SIBLING RESEMBLANCE AND INTERSIBLING EFFECTS IN EDUCATIONAL ATTAINMENT

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Is there reciprocal influence between siblings' educational attainments or a predominant influence of older on younger siblings? A reanalysis of Benin and Johnson's (1984) Nebraska data shows no unusually high level of resemblance between brothers, but there is an unusually low similarity between older sisters and younger brothers. We model reciprocal effects between brothers' educational attainments using Olneck's (1976) data from Kalamazoo, Michigan. We find evidence of reciprocal influence of brothers' levels of educational attainment, net of the common effects of family background and the effect of each brother's mental ability on his schooling. The unconstrained effect of older brother's schooling on younger brother's schooling is larger than the reciprocal effect, but a model of equal reciprocal effects fits almost as well. This leaves open the questions of whether reciprocal influence occurs in other types of sibling pairs and whether there is a predominant effect of older on younger siblings.

It is a sociological truism that the family is one of the important stratifying agents in the transmission of social and economic inequality. This was well stated many decades ago by Charles Horton Cooley (quoted in Blau and Duncan 1967, p. vii):

There is a certain opposition between the ideal of equal opportunity and that of family responsibility. Responsibility involves autonomy, which will produce divergence among families, which, in turn, will mean divergent conditions for the children; that is, unequal opportunities.

Well-known models of the stratification process represent family characteristics through a handful of variables—race, education, occupation, earnings, female headship, size of sibship, and the like—that cannot very well represent the full variety or influence of family environments. Although Blau and Duncan (1967) initiated modern studies of sibling resemblance in their discussions of the effects of "family climate" on achievement and the possibility of cross-sibling influence, the emphasis in research has been on between-family differences in easily measured social and economic characteristics, rather than on differences within the family in resources or social support, on typical patterns of activity of interaction, or on mutual influences among family members. For example, the classic status-attainment model, introduced by Blau and Duncan (1967) and then elaborated as the Wisconsin model (Hauser, Tsai, and Sewell 1983; Sewell, Haller, and Ohlendorf 1970; Sewell, Haller, and Portes 1969; Sewell and Hauser 1972; Sewell, Hauser, and Wolf 1980), focuses on the ways in which socioeconomic background affects individual status attainment in adulthood through social and psychological processes during adolescence.

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TWO CRITIQUES OF STATUS-ATTAINMENT MODELS

Beyond the neglect of family factors, there have been two other distinctive weaknesses of the status-attainment paradigm. One line of criticism typically centers on the contention that status-attainment models underestimate the extent or persistence of inequality, the importance of allocative mechanisms controlled by elites relative to socialization and self-selection, or the importance of labor markets relative to schooling and socializing experiences of youths as sources of social stratification. To a degree, these criticisms have coalesced in the development of the so-called new structuralism. This article does not address the major issues raised in the structural critique, except to the extent that it can be said to include a broader conception of family influence.

The second line of criticism argues that status-attainment models underestimate the importance of family background because of response errors, omitted variable bias, and other types of misspecification (Bowles 1972). The latter concerns sound methodological, but, in principle, their substantive implications could be large (Hauser, Tsai, and Sewell 1983). In this article, following Hauser and Mossel (1985) and Hauser and Sewell (1986), we respond to this critique by combining corrections for response error with an explicit model of family effects on educational attainment.

Family Influences on Status Attainment

If there is agreement that families are agents of stratification, questions remain about the size and mechanisms of family influence. Some believe that the family, together with other social institutions, mainly reflects and embodies the reproduction of class (Althusser 1971; Bourdieu and Passeron 1977; Bowles and Gintis 1976). Others, like Gritiches (1979), believe that the family also has a depolarizing effect, especially as a potential equalizer of income. That is, because there is heterogeneity among family members, parents may attempt to compensate for differences among offspring to equalize outcomes. In smaller families, parents may be more effective in equalizing outcomes, which may account for the greater heterogeneity in socioeconomic outcomes in large than in small sibships. As family size declines, between-family differences may then become relatively larger as a component of inequality; there is presumably a trade-off between these effects of lesser inequality within (smaller) families and declines in the main effects of size of sibship (Blake 1981; Heer 1985).

Even though research has found declining effects of socioeconomic background on achievement in recent cohorts (Featherman and Hauser 1978; Hout 1984, 1988), it is likely that the family unit will remain an important stratifying agent. There is ample evidence of heterogeneity among families in socioeconomic life chances, that is, of sibling resemblance. For example, after correcting for measurement error, Hauser and Featherman (1976, p. 117) found that about two-thirds of the variance in the educational attainments of cohorts of American men who were born between 1907 and 1951 can be explained by a common family factor and about half the variance in the common family factor can be explained by measured socioeconomic variables. After correction for measurement error, Olneck's (1976, p. 186; 1977, p. 132) findings for brothers from Kalamazoo, Michigan, imply that a common family factor explains 52 percent of the variance in scores on mental tests, 59 percent of the variance in educational attainment, 49 percent of the variance in status of first occupation, 37 percent of the variance in status of current occupation, and 27 percent of the variance in annual earnings. In their sample of brothers from Wisconsin, Hauser and Sewell (1986) found that a common family factor explains 49 percent of the variance in mental ability, 46 percent of the variance in educational attainment, 41 percent of the variance in status of first occupation, 38 percent of the variance in status of current occupation, and 27 percent of the variance in annual earnings. The difference in findings about educational attainment between the national sample (the 1973 Occupational Changes in a Generation [OCG] survey) and the Kalamazoo and

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1 One threat to this argument is the likelihood that increasing rates of nonmarital childbearing and marital dissolution will render it difficult to associate children with a common family environment because children's family environments change radically across time, because children have more than one parental home at the same time, or because biological siblings are not raised together.
Wisconsin samples may well be an effect of the restriction in range of the latter studies; that suggests we would find even larger family effects on ability, occupational status, and earnings if national data on them were available.

Some researchers, like Behrman and Taubman (1976), Behrman, Taubman, and Wales (1977), and Taubman (1976), studied the resemblance of siblings in an effort to resolve the old debate about nature versus nurture as sources of social inequality. The major emphasis in these studies is on comparisons of genetic and environmental sources of variation in education, occupation, and earnings by using data on the resemblance between monozygotic and dizygotic twins. Yet, as Jencks and Brown (1977) argued, this heredity-environment distinction is based on a false dichotomy. A high correlation between genotype and phenotype need not imply a low effect of environmental influences on phenotype. Moreover, data on twins do not provide any more identifying restrictions than do data on pairs of siblings (Chamberlain 1977; Goldberger 1977) without additional assumptions that are both implausible and restrictive. Our concern in studying siblings is not to estimate how much variance genes can explain in a given outcome, but to elucidate the effects of measured and unmeasured variables in the process of stratification (see Hauser and Mossel 1985; Hauser and Sewell 1986).

