# Correlations between Brothers and Neighboring Boys in Their Adult Earnings: The Importance of Being Urban

Marianne E. Page, *University of California at Davis*

Gary Solon, *University of Michigan*

A comparison of the correlations between brothers and neighboring boys in their adult earnings suggests that the earnings resemblance between brothers stems more from growing up in the same family than from growing up in the same neighborhood. Much of the neighbor correlation is explicable in terms of the large earnings differential between urban and nonurban areas combined with the strength with which urbanicity of childhood neighborhood predicts urbanicity of adult location. This pattern is subject to a variety of interpretations, but it is quite different from the usual view of neighborhood effects.

This research has been supported by National Science Foundation grants SBR-9615506 and SBR-9630918. Part of the work was completed while Page was a visiting scholar at the Joint Center for Poverty Research. Confidentiality restrictions prohibit us from sharing the database. Researchers seeking access to similar data from the Panel Study of Income Dynamics should contact the Panel Study staff at the University of Michigan's Institute for Social Research. We are grateful for comments from seminar participants at the University of Michigan, Australian National University, the University of California at Irvine, the University of California at Los Angeles, the University of California at Santa Barbara, the Joint Center for Poverty Research, the University of New South Wales, and Princeton University.

[*Journal of Labor Economics*, 2003, vol. 21, no. 4]

0734-306X/2003/2104-0004$10.00

## I. Introduction

Numerous researchers have used sibling correlations in various socioeconomic outcomes as omnibus measures of the extent to which inequality in those outcomes is attributable to family and community origins.[1] In the words of Griliches's (1979, p. S38) classic survey article, "Brothers are likely to be more alike than a randomly selected pair of individuals on a variety of socioeconomic measurements. This correlation arises from many sources: common heredity, both physical and cultural; similar access to financial resources; exposure to similar influences of friends, neighbors, schools, and other aspects of their particular community; the likelihood, even in adulthood, of closer location in space and hence exposure to similar regional price differentials and common business-cycle effects; and more. Some of these effects are measurable, but many are not, or only imperfectly so." Because of this imperfection in our ability to identify and measure exactly what matters about the background that siblings share, it has been common for researchers to find that sibling correlations far exceed the variation that can be explained by regressions of the outcome variables on particular measured family and neighborhood background characteristics.[2] What underlies the substantial sibling correlations, therefore, has remained an important, unresolved puzzle.

One aspect of this puzzle is the role of "exposure to similar influences of friends, neighbors, schools, and other aspects of their particular community." This question has become increasingly salient with the recent upsurge of interest in neighborhood effects. Initially, this interest stemmed largely from a concern about the role of "underclass" neighborhoods in perpetuating poverty and welfare dependency.[3] Many subsequent analyses, however, have proceeded to broader consideration of neighborhood influences on inequality, intergenerational mobility, and economic growth.[4]

In Solon, Page, and Duncan (2000), we proposed using correlations between neighboring children in their later socioeconomic status to bound the proportion of inequality in socioeconomic outcomes that can be attributed to disparities in neighborhood background. As we will explain below in Section II, this approach can identify only an upper bound on the explanatory power of neighborhood origins because neighbor cor-

[1] See sec. 3 of Solon (1999) for a recent survey of this literature.

[2] To our knowledge, Corcoran, Jencks, and Olneck (1976) were the first to emphasize this point.

[3] See, e.g., Murray (1984) and Wilson (1987).

[4] See Benabou (1996*a*, 1996*b*), Durlauf (1996), and Kremer (1997) for theoretical analyses. See sec. 5 of Solon (1999) for an overview of the empirical literature and further references.

relations reflect the influence of similar family backgrounds as well as the influence of shared community background.[5] When we used years of schooling as the outcome variable in our previous study, we found that, although the sibling correlation is above 0.5, the neighbor correlation is less than 0.2. Furthermore, once we accounted for the effects of a few readily observed family background characteristics, the upper bound on the proportion of schooling variation attributable to neighborhood background was tightened further to about 0.1. This proportion is substantial, but it remains inflated by neighbors' similarity in unmeasured aspects of family background. We concluded that the sibling resemblance in educational attainment arises much more from growing up in the same family than from growing up in the same neighborhood.

In this article, we extend our analysis to men's earnings.[6] Many previous researchers have measured sibling correlations in earnings, but, to the best of our knowledge, we are the first to obtain parallel correlations for unrelated individuals that grew up in the same neighborhood. Our analysis pays particular attention to Griliches's prescient point about "the likelihood, even in adulthood, of closer location in space and hence exposure to similar regional price differentials and common business-cycle effects" (Griliches 1979, p. S38). We find that much of the earnings correlation between brothers and most of the correlation between neighbors are explicable by what we call "the importance of being urban." Like Glaeser and Mare (2001) and Kim (2002), we find that workers in large cities earn much more than workers in small cities, who in turn earn much more than workers who are not located in cities at all. We also find that the urbanicity of where a worker grew up is an extremely strong predictor of the urbanicity of his adult residence. In combination, these facts account for most of the correlation between neighboring boys in their adult earnings. This finding is open to a variety of interpretations, but all of them differ considerably from the usual view of neighborhood effects.

The remainder of our article consists of three sections. In the next

[5] Jencks and Brown (1975), Altonji (1988), and Rivkin, Hanushek, and Kain (1998) have used a similar methodology for bounding the explanatory power of school effects.

[6] In a companion paper (Page and Solon, in press), we also consider women's family income. It is interesting to contemplate whether our present analysis of earnings could be combined with our earlier analysis of education to ascertain the extent to which the brother and neighbor correlations observed for earnings are caused by the brother and neighbor correlations in education. That research agenda, however, would face the same problems long recognized as formidable obstacles to identifying the causal impact of education on earnings. For thorough discussions of the reasons why observed earnings-education associations may reflect more than just causation running from education to earnings, see the classic survey article by Griliches (1977) and the more recent surveys by Card (1995) and Bound and Solon (1999).

section we explain our motivation, methodology, and data for estimating earnings correlations between brothers and neighboring boys, and then we present our empirical results. In Section III we proceed to our investigation of "the importance of being urban," and in Section IV we summarize our main findings.

## II. Estimating Earnings Correlations between Brothers and Neighboring Boys

### A. Econometric Model

In this subsection, we present a simple variance-components model that provides some intuition for the relationships among brother correlations, neighbor correlations, and regression analyses of neighborhood effects. Let $y_{cfi}$ denote the log of long-run earnings for individual $i$ from family $f$ in community $c$. Suppose that $y_{cfi}$ can be decomposed as

$$y_{cfi} = x_{cf} + z_c + u_{cfi}, \tag{1}$$

where $x_{cf}$ represents the combined effect of all observable and unobservable family characteristics that influence $y_{cfi}$, $z_c$ represents the combined effect of all observable and unobservable neighborhood characteristics, and $u_{cfi}$ is an orthogonal factor representing the effects of individual-specific characteristics unrelated to either family or neighborhood background.[7] We expect the family background factor $x_{cf}$ and the neighborhood factor $z_c$ to be positively correlated with each other because, for reasons discussed by Tiebout (1956) and others, advantaged families tend to sort into advantaged neighborhoods.

