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Sibling Similarity and Difference in Socioeconomic Status

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Abstract

Previous researchers have examined the effect of unmeasured family background on a variety of socioeconomic outcomes, such as educational attainment, welfare usage, and earnings among men. That research has used a variety of data sources and measurement techniques to arrive at estimates of the similarity between siblings in these outcomes. The current paper reviews this work and extends this line of inquiry by considering sisters in addition to brothers, by considering wealth in addition to income, by examining differences in sibling correlations across population subgroups, and by examining age-cohort differences in correlations across these population subgroups. Given the important role that women now occupy in the labor market and the overall system of economic stratification, it is important to document sister associations on a full range of outcome measures. Likewise, wealth is now taken to be a key component of socioeconomic status, so documenting sibling correlations in net worth is important in describing the degree of economic mobility in U.S. society. Finally, differences in sibling correlations in SES by demographic subgroups imply—but do not necessarily confirm—potentially different processes by which advantaged and disadvantaged families interact with the social structure of opportunity in the wider society. Results show that the sibling correlation among sisters is higher across the board than among brothers. Sibling correlations for wealth are similar to those for income. Finally, a mixed pattern regarding the relationship between level of disadvantage—measured through race, family size, and mother's education—and sibling resemblance emerges from comparisons without regard to cohort effects. However, analyses of only the most recent cohort of Americans show a clearer pattern, relating a disadvantaged background to greater sibling discordance. Net worth consistently demonstrates greater sibling resemblance among more disadvantaged families, perhaps reflecting a floor effect. The paper concludes with a discussion of the implications of these findings for stratification research and estimation of family background effects.

Sibling Similarity and Difference in Economic Status

INTRODUCTION

How similar are the socioeconomic statuses of siblings, and what does this tell us about the impact of family background in contemporary America? The current study attempts to document the degree of sibling resemblance in a number of components of socioeconomic status as well as a composite index. It also aims to determine for *whom* family background matters most. Its fivefold goals are the following: to update previous sibling correlations in socioeconomic status using the latest waves of the Panel Study of Income Dynamics (PSID); to add wealth to the mix (since household wealth has been shown to be important in a number of recent analyses, particularly with respect to race—see, e.g., Oliver and Shapiro 1995; Conley 1999; Keister 2000); to add sisters to the equation, a notable omission given the increased labor force participation of women in recent decades; to examine whether sibling resemblance varies by demographic subpopulation; and, finally, to examine whether sibling resemblance across these subpopulation groups has intensified or weakened over the past decades, and as individuals have aged.

The influence of family background or socioeconomic status on the outcomes of offspring is one of the most central concerns of stratification research in particular and sociology in general. Yet, as Hauser and Sewell have stated, "Nowhere has a research agenda of such substantive importance had to survive on such meager scraps of data" (1976: quoted in Solon et al. 1991: 512). Likewise, as Becker and Tomes (1986) write, "while sociologists have taken the lead in estimating the impact of family background on a number of outcomes—most notably educational and occupational attainment—they have lacked a clear explication of an underlying behavioral model." The current paper tries to address both of these concerns.

With respect to data, the current analysis represents a marked improvement over previous work. Here, we analyze data from the 1983 to 2001 waves of the PSID, a resource that provides a number of cases that dwarfs those used in previous sibling research (on income, at least). These waves of the PSID also provide data on individuals that are substantially older than those used by previous researchers (and who therefore may have much more stable earnings profiles). Also, the PSID allows the examination of a number of outcomes all in one data set: education, occupation, income, and wealth. Finally, the longitudinal nature of the data allows for estimates that are free of bias due to autocorrelative processes or random year-to-year fluctuation in socioeconomic status.

In addressing theoretical concerns, we make contributions by testing some of the implications of the Becker-Tomes (1986) model of human capital investment in offspring (to be discussed below). We also test for different implicit models of parental investment by comparing maximized status of siblings versus average status and by examining sibling correlations in residuals that have been purged of such other ascriptive characteristics as race and gender. Below we will discuss the underlying conceptions of social sorting that each of these approaches addresses, but first we briefly review some of the previous sociological and economic work on the impact of family background.

PREVIOUS WORK ON THE IMPACT OF FAMILY BACKGROUND

As summarized in Table 1, much of the earlier research on the impact of family background focuses on outcomes of educational attainment, occupational prestige, and the relationship between the two. Jencks (1972), drawing on a sample of white, nonfarm males from the Occupational Changes in a Generation (OCG) survey, examines the relationship between family background, which is defined as including but not synonymous with family economic status, and several outcomes, including test scores, educational attainment, and occupational status and earnings. Jencks finds a correlation of .55 between a white son's educational attainment and his father's income, and a correlation of less than .50 between a father's occupational status and that of his son. While he finds the effect of family background on a son's occupation to be stronger than that of father's income, family background is not found to be the decisive factor in predicting the son's occupational status, as family background, test scores, and educational attainment together at most account for half of the variance in men's occupational statuses. Further, in

TABLE 1
Results of Previous Studies

Authors	Outcome	Data Set	Findings	Limitations
Jencks, 1972	• Test scores, educational attainment, occupational status, earnings	• 1962 OCG	• Correlation between son's educational attainment and father's income=.55 • Correlation between educational attainment and occupational status=.65 • 15 percent of variation in income attributable to family background	• Model examines only linear relationships and not interactions • Disparate data sources
Hauser and Featherman, 1976	• Educational attainment	• 1962, 1973 OCG	• 2/3 of variance in length of schooling may be attributable to family influences • 55 percent of effect of family background on schooling explained by measured variables	• Do not conduct separate analysis of sibling resemblance for blacks and whites • Women represented only through husbands
Bielby et al., 1977	• Educational attainment, occupational status	• 1973 OCG	• Ignoring response error leads to an underestimation of the occupational returns to schooling by about 15 percent for nonblack men, and by about 30 percent for black men	• Lack of data on women
Jencks et al., 1979	• Test scores, educational attainment, occupational status, earnings	Includes: • NORC • 1973 OCG-II • Kalamazoo • Project Talent • NLS • PSID	• As corrected, family background explains 48 percent of variance in occupational status and 15–35 percent of variance in log earnings • Measured variables explain 22.6 percent of variance in occupational status and 8.9 percent of variance in log income	• Conclusions drawn from eleven separate samples • Lack of data on women
Benin and Johnson, 1984	• Educational attainment among sibling pairs	• Lincoln, NB, 1976 • 1982 Nebraska Annual Social Indicators Survey	• Residual covariance between siblings' educations differs significantly across sibling combinations • OB-YB=2.6, OS-YB=.63	• Sample restricted to state of Nebraska, children in geographically nonmobile families in stable marriages • Does not estimate cross-sibling effects on educational attainment

(table continues)

TABLE 1, continued

Authors	Outcome	Data Set	Findings	Limitations
Hauser and Sewell, 1986	• First occupational status; current occupational status	• WLS • Kalamazoo	• No evidence that effects of family background lead to bias in effect of mental ability on schooling or in effects of schooling on occupational status or earnings • Family background explains 27 percent variance in earnings, 49 percent in ability, and 46 percent in schooling	• Based on one annual observation of earnings, wages
Hauser and Wong, 1989	• Educational attainment	• Kalamazoo • Lincoln • Nebraska Annual Social Indicators Survey	• Effect of common family factor on younger sibling is 70 percent of the effect on older sibling • Common family factor accounts for 60 percent of the variance in individual educational attainment • Common variance in educational attainment in OB-YB pair is 67 percent and 49 percent respectively (NASIS), and 44 percent and 31 percent respectively (Lincoln)	• Nonrepresentative samples • Model of sibling reciprocal effect tested only on brothers • Impact of family factor on sibling pairs limited to educational attainment
Kuo and Hauser, 1995	• Educational attainment	• 1973 OCG	• Effects of measured and unmeasured family background variables on educational attainment of blacks and whites have declined • Family background accounts for at least half the variance in educational attainment; measured variables explain no more than half the common family effect	• Survey lacks measured ability of respondent/brothers • Males only; the oldest and youngest of his brothers are selected • Data set doesn't allow for understanding of changing effects in recent years
Teachman et al., 1995	• Grades	• HSB, 1980	• Family background affects grades of older and younger siblings differently • 43–48 percent of the variance in common family factor explained by measured variables	• Unique factors reduce model's ability to explain mental ability of older siblings in opposite-sex pairs

(table continues)

TABLE 1, continued

Authors	Outcome	Data Set	Findings	Limitations
Solon et al., 1988	• Brother correlation in permanent component of log earnings	• PSID	• Woman's probability of receiving welfare .20 if sister has not received welfare, .66 if her sister has • Woman's probability of being persistent welfare recipient is .12 if her sister is not a persistent recipient, .56 if her sister is	• Estimation problems, including small size of siblings sample
Solon et al., 1991	• Permanent earnings	• PSID	• Failure to distinguish between permanent and transitory income has underestimated effect of family background on earnings • Correlation of brothers' permanent log earnings is .448	• Small number of families in sample, with multiple siblings
Corcoran et al., 1992	• Income	• PSID	• Correlation between log family income and welfare income= -.52, between log family income and percent on welfare in zip code= -.53	• Use of zip code as unit of analysis may not accurately capture community
Mazumder and Levine, 2003	• Earnings	• NLS /PSID	• Correlation between brothers' earnings=.45 in 1979; up from .26 in 1966	• Mean age of respondents under 30 in all cohorts/datasets
Charles and Hurst, 2004	• Wealth	• PSID	• Parent-child correlation in wealth .37 before transfers	• Parent-child correlation, not siblings

examining the relationship between family background and income, Jencks concludes that only an estimated 15 percent of the variation in income can be attributed to family background.