In the present study, our interest is the microfoundation of familial equality or inequality, particularly sibling resemblance. There is a long-standing tradition of research on social and psychological differences among siblings, which centers on the measurement and explanation of the supposed effects of ordinal position. In recent years, research on birth order has developed in response to the Zajonc-Markus confluence model, which explains differences in mental ability within families by differences in the social environments faced by offspring of different ages across time (Zajonc 1976; Zajonc 1983; Zajonc and Markus 1975; Zajonc, Markus, and Markus 1979). Despite the efforts that have been devoted to this line of research, the bulk of the evidence appears to show that such effects are of minor importance, once controls have been introduced for the size of the sibship.\^[2]

Intersibling Effects on Educational Attainment

A related line of research, initiated by Blau and Duncan and revived by Benin and Johnson (1984), has postulated that the educational attainments of older siblings affect those of younger siblings by means of role modeling or social facilitation; this research interprets cross-sibling influence as a source of similarity in outcomes, rather than of differentiation. The main purpose of this article is to provide new evidence about this source of sibling resemblance. Specifically, we attempt to model reciprocal influence between brothers to determine whether there is a predominant flow of influence from older to younger siblings.

This study is motivated by Benin and Johnson’s (1984) study of two samples of adult siblings in Nebraska, from which they concluded that pairs of brothers resemble one another more, net of socioeconomic background, than do pairs of sisters or brothersister pairs. Benin and Johnson (pp. 18–19) stated that the greater similarity of pairs of brothers is better explained by cross-sibling influence, predominantly from older to younger siblings, than by the possibility that some unmeasured common family factors affect brothers but not sisters. They suggested that role modeling and facilitation (social contacts and support) by brothers are sources of this greater resemblance and that “role modeling has a stronger effect on sibling resemblance than does facilitation.”

OVERVIEW OF THE ANALYSIS

We first reanalyze the data on siblings reported by Benin and Johnson (1984), which was the first published report based on all four possible combinations of pairs of siblings ordered by sex and age. Contrary to Benin and Johnson, we find that differences in common family variance among types of sibling pairs are not derived from an unusually high level of fraternal resemblance, but

\^[2] See, for example, Hauser and Sewell (1985) and sources cited therein.
only from the unusually low similarity between older sisters and younger brothers. Since Benin and Johnson's interpretation rests partly on the finding of a high degree of fraternal resemblance, this leads us to look for more direct evidence of mutual influence between brothers.

Although the Nebraska data do not permit us to identify a model with reciprocal effects between brothers' educational attainments, suitable data are available in Olneck's (1976) important study of brothers from Kalamazoo, Michigan. In addition to data on educational attainments and parental status characteristics, the Kalamazoo data include scores on mental tests for each brother. These data permit us to estimate the reciprocal effects of brothers' educational attainments on the assumption that one man's mental ability does not affect his brother's schooling. We believe that this analysis is responsive to Benin and Johnson's (1984) call for the development of models for male siblings that include cross-sibling effects. In modeling the Kalamazoo data, we use a mixture of multiple proxy reports and self-reports to take account of random and correlated response errors in the measured variables. In brief, we find evidence of a reciprocal influence of brothers' levels of educational attainment, net of the common effects of family background and the effect of each brother's mental ability on his schooling. The unconstrained effect of the older brother's schooling on the younger brother's schooling is larger than the effect of the younger brother on the older brother, but the difference between the two effects is of borderline statistical significance. This leaves open the following questions: Does reciprocal influence truly occur in this or other types of sibling pairs? and Is there a predominant effect of older siblings on younger siblings?3

3 We are grateful to Michael R. Olneck for making his primary data available to us for this reanalysis. We take sole responsibility for any errors in their use herein.

ANOTHER LOOK AT THE NEBRASKA DATA

As Benin and Johnson (1984) noted, most sibling-based studies of the stratification process have included only brother-brother pairs and thus have not addressed potential differences in the size or sources of resemblance in female or mixed-sex sibling pairs. In their analysis, they were able to obtain sample data for all four different combinations of sibling pairs: older brother-younger brother (OB-YB), older brother-younger sister (OB-YS), older sister-younger brother (OS-YB), and older sister-younger sister (OS-YS). Their data were obtained from two independent samples: the 1981 Nebraska Annual Social Indicators Survey (NASIS) and a 1976 survey in Lincoln, Nebraska (Lincoln). In each survey, adult respondents were asked to report the sex and educational attainments of their offspring, ages 21 and older, along with the educational attainment of the mother and father and the father's occupation. On the one hand, the NASIS data were drawn from a statewide probability sample and represent the populations of adults residing in the state. On the other hand, the Lincoln sample was restricted to children from maritally stable families who had lived in the Lincoln area for 28–29 years. The NASIS sample covered a much broader age range among offspring (21–70 years) than did the Lincoln sample (21–50 years). The final weighted sample sizes for the NASIS and Lincoln data were 442 and 462, respectively. The Benin-Johnson analyses used all possible pairs formed from these data, weighted to compensate for clustering and for multiple appearances of individuals in pairs (pp. 13–15); our reanalysis uses the correlations and standard deviations reported by Benin and Johnson (1984, pp. 20–21).

The Benin-Johnson Model

Figure 1 shows a path diagram of the model of siblings' educational attainment that was used in most of Benin and Johnson's analysis. There are three exogenous variables—father's occupational status seem too small to be of substantive importance. Yet, one reviewer cautioned us not to let readers conclude that a nonsignificant effect is too small to be substantively important, and we second that caution.

4 Throughout our analysis, we relied heavily on tests of statistical significance and the closely related bic statistic (Raftery 1986) as a guide to deciding among alternative models. Sometimes, we found that there is no one best conclusion to a model-selection process using such tools, and we have tried to state alternative conclusions, when appropriate. In particular, we found it difficult to interpret effects that are nominally significant but
Figure 1. Structural Model of Sibling Resemblance in Educational Attainment as Hypothesized by Benin and Johnson

(\( \xi_1 = \text{POPDUNC} \)), father’s educational attainment (\( \xi_2 = \text{POPED} \)), and mother’s educational attainment (\( \xi_3 = \text{MOMED} \)). Occupational status is coded on the Duncan Socioeconomic Index (rescaled downward by a factor of 10 for convenience in reporting parameter estimates), and educational attainment is expressed in years. Each of the exogenous variables is permitted to covary with the others, as shown by the curved, two-headed arrows on the left side of the diagram, and each affects the educational attainments of each member of a sibling pair. In the diagram, the dependent variables are older sister’s education (\( \eta_1 = \text{OSED} \)) and younger sister’s education (\( \eta_2 = \text{YSED} \)), but there is a similar model for each type of sibling pair. Note that the same person may appear more than once in a paired subsample, not only in the same ordinal position, but also—within same-sex pairs—in the other ordinal position. To the extent that siblings’ educational attainments are not explained by the measured variables, they are determined by disturbances, \( \xi_1 \) and \( \xi_2 \), which are assumed to be independent of the exogenous variables but are not necessarily uncorrelated with one another. The latter possibility is indicated by the curved arrow connecting \( \xi_1 \) and \( \xi_2 \) at the right-hand side of the path diagram.