If all the family and neighborhood characteristics underlying $x_{cf}$ and $z_c$ could be measured with perfect accuracy, our research agenda would be simply to estimate the regression of $y_{cfi}$ on those characteristics. But, of course, not all of those characteristics can be observed, and those that are observed are often measured imperfectly. As a result, the many existing regression analyses of neighborhood and family background effects have been plagued by omitted-variables and errors-in-variables biases.[8] In this

[7] At the cost of additional complexity, we could generalize this model to allow interactions of family and neighborhood effects as well as other nonlinearities. Incorporating interaction terms into eq. (1) would add more ambiguous terms to the neighbor covariance shown below in eq. (4). The third term in eq. (4) already suffices to illustrate the inherent ambiguity of the variance decomposition into family and neighborhood effects when family and neighborhood variables are correlated. Therefore, for the sake of simplicity, we opt for the linear representation in eq. (1).

[8] See, e.g., Corcoran, Gordon, Laren, and Solon (1992), Kremer (1997), and the excellent survey article by Jencks and Mayer (1990). See Manski (1993) for a formal analysis of the underidentification of regression models of neighborhood effects.

study, instead of performing still another regression analysis, we use the earnings correlations between brothers and neighbors to explore the degree to which $x_{cf}$ and $z_c$ can explain earnings inequality. The beauty of this approach is that it provides us with information about the overall importance of all relevant neighborhood characteristics, including those we can never hope to measure.

To demonstrate this, we begin by noting that the population variance of $y_{cfi}$ is

$$\mathrm{Var}(y_{cfi}) = \mathrm{Var}(x_{cf}) + \mathrm{Var}(z_c) + 2\,\mathrm{Cov}(x_{cf}, z_c) + \mathrm{Var}(u_{cfi}). \qquad (2)$$

The covariance in $y_{cfi}$ between brothers $i$ and $i'$ from the same family is

$$\mathrm{Cov}(y_{cfi}, y_{cfi'}) = \mathrm{Var}(x_{cf}) + \mathrm{Var}(z_c) + 2\,\mathrm{Cov}(x_{cf}, z_c). \qquad (3)$$

This expression formalizes the obvious point that brothers have correlated outcomes because they share both family and neighborhood background. Brother correlations alone cannot identify the separate effects of family and neighborhood origins. But additional information might be gleaned from the covariance between neighbors from different families in the same community:

$$\mathrm{Cov}(y_{cfi}, y_{cf'i'}) = \mathrm{Cov}(x_{cf}, x_{cf'}) + \mathrm{Var}(z_c) + 2\,\mathrm{Cov}(x_{cf}, z_c). \qquad (4)$$

We expect the first term in equation (4) to be positive because neighborhoods usually contain families that are similar. Nevertheless, the neighbor covariance in equation (4) is smaller than the brother covariance in equation (3) because the neighboring boys' families are merely somewhat similar, not identical. If the neighbor covariance is only a small fraction of the brother covariance, the family effects generating the first term in equation (3) must be the main source of the brother covariance.

Equation (4) clarifies the two reasons why the neighbor covariance in $y_{cfi}$ should be viewed as an upper bound on the combined influence of the neighborhood characteristics underlying $z_c$. First, treating the neighbor covariance as an indicator of neighborhood effects generously attributes all of the third term to neighborhoods even though the proper allocation of that term between neighborhood and family effects is inherently ambiguous. Second, the first term in the neighbor covariance is unambiguously the effect of similar family backgrounds, rather than a neighborhood effect. Because the neighbor covariance provides an upper bound on the explanatory power of neighborhood background, very small neighbor correlations would indicate that neighborhoods cannot account for much of the variation in earnings. If they turn out to be large, important neighborhood contributions to earnings inequality remain in the realm of possibility, and further research on effects of particular neighborhood characteristics is strongly warranted.

### B. Data

Our data on brothers and neighboring boys come from the Panel Study of Income Dynamics (PSID), a longitudinal survey conducted by the University of Michigan's Survey Research Center. The PSID began by interviewing a national probability sample of families in 1968 and has reinterviewed the members of those families every year since. Like virtually every national household survey, the original PSID economized on interviewing costs by selecting a "cluster sample," that is, several households were selected in the same vicinity, usually within a block or two of each other. In the past, when social researchers have even been aware of the cluster design of household surveys, they have viewed it as a nuisance because the resulting nonindependence of household observations complicates the proper estimation of standard errors. But, for our purposes, the cluster design is an extraordinary blessing. Thanks to its cluster design, the 1968 PSID sample contains not only multiple siblings from the same families but also children from neighboring families. Furthermore, because the survey has followed those children as they have grown into adulthood and formed their own households, we can use the PSID data to examine the resemblance between both siblings and neighboring children in their later earnings.

As described in detail in Solon et al. (2000), families in the same PSID sampling cluster lived within a group of 20–30 contiguous dwelling units. Thus, although these families may or may not have been social neighbors in the sense of interacting closely with each other, they did live in close geographic proximity to each other. In urban areas, the sampling area they shared may have been a city block or even just part of a block. In rural areas, the families were spread further apart but still were among each other's closest neighbors. Thus, although the neighbor correlations we estimate will not capture every sort of environmental influence, they will be pertinent for assessing the effects of growing up in a particular residential location.

By defining families in the same sampling cluster as neighbors, we measure a child's neighborhood environment in terms of where the child lived when the PSID sample was selected in 1968. In many cases, however, the family moved elsewhere after 1968. If 1968 neighborhood is a poor proxy for longer-run neighborhood environment, then our estimation of neighbor correlations may be subject to downward errors-in-variables bias.[9] As discussed in detail in Kunz, Page, and Solon (2003), this problem probably is not severe because, even when families move, the neighborhoods they move to usually are similar to the ones they move from. The analyses in that paper find strikingly high autocorrelations in measured

[9] Regression analyses of neighborhood effects that use single-year measures of observable neighborhood characteristics also are subject to this problem.

characteristics of the "geocodes" (usually census tracts) inhabited by the PSID children. For example, the sample autocorrelation between log mean income (in the geocode) in each year between 1970 and 1974 and the average of log mean income over the entire period is at least 0.96 for every year and averages 0.97. That correlation averages 0.89 even for the subsample of children that changed geocodes. These findings suggest that the neighborhood that a child inhabits in a particular year is usually a very good indicator of his longer-run neighborhood environment and, therefore, that our reliance on 1968 neighborhood will not lead to large biases.

One reason that our earlier study used educational attainment as the outcome variable was that the distribution of educational attainment is similar between women and men, and we could, therefore, boost our sample size by pooling the genders in our analysis. This argument clearly does not hold for earnings, and our analysis here focuses only on men. Because we wish to study inequality in long-run earnings, we reduce the impact of transitory earnings fluctuations and random measurement error by using the 5-year average of the natural logarithm of total labor income over the 1987–91 period (as reported in the 1988–92 interviews). We exclude men whose earnings were imputed by "major assignments," and we remove outliers by restricting our sample to men whose earnings in each of the 5 years were at least $1,000 in 1991 dollars (as measured by the CPI-U).[10]

Our analysis pertains to the cohort born between 1952 and 1962. Men born before 1952, who were older than 16 at the 1968 interview, are excluded to avoid overrepresenting individuals who left home at late ages. The 1962 birth year restriction assures that the earnings measures from 1987 on are observed at ages of at least 25. As of 1991, the oldest cohort is age 39. To account for the remaining life-cycle variation in earnings, we will adjust our earnings variable with a preliminary regression on a quadratic in age.