Hauser and Featherman (1976), focusing on several socioeconomic background variables, find that the total inequality in the distribution of schooling among men has declined during the twentieth century. They also find a decline in the variability in schooling attributable to differences in social background, as well as variability independent of social background for white, black, and Hispanic minority groups. Thus, while the effects of certain background variables on educational attainment have lessened, other family socioeconomic conditions, specifically father's education, father's occupation, and sibship size, have remained unchanged in their impact on variability in educational attainment. Yet Hauser and Featherman note that the impact of social background on schooling exists in a way construed more broadly than they have specified; thus for the cohorts they examine, though at least half of the variance in schooling is attributable to the effect of family background, only 55 percent of this effect can be explained by the identified social background variables.

Bielby, Hauser, and Featherman (1977) examine the extent to which measurement error may have led to an underestimation of the effects of socioeconomic variables on educational attainment and economic status for black and nonblack men and an overestimation of the amount of socioeconomic achievement that cannot be attributed to background variables. While the direction of bias for both nonblack and black men is the same for the outcomes examined, the magnitude of the bias is greater for blacks in each instance. For example, while occupational returns to schooling for nonblacks may be underestimated by 15 percent, for black men this figure may be underestimated by as much as 30 percent. This suggests a tendency to overestimate differences between blacks and nonblacks, both in returns to schooling and in terms of the portion of socioeconomic achievement that cannot be attributed to the influence of background variables.

Jencks et al. (1979) use a variety of surveys to reexamine many of the earlier conclusions in Jencks (1972) and ultimately disagree with the earlier finding that background characteristics exert minimal influence on men's occupational status and earnings. Using the OCG-II, they estimate that the survey's ten measured demographic variables explain 22.6 percent of the variance in occupational status and 8.9 percent of the variance in natural logarithm earnings. Using these figures (as well as adding religion as a variable), correcting for random error, and assuming that brothers do not influence each other, Jencks et al. estimate that, were demographic characteristics of both brothers to be precisely the same, the measured family background would explain 48 percent of the variance in occupational status and 15 to 35 percent of the variance in natural logarithm earnings. Since realistically it is unlikely that this set of criteria will be met, Jencks et al. use the Kalamazoo and Project Talent data to explore the difference in explanatory power between demographic variables and unmeasured family background, concluding that measured family background variables can explain roughly two-thirds of the resemblance between brothers.

Benin and Johnson (1984), using data from two Nebraska surveys, examine the difference in resemblance of educational attainment among four combinations of male and female sibling pairs (older brother-younger brother, older sister-younger sister, older brother-younger sister, older sister-younger brother). They find that residual covariances do differ significantly across the four sibling combinations, with the OB-YB pair having the highest covariance of residuals, at 2.6, and the OS-YB pair with the lowest covariance of residuals, at .63. They attribute the differing levels of resemblance to an unusually high level of fraternal resemblance, which they posit is a function of intersibling effects. Hauser and Wong (1989), however, reexamine the data used by Benin and Johnson and conclude that the variance in similarity of educational attainment across sibling pairs is due not to an unusually high resemblance between brothers, but rather to an unusually low resemblance between the OS-YB pair; that is, the common unmeasured family background factors affect the other three pairs to a greater degree than they affect the OS-YB pair. From the Kalamazoo data they find that the common family factor may account for as much as 60 percent of the variance in individual educational attainment, although the Lincoln and Nebraska Annual Social Indicators Survey data yield lower estimates of the variance explained in the OB-

YB pair, 67 and 49 percent and 44 and 31 percent, respectively. Additionally, using the Kalamazoo data, they find that a reciprocal influence exists in the OB-YB pair, although, given the nature of the data set, they are unable to ask whether a reciprocal influence exists in the three other sibling pairs, nor do they examine intersibling effects on other outcomes, such as occupational status or economic success.

Given the importance of schooling to socioeconomic outcomes, Hauser and Mossel (1985) and Hauser and Sewell (1986) seek to determine the extent to which the relationship between educational attainment and socioeconomic outcomes is actually causal, or whether family background biases the apparent effect of schooling on economic attainment. Hauser and Mossel argue that the effects of family background on social and economic achievement are not adequately addressed by the measures generally employed in models to encompass family background, and that a failure to control for common family factors may lead to findings of a more robust relationship between schooling and economic attainment than is warranted. Using pairs of brothers from the Wisconsin Longitudinal Study, they find that whereas family membership accounts for half of the variance in schooling and over a third of the variance in occupational status, family background does not affect the influence of schooling on occupational status. Similarly, Hauser and Sewell (1986), using data from Wisconsin as well as Kalamazoo, find no net family bias in terms of the effect of mental ability on schooling, mental ability on socioeconomic outcomes, or schooling on socioeconomic outcomes. They do find, however, that family background has an independent effect on earnings (27 percent of the variance in annual earnings), though to a lesser degree than the effects found for ability (49 percent) and schooling (46 percent), and that an important portion of the between-family variance in these variables is unexplained by socioeconomic background.

Kuo and Hauser (1995) extend the examination of educational attainment among black and white men to include the impact of family beyond the social and economic variables generally utilized. They find that for black and white men born in the first half of the twentieth century, effects of measured and unmeasured family background variables have declined, though for blacks the decline has been among all measured family background variables, and they find that for white men the effect of only certain variables, specifically farm background, intact family, and Southern birth, have declined. Kuo and Hauser also find that family background accounts for at least half of the variance in educational attainment, and that their measured variables, including parental schooling, father's occupational status, and size of sibship, explain at most half of the common family effect.

Teachman (1995a) and Teachman, Carver, and Day (1995), in advocating the use of sibling pairs as a means of estimating family influence both within and between families, also address the point that many of the commonly accepted measures of family background, such as parental education and income, may not be the most appropriate means for estimating how the family influences the educational attainment of its children. Teachman et al. (1995) look at the effect of family background on mental ability and grades and find that 43 to 48 percent of the variance in common family factor is explained by the family background variables that are measured, while over 50 percent of the variance cannot be explained by standard socioeconomic variables. Additionally, in terms of estimating the effects of educational attainment on income, Teachman (1995b) suggests that families may affect their children's income in ways not directly tied to schooling, perhaps leading to inaccurate conclusions as to the nature of the relationship between education and income.

More recently, sociologists and economists have begun to examine explicitly economic outcomes, such as earnings. Solon et al. (1991) address the impact of family background on economic attainment of brothers, and to a lesser extent, sisters. By distinguishing between permanent and transitory income, they find higher correlations for brothers' log wages and income than previous studies have yielded. In terms of log earnings, they estimate a correlation of .448 for brothers' permanent status, as opposed to the .248 correlation that does not distinguish between permanent and transitory components of earnings, and a correlation of .276 for women's log family income. Their findings suggest that the effect of family background on permanent earnings is greater than previously thought, that family background may exert a stronger influence on the likelihood of upward mobility than previous findings suggest, and that earlier studies may have underestimated the effect of family background on income variables by using single-year income data.

Corcoran et al. (1992) also examine the effects of welfare receipt, both at the family and community levels, on the log earnings of black and white men, finding a correlation between log family income and welfare income of -.518 and a correlation between log family income and the percentage of the community (measured by zip code) receiving welfare of -.530. Additionally, the correlation between log family income and the interaction between the family's welfare income and the percentage of the community receiving welfare is -.019. Finally, Solon et al. (1988) examine the sibling (sister) similarity in likelihood of welfare receipt and find that between the time of leaving home and the age of 27, a woman's probability of receiving welfare is .20 if her sister has not received welfare and .66 if her sister has received welfare. Similarly, the probability of a woman's persistent recipient of welfare is .12 if her sister does not receive welfare persistently and .56 if her sister does.

Mazumder and Levine (2003), using data from the 1966 and 1979 National Longitudinal Surveys,[1] find that not only are family influences[2] higher in determining economic outcomes than previous studies have yielded, but they also find a significant increase in correlation between brothers' earnings between the two cohorts. For the more recent cohort, .45 of the variance in earnings can be attributed to family background, an increase of .19 from the 1966 cohort. And, since they find no increase in correlation in brothers' years of education between cohorts, they conclude that this increase in earnings correlation cannot be attributed to higher returns to education.

Finally, one recent paper addresses wealth, in contrast to income. However, Charles and Hurst (2003) examine (age-adjusted) parent-child correlations, not sibling correlations. In this respect the paper

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[1]They advocate in favor of the NLS and against the PSID because "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." However, in contrast to the NLS, the PSID provides better data on wealth.

[2]They use the term "family and community influences." However, since they appear to be answering the same question, we just use "family."

is answering a slightly different question from that addressed in the current study with respect to wealth. Parent-child correlations may be affected by the change in the distribution of wealth across cohorts to a much greater extent than are sibling correlations.