The effects of substantive interest in the Benin-Johnson analysis are the slopes of educational attainments on the background variables, where \( \gamma_{ij} \) is the effect of \( \xi_j \) on \( \eta_i \), and the covariances between the disturbances, denoted by \( \psi_{12} \). If the \( \psi_{12} \) are positive, then there must be at least one source of variance in educational attainment that is common to siblings but is not reflected in measured social background. Although this model consists of two multiple regression equations in measured variables sharing the same set of regressors, Benin and Johnson used the LISREL program to estimate the parameters because the maximum likelihood estimation method used by LISREL permits the imposition of equality constraints on parameters across equations and across multiple samples (Joreskog and Sorbom 1984). Thus, Benin and Johnson were able to test equalities of the form \( \gamma_{11} = \gamma_{21} \), which says that the effect of father’s occupational status on each sibling’s educational attainment is the same. Also, they were able to test equalities of the form, \( \psi_{12} = \psi_{12} \), where \( \psi_{12} \) and \( \psi_{12} \) pertain to disturbance covariances in different types of sibling pairs.

We have deliberately used the LISREL notation for the Benin-Johnson model because we use an elaboration of this model in subsequent analyses.\(^5\) In its general form, the LISREL model has three sets of equations. The structural model, part of which is used in Figure 1, is

\[
\eta = B\eta + \Gamma \xi + \xi,
\]

where \( \eta \) is a vector of endogenous latent variables, \( \xi \) is a vector of exogenous latent variables, \( \xi \) is a vector of disturbances that are independent of the \( \eta \) and \( \xi \), and \( B \) and \( \Gamma \) are parameter matrices; \( B \) is a square matrix, defined with zero entries on its main diagonal.

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\(^5\) All our estimates were obtained by the method of maximum likelihood using procedures for multiple group analyses in LISREL V and LISREL VI (Joreskog and Sorbom 1984).
to indicate which is the left-hand variable in each equation. The variance-covariance matrices of the $\xi$, denoted by $\Phi$, and that of the $\zeta$, denoted by $\Psi$, are also parameter matrices of the structural model. The two measurement models that specify the dependence of observed variables on the latent variables are

$$X = \Lambda_x \xi + \delta$$ \hspace{1cm} (2)

and

$$Y = \Lambda_y \eta + \epsilon,$$ \hspace{1cm} (3)

where $X$ and $Y$ are vectors of observed indicators of exogenous and endogenous variables, respectively, $\Lambda_x$ and $\Lambda_y$ are parameter matrices, $\delta$ and $\epsilon$ are vectors of independently distributed errors in variables, and the variance-covariance matrices of $\delta$ and of $\epsilon$ are denoted by $\Theta^\delta$ and $\Theta^\epsilon$, respectively. In the model of Figure 1, Equations 2 and 3 are trivial because observables are equivalent to latent variables.

**Findings of Benin and Johnson**

Benin and Johnson's findings are provocative and consistent across samples. First, the residual covariances, $\psi_{12}$, between siblings' educational attainments differ significantly across types of pairs; the OB-YB pairs have the largest residual covariances and the OS-YB pairs have the smallest. According to Benin and Johnson, this finding implies that "social mobility studies which use the residual correlation between brothers' educational attainments as an indicator of unmeasured family-background effects have overestimated that effect for the other sex combinations" (1984, pp. 17-18).

Second, Benin and Johnson used common heredity; common environment; and, most important, intersibling effects to explain the cross-group differences in the residual covariance terms. In their interpretation, the smallest residual correlation, that is OS-YB pairs, is "an upper bounds [sic] of a common-heredity effect" (p. 18). Benin and Johnson invoked role modeling and facilitation as mechanisms of intersibling effects. Role modeling should be greater between like-sex than between mixed-sex pairs of siblings. Older brothers are more likely than are older sisters to become role models for younger siblings and to contribute social contacts or resources that will facilitate the schooling of their younger siblings; they are also more likely to aid a younger brother than a younger sister (p. 12). These arguments lead to the prediction, apparently supported by the data (p. 16), that OB-YB pairs will have the largest residual covariance, that OS-YB pairs will have the smallest residual covariance, and that the other two types of pairs will display intermediate values.

Benin and Johnson also reestimated the same model for each type of pair after classifying the sibling pairs by the age difference between the siblings. The overall pattern of differences in residual covariances by type of pair is replicated within the classification by age differences, and there is some evidence that, within the type of pair, the residual covariance is larger for siblings who are separated in age by three or fewer years than among siblings who are more widely separated in age. Benin and Johnson said that the latter finding is more consistent with role modeling than with social facilitation as a mechanism of cross-sibling influence.

**Critique of Benin and Johnson's Analysis**

Although Benin and Johnson's work makes a novel and significant contribution to our understanding of sibling resemblance, there are weak points in their argument and in the supporting analysis. First, we disagree with their claim that previous studies of pairs of brothers overestimated a common family influence. We think that earlier studies correctly estimated the common family influence in pairs of brothers, and Benin and Johnson cited no inappropriate generalizations from those estimates. Obviously, Benin and Johnson have advanced knowledge by studying resemblance among other types of sibling pairs (see Hauser 1984), but this does not render research on brothers any less valuable.

Second, we disagree with Benin and Johnson's claim that the residual covariance in educational attainments of OS-YB pairs provides an upper bound on the effect of heredity on educational attainment. If parental statuses are positively correlated with genetic determinants of schooling, then the genetic variance in schooling could exceed the residual covariance. We would expect this confounding to have occurred if genetic background has any influence in the child's
generation; that is, if genes affect children’s success, it is logical to assume that they also affected parents’ success.

Third, Benin and Johnson did not actually estimate any cross-sibling effects on educational attainment. No such effects were identified in their model because they have no explanatory variables that can be said to influence the schooling of one sibling in a pair, but not of the other. The only evidence of simultaneity offered by Benin and Johnson is the difference in residual covariances among the groups; these covariance terms are irrelevant in the identification and estimation of reciprocal effects.6

Fourth, although Benin and Johnson’s exposition appears to invite the specification of a common factor in educational attainment, composed of measured and unmeasured family influences, Benin and Johnson did not specify a model of this form. Figure 2 shows the path diagram of this more parsimonious model, in which the effects of the social background variables influence educational attainments by way of a common family factor. Here, the educational outcomes, \( Y_1 = \text{OSED} \) and \( Y_2 = \text{YSED} \), appear as indicators of a common family factor, \( \eta_1 \), which is directly affected by the exogenous variables, \( \xi_1, \xi_2, \text{ and } \xi_3 \). The effects of the common factor, \( \eta_1 \), on the siblings’ educational attainments, \( Y_1 \) and \( Y_2 \), are expressed in the LISREL measurement model as loadings, \( \lambda_{11} \) and \( \lambda_{21} \). The specification of the common factor in Figure 2 imposes a proportionality constraint on coefficients of the reduced form regressions of the \( Y_i \) on the \( \xi_j \), that is, equations of the form of those in Figure 1. Also, in the model of Figure 2, the variance, \( \psi_{11} \), of the disturbance of the family factor, \( \xi_1 \), is equivalent to the residual covariance under the model of Figure 1. Thus, Figure 2 displays a multiple indicator–multiple cause (MIMIC) model (Hauser and Goldberger 1971; Jöreskog and Goldberger 1975).