[10] The influence of outliers on estimated sibling correlations is discussed at length in Solon, Corcoran, Gordon, and Laren (1991). Excluding outliers clearly produces a more homogeneous sample, but it is not clear in which direction this pushes estimates of sibling and neighbor correlations. The variance in the denominator of each correlation is depressed by the exclusion of outliers, but so is the covariance in the numerator. For example, following a suggestion from Derek Neal, we have experimented with including the outliers in an analysis in which our outcome variable is the log of the 5-year average of labor income, instead of the average of the logs. Switching outcome variables without adding the outliers has very little effect on the results we report below. Adding the outliers more than doubles the estimated variance of the outcome variable, but it also increases the estimated neighbor covariance by a similar proportion. The resulting 0.15 estimate of the neighbor correlation is very close to the 0.16 estimate we report below for the sample excluding outliers.

**Table 1**
**Sample Means**

| Variable | Sample Mean |
|---|---|
| Age in 1989 | 32.3 |
| Average log earnings (1991 $), 1987–91 | 10.2 |
| Black | .045 |
| 1968 region: | |
| Northeast | .246 |
| North Central | .370 |
| South | .228 |
| West | .156 |
| 1968 urbanicity: | |
| Large city | .230 |
| Small city | .332 |
| Noncity | .438 |
| 1987–91 average region: | |
| Northeast | .239 |
| North Central | .300 |
| South | .278 |
| West | .183 |
| 1987–91 average urbanicity: | |
| Large city | .349 |
| Small city | .366 |
| Noncity | .285 |
| Sample size | 443 |

Our analysis sample excludes anyone missing any of the above information or the region or urbanicity information discussed later in Section III. It also excludes the Survey of Economic Opportunity component of the PSID sample, commonly known as the "poverty sample." This component is uninformative for our purposes because two neighboring families could enter that component of the PSID only if they both had sufficiently low income. Consequently, the within-neighborhood variation in outcomes in the Survey of Economic Opportunity sample cannot be informative about the typical variation among all families within a neighborhood. Finally, because we are primarily interested in correlations across neighboring families, we restrict our sample to clusters containing sample members from at least two different families.

The resulting sample contains 443 men from 287 families in 120 clusters. Table 1 displays the sample means of relevant variables. With ages ranging from a low of 25 in 1987 to a high of 39 in 1991, the sample mean age as of 1989 is a little above 32. The sample mean of 10.2 for the 5-year average of log earnings (in 1991 dollars) implies a geometric mean of about $27,000. A comparison of the regional distributions for 1968 and 1987–91 reveals some tendency toward migration to the South and West. The low 0.045 value for proportion black arises from the combination of our earnings restrictions and the well-documented tendency for higher sample attrition among the lower-income members of the PSID sample.[11]

[11] See Solon (1992) and Fitzgerald, Gottschalk, and Moffitt (1998).

As already discussed above in note 10, the resulting homogenization of the sample has an ambiguous effect on the estimation of brother and neighbor correlations because it depresses both the variance in the denominator of each correlation and the covariance in the numerator. A related question is how our results might be affected by a greater propensity for attrition among movers than stayers. If the children who tend to disappear from the sample because they move furthest from their families and neighborhoods of origin also deviate the furthest in their socioeconomic attainment, the remaining sample tends to overstate the resemblance among both brothers and neighboring children.

## C. Estimation Strategy

Our initial measure of adult earnings is the 5-year average of log labor income over the 1987–91 period. The age of our sample members ranges from a low of 25 in 1987 to a high of 39 in 1991, and this age range is a stage of particularly rapid earnings growth. We therefore adjust our initial earnings measure for stage of life cycle by applying least squares to the regression of the 5-year average of log earnings on age as of 1989 and its square. In the remainder of our analysis, we use the residual from this regression as our measure of earnings status.[12]

Let $y_{cfi}$ denote our "residualized" earnings measure for individual $i$ from family $f$ in geographic cluster $c$. If $y_{cfi}$ is measured in deviation-from-mean form, the variance of $y_{cfi}$ is $E(y_{cfi}^2)$, the covariance between two brothers from the same family is $E(y_{cfi}y_{cfi'})$, and the brother correlation is the ratio of the brother covariance to the variance. The covariance between two unrelated neighbors is $E(y_{cfi}y_{cf'i'})$, and the neighbor correlation is the ratio of the neighbor covariance to the variance.

A natural estimator of the variance $E(y_{cfi}^2)$ is the sample mean square of $y_{cfi}$:

$$\hat{\sigma}^2 = \sum_{c=1}^{C}\sum_{f=1}^{F_c}\sum_{i=1}^{I_{cf}} y_{cfi}^2 \Big/ \sum_{c=1}^{C}\sum_{f=1}^{F_c} I_{cf}, \tag{5}$$

where $C = 120$ is the number of clusters in the sample, $F_c$ is the number of sample families in cluster $c$, $I_{cf}$ is the number of sample individuals from family $f$ in cluster $c$, and $\sum_{c=1}^{C}\sum_{f=1}^{F_c} I_{cf} = 443$ is the total number of individuals in the sample. If we were working with "balanced" data—for

[12] We wondered whether our results might change if we reversed the sequence in which we average earnings and adjust for stage of life cycle. Therefore, instead of averaging first and then adjusting, we also have tried starting with least squares estimation of the regression of the annual log earnings observations on age, age squared, and year dummies, and then averaging the residuals from that regression over the 5 years. Our results with that alternative earnings variable are virtually identical to those reported in table 2.

example, if every cluster contained two families and every family contained two brothers who met our selection criteria—our formulas for estimating the brother and neighbor covariances would be equally straightforward. But, of course, the number of sample families per cluster and the number of sample brothers per family do vary, and this creates some complications.

To see the issue, imagine at first that we are required to estimate the brother covariance with data from only one family, family $f$ in cluster $c$. Then the obvious estimator of $E(y_{cfi}y_{cfi'})$ is this family's corresponding sample mean $\sum_{i \neq i'} y_{cfi}y_{cfi'}/[I_{cf}(I_{cf}-1)/2]$, where $I_{cf}(I_{cf}-1)/2$ is the family's number of distinct brother pairs. But now suppose we are permitted to use data from all the sample families in the cluster. Then we have as many estimators like the above as we have families, and the question becomes how to combine them. And once we are permitted to use data from all the clusters, we face the similar question of how to combine estimators across clusters.