TEMPORAL SHIFTS IN THE IMPACT OF FAMILY BACKGROUND

The general consensus among stratification researchers has been that the impact of family background on a variety of outcomes has declined over the course of the twentieth century. In fact, the putative waning significance of family background with the arrival of industrial capitalism and modernity has been one of the main themes of stratification theory (DiPrete and Grusky 1990; Erikson and Goldthorpe 1992; Goldthorpe 1980; Grusky and Hauser 1984; Hout 1989; Kaelble 1981). The rise of meritocracy has been shown empirically through increases in both exchange and structural intergenerational mobility (see, e.g., Erikson and Goldthorpe 1992; Goldthorpe 1980; Hout 1984, 1988; Sobel, Hout, and Duncan 1985) and the increased salience of schooling and ability over ascriptive characteristics (Blau and Duncan 1967; Featherman and Hauser 1978; Hauser and Huang 1997). However, while empirical analysis has shown that the impact of measured parental variables may be declining, it is less certain whether the unmeasured component of family background is also waning (see, e.g., Hauser and Featherman 1976). In fact, at least one study finds that sibling correlations in income may have increased between the 1960s and 1970s (Mazumder and Levine 2003).

Changing dynamics of the American economy and family life add to this empirical uncertainty, raising questions about whether such trends toward universalism are continuing in contemporary American society—that is, whether the total impact of family background is still declining. First, on a cross-sectional basis, inequality has increased. Income and wealth levels have become increasingly polarized in recent decades (see Atkinson, Rainwater, and Smeeding 1995; Danziger and Gottschalk 1995; Danziger, Sandefur, and Weinberg 1994; Lichter 1997; Lichter and Eggebeen 1993; Marmor, Smeeding, and Greene 1994; Smeeding, O'Higgins, and Rainwater 1990; Wolff 1996; Ryscavage 1999). When viewed through an intergenerational lens, such increases in inequality will result in either greater intrafamily differences in SES or greater interfamily differences, or both. How “the great U-turn”—as this polarization is often called (Harrison and Bluestone 1981; Nielsen and Alderson 1997)—plays out across generations is an empirical question that can only be answered by analysis of the newer cohorts of Americans.

Second, as inequality measures have risen, families have become more fluid (see, e.g., Cherlin 1992; Furstenberg and Cherlin 1991; Gerson 1993). “Family background” may have become more difficult to measure, not less important. For instance, father’s occupation may have less impact on children’s outcomes, but this may be compensated for by greater maternal, grandparental,[3] or even community-level effects that cannot be detected in father-son correlations. Put another way, this decline in the influence of measured variables may result from increased misspecification of “background.”[4] Finally, assortive mating may have increased in extent while women have entered the labor force in greater numbers, thereby redoubling financial and cultural advantages for certain families (Kalmijn 1991; Qian and Preston 1993). Increased social sorting may be taking place before children are even born as high SES women and men select each other with increasing precision.

The current study represents an effort to update these trends and to determine whether the impact of family background is indeed waning for all Americans, or waning for only certain subgroups of Americans.

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[3]Warren and Hauser (1997) examined patterns of social stratification across three generations using data from the Wisconsin Longitudinal Study (WLS). They find no effects of grandparents using direct measurement of grandparental variables, but they do not model latent, unmeasured effects of extended family (i.e., compare cousins net of sibling resemblance).

[4]However, families have also become smaller, suggesting the possibility of less regression to the mean and therefore more dissimilarity between individuals from the same family of origin (Blake 1984); in fact, Kuo and Hauser (1997) show that in the WLS there is more heterogeneity in small sibships than large.

THE PRESENT STUDY

The current study makes several important contributions to the literature, specifically by including sisters in sibling economic correlations, by examining wealth in addition to income and earnings, by utilizing the most recent waves of PSID data, by examining subgroup differences within the population, and by examining subgroup differences for the most recent cohort of Americans. The current study makes several contributions to the literature, specifically by including sisters in sibling economic correlations, by examining wealth in addition to income and earnings, by utilizing the most recent waves of the PSID data, by examining subgroup differences within the population, and by examining subgroup differences for the most recent cohort of Americans. We note at the outset that any observed cohort differences may also be due to differences between older and younger samples, as the PSID and our methodology do not allow us to isolate cohort from age effects. Previous studies have been limited in their explanatory power due in part to their tendency to use all-male, young, and/or localized samples (e.g., WLS; Kalamazoo; and Lincoln, Nebraska). The current study benefits from its use of the PSID, in that it both allows for the inclusion of sisters and avoids the limitations inherent in a sample that is not nationally representative and/or includes only young adults.

The inclusion of sisters is important not just because of the extent to which women have become active participants in the labor force; rather, as previous studies (e.g., Benin and Johnson, Hauser and Wong) have shown, resemblance may differ across the four types of sibling pairs. Furthermore, even studies that include correlations for sisters and for economic variables, such as Solon et al. (1991), focus on income rather than accumulated family wealth. This is an important distinction, as wealth is more unequally distributed than income, and by not taking wealth into account, previous studies have missed a significant amount of variation in families' level of economic resources (Conley 1999).

In terms of the relative importance of family background for each of these measures, we can envision two general patterns that might emerge. The first would suggest a declining sibling correlation as we move across outcomes that increasingly pertain to older stages of the life cycle. In this scenario, we would expect the highest sibling correlation for education, followed by occupation (since this may, to a great extend, reflect the legacy of decisions and tradeoffs made relatively early in adulthood), then followed by income, and finally by net worth (which typically starts to vary most in later adulthood). Alternatively, it could be the case that siblings' social statuses "converge"—i.e., there are multiple routes to economic success and it is the downstream economic measures (the ultimate rewards) that are maximized by parents. In other words, parents may invest in the education of academically oriented offspring, but in the business ventures of those who are not educationally inclined. This dynamic would suggest that we might observe lower degrees of sibling resemblance in educational level and occupational prestige, but higher levels for income and, ultimately, wealth. In contrast to the life stage hypothesis, we call this potential dynamic the "it doesn't matter how you get there, it's where you end up" hypothesis.

Last but not least, this paper makes a contribution by examining differences in the strength of sibling resemblance among various subgroups in the population. We split the sample on a number of dimensions, including the sex mix of the siblings; race (black and nonblack); maternal education (completed 12 or more years of schooling and completed less than 12 years of schooling); family size (three or fewer children in the family and four or more children in the family); maternal age at birth of child (less than 27 years old and 27 years or older); and sibling spacing (years between youngest and oldest siblings in the sample is four or fewer and years between youngest and oldest siblings in the sample is greater than four years). There is a theoretical logic guiding the choice of these dimensions along which to cleave our data: the motivating hypothesis is that among families that are disadvantaged, we should observe greater sibling disparities (i.e., lower correlations), as previous qualitative work (Conley 2004) has suggested that among disadvantaged households, sibling disparities tend to increase, since limited opportunities and resources may evince parenting strategies that accentuate sibling differences by directing family resources to the one (or few) sibling(s) for whom upward mobility is most likely. This research also suggested that among families that were well-endowed with class resources (and were racially privileged as well), parents often invested more heavily in those offspring they saw as having the worst chances for success in the education system and/or labor market—in a compensatory fashion. Put another way, disadvantaged families were seen to be behaving efficiently (investing more in the offspring for whom they expect higher returns), thus reinforcing sibling differences, whereas better-off families appeared to be behaving inefficiently (investing more in the child for whom they expect lower returns), thus compensating, i.e., trying to bring about more equity in the outcomes of offspring.

With this in mind, we hypothesize that blacks have lower sibling correlations than nonblacks; that siblings whose mothers have completed fewer years of education have lower correlations in SES than do those whose mothers have completed more years of schooling; and that those with larger families have lower sibling correlations than do those with smaller families. Of course, the empirical observation of such a pattern does not constitute sufficient evidence to confirm the theory about differences in parental investment. It could also reflect other dynamics, such as differential distributions of ability within families of various subpopulations; the greater or lesser influence of outside forces, such as school or peer effects (to the extent that they differ across siblings); or differences in measurement error across subpopulations. That said, rejection of this hypothesis would cast serious doubt on the qualitative inference that different parenting strategies exist (or at least that they have the consequences noted).

This hypothesis stands in contrast to the theoretical prediction of Becker and Tomes (1986), who posited that with capital constraints, low-income parents may not be able to optimally invest in their children's human capital. Such underinvestment may lead to higher degrees of sibling resemblance at lower incomes since "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" (Mazumder and Levine 2003: 16). Indeed, this is what Mazumder and Levine find: lower correlations among high-income siblings in both the 1968 and 1979 waves of the PSID. However, when they split the sample along the median, they end up with only a maximum of 185 multiple sibling sets in an income group (in the 1979 wave of the PSID, they have 1,086 cases from 901 families in all). Even if these were all two-sibling sets, then the maximum number of pairs for each income group would be 92—quite a small number. More important, however, is the fact that they do not split the sample based on the parental characteristics—as would be appropriate for the Becker-Tomes model—but rather by the incomes of the adult siblings themselves. This makes the sample split endogenous to their outcomes. In other words, what they may be observing could be a result of sibling decisions regarding tradeoffs between equity and efficiency. If certain sibships value equality, they may sacrifice the attainment of the better-endowed sibling, bringing the overall mean down, but resulting in a higher correlation between siblings. By contrast, we split the sample by parental measures, which are at least temporally anterior to the sibling outcomes.