The MIMIC model explicitly postulates that families have an effect that is common to offspring; this imagery is appropriate, as well as traditional, in studies of sibling resemblance. The MIMIC model tells exactly how much of the common effect is due to measured social background effects and how much is due to other common influences on siblings. The MIMIC model is useful here because it yields evidence of an association of ordinal position with the influence of parental statuses and lays the basis for our presentation of an extended model that permits true cross-sibling effects.

One may think this MIMIC specification makes little difference in the analysis. Benin and Johnson were unable to reject a model in which the effects of socioeconomic variables on schooling (\( \gamma_{ij} \)) are the same regardless of sex or ordinal position (Benin and Johnson, Model 4 in Table 1, p. 16). Their tests of equality in residual covariances were conditioned on this model, which is formally equivalent to a MIMIC model in which the loadings of \( Y_1 \) and \( Y_2 \) are both set equal to unity. However, not only does the MIMIC model fit the Lincoln and NASIS data, it provides more flexibility and parsimony in our tests of cross-group differences, which yield different substantive findings than those of Benin and Johnson. Specifically, we find some indications of a difference in effects of social background, such that younger children, regardless of sex, are less affected by parental status characteristics than are their older siblings.7 This is an important possibility because of the suggestion of Jencks et al. (1979, pp. 68–70), noted by Benin and Johnson (p. 13), that “if older brothers are role models for younger brothers, the importance of the father as a role model would be diminished . . . [but] they were unable to find differences in the effects of fathers’ attainments on younger and older brothers’ educations.”8

Fifth and most important, in Benin and Johnson’s model, as well as in the revised model, we find that there are no significant differences between common variance components of the OB-YB, OB-YS, or OS-YS types.

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6 See Duncan (1975, chaps. 5 and 6) for a useful, elementary exposition of identification in simultaneous equation models.

7 We were not convinced on this point by the Nebraska data alone, but cross-validated it in several other bodies of data, as described later.

8 Also, Jencks et al. (1972, pp. 346–348) considered the possibility that older brothers positively influence the educational attainments of younger brothers. It is interesting that they sought evidence about interpersonal influence in the reduced-form equations for nonoverlapping samples of older and younger brothers in the Project Talent data. However, the findings lent no support to their hypothesis.
Where \( OSED \) = Older sibling's educational attainment
\( YSED \) = Younger sibling's educational attainment
\( POPDUNC \) = Father's occupational attainment
\( POPED \) = Father's educational attainment
\( MOMED \) = Mother's educational attainment

Figure 2. Alternative Structural Model of Sibling Resemblance in Educational Attainment

of pairs; the only significant contrast is that predicted between the OS-YB type and the others. This finding is inconsistent with Benin and Johnson's argument that cross-sibling effects are especially large in the OB-YB pairs.

Reanalyses of the NASIS and Lincoln Data

Before turning to our models of intersibling influence in the Kalamazoo data, we first report our reanalyses of the samples of siblings in the Nebraska studies used by Benin and Johnson. To test the hypotheses that were of most substantive interest, that is, those pertaining to the relative influence of parental status on older and younger siblings and to the residual family effects on sibling resemblance, we first estimate an unrestricted version of our model, followed by successive restrictions on its parameters, either within or across groups defined by the type of pair. It is inevitable that the outcomes of these several tests will depend on the order in which the restrictions are introduced; for that reason, we use an explicit set of decision-making guidelines. In many cases, we verified whether our decisions would be affected by changes in the order of successive restrictions.

To evaluate the model fit, we used conventional tests based on the likelihood ratio statistic, \( L^2 \).\(^9\) We also used the \( bic \)

\[ bic = L^2_m - df_m \times \log N, \quad (4) \]

where \( L^2_m \) is the likelihood ratio test statistic under Model \( m \), \( df_m \) is the degrees of freedom under Model \( m \), and \( N \) is the sample size. Satisfactory fit is indicated by a negative \( bic \) statistic, and in comparisons of fit between models, those with lower (more negative) \( bic \) statistics are preferred. Sometimes, a conventional chi-square test yields a significant contrast between a more parsimonious and a less parsimonious model when \( bic \) suggests that the more parsimonious model is to be preferred. In those cases, we used \( bic \) to guide our decisions because the probability of falsely rejecting a null hypothesis in a large set of contrasts is higher than the nominal probability level would suggest.

The NASIS Sample

Table 1 reports tests of the model fit in our reanalysis of the NASIS data; parameter estimates from our preferred model are

\(^9\) The likelihood ratio test statistic, \( L^2 \), is distributed as \( \chi^2 \) in large samples under the assumption of multivariate normality, and the difference between likelihood ratio test statistics of two nested models will be distributed as \( \chi^2 \) with \( df_s \) equal to the difference of those under the two models.
Table 1. Summary of Model Selection for Comparisons between Types of Sibling Pairs: NASIS Sample

<table>
<thead>
<tr>
<th>Model</th>
<th>L²</th>
<th>df</th>
<th>p</th>
<th>bic</th>
<th>Contrasts</th>
<th>L²</th>
<th>df</th>
<th>p</th>
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<td>A. MIMIC model</td>
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<td>33</td>
<td>&lt;.82</td>
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<td>7</td>
<td>&lt;.13</td>
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<td>11.56</td>
<td>9</td>
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<td>(F)-(E)</td>
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<td>G. F + ( \lambda_{21}^2 = 1 )</td>
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</tr>
<tr>
<td>I. F + ( \Psi ) invariant</td>
<td>40.18</td>
<td>48</td>
<td>&lt;.79</td>
<td>-252.2</td>
<td>(I)-(F)</td>
<td>1.62</td>
<td>2</td>
<td>&lt;.45</td>
</tr>
</tbody>
</table>

The second model imposes the constraint that all the variances and covariances of parental background variables are the same across the four types of siblings, that is, \( \Phi \) is invariant across types of pairs. The difference between the likelihood ratio test statistics of these two models is 6.1 with 18 \( df \), so there are no significant differences in the variances or covariances of parental characteristics across groups. The model of line C imposes the constraint that \( \Theta^e \) is the same for all groups, regardless of sex or age. This is a sociologically interesting hypothesis; it says that factors that are unique to each sibling and unrelated to common family background contribute equally to the variance of schooling, regardless of sex or ordinal position. These sources of variance in schooling should not be regarded as “nonfamilial.” They may well originate within as well as outside the family; their only distinguishing characteristic is that they are not the same for each sibling in the pair. Thus, for example, efforts of parents to accommodate the special needs of each child may well induce variability in \( \Theta^e \). Equality in \( \Theta^e \) within each type of sibling implies that within-family variation does not depend on birth order. The contrast yields \( L^2 = 11.43 \) with 7 \( df \), which is not statistically significant; this hypothesis cannot be rejected.