Following Karlin, Cameron, and Williams (1981), we can take a weighted average of all the family-specific estimators to get a single estimator of the brother covariance:

$$\hat{\Theta} = \sum_{c=1}^{C} W_c \left\{ \sum_{f=1}^{F_c} W_{cf} \left\{ \sum_{i \neq i'} y_{cfi}y_{cfi'}/[I_{cf}(I_{cf}-1)/2] \right\} / \left( \sum_{f=1}^{F_c} W_{cf} \right) \right\} / \sum_{c=1}^{C} W_c, \quad (6)$$

where $W_{cf}$ is the weight assigned to family $f$ in cluster $c$ and $W_c$ is the weight assigned to cluster $c$. The simplest version of this estimator gives equal weight to all families within a cluster and equal weight to all clusters by setting $W_{cf} = W_c = 1$. That estimator is inefficient, though, because it overlooks the fact that families with more brothers and clusters with more families contain more information. Another version, used by Altonji and Dunn (1991), weights in proportion to the number of distinct brother pairs by setting $W_{cf} = I_{cf}(I_{cf}-1)/2$ and $W_c = \sum_{f=1}^{F_c} W_{cf}$. As pointed out by Donner (1986), however, this estimator gives six times as much weight to a family with four brothers as to a family with two brothers, and that weighting is probably excessive. In Solon et al. (2000), we investigated the relative efficiencies of alternative weighting schemes and found that both of the above schemes were usually outperformed by an intermediate scheme that weights by $W_{cf} = \sqrt{I_{cf}(I_{cf}-1)/2}$, the square root of the number of distinct brother pairs. This estimation approach, which gives $\sqrt{6} \cong 2.45$ as much weight to the family with four brothers as to the family with two, is the one we use throughout the present study.[13] Based on similar reasoning, we estimate the neighbor covariance by

$$\hat{\eta} = \sum_{c=1}^{C} W_c \left\{ \sum_{f \neq f'} W_{cff'} [\sum_{i=1}^{I_{cf}} \sum_{i'=1}^{I_{cf'}} y_{cfi} y_{cf'i'} / (I_{cf} I_{cf'})] / \sum_{f \neq f'} W_{cff'} \right\} / \sum_{c=1}^{C} W_c, \qquad (7)$$

with $W_{cff'} = \sqrt{I_{cf} I_{cf'}}$ and $W_c = \sum_{f \neq f'} W_{cff'}$.

## D. Standard Error Estimation

The estimation of standard errors for all the estimators described above is complicated by the sequential nature of our estimation (starting with the preliminary regression of log earnings on age and age squared), the unbalanced structure of the sample, the nonindependence of the (sometimes overlapping) pairs of brothers and neighbors, and the weighting procedures. To finesse all of these issues at once, we resort to the nonparametric approach of balanced half-sample replications. This procedure, which is a cousin to the jackknife and bootstrap procedures, is explained in Wolter (1985) and has been applied previously in sibling and neighbor studies by Solon, Corcoran, Gordon, and Laren (1988), Solon, Corcoran, Gordon, and Laren (1991), and Solon et al. (2000). This approach—described in detail in the appendix—repeatedly applies the entire estimation procedure to a succession of strategically chosen half-samples. Each estimator's observed variance across the half-sample replications is then used to infer an estimate of the variance of that estimator as applied to the full sample.

## E. Results

The first entry in table 2 shows that the sample mean square of our "residualized" log earnings variable $y_{cfi}$ estimates the variance of $y_{cfi}$ at 0.247. The remainder of our analysis is directed toward studying the sources of this earnings variation. First, we use the brother correlation in $y_{cfi}$ as an omnibus measure of the proportion of earnings variation attributable to all the family and community background characteristics shared by brothers. Using the estimator shown in equation (6), we estimate the brother covariance in $y_{cfi}$ to be 0.078. Dividing this by the estimated variance 0.247 yields an estimated brother correlation of 0.32. The numerator of this ratio, however, is necessarily based only on families that contribute at least two brothers to the sample, while the denominator is based on all the sample families. Most previous studies of sibling correlations have treated the numerator and denominator conformably by also basing the variance estimate in the denominator only on families con-

[13] The point estimates in Solon, Page, and Duncan (2000) do not vary greatly across alternative weighting schemes.

**Table 2**
**Estimates and Decompositions of Variances and Covariances**

| | $y$ | $b'D$ | $e$ | 2 × Cross-Covariance between $b'D$ and $e$ |
|---|---|---|---|---|
| Variance | .247 | .043 | .204 | 0 |
| | (.027) | (.012) | (.022) | |
| Brother covariance | .078 | .025 | .038 | .015 |
| | (.028) | (.011) | (.018) | (.010) |
| Neighbor covariance | .040 | .022 | .003 | .014 |
| | (.024) | (.010) | (.011) | (.016) |
| Variance explained by childhood urbanicity and region | .033 | .020 | .002 | .011 |
| | (.011) | (.009) | (.009) | (.008) |

tributing at least two siblings. Doing so with our data slightly raises our variance estimate to 0.253 (with estimated standard error 0.028) and reduces our estimated brother correlation to 0.31 (with estimated standard error 0.09).

The previous literature on earnings correlations among American brothers, surveyed in section 3 of Solon (1999), contains a wide range of estimates, which is unsurprising in light of the small samples on which many of the estimates are based.[14] The central tendency of the estimates seems to be about 0.25. In most of these studies, though, the outcome variable is only a single-year measure of log earnings, and—as emphasized by Solon et al. (1991)—this induces a downward errors-in-variables bias for estimating the brother correlation in longer-run earnings. It therefore was to be expected that our estimate based on a 5-year average of log earnings would come out somewhat higher, and indeed it is fairly similar to most of the other estimates based on multiyear averages. Also using multiyear averages from the PSID, Altonji (1988) estimates brother correlations of 0.37 for the log of average hourly earnings and 0.44 for the log of a directly reported hourly wage rate. Using multiyear averages from the National Longitudinal Surveys of labor market experience, Altonji and Dunn (1991) estimate brother correlations of 0.32 for log earnings and 0.33 for log hourly wage, and Ashenfelter and Zimmerman (1997) estimate a 0.31 brother correlation in log hourly wage.

As far as we know, our study is the first to supplement our evidence on the brother correlation in earnings with parallel evidence for unrelated boys that grew up in the same neighborhood. As shown in the third row of table 2, the estimator in equation (7) generates an estimated neighbor

[14] Interestingly, one of the smallest estimates—the 0.11 estimate reported by Bound, Griliches, and Hall (1986)—is based on a "residualized" wage measure from which urbanicity and region effects have been partialed out.

covariance of 0.040. Dividing by the 0.247 variance estimate yields an estimated neighbor correlation of 0.16 (with estimated standard error 0.09). As it happens, this estimate is quite similar to the estimated neighbor correlations in educational attainment reported in Solon et al. (2000). It is about half of our estimate of the brother correlation in earnings. The neighbor correlation reflects not only true neighborhood effects but also the effects of growing up in somewhat similar families, so this comparison suggests that the majority of the brother correlation stems from growing up in the same family, not from growing up in the same neighborhood.

The neighbor correlation provides an upper bound for the proportion of earnings variation attributable to neighborhood background, but what do our results imply in the metric of "slope effects"? That is, how much would exogenously transporting a child to a better neighborhood increase the child's expected value for $y_{cfi}$? This is a tricky question because we cannot observe all the neighborhood variables underlying $z_c$ in equation (1), but consider the thought experiment of increasing $z_c$ by one standard deviation. Inspection of equation (1) makes clear that, other things equal, this improvement in neighborhood quality would translate into raising the earnings variable $y_{cfi}$ also by one standard deviation in $z_c$. To bound the standard deviation of $z_c$, refer back to equation (4), which shows that the neighbor covariance is the sum of the variance of $z_c$ and two other terms. Also recall that we expect both of those other terms to be positive because advantaged families sort together into advantaged neighborhoods. Since our 0.040 estimate of the neighbor covariance is therefore an estimated upper bound for the variance of $z_c$, we can estimate the upper bound of a one-standard-deviation increase in neighborhood quality as increasing log earnings by $\sqrt{0.040} = 0.20$. Whether that is a surprisingly large impact or a surprisingly small one is in the eyes of the beholder. On the one hand, a 20% earnings increase is substantial. On the other hand, it takes all of a standard-deviation increase in neighborhood quality to generate that increase, and our estimate of that standard deviation is probably too high.