DATA AND VARIABLES

The PSID began in 1968 with a nationally representative sample of 5,000 American families and has followed them each year since. Needless to say, it is a complicated study design and cannot be done justice in the space allowed here. For a fuller description, see Hill (1992) or Duncan and Hill (1989). By virtue of this complex design, the study has information on the socioeconomic histories of families as well as on the outcomes of multiple children from the same families who were in the original sample, moved into it, or were born to sample members. We select adult respondents ages 25 and older who were head or wife of their household in any (or all) years between 1983 and 2001. Further, these individuals had to have a valid person number for their mother; that is, their mother had to have been in the sample at some time. They were then linked to their siblings through this maternal connection. A trivial number (less than 1 percent) of respondents had a father in the sample but not a mother. The majority had both parents. But since many more of the fathers were missing, we decided to identify siblings based on their mother's identification. This said, results are not statistically different if we rely on the father's identification or include only those who have both parents' sample identifiers. The reason we truncate the person-years at 1983 is that prior to that year "wives" were classified differently: there was no category for cohabiting women (what the PSID subsequently called "wife" in quotes, relationship-to-head code 22). Further, prior to 1981, occupation—one of our key dependent measures—was not coded in the same way (it was coded in a one- or two-digit format in contrast to the standard three-digit census classification used consistently from 1981 onward). For both of these reasons—and because of the desire to have a relatively more mature sample than previous researchers—we truncate the survey years at 1983.

We examined a number of socioeconomic outcomes and split the sample on a number of different dimensions. The measures that we used to capture siblings' socioeconomic statuses are described below; the unit of analysis is the person-year. (Mean values—which generally conform to national averages—are presented in Table 2.)

Education: This is measured as total years of formal schooling completed—a continuous variable from 1 to 17, with the topcode representing any graduate work, regardless of whether a degree was received (the PSID does not, unfortunately, distinguish between various levels of graduate schooling).

Occupational Prestige: This is measured as a Hodge-Siegel-Rossi prestige score (see, Hodge, Siegel, and Rossi 1964).

Family Income: We tested a number of formulations of income including logged and unlogged forms; income-to-needs ratios and straight income; and total household income as well as individual income. We present sibling correlations for total household income (logged to the base e).

Household Wealth: This variable is taken from the 2001, 1999, 1994, 1989, and 1984 waves of the PSID. As was the case with income, we tried a number of different formulations. The highest sibling correlation appeared to be for the natural logarithm of total net worth minus equity in the main family home—with siblings who had zero or negative values set to zero.

Socioeconomic Status: We then tested the sibling correlation on a measure of the "class" or socioeconomic status of the respondents. We examined two formulations of this concept: the first was the standardized average of the standardized values of education level, occupational prestige, and income (natural logarithm). The second measure added wealth to this measure, but since wealth is only available for select years, the sample size of person-years for this measure is much lower. The rationale behind this approach is that individuals may be maximizing a latent variable called "class" or "status"; in doing so, they may be making tradeoffs between the various components of social status listed above which lead to lower sibling correlations for each individual measure, while preserving a high degree of social reproduction in the overall latent measure of “class” or “social status.” For example, one sister may take a

TABLE 2
Descriptive Statistics: Means (Standard Deviations, and Within-Family Standard Deviations Below)

Variable:	Education: Two or More Sibling Families 1983–2001	Education: Two or More Sibling Families 1997–2001	Occupational Prestige: Two or More Sibling Families 1983–2001	Occupational Prestige: Two or More Sibling Families 1997–2001	Ln Total Family Income: Two or More Sibling Families 1983–2001	Ln Total Family Income: Two or More Sibling Families 1997–2001	Ln Total Net Worth: Two or More Sibling Families 1983–2001	Ln Total Net Worth: Two or More Sibling Families 1997–2001
Education	13.61	13.65						
	2.26	2.22						
	0.28	0.00						
Occupational Prestige			43.91	43.77				
			14.48	14.26				
			7.41	5.59				
Ln Total Family Income					10.52	10.97		
					1.32	1.69		
					1.05	1.30		
Ln Net Worth (minus home equity)							8.70	9.31
							3.97	4.00
							2.35	1.56
Mixed Sex	0.64	0.64	0.63	0.62	0.63	0.62	0.64	0.63
	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Brothers Only	0.18	0.18	0.29	0.30	0.27	0.26	0.25	0.23
	0.39	0.39	0.46	0.46	0.44	0.44	0.43	0.42
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Sisters Only	0.18	0.18	0.07	0.08	0.10	0.11	0.11	0.15
	0.38	0.39	0.26	0.27	0.30	0.32	0.31	0.35
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

(table continues)

TABLE 2, continued

	Education		Occupational Prestige		Ln Total Family Income		Ln Total Net Worth	
Variable:	Two or More Sibling Families 1983–2001	Two or More Sibling Families 1997–2001	Two or More Sibling Families 1983–2001	Two or More Sibling Families 1997–2001	Two or More Sibling Families 1983–2001	Two or More Sibling Families 1997–2001	Two or More Sibling Families 1983–2001	Two or More Sibling Families 1997–2001
Age	36.39	41.89	36.58	41.68	36.75	42.10	38.40	42.69
	7.41	6.79	7.25	6.67	7.55	6.97	7.89	6.81
	4.93	1.55	4.64	1.49	4.76	1.46	5.05	0.92
Black	0.07	0.07	0.07	0.08	0.08	0.08	0.08	0.08
	0.25	0.25	0.26	0.27	0.27	0.27	0.27	0.27
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Sibling Age Range <5 Yrs.	0.60	0.60	0.61	0.61	0.60	0.60	0.60	0.60
	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Age of Mother at Birth of Child	27.24	27.21	27.32	27.21	27.27	27.22	27.28	27.25
	5.61	5.63	5.41	5.46	5.48	5.51	5.50	5.58
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Mother's Educational Attainment	11.86	11.97	11.92	12.00	11.85	11.95	11.88	11.96
	2.68	2.65	2.64	2.59	2.68	2.65	2.68	2.65
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of Siblings within Family	4.24	4.18	4.16	4.10	4.22	4.15	4.20	4.14
	2.16	2.14	2.15	2.08	2.20	2.13	2.18	2.11
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Number of Person-Years	25,554	7,108	15,277	4,440	18,144	5,363	5,041	2,468
Number of Individuals	1,777	1,484	1,388	1,025	1,871	1,567	1,767	1,537

high-prestige occupation that pays little, in contrast to her sister who takes a low-prestige job that pays a lot of money. Under one conceptual model, at least, they would be considered equivalent in socioeconomic status, having comparatively maximized different aspects of SES. An alternative way to think about this composite measure is that each of the individual measures may be subject to a large degree of measurement error and that by averaging them, we reduce the errors-in-variables bias in the correlation between siblings.

Sample Splits: The sample is divided along the lines of race (black versus nonblack); maternal education (12 or more years of schooling versus less than 12 years of schooling); maternal age at birth of sibling (mother is less than 27 years old versus mother is at least 27 years old); sibship size (less than four siblings in sibship versus at least four); sibship age spacing (less than five years between the youngest and oldest versus more than five years); and by the sex mix of the sibship (all female versus all male, and mixed sex versus same sex).

Age-Cohort Split: To examine whether subpopulation effects have intensified or weakened for the most recent and older cohort of Americans, we compare subpopulation effects for the 1997–2001 cohort to subpopulation effects for the entire 1983–2001 sample.

STATISTICAL APPROACH

The general approach we take to estimate the sibling resemblance is a variance decomposition method, following the strategy for income used by Mazumder and Levine (2003) and Solon el al. (1991). However, we also consider several variations on their approaches to estimating the sibling correlation in social and economic status (which we will describe below). The total variance of the outcome, Y_{ij}, can be expressed as:

$$\sigma_{\varepsilon}^{2} = E(\overline{\varepsilon} - \varepsilon_{ijt})^{2} \tag{1}$$

This total variance can be decomposed into the sum of expected values of three components (as shown in equation 2 below): the between-family component in permanent status (that is, the difference between the family means and the grand mean), a within-family component (differences between the mean of the *i*th individual's status in family *j* from the mean for family *j*), and a within-subject component (the transitory component of income or wealth; that is, the differences between a given year's income or wealth and the mean for that individual). For our single-year measures—the maximized values—the third component essentially drops out of the equation.

$$\sigma_{\varepsilon}^{2} = \mathrm{E}[(\bar{\varepsilon}_{i} - \bar{\varepsilon})^{2} + (\bar{\varepsilon}_{ij} - \bar{\varepsilon}_{j})^{2} + (\varepsilon_{ijt} - \bar{\varepsilon}_{ij})^{2}] \tag{2}$$

Multiplying this out gives us the well-known formula that the total variance equals the sum of the three variance components minus two times their respective covariances.

$$\begin{aligned}\sigma_{\varepsilon}^{2} &= \mathrm{E}(\bar{\varepsilon}_{j} - \bar{\varepsilon})^{2} + \mathrm{E}(\bar{\varepsilon}_{ij} - \bar{\varepsilon}_{j})^{2} + \mathrm{E}(\varepsilon_{ijt} - \bar{\varepsilon}_{ij})^{2} \\ &- 2\mathrm{E}(\bar{\varepsilon} - \bar{\varepsilon}_{j})(\varepsilon_{ij} - \bar{\varepsilon}_{j}) - 2\mathrm{E}(\bar{\varepsilon}_{ij} - \bar{\varepsilon}_{j})(\varepsilon_{ijt} - \bar{\varepsilon}_{jt}) - 2\mathrm{E}(\bar{\varepsilon} - \bar{\varepsilon}_{ij})(\varepsilon_{ijt} - \bar{\varepsilon}_{jt})\end{aligned} \tag{3}$$

Like others before us (e.g., Solon et al. 1991 and Mazumder and Levine 2003), we will proceed (for now) on the assumption that the covariance of between-family, between-sibling, and within-sibling differences is zero. This assumption is akin to positing:

A. No relationship between where your family is on the distribution and how unstable your income (or occupation, education or wealth) is,…

B. No relationship between where you are relative to your family mean and how unstable your status is, and…

C. No relationship between where your family is in the distribution and the degree of similarity among you and your siblings.