In the model of line D, we pool the effects of the parental background variables across the four groups, that is, \( \Gamma \) is invariant. Given our normalization of \( \Lambda_e \), this constraint implies that effects of social background variables are equal for older brothers and older sisters but not necessarily for younger brothers and younger sisters, for whom \( \Lambda_e \) is permitted to differ across groups. The change in fit under this constraint is not statistically significant (\( L^2 = 11.56 \) with 9 \( df \)). The model in line E introduces the constraint that the effect of father’s education on the common family background factor is the same as that of mother’s education. The change in fit under this constraint is negligible.

In the model of line F, we specify that the common family factor affects the schooling of
Table 2. Estimates of Structural Parameters in Models of Sibling Resemblance in Educational Attainment: Nebraska Data

<table>
<thead>
<tr>
<th>Sample and Type of Pair</th>
<th>$\psi_{11}$</th>
<th>$\theta^s_{11}$</th>
<th>$\theta^s_{22}$</th>
<th>$\gamma_{12} = \gamma_{13}$</th>
<th>$\gamma_{11}$</th>
<th>$\lambda^s_{21}$</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>NASIS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>OB-YS, OB-YS, and OS-YS</td>
<td>2.623 (.386)</td>
<td>2.593 (.175)</td>
<td>2.593 (.175)</td>
<td>.228 (.023)</td>
<td>.143 (.046)</td>
<td>.733 (.057)</td>
</tr>
<tr>
<td>OS-YB</td>
<td>.977 (.733)</td>
<td>2.593 (.175)</td>
<td>2.593 (.175)</td>
<td>.228 (.023)</td>
<td>.143 (.046)</td>
<td>.733 (.057)</td>
</tr>
<tr>
<td><strong>Lincoln</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>OB-YS</td>
<td>2.018 (.375)</td>
<td>5.340 (.426)</td>
<td>5.340 (.426)</td>
<td>.192 (.029)</td>
<td>.242 (.056)</td>
<td>.768 (.065)</td>
</tr>
<tr>
<td>OS-YS</td>
<td>2.018 (.375)</td>
<td>2.300 (.233)</td>
<td>2.300 (.233)</td>
<td>.192 (.029)</td>
<td>.242 (.056)</td>
<td>.768 (.065)</td>
</tr>
<tr>
<td>OS-YB</td>
<td>.645* (.375)</td>
<td>2.300 (.233)</td>
<td>2.300 (.233)</td>
<td>.192 (.029)</td>
<td>.242 (.056)</td>
<td>.768 (.065)</td>
</tr>
</tbody>
</table>

*a All estimates are significant at the .05 level except the one marked with "*." Entries in parentheses are estimated standard errors. Estimates for the NASIS data are from Model I in Table 1, and estimates for the Lincoln data are for Model J in Table 3.

all younger siblings equally, regardless of sex. Again, the change in fit is small ($L^2 = 0.98$ with 3 df), and we are unable to reject this model. In the model of line G, we add the restriction that the effect of the common family factor on educational attainment is the same for all individuals, regardless of sex or birth order. Here, the deterioration of fit is large and statistically significant. If one compares lines F and G, $L^2$ is 15.77 with only 1 df. The contrast is still significant, even between lines G and E. That is, on the basis of this model, we have reason to believe that common family effects differ between younger and older siblings. In our final model, for which estimates are shown in Table 2, we find that the effect of the common factor on the schooling of younger siblings is only .73 times as large as its effect on older siblings, regardless of sex. In line H, we add the restriction to the model of line F that the variance of the disturbance in the common family factor, $\psi_{11}$, is equal across pair types. Violation of the corresponding restriction in the model of Figure 1 is the basis for Benin and Johnson's major finding. Although the overall fit of the model is still good ($L^2 = 49.06$ with 49 df with an improved—more negative—bic statistic), the deterioration in fit relative to Model F suggests that we may want to improve on this model. When we compared the deviations of estimated from observed values of the residual covariances, the only group that stood out was the OS-YB pair. For this reason, in the model of line I, we equate the other variances in the other 3 groups but not among OS-YB pairs. This model fits very well ($L^2 = 40.18$ with 48 df and bic = $-252.2$), and the contrasts with lines F and H clearly demonstrate that much of the discrepancy lies in the OS-YB pair. That is, Model I is nested between Models F and G: The closeness of fit to that of Model F shows that the restriction on the common variances

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11 We carried out similar analyses with an alternative ordering of the parameter restrictions suggested by a reviewer. After Model B, we first imposed the restrictions of Models D and E on $\Gamma$, followed by those of Models F and G on $\Lambda_{mm}$, and, finally, those of Model C on $\Theta^s$. We find that $\Theta^s$ is invariant across types of siblings but not equal across birth orders within types of siblings. Although the point estimate of the slope of the younger sibling's schooling on the family factor, $\lambda^s_{21} = .85$, remains less than that of the older sibling, the difference between the two loadings is not statistically significant. Thus, the finding of lesser parental influence on younger siblings is not entirely reliable. These results are available on request from the authors.

12 We also estimated effects of sex on the loadings of educational attainments on the common family factor, and these are not significant in either the NASIS data or the Lincoln data.
in the 3 other groups is not substantially violated by the data, and the deterioration in fit from Model I to Model H shows how different the common variance is among OS-YB pairs. The lack of homogeneity between OS-YB pairs and the others is consistent with Benin and Johnson’s hypotheses about the weak basis for role modeling and facilitation in such pairs. At the same time, the homogeneity of common variance in the other 3 groups is inconsistent with Benin and Johnson’s hypothesis that cross-sibling effects are strongest between OBs and YBs.13

The Lincoln Sample

Our reanalysis of the Lincoln data, reported in Table 3, follows exactly the same pattern as that of the NASIS data; estimates from our preferred model are reported in the lower panel of Table 2. In the Lincoln sample, the within-family variances of the siblings’ educational attainments are not equal for all types of pairs; they are equal only for siblings of the same sex (line D). In the Lincoln sample, the within-family variance among men is much larger than that among women (5.340 and 2.300, respectively, in Table 2), while the within-family variance among women is similar to that among men and women in the NASIS sample (2.593). This large residual variance implies the importance of some other variables that uniquely affect the schooling of urban men; one possibility, suggested by an examination of the tables of mean years of schooling, is that men in the Lincoln sample may have been far more likely than all women or men from elsewhere in the state to have completed college or obtained some postgraduate schooling (Benin and Johnson 1984, pp. 20–21).14

Although some of our findings are sensitive to the order in which tests of homogeneity are carried out, under the alternative ordering of tests described in footnote 11, there were no significant differences among groups in the variance of the unmeasured family factor. That is, under the alternative testing sequence, we found no support in the NASIS sample for the hypothesis that residual family effects differ among types of sibling pairs.15

In the case of the Lincoln data, the alternative order of model selection, described in footnote 11, had no effect on our findings. In particular, it did not affect our findings that parental influence is less on younger than on older siblings and that the residual family effects differ only between OS-YB pairs and all other types of pairs combined. Also, we note that the invariance restriction on \( \Phi \), which was rejected in our effort to replicate the Benin-Johnson analyses, is not rejected here when we use the \( \text{bic} \) statistic to guide our decision.