In any case, our point estimate of the neighbor correlation is substantial, and it is worthwhile to investigate its sources. Because Griliches's (1979, p. S38) point about "the likelihood, even in adulthood, of closer location in space and hence exposure to similar regional price differentials and common business-cycle effects" may apply to neighboring boys as well as to brothers, we next explore the degree to which location can explain earnings similarities between brothers and neighbors.

**Table 3**
**Estimated Coefficients (and Standard Errors) in Regressions of Log Earnings on Location Variables**

| Explanatory Variable | Adult Location | Childhood Location |
|---|---|---|
| Large city | .499 | .377 |
| | (.059) | (.062) |
| Small city | .257 | .184 |
| | (.058) | (.052) |
| Northeast | .081 | .066 |
| | (.069) | (.075) |
| North Central | .138 | .086 |
| | (.066) | (.067) |
| South | −.020 | −.094 |
| | (.068) | (.073) |
| $R^2$ | .175 | .135 |

## III. Estimating Region and Urbanicity Effects

### A. Data

To explore the effects of region and urbanicity, we will use information on both adult and childhood location. For each of the PSID's 1987–91 interviews, we know if the individual resided in the Northeast, North Central, South, or West. We measure childhood region with the same classification from the 1968 interview. For each year 1987–91, we also classify adult residence in one of three urbanicity categories: large city (metropolitan areas with population of at least a million), small city (metropolitan areas with population less than a million), or noncity. For urbanicity of childhood location, we use a similar, but slightly different categorization from the 1968 interview: large city (the 12 largest metropolitan areas), small city (other metropolitan areas), or noncity.

### B. The Importance of Adult Urbanicity

The first step in our exploration is to perform least squares estimation of

$$y_{cfi} = b'D_{cfi} + e_{cfi}, \tag{8}$$

where $D_{cfi}$ is a vector containing the 1987–91 averages of dummy variables for the adult region and urbanicity categories described above, $b$ is the associated vector of estimated coefficients, and $e_{cfi}$ is the residual. The region dummy variables are for Northeast, North Central, and South, with West as the omitted category. The urbanicity dummy variables are for large city and small city, with noncity as the omitted category. Because equation (8) does not include a number of variables that may be correlated with $y$ and $D$, $b$ should not be given a structural interpretation; it simply indicates the degree to which earnings vary across geographic areas.

The results appear in the first column of coefficient estimates in table 3.

Like many previous researchers, we estimate moderate region effects on earnings. More strikingly, we find huge effects of urbanicity. The coefficient estimates of 0.499 for large city and 0.257 for small city imply that workers in large cities earn 27% more than workers in small cities, who in turn earn 29% more than workers not in cities.[15] These estimates are consistent with the estimates Glaeser and Mare (2001) and Kim (2002) report for broader PSID samples than ours.[16]

The economics underlying the large earnings gap between urban and nonurban workers is a very interesting topic. An equilibrium interpretation requires answers to two questions: (1) why don't all the workers in the hinterland move to the cities to get higher wages and (2) how can the urban employers pay higher wages and still stay in business? Glaeser and Mare summarize the likely answers, some of which are adapted from the literature on wage differentials across cities.[17] One answer to question 1 is that disamenities of urban life, including the higher housing prices that stem from higher land prices, may deter the marginal worker from moving to the city. Another factor is that the measured urban wage premium may not be fully available to the workers in the hinterland because they are not qualified for it. In at least some occupations, workers with greater talent or motivation may have a comparative advantage for urban employment. According to this story, it is no coincidence that the best basketball player worked in Chicago or that the best dancers work in New York City. Would-be basketball players in the hinterland do not

[15] That is, $\exp(0.499 - 0.257) - 1 = 0.27$, and $\exp(0.257) - 1 = 0.29$.

[16] Using the 1985–91 average of log hourly earnings as his dependent variable, Kim (2002) estimates coefficients of 0.410 for large cities and 0.210 for small cities. This regression, however, controls for education and race as well as region and stage of life cycle. At our request, Kim has reestimated the coefficients without controlling for education and race. Doing so raises the large- and small-city coefficient estimates to 0.535 and 0.281, which are even larger than our estimates. Glaeser and Mare's (2001) estimates for log hourly earnings over the 1968–85 period, again with controls for education and race, are somewhat smaller than Kim's for 1985–91. Kim shows that the discrepancy is due to two factors. First, the urban/nonurban wage gap actually was smaller in 1968–85 than in 1985–91. Second, Glaeser and Mare's estimates are based on applying ordinary least squares to the pooled longitudinal data from their period. This OLS estimator is a weighted average of the "between estimator" (i.e., the cross-sectional estimator applied to time averages, which is what we use) and the "within estimator" (i.e., the fixed-effects estimator that controls for worker-specific dummy variables). Both Kim and Glaeser and Mare report that the within estimator generates dramatically smaller estimates of the urban/nonurban wage gap. There are two reasons for this. First, by controlling for worker-specific fixed effects, the within estimator partials out the impact of skill differences between urban and nonurban workers. Second, the within estimator is notoriously susceptible to attenuation bias from errors in variables (Griliches and Hausman 1986).

[17] See, e.g., Roback (1982), Johnson (1983), and Rauch (1993).

move to Chicago to get Michael Jordan's salary because they would not get it even if they did.

Some evidence is available on the magnitude of the wage premium that urban employers must pay to compensate for higher housing prices. Estimates reported by the National Research Council's Panel on Poverty and Family Assistance suggest that housing prices in the largest cities within a region typically are no more than about double the prices in the least populous areas.[18] Given that the expenditure share for housing is typically about 25%, this suggests that a 25% wage differential between the most and least urban areas would suffice to compensate for the cost-of-living difference. Our estimate of the actual earnings differential, however, is considerably larger than that, suggesting that cost of living is not the entire story. Furthermore, there are two reasons to suspect that the 25% estimate of the cost-of-living difference may be excessive. First, as noted in the National Research Council report,[19] the lower housing costs in less urban areas may be at least partly offset by higher transportation costs. Second, as emphasized by Kaplow (1995) and Glaeser (1998), if high urban housing prices are due at least partly to superior consumer amenities, mechanically deflating for housing costs overadjusts for true cost-of-living differences.

A best guess, then, is that the urban earnings premium is only partly accounted for by cost of living and also stems from a tendency for more productive workers to locate in larger cities.[20] To the extent that the higher urban wages are paid to more productive workers, the ability of urban employers to stay in business is not so puzzling. But how can urban employers afford to pay a wage premium to compensate for a higher cost of living, especially if the employers themselves have to pay the higher land prices as well? As Glaeser and Mare note, these cost disadvantages must be offset by cost advantages such as lower transportation costs, learning externalities, and other economies of agglomeration.

In our view, the relationship between urbanicity and earnings deserves much more attention than it has received to date. For purposes of the present article, however, our concern is the extent to which urbanicity and region effects contribute to the observed brother and neighbor correlations in earnings. For example, what if the neighbor correlation in earnings arises merely because kids in big cities tend to become adults in big cities and, therefore, exhibit positively correlated earnings because they share in receiving the urban wage premium? If this wage premium

[18] Citro and Michael (1995), especially table 3-6. Also see Moulton (1995).