We will revisit lemma C in the analysis of sibling resemblance by race and SES; but in the meantime, with no covariance by design, the total variance in total SES can thus be represented merely as a sum of the three variance components:

$$\sigma_{\varepsilon}^{2} = \sigma_{a}^{2} + \sigma_{u}^{2} + + \sigma_{v}^{2} \tag{4}$$

where σ_{a}^{2} is the explained variance between families, and σ_{u}^{2} is the unexplained (or within-family variance in permanent status), and σ_{v}^{2} is the variance in individual economic characteristics (or transitory SES). This assumption of zero covariance—not discussed thoroughly elsewhere—makes the variance decomposition possible and results in a sibling correlation in permanent status according to equation 5, below.

$$\rho = \frac{\sigma_a^2}{\sigma_a^2 + \sigma_u^2} \tag{5}$$

We can also estimate ϕ, the proportion of permanent status that is captured in a single-year measure:

$$\phi = \frac{\sigma_a^2 + \sigma_u^2}{\sigma_a^2 + \sigma_u^2 + \sigma_v^2} \tag{6}$$

We present estimates of sibling correlations in socioeconomic status (ρ) using several approaches:

1. We decompose the raw (unadjusted) value on a given measure for those families with two or more siblings with at least one valid person-year each in the sample.

2. The second approach includes persons who do not also have a sibling in the sample (something that both our predecessors also do); while these lone children do not affect the family effect, they affect the "denominator," in that they affect the total variance. If they display a systematically greater (or lesser) variance than do multiple sibling respondents, then their inclusion or exclusion may affect the proportion of variance explained by the family (i.e., the sibling effect).

3. The third approach takes the maximum value of the given measure for that sibling in all the person-years for that sibling—and by design collapses the person-year analysis into person-level analysis, eliminating the "transitory" component of our measures. The rationale behind this sensitivity analysis is that perhaps siblings' average or permanent status is not what is maximized by parents (or by the siblings themselves), but rather the highest attained status on any given measure. It could be the case, for example, that siblings make sacrifices at different points over the life cycle in order to benefit each other's status, but in the process create an apparently inverse influence on each other, depressing the correlation in permanent status. If this were the case, a focus on the maximum attained income or wealth, for example, might show a higher correlation than permanent measures.

4. In our desire to compare sibling resemblance across outcomes, it is necessary to standardize the measures in some way. We do this by calculating sibling rank order correlations (Spearman's r). This takes away issues of scale or variance and instead conceptualizes each outcome as a ladder with the same number of rungs spaced the same width apart. This approach is also useful in adjusting for year effects—i.e., different degrees of total variance (i.e., inequality) by year.

5. Next, we follow our predecessors' lead and estimate sibling resemblance in a residual purged of life cycle and cohort effects. To obtain the residual, the first step is to estimate the following regression:

$$y_{ijt} = \beta \ X_{ijt} + \varepsilon_{ijt} \tag{7}$$

where the outcome for a given sibling *j* in family *i* in year *t* is shown above as y_{itj}. The vector X_{ijt} includes dummy variables for respondent's age and survey years to account for career effects and for the timing of exogenous economic shocks.

6. The next approach is a variation on number 5. Here not only do we purge the residual of life cycle and cohort effects, we also purge it of mean race and gender differences (and the interactions between race and gender). Implicit in this approach is a model that posits separate markets for white women, black women, white men, and black men. In other words, if by taking out the mean effect of being a black man, for example, sibling correlations decline between black brothers, it suggests that race dynamics are such that blacks have a degree of socioeconomic mobility that is constrained by overall racial inequality writ large in society.

7. We then weight our residuals purged of life cycle and cohort effects to see if sample weights affect the correlation in a significant way.

8. And finally, for the occupational prestige, income, and socioeconomic status measures that may demonstrate autocorrelative processes—leading to an underestimation of sibling resemblance—we also estimate family effects for the residuals adjusted for first-order serial correlation. To relax the assumption of no serial correlation in income or wealth, we follow Solon et al. (1991) and posit the following autoregressive process for income/wealth in a given year for person *j*:

$$v_{ijt} = \lambda v_{ij,t-1} + z_{ijt} \tag{8}$$

where z is a nonautocorrelated residual and the variance of v_{ijt} can now be expressed as:

$$\sigma_v^2 = \sigma_z^2 / (1 - \lambda^2) \tag{9}$$

If we substitute this equation back into our original variance decomposition (equation 4) we get the following:

$$\varepsilon_{ijt} - \hat{\lambda} \varepsilon_{ij,t-1} = (1 - \lambda) a_i + (1 - \lambda) u_{ij} + z_{ijt} \tag{10}$$

The variance of this estimate can then be decomposed in the same way as the simpler model (equation 4) was, into: $(1-\lambda)\sigma_a^2 + (1-\lambda)\sigma_u^2 + \sigma_z^2$. To estimate λ, we follow Solon et al.'s (1991) lead and regress the difference in residuals at time t and t-1 by the difference between residuals at t-1 and t-2. The resulting regression coefficient represents a parameter estimate of the first-order serial correlation which can be expressed as (λ – 1)/2. Solving for λ gives us a predicted value for λ of $\hat{\lambda} = 1 + 2r$.

Standard error estimates for all models are obtained by bootstrapping.

FINDINGS

Table 3 presents our analysis of the entire sample of PSID siblings. Beginning with education, we find that the PSID data yield a correlation of .576 for years of schooling for all sibling sets. To put this in the context of other previous work, earlier studies such as Hauser and Wong (1989) have suggested that family background accounts for roughly 50 percent of the variation in educational attainment, while Kuo and Hauser (1995) put the figure closer to 60 percent. When we include children who are alone in the sample as well, we find that the figure jumps to .706. We think that this reflects the different natures of the two samples. When we compare those lone siblings with the two-plus group, we find that their standard deviations are indeed higher for the education (and age) variables. The group with one respondent per family is significantly older and significantly less educated (results not shown).

When we use only the maximum value attained in any person-year (something that would make sense for education more than for, perhaps, income or wealth), we instead find that the value is slightly lower, at .567. This suggests that siblings' average education over the life course is more similar than their ultimate education received. Given that our respondents are at a minimum 25 years old in a given year, this indicates that there must be some later-life degree attainment that is less related to family background effects (of course, the difference between these two measures is really trivial and not statistically significant). At the very least, it casts doubt on the idea that siblings trade off the timing of their schooling to achieve a relatively equal maximum level in the end. When we purge the education variable of age and cohort effects, we find that the sibling correlation holds relatively steady at .570, and when we weight this analysis, it does not change significantly. Purging the variable of age, cohort, and mean gender and race effects lowers the sibling correlation to .554. We do not adjust for autocorrelation for this measure, since it is hard to envision such a dynamic at work with the highest level of schooling attained.

In terms of occupational prestige, we find a correlation of .411. When we include lone children this correlation declines slightly, to .407. Purifying the measure of age and cohort effects yields a

TABLE 3
PSID Sibling Correlations: 1983–2001 Waves (Number of Person-Years, Number of Individuals, and Number of Families Below)

	Education	Occ. Prestige	Ln Income	Ln Net Worth (minus home equity)	Class Position I (educ., occ., ln income)	Class Position II (+ ln net worth)
Actual Value	0.576	0.411	0.458	0.371	0.612	0.605
	25,554	15,277	18,144	5,041	14,808	3,899
	1,777	1,388	1,871	1,871	1,297	1,194
	780	705	806	806	672	645
Actual Value Including Lone Children	0.706	0.407	0.599	0.291	0.616	0.611
	63,122	28,077	43,338	11,653	15,988	4,210
	4,119	2,586	4,151	3,924	1,399	1,288
	3,022	1,849	2,984	2,861	726	695
Maximum Value	0.567	0.354	0.063	0.179	0.429	0.519
	N/A	N/A	N/A	N/A	N/A	N/A
	1,777	1,388	1,871	1,871	1,297	1,194
	780	705	806	806	672	645
Rank Order Value	0.560	0.439	0.346	0.371	0.616	0.623
	25,554	15,277	18,144	5,041	14,808	3,899
	1,777	1,388	1,871	1,767	1,297	1,194
	780	705	806	788	672	645
Residual Purified of Age and Year	0.570	0.409	0.458	0.347	0.614	0.602
	25,554	15,277	18,144	5,041	14,808	3,899
	1,777	1,388	1,871	1,767	1,297	1,194
	780	705	806	788	672	645
Residual Purified of Age, Year, Race, Sex, and Race-Sex Interaction	0.554	0.398	0.470	0.334	0.595	0.570
	21,717	13,765	15,921	4,462	13,331	3,566
	1,450	1,127	1,528	1,459	1,045	998
	712	633	735	717	601	587
Weighted Residual Purified of Age and Year	0.570	0.406	0.472	0.332	0.611	0.596
	25,186	15,032	17,803	4,881	14,627	3,829
	1,756	1,359	1,835	1,712	1,279	1,171
	776	699	799	778	670	642
Residual Purified of Age and Year Adjusted for 1st Order Autocorrelation	N/A	0.512	0.270	N/A	0.451	N/A
		12,781	15,873		12,343	
		1,247	1,409		1,171	
		667	716		636	

correlation similar to the raw value correlation, and weighting the analysis of that residual actually lowers the correlation slightly, to .406. As with education, purifying the occupational prestige measure of age, cohort, mean gender, and mean race effects lowers the sibling correlation, but only by a small degree. Similar to the analysis for education, using the maximum value of respondents' occupational prestige actually lowers the sibling correlation, suggesting that siblings' average occupational correlations are more similar than their peak occupational status. Accounting for autocorrelation in job prestige actually increases the correlation to .512.