With this exception, our findings from the Lincoln data are similar to those from the NASIS data. First, the MIMIC model fits the data very well; background can be said to affect siblings’ schooling through a common factor. Second, the effects of parental socioeconomic background can be pooled across types of sibling pairs. Third, the effects of father’s education and mother’s education are essentially the same. Fourth, the effect of the common family background factor on the younger sibling is only about 70 percent as large as its effect on his or her older sibling. Finally, the variance of the disturbance of the common family factor is much lower in OS-YB pairs than in the other 3 types of pairs. Common, unmeasured family factors affect OB-YB, OB-YS, and OS-YS pairs more than they affect OS-YB pairs.15

To summarize, our reanalysis suggests some modifications of Benin and Johnson’s conclusions. Although our results were not radically different from theirs, we found, contrary to their contention, that previous empirical research has not led to a significant overestimation of the effect of unmeasured common family background on schooling, except in OS-YB pairs. Nevertheless, the low level of resemblance in OS-YB pairs raises an interesting sociological question. One plausible explanation, of course, is the theory of role modeling and facilitation proposed by Benin and Johnson, but this theory also predicts higher resemblance among pairs of brothers, for which the evidence is not persuasive. Additional empirical research is needed to strengthen the evidence in support of Benin and Johnson’s hypothesis or to provide an alternative explanation for the low level of resemblance in OS-YB pairs.

Fortunately, we can provide some additional evidence, for we learned of two efforts to replicate the Benin-Johnson analysis in
Table 3. Summary of Model Selection for Comparisons between Types of Sibling Pairs: Lincoln Sample

<table>
<thead>
<tr>
<th>Model</th>
<th>$L^2$</th>
<th>$df$</th>
<th>$p$</th>
<th>$bic$</th>
<th>Contrasts</th>
<th>$L^2$</th>
<th>$df$</th>
<th>$p$</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. MIMIC model</td>
<td>1.59</td>
<td>8</td>
<td>&lt;.99</td>
<td>-47.5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>B. A + $\Phi$ invariant</td>
<td>38.42</td>
<td>26</td>
<td>&lt;.06</td>
<td>-121.1</td>
<td>(B)-(A)</td>
<td>36.83</td>
<td>18</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>C. B + $\Phi'$ invariant and equal</td>
<td>64.00</td>
<td>33</td>
<td>&lt;.01</td>
<td>-138.5</td>
<td>(C)-(B)</td>
<td>25.58</td>
<td>7</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>D. B + $\Phi'$ equal for same sex</td>
<td>43.67</td>
<td>32</td>
<td>&lt;.01</td>
<td>-152.7</td>
<td>(D)-(B)</td>
<td>5.25</td>
<td>6</td>
<td>&lt;.52</td>
</tr>
<tr>
<td>E. D + $\Gamma$ invariant</td>
<td>49.73</td>
<td>41</td>
<td>&lt;.17</td>
<td>-201.8</td>
<td>(E)-(D)</td>
<td>6.06</td>
<td>9</td>
<td>&lt;.74</td>
</tr>
<tr>
<td>F. E + $\gamma_2$ = $\gamma_3$</td>
<td>52.73</td>
<td>42</td>
<td>&lt;.13</td>
<td>-205.0</td>
<td>(F)-(E)</td>
<td>3.00</td>
<td>1</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>G. F + $\Lambda_e$ invariant</td>
<td>57.19</td>
<td>45</td>
<td>&lt;.11</td>
<td>-218.9</td>
<td>(G)-(F)</td>
<td>4.46</td>
<td>3</td>
<td>&lt;.22</td>
</tr>
<tr>
<td>H. G + $\lambda_{21} = 1$</td>
<td>67.50</td>
<td>46</td>
<td>&lt;.01</td>
<td>-214.7</td>
<td>(H)-(G)</td>
<td>10.31</td>
<td>1</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>I. G + $\Psi$ invariant</td>
<td>66.56</td>
<td>48</td>
<td>&lt;.01</td>
<td>-227.9</td>
<td>(I)-(G)</td>
<td>9.37</td>
<td>3</td>
<td>&lt;.01</td>
</tr>
<tr>
<td>J. G + $\Psi$ invariant for OB-YB, OB-YS, and OS-YS pairs</td>
<td>59.97</td>
<td>47</td>
<td>&lt;.10</td>
<td>-228.4</td>
<td>(J)-(G)</td>
<td>2.78</td>
<td>2</td>
<td>&lt;.25</td>
</tr>
</tbody>
</table>

European samples.16 Dronkers (1988) reported an analysis of 1,072 Dutch families, based on a national sample of youths who left primary school in 1965. Using his data, we found that differences among types of pairs in residual family effects are not statistically significant in a global test. The estimate of residual variance among OB-YB pairs is larger than that in the other 3 types of pairs,17 but the variance among OS-YB pairs is as large or larger than that in OB-YS or OS-YB pairs. As in the Nebraska data, we found that the effect of the family factor on the schooling of younger siblings is less than that on older siblings, irrespective of gender; the point estimate of the loading is 25 percent smaller among younger siblings.

De Graaf and Huinink (1988) reported an analysis of 1,671 German families in which one sibling was born in 1930, 1940, or 1950. In separate analyses of each cohort of families, we found no evidence of heterogeneity among types of sibling pairs that would even tend to support the Benin-Johnson theory. None of the differences among types of pairs in residual family variances is statistically significant in a global test, and in no cases are the effects larger among OB-YB pairs or smaller among OS-YB pairs. However, in the German data, there is also very little evidence that family background has a smaller effect on younger than on older siblings. Although all the point estimates of loadings for younger siblings are less than 1.0, none is statistically significant, and this leaves open the generality of our finding in the Nebraska and Dutch data.

RECI PROCAL EFFECTS

Kalamazoo Brothers Sample

In the second part of our analysis, we estimate models of resemblance in the educational attainment of brothers that permit direct estimates of cross-sibling effects. The data from the survey of brothers from Kalamazoo were originally collected and analyzed by Olneck (1976, 1977, 1979). The sample consisted of men, identifiable as brothers, for whom there were sixth-grade school records in the Kalamazoo, Michigan, public school system for the years 1928–50. After the interviewing process, in which Olneck attempted to interview each member of each pair of siblings, the data contained 692 individual respondents, or 346 weighted pairs aged 35–59 in 1973 for whom complete self-reported data had been obtained. In the interviewing process Olneck first chose one (primary) brother at random from each sibship to interview; the interview with this individual ascertained data on social background, current social and economic status, and the social and economic status of each of the respondent’s brothers. If the selected person could not be interviewed, then another primary brother was selected. When the

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16 We thank Jaap Dronkers, Paul de Graaf, and Johannes Huinink for their generosity in permitting us to replicate their analyses and report additional findings. We are responsible for any errors in the interpretation of their data. The details of these analyses are available on request from the authors.