[19] Citro and Michael (1995), p. 185.

[20] This conjecture is consistent with Kim's (2002) and Glaeser and Mare's (2001) evidence, referred to in n. 16 above, that controlling for worker-specific fixed effects dramatically reduces the estimated urban/nonurban wage gap.

were merely a compensating premium for a higher cost of living, the positively correlated nominal earnings would not signify a positive correlation in economic well-being. And, even if the urban wage premium is a return to greater ability or effort, the interpretation of the neighbor correlation still may differ in important ways from what is usually meant by neighborhood effects.

We next investigate the role that adult location plays in earnings correlations between brothers and neighbors. Equation (8) decomposes earnings into a component associated with the region and urbanicity of where the individual lives and an orthogonal component unrelated to region and urbanicity. The orthogonality between the two components $b'D_{cfi}$ and $e_{cfi}$ means that the overall earnings variance can be expressed as the sum of the variances of $b'D_{cfi}$ and $e_{cfi}$:

$$\mathrm{Var}(y_{cfi}) = \mathrm{Var}(b'D_{cfi}) + \mathrm{Var}(e_{cfi}). \tag{9}$$

Similarly, the earnings covariance between brothers can be written as

$$\begin{aligned}\mathrm{Cov}(y_{cfi}, y_{cfi'}) &= \mathrm{Cov}(b'D_{cfi} + e_{cfi}, b'D_{cfi'} + e_{cfi'}) \\ &= \mathrm{Cov}(b'D_{cfi}, b'D_{cfi'}) + \mathrm{Cov}(e_{cfi}, e_{cfi'}) \\ &\quad + 2\,\mathrm{Cov}(b'D_{cfi}, e_{cfi'}),\end{aligned} \tag{10}$$

and the covariance between neighboring boys can be written as

$$\begin{aligned}\mathrm{Cov}(y_{cfi}, y_{cf'i'}) &= \mathrm{Cov}(b'D_{cfi} + e_{cfi}, b'D_{cf'i'} + e_{cf'i'}) \\ &= \mathrm{Cov}(b'D_{cfi}, b'D_{cf'i'}) + \mathrm{Cov}(e_{cfi}, e_{cf'i'}) \\ &\quad + 2\,\mathrm{Cov}(b'D_{cfi}, e_{cf'i'}).\end{aligned} \tag{11}$$

Just as the variance of $y_{cfi}$ can be estimated by its sample mean square, the two variance components in equation (9) can be estimated by the sample mean squares of $b'D_{cfi}$ and $e_{cfi}$. Equivalently, the proportion of the variance in $y_{cfi}$ accounted for by adult residential location can be estimated by the $R^2$ from the regression in equation (8). And all the covariance terms in equations (10) and (11) can be estimated by applying the same brother and neighbor covariance estimators developed in Section IIC to $b'D$ and $e$ instead of to $y$. Note that the last term in equation (10), double the "cross-covariance" between one brother's $b'D$ and the other brother's, is generally nonzero. The least squares normal equations impose orthogonality between the same individual's $b'D$ and $e$ (which is why the last entry in the first row of table 2 is identically zero), but no such orthogonality is imposed across brothers. The same point pertains to the last term in equation (11).[21]

We now return to table 2 to decompose the 0.247 variance in $y_{cfi}$ into components related and unrelated to urbanicity and region effects. In the first row of the table, when we multiply 0.247 by the $R^2$ of 0.175 from the regression of $y_{cfi}$ on $D_{cfi}$, we attribute 0.043 of the variance to the effects of urbanicity and region of adult location. The remaining 0.204 attributed to $e_{cfi}$ is simply the mean squared residual from the regression. Most of the 0.043 attributed to adult location is related to urbanicity, not region. Regressing $y_{cfi}$ on only the city dummies produces an $R^2$ of 0.158; regressing it on only the region dummies produces an $R^2$ of only 0.039.

Next, we come to Griliches's (1979) question of how much of the brother covariance in earnings is connected to a similarity in brothers' adult location. In the second row of table 2, we decompose the brother covariance in $y_{cfi}$ by estimating the three components of the right side of equation (10). We estimate that 0.025 of the 0.078 brother covariance in earnings is connected to the brothers' adult urbanicity and region. This allows considerable scope for the location phenomenon Griliches postulated, and it leaves open the possibility that some of the brother correlation in earnings may result simply from cost-of-living differences across locations combined with a tendency to reside in similar geographic areas. Nevertheless, a larger share of the brother covariance is attributed to the earnings component orthogonal to the adult location variables. Whatever it is about brothers' shared background that leads them to have correlated earnings goes well beyond a tendency to locate in the same region or the same city type.

The pattern in the third row is very different. When we perform the same exercise for decomposing the earnings covariance between boys that grew up in the same neighborhood, almost none of the neighbor covariance is attributed to the component orthogonal to the adult location variables, and a majority share is assigned to the neighbor covariance in $b'D$. "The importance of being urban" appears to loom large in the neighbor covariance of earnings.

There are two reasons to be cautious about drawing conclusions from these estimates. First, some of the estimates are not very precise. Like other studies of neighborhood effects, ours uses a small sample of neighborhoods (120), and this impedes the precision with which we can estimate earnings correlations and their components. Second, estimation error in $b$ will lead

[21] Our point estimates of these cross-covariance terms turn out to be positive (though not statistically significant). One implication, e.g., is that, if neighbor A lives in a big city as a grown-up and neighbor B does not, the fact that neighbor A does is predictive of higher earnings for neighbor B. It is not clear whether this predictive power stems from a neighborhood effect or from similar family effects.

**Table 4**
**Adult Urbanicity in 1989 by Childhood Urbanicity in 1968**

| Childhood Location | Adult Location | | | |
|---|---|---|---|---|
| | Large City | Small City | Noncity | Total |
| Large city | 85 | 11 | 6 | 102 |
| Small city | 40 | 90 | 17 | 147 |
| Noncity | 27 | 63 | 104 | 194 |
| Total | 152 | 164 | 127 | 443 |

us to overadjust for location effects because $\mathrm{Cov}(b'D_{cfi}, b'D_{cf'i'})$ exceeds its population analog by the expectation of $D'_{cfi}V(b)D_{cf'i'}$, where $V(b)$ is the variance-covariance matrix of $b$. Fortunately, this expectation can be estimated by its sample analog, with $V(b)$ replaced by an estimate based on the half-sample replication method discussed in Section IID. Our estimate of this expectation is only 0.003, which suggests that our estimation of $\mathrm{Cov}(b'D_{cfi}, b'D_{cf'i'})$ is not severely biased. Therefore, an important part of the story still seems to be that boys who grow up in the same neighborhood tend to locate as adults in areas of similar urbanicity.

### C. The Importance of Childhood Urbanicity

One possible explanation for our finding that adult urbanicity "explains" much of the neighbor and brother correlations is simply that kids who grow up in cities tend to end up in cities, and kids who grow up outside of cities remain outside of cities as adults. The cross-tabulation in table 4 strongly indicates such a pattern. For example, of the 102 kids in our sample who lived in a large city in 1968, 85 still lived in a large city in 1989. Of the 194 kids who lived in a noncity in 1968, only 27 lived in a large city in 1989. Of the 127 men who lived in a noncity in 1989, 104 had lived in a noncity in 1968.

