)	(0.16)		
	790	901			
2. Using Solon et al (1991) methodology					
NLS	--	0.450			
	--	(0.07)			
	--	3971			
delta	--	0.286			
	--	(0.01)			
PSID	0.476	0.543	0.067	0.239	0.405
	(0.25)	(0.13)	(0.28)		
	760	813			
delta	0.357	0.265			

Table 6: Sibling Correlation in Residuals Purged of Education

Brother Correlation in Years of Education, NLS samples

	Early Cohorts	*Later Cohorts*
estimate	0.620	0.590
N	341	1008

Brother Correlation in Earnings, NLS samples

	Early Cohorts	*Later Cohorts*	diff.	*% change*
1. Results from Table 2				
	0.263	0.452	0.189	72.0
	(0.13)	(0.05)		
	3079	4285		
2. Earnings Correlation Purged of Education effects				
	0.204	0.358	0.154	75.6
	(0.13)	(0.05)		
	3079	4285		
Difference	-0.059	-0.094		
% change	-22.5	-20.9		

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