17 This difference is nominally significant at the .05 level in a one-tailed test.
second or other brothers from the same sibship were interviewed, the social background data and self-reports of status variables were ascertained again, but proxy reports about siblings were not obtained from secondary brothers. Thus, for each sibship, there are reports from a primary respondent that include proxy data for his brother or brothers and reports from each secondary brother that repeat measurements of social background but do not include proxy data for other brothers.

The data for the present analysis differ slightly from those used by Olneck. First, we verified all the data against responses from the original questionnaires; in a small number of cases, this verification altered matches between records. We recoded detailed occupational codes for fathers and brothers into the Duncan SEI (rescaled downward by a factor of 10 for convenience in reporting parameter estimates). Second, except in Olneck’s useful estimates of response error (1976, chap. 4; 1977, pp. 149–150), his published analyses used only the self-reported responses, but we also used proxy reports from the primary brother. By so doing, we obtain two reports of the secondary brother’s education attainment, which permit us to estimate the structural model with corrections for response variability in self-reports and proxy reports. Similarly, we use the reports of parental status characteristics by primary and secondary respondents to estimate their response variability. Third, to test hypotheses about directionality in cross-sibling effects, we order the members of each pair of brothers by age. This ordering leads to a two-sample design in which the older brother is the primary respondent in one sample and the younger brother is the primary respondent in the other sample. Finally, our effective sample size also differs from that reported by Olneck because we choose weights so the sum over pairs from each family is equal to \(1.18\). The weighting scheme depends on the number of siblings in each family or, to be more exact, the number of male siblings included in the data. The downward correction permits us to treat the data as if they were obtained by simple random sampling.

When the younger brothers are primary respondents.

In our analyses of the Kalamazoo data, as in Benin and Johnson’s analyses, each of the educational attainment variables is expressed in years of schooling and each of the occupational status variables is expressed in the Duncan SEI. Table 4 gives the means, standard deviations, and descriptions of variables in the two subsamples of brother pairs, and Table 5 gives the correlations within each subsample.

A Model of Intersibling Effects

Our model, shown in Figure 3, is similar in structure to that of Figure 2 in hypothesizing a latent common family background variable, \(\eta_6\), that is affected by father’s occupational status, \(\eta_2\), father’s educational attainment, \(\eta_3\), mother’s educational attainment, \(\eta_4\), and a disturbance, \(\varepsilon_6\). In turn, the common family factor affects the educational attainment of the primary brother, \(\eta_7\), and of that of the secondary brother, \(\eta_8\). Because the variables in Figure 3 refer to primary or secondary respondents, while our substantive interest lies in comparisons between older and younger brothers, we sometimes refer to parameters with a sample designation. When such a reference is necessary, we use the superscript (1) to refer to parameters in the sample in which older brothers were the primary respondents and the superscript (2) when younger brothers were the primary respondents. For example, in an initial specification of the model in Figure 3, we normalize the effect of the common factor by fixing \(\beta^{(1)}_{2,6} = \beta^{(2)}_{2,6} = 1\), where \(\beta^{(1)}_{2,6}\) is the effect of the common family factor on the educational attainment of older primary brothers, and \(\beta^{(2)}_{2,6}\) is the effect of the common family factor on the educational attainment of older secondary brothers.

There are three main differences between the models of Figure 2 and Figure 3. First, in Figure 3 all the variables in the structural model are latent constructs. Except for the educational attainment of the primary brother (ED), there are two indicators of each construct; for that variable, we estimate the error variance in the self-report by that estimated for self-reports by secondary brothers (ZED). Second, in the model of Figure 3, all the constructs are specified to be endogenous variables (\(\eta\)) in the LISREL model.
Table 4. Means, Standard Deviations, and Descriptions of Variables: Kalamazoo Brothers

<table>
<thead>
<tr>
<th>( Y_i )</th>
<th>Name</th>
<th>Older Primary</th>
<th>Older Secondary</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>( Y_1 )</td>
<td>IQ</td>
<td>10.110</td>
<td>9.811</td>
<td>Score on Otis test</td>
</tr>
<tr>
<td>( Y_2 )</td>
<td>EDQ</td>
<td>9.839</td>
<td>9.409</td>
<td>Score on Metropolitan Achievement Test</td>
</tr>
<tr>
<td>( Y_3 )</td>
<td>POPDUNC</td>
<td>3.856</td>
<td>3.716</td>
<td>Father’s occupation</td>
</tr>
<tr>
<td>( Y_4 )</td>
<td>ZPOPDUNC</td>
<td>3.852</td>
<td>3.742</td>
<td>Father’s occupation</td>
</tr>
<tr>
<td>( Y_5 )</td>
<td>POPED</td>
<td>9.909</td>
<td>9.432</td>
<td>Father’s education</td>
</tr>
<tr>
<td>( Y_6 )</td>
<td>ZPOPED</td>
<td>9.852</td>
<td>9.355</td>
<td>Father’s education</td>
</tr>
<tr>
<td>( Y_7 )</td>
<td>MOMED</td>
<td>10.851</td>
<td>10.059</td>
<td>Mother’s education</td>
</tr>
<tr>
<td>( Y_8 )</td>
<td>ZMOMED</td>
<td>10.536</td>
<td>10.116</td>
<td>Mother’s education</td>
</tr>
<tr>
<td>( Y_9 )</td>
<td>ZIQ</td>
<td>10.254</td>
<td>10.023</td>
<td>Score on Otis test</td>
</tr>
<tr>
<td>( Y_{10} )</td>
<td>ZEDQ</td>
<td>9.761</td>
<td>9.759</td>
<td>Score on Metropolitan Achievement Test</td>
</tr>
<tr>
<td>( Y_{11} )</td>
<td>ED</td>
<td>13.020</td>
<td>12.570</td>
<td>Education</td>
</tr>
<tr>
<td>( Y_{12} )</td>
<td>ZED</td>
<td>13.646</td>
<td>13.087</td>
<td>Education</td>
</tr>
<tr>
<td>( Y_{13} )</td>
<td>ZEDPR</td>
<td>13.703</td>
<td>12.869</td>
<td>Education by proxy</td>
</tr>
</tbody>
</table>

* Father’s occupational status is in Duncan’s SEI (scaled downward by a factor of 10). Educational attainments are in years of schooling. Scores on the Otis test and on the Metropolitan Achievement Test have also been scaled downward by a factor of 10. Names of variables pertaining to the secondary brother begin with “Z.”

...even the parental status characteristics. This is of no substantive importance, but it gives us greater freedom in specifying and testing correlated response errors (Hauser, Tsai, and Sewell 1983).