In terms of income, our results diverge from the most recent analyses of Solon et al. (1991) and Mazumder and Levine (2003). Our correlation in the raw value (to the base e) is .458. Including lone children increases, as with education, the correlation to .599, suggesting that the incomes of lone individuals vary less substantially than do those of multiple sibling families. Most interesting is that when we use the maximization standard—examining the sibling correlation in the maximum log-family income over the time period—we find that the sibling correlation drops to .063. Other researchers have shown that sibling correlations in permanent income were substantially higher than sibling correlations in single-year measures. We add to this by showing that selecting each sibling's maximum income lowers the correlation even more dramatically, suggesting a lot of individual-level variability that has little to do with the family (at least the family as an institution that instills equity among offspring). When we purify the income measure of age and cohort effects, the correlation stays the same, and when we add to this a purification of race and gender effects, the correlation increases to .470. Weighting this analysis raises it back to .472. As was the case for occupation, correcting for negative autocorrelation significantly reduces the correlation.

When we examine net worth, we find that in the raw score (natural logarithm of net worth with zero or negative values set to zero) yields a lower correlation than the corresponding value for income: .371. When we include lone children, it lowers to .291; using the maximum value for each sibling's person-years lowers the correlation to .179. This is a much less dramatic drop than the drop for income, perhaps reflecting the more stable nature of wealth over time. When we purge the measure of age and cohort effects, the correlation is lower, at .347, and lowers even more, to .334, when we purge the measure of age, cohort, race, and gender effect. Weighting it lowers it still, to .332. Since wealth is measured intermittently in the PSID, we were not able to correct for negative first-order serial correlation in the data. Now that the PSID documents wealth during every wave (even if those waves are now every other year) future researchers will be able to estimate a λ value for net worth and adjust the sibling correlation upward.

The next two columns show results for global measures of socioeconomic status. The first column shows the standardized combination of education, occupation, and income. The sibling correlation for this measure is .612, higher than for any of the single indicators alone. When we include lone individuals in the sample, this stays relatively the same, at .616. Using the maximum attained SES drops the correlation to .429, while using the residual leaves it virtually unchanged, at .614. When we purify the variable from race and gender effects as well as age and cohort effects, the correlation drops to .595. Weighting this residual again leaves it unchanged, at .611. Finally, accounting for serial autocorrelation results in a value of .451. When we consider the second measure of global socioeconomic status—the one that includes net worth as well as education, occupation, and income (last column)—we find that the sibling correlation is slightly lower than it was for the measure that excluded wealth, at .605. Keep in mind, however, that the sample size of person-years is much lower (3,899 versus 14,808) given that wealth was included in only select waves. Including lone children raises this correlation to .611, while using the maximum value reduces it to .519. The residual demonstrates a correlation of .602, and the weighted residual .596. Since wealth is included in this measure, it was not feasible to correct for negative first-order serial correlation.

The rank order model allows us to compare sibling correlations across outcome. Here we see that the composite measures of SES, the measure that includes only income, education, and occupational prestige, and the measure that adds wealth to the index, yield the greatest sibling correlations, at .616 and

.623 respectively. Education and occupation follow this, at .560 and .439. Siblings' correlations in income and wealth are quite similar, at .346 and .371 respectively.

Next we examine sibling correlations by subgroup. The sample splits that we employ result in relatively sparse data, and for some of our splits, we cannot distinguish between family effects and sibling effects. In these cases of extreme data sparseness, we do not report results. Further, because of the sparseness of our data, bootstrapping to derive standard errors of our estimates does not yield accurate results. We therefore treat findings from these subpopulation splits as indicative of, but not confirming overall trends. In most cases, differences between subpopulation groups are probably not significant, and we use the magnitude of such differences to shape our discussion of findings.

We first split the sample by the sex mix of the respondents. (Note: for these subgroup comparisons, we use the raw values, though using residuals or any of the other formulations does not change the results. We also exclude lone children from the sample.) In the first two rows of Table 4, we compare same-sex respondent groups and mixed-sex respondent groups. This does not necessarily reflect the sex composition of the entire sibship, rather just those that found their way into the sample. We find that across all measures same-sex sibships reveal greater similarity, such that family background explains over 60 percent of schooling levels for same-sex sibships. More interesting is that when we split the same-sex group along gender lines, we find that sisters demonstrate greater sibling resemblance than do brothers. The magnitude of these differences is relatively low, except for wealth, where brothers have a correlation of .299 and sisters have a correlation of .675. This means that family background explains almost three-quarters of SES—so measured—for sisters, an issue we will return to in the discussion section below.

Moving to the next panel of Table 4, we split the sample by demographic and economic characteristics. The first split we pursue is by race: black respondents versus nonblack respondents. Here we find mixed results. Differences between SES based on measures of education, occupation, and composites are relatively small. However, differences based on income and wealth are substantial. Sibling

TABLE 4
PSID Sibling Correlations: 1983–2001 Waves, by Subgroups (Number of Person-Years, Number of Individuals, Number of Families Below)

	Education	Occ. Prestige	Ln Income	Ln Net Worth (minus home equity)	Class Position I (educ., occ., ln income)	Class Position II (+ ln net worth)
Mixed Sex	0.556	0.381	0.404	0.349	0.594	0.575
	16,350	9,679	11,484	3,204	9,377	2,471
	1,127	884	1,180	1,120	827	762
	366	365	369	368	352	343
Same Sex	0.631	0.505	0.565	0.429	0.662	0.683
	9,204	5,598	6,660	1,837	5,431	1,428
	650	504	691	647	470	432
	414	340	437	420	320	302
Difference	*-0.075*	*-0.124*	*-0.161*	*-0.080*	*-0.067*	*-0.108*
Brothers Only	0.607	0.506	0.550	0.299		
	4,686	4,474	4,847	1,276		
	331	350	363	349		
	214	223	233	227		
Sisters Only	0.651	0.550	0.573	0.675		
	4,518	1,124	1,813	561		
	319	154	328	298		
	200	117	204	193		
Difference	*-0.043*	*-0.043*	*-0.023*	*-0.376*		
Black	0.548	0.490	0.158	0.538	0.597	0.589
	1,447	985	1,248	352	938	258
	108	102	120	113	90	83
	59	56	62	56	52	49
Nonblack	0.571	0.396	0.525	0.331	0.608	0.584
	20,270	12,780	14,673	4,110	12,393	3,308
	1,342	1,025	1,408	1,346	955	915
	655	579	675	663	551	539
Difference	*-0.023*	*0.094*	*-0.367*	*0.207*	*-0.011*	*0.006*
Mother Has Less Than 12 Years Education	0.547	0.486	0.466	0.549	0.651	0.694
	7,026	3,915	4,982	1,364	3,816	993
	507	384	526	481	361	323
	216	186	220	212	177	167
Mother Has 12+ Years Education	0.477	0.309	0.359	0.277	0.508	0.489
	18,528	11,362	13,162	3,677	10,992	2,906
	1,270	1,004	1,345	1,286	936	871
	564	519	586	576	495	478
Difference	*0.070*	*0.177*	*0.107*	*0.272*	*0.143*	*0.204*

(table continues)

TABLE 4, continued

	Education	Occ. Prestige	Ln Income	Ln Net Worth (minus home equity)	Class Position I (educ., occ., ln income)	Class Position II (+ ln net worth)
Sibling Age Range <5 Years	0.529	0.295	0.150	0.202	0.534	0.532
	13,394	8,085	9,498	2,626	7,850	2,077
	946	739	983	919	695	637
	374	352	375	370	340	328
Sibling Age Range 5+ Years	0.621	0.425	0.505	0.351	0.664	0.664
	9,015	5,262	6,293	1,752	5,108	1,335
	598	465	631	604	434	400
	173	169	174	174	164	160
Difference	*-0.093*	*-0.131*	*-0.355*	*-0.149*	*-0.130*	*-0.132*
Mother < 27 at Birth	0.555	0.474	0.377	0.392	0.639	0.645
	16,274	9,712	11,553	3,205	9,403	2,484
	1,151	893	1,204	1,128	835	769
	629	554	645	628	532	504
Mother 27+ at Birth	0.594	0.342	0.533	0.295	0.605	0.600
	9,280	5,565	6,591	1,836	5,405	1,415
	626	495	667	639	462	425
	389	336	409	399	315	295
Difference	*-0.039*	*0.132*	*-0.156*	*0.098*	*0.034*	*0.045*
Large Family	0.555	0.365	0.487	0.395	0.595	0.602
	14,135	8,105	9,812	2,711	7,876	2,066
	974	743	1,018	957	701	643
	320	295	328	322	285	277
Small Family	0.552	0.437	0.417	0.322	0.606	0.566
	11,419	7,172	8,332	2,330	6,932	1,833
	803	645	853	810	596	551
	460	410	478	466	387	368
Difference	*0.002*	*-0.072*	*0.069*	*0.072*	*-0.011*	*0.035*

resemblance among blacks is much lower than it is for nonblacks on the income measure (ρ = .158 and ρ =.525 respectively), while sibling resemblance among blacks is much higher than it is for nonblacks on the wealth measure (ρ = .538 and ρ = .331). We will return to this dynamic in the discussion below.