To assess the role of childhood urbanicity and region in adult earnings variation, we now apply least squares to the regression of our log earnings variable on the childhood instead of the adult location variables. In other words, we estimate

$$y_{cfi} = a'U_{cf} + v_{cfi}, \tag{12}$$

where $U_{cf}$ is a vector of dummy variables representing the childhood region and urbanicity categories described earlier. The results are shown in the last column of table 3. The estimated coefficients of the childhood urbanicity variables are essentially attenuated versions of the estimated coefficients of the corresponding adult variables, reflecting that childhood urbanicity predicts adult urbanicity strongly, but not perfectly. The $R^2$ from the regression on the childhood location variables is 0.135. Again, the explanatory power comes more from the urbanicity variables than from the region variables. The regression on only the urbanicity variables generates an $R^2$ of 0.115; the regression on only the region variables produces an $R^2$ of only 0.058.

We use these results to estimate the portion of the variance in each of the components of equation (8) that can be explained by childhood location, and we report our findings in the last row of table 2. To estimate the portion of the earnings variance explained by childhood urbanicity and region, we multiply $R^2 = 0.135$ by the overall earnings variance of 0.247, which yields an estimate of 0.033. The other entries in the last row are similarly based on the $R^2$s from the regressions of $b'D$ and $e$ on the childhood location variables. Not surprisingly, most of the explanatory power of the childhood location variables is associated with the earnings impact of adult location. Almost none is associated with the earnings component orthogonal to adult location.

Comparison of the last two entries in the column of table 2 indicates that 0.033 out of the 0.039 neighbor covariance is spanned merely by the five explanatory variables indicating whether the individual's childhood neighborhood was in a large city, a small city, or no city and which region it was in.[22] Is that what we usually have in mind when we talk about neighborhood effects? On one hand, if the real story is mainly that neighboring kids, who necessarily share childhood urbanicity, thereby tend to share adult urbanicity, and if the earnings premium associated with living in a city is merely compensation for a higher cost of living, the positive correlation between neighboring boys in their later nominal earnings does not even signify a positive correlation in their economic well-being. On the other hand, the higher earnings received by urban workers probably is at least partly a real return to greater ability or effort. Insofar as growing up in a city somehow imparts greater ability or motivation that later translates into a match with a high-paying urban job, that might be construed as a sort of neighborhood influence on later earnings.

But notice how different that story is from the story usually told about neighborhood effects. The stereotypical account of neighborhood effects is about the advantages of growing up in a wealthy suburb instead of a poor inner-city neighborhood. Our results indicate instead that most of the neighbor correlation is explained by whether or not one grew up in a city, not by which part of the city one grew up in.

## IV. Summary

Using the Panel Study of Income Dynamics, we have replicated the finding of earlier studies that the brother correlation in a multiyear mea-

[22] As is the case when estimating the explanatory power of adult location, estimation error in $a$ may result in overestimation of $\mathrm{Var}(a'U_{cf})$. We estimate that the upward bias is only about 0.005, however, so the resulting biases in the estimates in the fourth row of table 2 are probably not severe.

sure of log earnings exceeds 0.3. The novel feature of our study is that we also have estimated the earnings correlation between unrelated men who grew up in the same neighborhood. Even though that correlation reflects the effects of somewhat similar family backgrounds as well as true neighborhood influences, our estimated neighbor correlation is about half the brother correlation. This suggests that the resemblance between brothers in their later earnings stems more from growing up in the same family than from growing up in the same neighborhood.

Further examination of the neighbor correlation has highlighted "the importance of being urban." Like Glaeser and Mare (2001) and Kim (2002), we find huge earnings differentials among workers in large cities, small cities, and noncities. We also find that childhood location is a strong predictor of adult location. In combination, these facts account for a substantial part of the earnings correlation between brothers, but a larger share is attributed to the earnings component orthogonal to the region and urbanicity variables. In contrast, the main reason that childhood neighborhood predicts future earnings is that the urbanicity of that neighborhood predicts the urbanicity of adult location. While this might be interpreted as a sort of neighborhood influence, it is quite different from what writers on neighborhood effects usually seem to have in mind. The portion of earnings inequality that is connected to where one grew up has more to do with whether one grew up in a city than with which part of the city one grew up in.

Although which part of town one grew up in seems to play a limited role in accounting for population-wide earnings variation, it still may matter greatly for some children growing up in extreme neighborhood environments or with special sensitivity to their environments. Evidence from the ongoing Moving to Opportunity project may eventually shed more light on that possibility.

## Appendix

### Balanced Half-Sample Replications

To facilitate half-sample replications, the Institute for Social Research has characterized the PSID sample as consisting of two independent "primary selections" from each of 32 strata. The pair of selections in the *k*th stratum might be, say, the PSID samples from the Milwaukee and Minneapolis areas. A half-sample composed of only one selection from each of the 32 strata more or less duplicates the complex survey design of the PSID, but at only about half the size.

We use the 32 × 32 Hadamard matrix on page 325 of Wolter (1985) to select a set of 32 "balanced" half-samples. For any parameter $\mu$, if $\hat{\mu}$

denotes the estimate from the full sample and $\hat{\mu}_k$ the estimate from the $k$th half-sample, we estimate the variance of $\hat{\mu}$ with

$$\hat{\mathrm{Var}}\,(\hat{\mu}) = \sum_{k=1}^{32} (\hat{\mu}_k - \hat{\mu})^2/32.$$

Why is this a sensible estimator of $\mathrm{Var}\,(\hat{\mu})$? Let $\hat{\mu}_{k'}$ denote the estimate of $\mu$ from the complement of the $k$th half-sample, and suppose $\hat{\mu} = (\hat{\mu}_k + \hat{\mu}_{k'})/2$ , as is exactly true if $\hat{\mu}$ is a linear estimator and is likely to be approximately true otherwise. Then, for any arbitrary half-sample $k$,

$$\begin{aligned} E\,(\hat{\mu}_k - \hat{\mu})^2 &= E\,[\hat{\mu}_k - (\hat{\mu}_k + \hat{\mu}_{k'})/2]^2 \\ &= E\,[(\hat{\mu}_k - \hat{\mu}_{k'})/2]^2 \\ &= E\,(\hat{\mu}_k - \hat{\mu}_{k'})^2/4 \\ &= E\,[(\hat{\mu}_k - \mu) - (\hat{\mu}_{k'} - \mu)]^2/4 \\ &= 2\ \mathrm{Var}\,(\hat{\mu}_k)/4 \\ &= \mathrm{Var}\,(\hat{\mu}_k)/2 \\ &= \mathrm{Var}\,(\hat{\mu}). \end{aligned}$$

Thus, for any particular half-sample $k$, the squared deviation of $\hat{\mu}_k$ from $\hat{\mu}$ is an approximately unbiased estimator of $\mathrm{Var}\,(\hat{\mu})$. The point of taking 32 different half-samples and averaging the squared deviations of the $\hat{\mu}_k$ from $\hat{\mu}$ is to improve the precision of the variance estimator. The optimal method of choosing "balanced" half-sample replications is discussed in detail in Wolter (1985).

## References


Altonji, Joseph G. "The Effects of Family Background and School Characteristics on Education and Labor Market Outcomes." Unpublished manuscript. Yale University, 1988.