Is race acting as a proxy for parental socioeconomic status in our data? When we split the sample by the mother's educational attainment, we find that for entire sample of cohorts, this may be the case. Our findings, however, work in the opposite direction of our hypothesis, as families where mother's educational attainment is less than high school yield higher sibling correlations. We will return to this issue as well in the discussion below.

In the last panels of Table 4, we contrast subgroups based on the demographic characteristics of the family: sibship age range (less than five years versus five or more years); the age of the mother (less than 27 years at the time of birth of the respondent versus age 27 and older); and sibship size (fewer than four children born to the respondents' mother versus four or more). Most of these differences are small in magnitude. For the maternal age and sibship age range splits, not only are the differences rather small, but the sign of the difference between the subgroups is not consistently in one direction across measures. However, the sibship age spread analysis yields substantial differences between the two groups across all measures: respondents from families where all siblings are within four years of age have substantially lower correlations than respondents from families where children are more spread out. For income, the difference is substantial, with ρ = .150 for concentrated sibships and ρ = .505 for diffuse sibships. For occupational prestige and net worth, ρ = .295 and ρ = .202 respectively for concentrated sibships, and ρ = .425 and ρ = .351 for diffuse sibships.

These subpopulation splits reveal mixed findings regarding our hypothesis of greater sibling discordance among siblings with disadvantaged social backgrounds. We have found that disadvantage measured through sibship concentration leads to greater sibling discordance. However, disadvantage measured through mother's educational attainment leads to greater sibling concordance. Findings from splits along race and family size are inconclusive in that the signs of the differences across measures are not uniform and the differences are small in magnitude. In the following analysis, we explore the relationship between family disadvantage and sibling discordance for the most recent and older cohort of Americans, and results do indeed confirm our hypothesis of sibling discordance among disadvantaged families. Further, a comparison of findings from all cohorts to findings based on the most recent cohort indicate that processes by which advantaged and disadvantaged families interact with the wider social structure of opportunity have shifted.

We note first in Table 5 that sibship sex composition effects have not changed for this recent cohort, as siblings from mixed-sex families experience greater discordance, with the exception of income. For income, the correlation among mixed-sex siblings is high, at $\rho = .712$. This means that for mixed-sex siblings, family background explains almost three-quarters of current income. For same-sex siblings, the correlation among siblings in income is much lower, at $\rho = .328$. Sample splits by all brothers and all sisters result in data too sparse to derive accurate estimates.

Moving to the next panels of Table 5, we split the sample by race and other measures of family disadvantage. Unlike findings reported in Table 4, which did not disentangle age-cohort effects, here we see that black siblings are less like each other than nonblack siblings, except for wealth. Comparing findings from this most recent cohort to findings from all cohorts, we see an attenuation in the effects of mother's educational attainment on sibling resemblance. For most measures, there is less than a 10 percent difference in sibling correlations between families where maternal educational attainment is less than 12 years and families where maternal educational attainment is 12 or more years. A comparison of these results to Table 4, where differences in sibling correlations by maternal education are all over 10 percent (save education), indicates a substantial waning of the effects of this specific measure of social disadvantage on sibling concordance. Like maternal educational attainment, we see a shift in the effects of family size on sibling resemblance, such that siblings from larger families are more discordant than siblings from smaller families (except for income). And again, the magnitude of these differences can be contrasted to the magnitude of the differences for the entire cohort analysis, so that we see general

TABLE 5
PSID Sibling Correlations: 1997–2001 Waves by Subgroups (Number of Person-Years, Number of Individuals, Number of Families Below)

	Education	Occ. Prestige	Ln Income	Ln Net Worth (minus home equity)	Class Position I (educ., occ., ln income)	Class Position II (+ ln net worth)
Mixed Sex	0.516	0.301	0.712	0.287	0.577	0.530
	4,524	2,768	3,350	1,551	2,582	1,016
	945	646	991	974	598	553
	355	336	361	360	319	309
Same Sex	0.632	0.483	0.328	0.296	0.646	0.729
	2,584	1,672	2,013	917	1,557	616
	539	379	576	563	350	335
	365	269	388	381	251	242
Difference	*-0.116*	*-0.182*	*0.383*	*-0.010*	*-0.069*	*-0.199*
Black	0.479	0.202	0.483	0.376	0.592	0.409
	426	331	417	176	302	120
	90	84	101	98	75	68
	52	49	55	53	46	44
Nonblack	0.548	0.342	0.597	0.280	0.591	0.575
	5,820	4,009	4,688	2,095	3,751	1,478
	1,212	918	1,289	1,262	853	801
	625	545	649	641	514	497
Difference	*-0.069*	*-0.140*	*-0.113*	*0.097*	*0.001*	*-0.166*
Mother Has Less Than 12 Years Education	0.483	0.335	0.180	0.338	0.587	0.611
	1,861	1,104	1,419	648	1,034	407
	394	271	414	402	250	228
	191	154	196	194	144	137
Mother Has 12+ Years Education	0.466	0.239	0.156	0.224	0.517	0.502
	5,247	3,336	3,944	1,820	3,105	1,225
	1,090	754	1,153	1,135	698	660
	529	451	553	547	426	414
Difference	*0.017*	*0.096*	*0.024*	*0.114*	*0.070*	*0.110*
Sibling Age Range <5 Years	0.531	0.303		0.383	0.563	0.577
	3,725	2,355		1,280	2,211	870
	784	545		796	509	469
	356	308		358	296	287
Sibling Age Range 5+ Years	0.572	0.402		0.280	0.684	0.652
	2,456	1,488		845	1,390	551
	507	343		531	315	302
	171	160		173	150	147
Difference	*-0.041*	*-0.099*		*0.104*	*-0.121*	*-0.075*

(table continues)

TABLE 5, continued

	Education	Occ. Prestige	Ln Income	Ln Net Worth (minus home equity)	Class Position I (educ., occ., ln income)	Class Position II (+ ln net worth)
Large Family	0.508	0.304	0.779	0.259	0.555	0.521
	3,837	2,333	2,852	1,315	2,187	861
	805	545	844	825	509	470
	299	261	309	307	249	240
Small Family	0.575	0.417	0.463	0.450	0.674	0.687
	3,271	2,107	2,511	1,153	1,952	771
	679	480	723	712	439	418
	421	344	440	434	321	311
Difference	*-0.067*	*-0.113*	*0.317*	*-0.191*	*-0.119*	*-0.165*

movement towards greater sibling discordance among more disadvantaged families for this most recent and older cohort of Americans.

DISCUSSION

We are not the first to propose examining sibling correlations in social and economic status as a way of measuring the impact of family background. For example, Daphne Kuo and Robert Hauser analyze the Occupational Changes in a Generation (OCG) survey data and find that for education, sibling differences (within family variance components) among various age groups of black and white brothers range between 38 percent and 52 percent (see Kuo and Hauser 1995). Mary Corcoran, Roger Gordon, Deborah Laren, and Gary Solon (1990: 364) estimate a brother-brother correlation in *permanent* income of .45 using data from the PSID. Bhashkar Mazumder and David Levine (2003) examine the NLS and the PSID and argue that between the 1960s and the 1970s, the correlation in earnings between brothers rose from .26 to .45. Sibling resemblance in such other outcomes as welfare usage, education, and occupation follow similar patterns and are sensitive to the specification deployed—particularly for nonlinear measures. For example, if a woman's sister has received welfare, she is over three times more likely to use it herself (.66 versus .20 probability in their PSID sample).[5]

When we reanalyze more recent waves of PSID data—in which the siblings are on average older and more stable economically—with a substantially larger sample size of person-years and sibling sets we obtain similar estimates for the impact of family background on education, occupational prestige, and income. When we correct for negative first-order serial correlation, we obtain a sibling correlation of .512 for occupational prestige—not remarkably different from either Solon et al. (1991) or Mazumder and Levine (2003). However, they were examining only brothers (as were most of the previous researchers who investigated education and occupational resemblance as well). When we break out our analysis by

[5]Differences for "persistent participation" in welfare programs by sibling welfare status are even greater.

sex, however, we find that sisters have generally higher sibling correlations than do brothers. This stands in contrast to Solon et al. (1991) who found that sisters demonstrated a significantly lower correlation ($\rho = .276$) than did brothers. Most notably, we find that sisters demonstrate a correlation of .675 for the wealth measure (in contrast to the $\rho = .299$ figure for brothers). This means that family background explains almost 70 percent of "class" status for women, when measured through wealth, and that family background is about 50 percent weaker for brothers than it is for sisters. Such a finding suggests that social background exerts a far greater influence on sisters' economic fortunes than was previously believed, or that now that women have entered the labor market in greater numbers their degree of social reproduction within the family has increased. It is not clear, however, whether this reflects social sorting by family background in the labor market or the marriage market. Since these, in turn, may be jointly dependent processes, finding an instrument to separate out the effects of background on economic success through marriage and through the labor market would be in order—though it would constitute a tall order to find a valid instrument to resolve this issue.

When we break out this analysis by family background—race, maternal education, maternal age, maternal marital status, and family size— without disaggregating by cohort, we arrive at mixed results. Becker and Tomes (1986) *predict* and Mazumder and Levine (2003) *find* that siblings who have fewer economic resources tend to demonstrate greater concordance in their socioeconomic statuses. Our analysis of maternal educational attainment does lend support to this theory, as we find greater concordance among siblings from families where mother's educational attainment is lower. However, race differences for income work in the opposite direction. Namely, black families (who presumably have fewer economic resources and more credit constraints) demonstrate greater sibling differences than nonblack families (a split along the nonwhite/white axis reveals the same pattern). This dynamic is reversed for wealth, such that black families demonstrate greater sibling resemblance than nonblack families. We take up this income-wealth discrepancy below. Other family characteristics—such as the age of the mother at the birth of her children and family size—lead to inconclusive results. One important distinction between the findings of Mazumder and Levine and ours is that we split the sample based on *parental* characteristics, avoiding endogeneity problems that may ensue from splitting on the sibling adult characteristics themselves.

Whereas results from analysis of all the cohorts from 1983 to 2001 seem to contradict our hypothesis relating disadvantage to sibling discordance, results from analysis of the most recent cohort support our hypothesis and indicate an increase in the effects of disadvantage on sibling discordance. For this most recent cohort, siblings from disadvantaged social backgrounds, measured through race and family size, show greater discordance than siblings from less disadvantaged social backgrounds. Most importantly, findings indicate that the substantial changes we have witnessed in the distribution of inequality, i.e., the growth of inequality and the polarization of Americans, has in fact resulted in greater intrafamily differences.

One of the most interesting findings is that net worth—i.e., wealth, or assets—seems to frequently display a different dynamic from other measures of SES when we break the sample out. In the total sample the sibling correlation is not much different than that for income ($\rho = .371$ and $\rho = .458$, respectively); however, when we divide the sample into subgroups, we often find that wealth behaves differently from income. For example, wealth demonstrates a most substantial difference between brother-only and sister-only sibships and between mothers with higher and lower educational attainment for the entire cohort analysis. Further, while black siblings generally demonstrate weaker correlations than do nonblack siblings in the most recent cohort analysis, for the natural logarithm of net worth this dynamic is reversed. The general pattern of results for wealth—in contrast to income and other measures—is supportive of the Becker-Tomes hypothesis. That is, families from more disadvantaged backgrounds—by virtue of sibship age concentration, maternal educational attainment, or black race—have a greater degree of resemblance than do siblings from more advantaged backgrounds. However, we are hesitant to infer that these differences are supportive of the investment constraints model developed by Becker and Tomes (1986) since it is not supported by other socioeconomic measures (for our most recent cohort analysis) and may instead be an artifact of the distribution of the natural logarithm of the wealth variable—that is, with a huge spike at zero.

These results are also worth viewing within a larger theoretical debate about the proper unit of analysis in stratification research, since wealth is not an individual variable but rather a household one. There has been a lively debate regarding how to conceptualize the appropriate unit of analysis in class-related research. As early as 1973, Acker criticized the literature for relying on the male head of household as the unit of class analysis, arguing that this methodology rests on several sexist and outdated assumptions. This conventional paradigm assumes that women's status within the stratification system is determined by that of her husband or other man to whom she is primarily attached. A woman's status is equal to her man's status, since different members within the family unit are, presumably, of equivalent evaluation. The only case in which a woman determines her own status is when she is not attached to a man (Acker 1973). Acker points to the declining prevalence of the traditional nuclear family in the United States to make a case for considering the individual, rather than the male-headed family, as the primary unit of stratification analysis. She argues that this approach would be able to account for the ways in which sex stratification cuts across class lines, more accurately representing our current system of social ordering.

Acker's argument, along with other feminist critiques of the male-centered paradigm (Delphy 1981, 1984; Delphy and Leonard 1986; Walby 1986), has provoked a defense of the conventional framework initiated by Goldthorpe (1983). Goldthorpe argued that it is because of sexual inequality that wives' life chances are dependent on those of their husbands. Gender inequality limits the paid work of women to the extent that it is not a significant enough factor to take precedence over the husband's status (Goldthorpe 1983). This debate has led to various attempts to reconceptualize the theoretical and empirical tools used to account for gender in stratification analysis (e.g., Curtis 1986; Wallerstein and Smith 1991). Curtis (1986) has argued that, while he agrees with the feminist critique that stratification analysis has been biased toward androcentrism, the individualist approach advocated by Acker is not an adequate replacement for an approach centered on the male breadwinner. Curtis calls for a framework that uses the family as the unit of analysis, while accounting for inequalities within the family. He argues that inequalities, be they by gender or other categories, emerge through processes of redistribution and social exchange among family members, given the structure of noneconomic relations of power and authority within the family. This framework considers the variation in household structure and demographic features as an important factor in these redistributive processes (Curtis 1986). Sorensen and McLanahan (1987) make a similar case for using the family as the primary unit of analysis, arguing that, because of the redistribution that occurs within families, individualistic analyses misrepresent women's life chances.

The current results inform this debate in two ways: by demonstrating robust sister correlations in economic status, and by considering wealth. The sister-sister correlation that exceeds the brother-brother figure also suggests that the male-head-of-household approach is outdated. Likewise, since wealth comes to families from various sources—including labor market earnings, return on investments, and inheritance or transfers—and is held jointly by the family, it suggests that neither the head nor the individual, but rather the household, is the proper unit of analysis for mobility studies.

This issue leads to another important theoretical consideration: whether or not parent-child or sibling-sibling correlations (or mobility tables) can accurately represent the openness of a society and the impact of family background. The short answer is "no"—once we open up the possibility of within-family heterogeneity in parental investment, siblings affecting each other, and potentially differing responses to familial investment in households with multiple offspring. While it is trivially true that if there is a zero correlation between siblings (or between parent and child for that matter), family background can be said to have no impact, this is a descriptive account only. For example, it could be the case that family background matters enormously, but that within-family dynamics obscure this fact in sibling or parent-child associations of socioeconomic status.[6] For example, envision the case of a two-child family in which the elder child is expected to sacrifice for the benefit of the younger sibling. If such a dynamic were widespread in a given society and resulted in downward mobility for the sacrificing sibling and upward mobility for the sibling who benefited from the sacrifice, we could actually observe a negative sibling correlation and a zero parent-child correlation (since the upwardly mobile offspring would be cancelled out by the downwardly mobile one).

If such dynamics were systematically stratified by a measurable variable such as gender or birth position, then we could accurately describe the intrafamily dynamics by observing correlations for within-family subgroups such as first-borns or boys. But if the way that families generated outcomes among children was based on some unobservable factor—such as parental belief in child ability—then to the researcher, the apparent result may be randomness and a potentially faulty observation that family background means little. In fact, what it would mean is that the family would act as a primary queuing mechanism for socioeconomic opportunity.

How to resolve such ambivalent interpretations of sibling or parent-child associations? The best way would be to find an instrumental variable that affects individual success but which does not affect a sibling's outcomes *or* the distribution of resources within the family. Birth weight would seem to provide just such an instrument, given its individual nature and the associations it has been shown to have with socioeconomic outcomes (see, e.g., Conley and Bennett 2000, or Behrman and Rosenzweig forthcoming). However, parents may invest differentially in their children by estimating the rate of return according to birth weight. So, it is unlikely that there will appear an instrument that satisfies these two criteria, and as a result, a fully specified model is by definition, underidentified. (Add in selective fertility—parity progression bias—based on parental perceptions of whether or not they got a "winner" with the first child

[6]For a good example of how these processes can be modeled, see, e.g., Behrman, Rosenzweig, and Taubman (1994).

or not—and the problem of understanding social transmission within the family gets even more complicated.)

Given the apparent intractability of modeling the true impact of family background(s), what are we to make of sibling correlations? The answer is that we can read a sibling correlation as a global effect of family background if we assume a model in which offspring are invested in equally (or at least that any favoritism is randomly distributed) and in which siblings have only a mean-regressive effect on each other. That is, that they tend to cause each other to be more alike than they would in each other's absence. This is not an entirely unreasonable assumption, but it is an assumption nonetheless. It is for future researchers—both ethnographic and quantitative—to determine whether this assumption (and that of nonfavoritism) is accurate or not.

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