Altonji, Joseph G., and Dunn, Thomas A. "Relationships among the Family Incomes and Labor Market Outcomes of Relatives." *Research in Labor Economics* 12 (1991): 269–310.

Ashenfelter, Orley, and Zimmerman, David J. "Estimates of the Returns to Schooling from Sibling Data: Fathers, Sons, and Brothers." *Review of Economics and Statistics* 79 (February 1997): 1–9.

Benabou, Roland. "Equity and Efficiency in Human Capital Investment: The Local Connection." *Review of Economic Studies* 63 (April 1996): 237–64. (*a*)

———. "Heterogeneity, Stratification, and Growth: Macroeconomic Implications of Community Structure and School Finance." *American Economic Review* 86 (June 1996): 584–609. (*b*)

Bound, John; Griliches, Zvi; and Hall, Bronwyn H. "Wages, Schooling and IQ of Brothers and Sisters: Do the Family Factors Differ?" *International Economic Review* 27 (February 1986): 77–105.

Bound, John, and Solon, Gary. "Double Trouble: On the Value of Twins-Based Estimation of the Return to Schooling." *Economics of Education Review* 18 (April 1999): 169–82.

Card, David. "Earnings, Schooling, and Ability Revisited." *Research in Labor Economics* 14 (1995): 23–48.

Citro, Constance F., and Michael, Robert T., eds. *Measuring Poverty: A New Approach*. Washington, DC: National Academy Press, 1995.

Corcoran, Mary; Gordon, Roger; Laren, Deborah; and Solon, Gary. "The Association between Men's Economic Status and Their Family and Community Origins." *Journal of Human Resources* 27 (Fall 1992): 575–601.

Corcoran, Mary; Jencks, Christopher; and Olneck, Michael. "The Effects of Family Background on Earnings." *American Economic Review* 66 (May 1976): 430–35.

Donner, Allan. "A Review of Inference Procedures for the Intraclass Correlation Coefficient in the One-Way Random Effects Model." *International Statistical Review* 54 (April 1986): 67–81.

Durlauf, Steven N. "A Theory of Persistent Income Inequality." *Journal of Economic Growth* 1 (March 1996): 75–93.

Fitzgerald, John; Gottschalk, Peter; and Moffitt, Robert. "An Analysis of the Impact of Sample Attrition on the Second Generation of Respondents in the Michigan Panel Study of Income Dynamics." *Journal of Human Resources* 33 (Spring 1998): 300–334.

Glaeser, Edward L. "Should Transfer Payments Be Indexed to Local Price Levels?" *Regional Science and Urban Economics* 28 (January 1998): 1–20.

Glaeser, Edward L., and Mare, David C. "Cities and Skills." *Journal of Labor Economics* 19 (April 2001): 316–42.

Griliches, Zvi. "Estimating the Returns to Schooling: Some Econometric Problems." *Econometrica* 45 (January 1977): 1–22.

———. "Sibling Models and Data in Economics: Beginnings of a Survey." *Journal of Political Economy* 87 (October 1979): S37–S64.

Griliches, Zvi, and Hausman, Jerry A. "Errors in Variables in Panel Data." *Journal of Econometrics* 31 (February 1986): 93–118.

Jencks, Christopher S., and Brown, Marsha D. "Effects of High Schools on Their Students." *Harvard Educational Review* 45 (August 1975): 273–324.

Jencks, Christopher, and Mayer, Susan E. "The Social Consequences of Growing Up in a Poor Neighborhood." In *Inner-City Poverty in the United States*, edited by Laurence E. Lynn, Jr., and Michael G. H. McGeary, pp. 111–86. Washington: National Academy Press, 1990.

Johnson, George E. "Intermetropolitan Wage Differentials in the United States." In *The Measurement of Labor Cost*, edited by Jack E. Triplett, pp. 309–30. Chicago: University of Chicago Press, 1983.

Kaplow, Louis. "Regional Cost-of-Living Adjustments in Tax-Transfer

Schemes." Working Paper no. 5008. Cambridge, MA: National Bureau of Economic Research, 1995.

Karlin, Samuel; Cameron, Edward C.; and Williams, Paul T. "Sibling and Parent-Offspring Correlation Estimation with Variable Family Size." *Proceedings of the National Academy of Sciences USA* 78 (May 1981): 2664–68.

Kim, Bonggeun. "The Wage Gap between Metropolitan and Non-metropolitan Areas." Chap. 2 in "Essays on Earnings, Employment and Location." Ph.D. dissertation, University of Michigan, 2002.

Kremer, Michael. "How Much Does Sorting Increase Inequality?" *Quarterly Journal of Economics* 112 (February 1997): 115–39.

Kunz, Jim; Page, Marianne E.; and Solon, Gary. "Are Point-in-Time Measures of Neighborhood Characteristics Useful Proxies for Children's Long-Run Neighborhood Environment?" *Economics Letters* 79 (May 2003): 231–37.

Manski, Charles F. "Identification of Endogenous Social Effects: The Reflection Problem." *Review of Economic Studies* 60 (July 1993): 531–42.

Moulton, Brent R. "Interarea Indexes of the Cost of Shelter Using Hedonic Quality Adjustment Techniques." *Journal of Econometrics* 68 (July 1995): 181–204.

Murray, Charles. *Losing Ground: American Social Policy, 1950–1980*. New York: Basic, 1984.

Page, Marianne E., and Solon, Gary. "Correlations between Sisters and Neighboring Girls in Their Subsequent Income as Adults." *Journal of Applied Econometrics* (in press).

Rauch, James E. "Productivity Gains from Geographic Concentration of Human Capital: Evidence from the Cities." *Journal of Urban Economics* 34 (November 1993): 380–400.

Rivkin, Steven G.; Hanushek, Eric A.; and Kain, John F. "Teachers, Schools and Academic Achievement." Working Paper no. 6691. Cambridge, MA: National Bureau of Economic Research, 1998.

Roback, Jennifer. "Wages, Rents, and the Quality of Life." *Journal of Political Economy* 90 (December 1982): 1257–78.

Solon, Gary. "Intergenerational Mobility in the United States." *American Economic Review* 82 (June 1992): 393–408.

———. "Intergenerational Mobility in the Labor Market." In *Handbook of Labor Economics*, vol. 3A, edited by Orley C. Ashenfelter and David Card, pp. 1761–1800. Amsterdam: North-Holland, 1999.

Solon, Gary; Corcoran, Mary; Gordon, Roger; and Laren, Deborah. "Sibling and Intergenerational Correlations in Welfare Program Participation." *Journal of Human Resources* 23 (Summer 1988): 388–96.

———. "A Longitudinal Analysis of Sibling Correlations in Economic Status." *Journal of Human Resources* 26 (Summer 1991): 509–34.

Solon, Gary; Page, Marianne E.; and Duncan, Greg J. "Correlations between Neighboring Children in Their Subsequent Educational Attainment." *Review of Economics and Statistics* 82 (August 2000): 383–92.

Tiebout, Charles M. "A Pure Theory of Local Expenditures." *Journal of Political Economy* 64 (October 1956): 416–24.

Wilson, William Julius. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*. Chicago: University of Chicago Press, 1987.

Wolter, Kirk M. *Introduction to Variance Estimation*. New York: Springer-Verlag, 1985.