Third, and most important, we add constructs for the academic ability of each brother, \( \eta_1 \) and \( \eta_5 \), for which we use scores on the Otis or Terman group tests (IQ and ZIQ) and scores on the Metropolitan Achievement Test (EDQ and ZEDQ) as indicators.\(^{19}\) Thus, the educational attainment of the primary brother, \( \eta_7 \), is affected directly by his academic ability, \( \eta_1 \), as well as by the family factor, \( \eta_6 \). Similarly, the educational attainment of the secondary brother, \( \eta_8 \), is affected directly by his academic ability, \( \eta_5 \), and by the family factor, \( \eta_6 \). Not only is the specification of each sibling’s schooling improved by introducing an academic achievement construct, but that construct serves as an instrument for the estimation of simultaneous cross-sibling effects of educational attainment. That is, the primary brother’s academic ability, \( \eta_1 \), affects the secondary brother’s educational attainment, \( \eta_8 \), indirectly by way of his own educational attainment, \( \eta_7 \), and the secondary brother’s academic ability, \( \eta_5 \), affects the primary brother’s educational attainment, \( \eta_7 \), indirectly by way of his own educational attainment, \( \eta_8 \). There is no direct effect of \( \eta_1 \) on \( \eta_8 \) or of \( \eta_5 \) on \( \eta_7 \).

Our specification of reciprocal effects has at least two serious weaknesses. First, the model is misspecified to the degree that the academic ability of a man affects that of his brother directly or that the schooling of both brothers is affected by omitted common factors, correlated with academic ability, that explain the covariance of ability with schooling across brothers. Second, educational attainment measures the cumulative school experience of each brother; attainment occurs across time, and school completion need not be ordered in time to match the ordinal position of members of the fraternal pair. Thus, if we take educational attainment to represent schooling at the time of final school leaving, there is no temporal basis for specifying either an effect of the schooling of the older brother on the younger or the reverse. For our present purposes, and presumably consistent with past efforts to establish reciprocal effects of schooling among siblings, we took educational attainment as a proxy for decisions about the length of completed schooling that were not necessarily ordered in time as dates of school leaving. It will be useful for future research to test these critical assumptions by measuring additional background and intervening variables and, perhaps, by focusing on intervening variables, like aspirations, as the key endogenous variables that are subject to reciprocal influence.

Neglecting the distinction between older and younger brothers, within either subsample the structural model can be represented by the following equations:

\(^{19}\) We rescaled all the test scores downward by a factor of 10 for convenience in reporting parameter estimates.
\[ \eta_6 = \beta_{62} \eta_2 + \beta_{63} \eta_3 + \beta_{64} \eta_4 + \zeta_6, \quad (5) \]
\[ \eta_7 = \beta_{71} \eta_1 + \beta_{76} \eta_6 + \beta_{78} \eta_8 + \zeta_7, \quad (6) \]
and
\[ \eta_8 = \beta_{85} \eta_5 + \beta_{86} \eta_6 + \beta_{87} \eta_7 + \zeta_8. \quad (7) \]

where \( E [\zeta_i \eta_j] = 0 \) for \( i = 6, 7, 8 \) and \( j = 1, 2, 3, 4, 5 \); \( E [\zeta_6 \zeta_7] = E [\zeta_6 \zeta_8] = E [\zeta_7 \zeta_8] = 0 \); and \( E [\zeta_7 \eta_8] = E [\zeta_8 \eta_6] = 0 \). That is, \( \zeta_6, \zeta_7, \) and \( \zeta_8 \) are all uncorrelated with prior latent variables \( (\eta_i) \) on the right-hand side of Equations 5, 6, and 7, but \( E [\zeta_7 \eta_8] \) and \( E [\zeta_8 \eta_7] \) are not equal to zero because of simultaneity. The critical theoretical assumption of our analysis of cross-sibling effects is that each brother’s academic achievement affects the other brother’s schooling only indirectly.

**Findings**

We began our analysis of the model in Figure 3 by developing a measurement model that permits random measurement errors and selected correlations of measurement errors and that pools selected parameters between subsamples or between OBs and YBs. The loadings of both indicators of each parental variable, of the scores on the Otis or Terman test (but not those of the Metropolitan Achievement Test), and of self- and proxy reports of son’s educational attainment are fixed at unity a priori. That is,

\[
\lambda_{1,1} = \lambda_{3,2} = \lambda_{4,2} = \lambda_{5,3} = \lambda_{6,3} = \lambda_{7,4} = \lambda_{8,4} = \lambda_{9,5} = \lambda_{11,7} = \lambda_{12,8} = \lambda_{13,8} = 1
\]

Since there is only one indicator of the primary brother’s educational attainment, we use the estimate of error variance in the secondary brother’s self-report of educational attainment, \( \theta_{11,11} = \theta_{12,12} \). Although the loadings of academic achievement (EDQ or ZEDQ) on the academic ability factor \( (\eta_1 \text{ or } \eta_5) \) are free parameters of the model, preliminary analyses

---

20 Again, note that one of \( \beta_{76} \) and \( \beta_{86} \) is initially fixed at unity within each subsample.
indicated that their values were quite similar, and we constrained all four of these parameters to be equal, that is,

$$\lambda_{2,1}^{(1)} = \lambda_{2,1}^{(2)} = \lambda_{10,5}^{(1)} = \lambda_{10,5}^{(2)}.$$ (9)

To obtain a satisfactory fit, it is necessary to introduce two sets of correlations between errors in variables; these are not shown in Figure 3. First, as in previous research, we find clear evidence of a correlation between errors in reports of mother’s education and of the father’s education by the same respondent (Corcoran 1980; Hauser, Tsai, and Sewell 1983). Thus, we introduce free parameters for $\theta_{1,5}$ and $\theta_{6,6}$ in each subsample.

Second, we find strong evidence of correlation between errors in academic achievement (EDQ or ZEDQ) and errors in reports of parental status variables. For this reason, we introduce free parameters in each subsample for $\theta_{2,3}^{*}, \theta_{5,6}^{*}, \theta_{5,2}^{*}, \theta_{5,6}^{*}, \theta_{5,7}^{*}, \theta_{5,8}^{*}, \theta_{5,10,3}^{*}, \theta_{5,10,4}^{*}, \theta_{10,5}^{*}, \theta_{10,6}^{*}, \theta_{10,7}^{*}, \theta_{10,8}^{*}$. These error covariances are not clearly patterned; for example, some are significant and positive, while others are significant and negative. Thus, for instance, they do not appear to support either the argument that academic achievement is more highly correlated with parental status or that it is less highly correlated with parental status than one might expect from the factor model specified in Figure 3. Finally, we constrain all the error variances and covariances to be equal in the two subsamples.

The measurement model does not fit the data satisfactorily by conventional standards of statistical significance ($L^2 = 144.47$ with 99 df), but, given the number of observed variables in the model, their high degree of intercorrelation, the sample size, and the lack of systematic departures from fit, we find this to be a satisfactory baseline model. In the measurement model, we find $bic = -429.04$, which suggests a better fit than is indicated by the nominal significance level.

Table 6 shows selected parameters of the measurement model, estimated in the final version of the structural model (line F of Table 7). Our estimates of reliabilities are similar to those of Olneck (1976, p. 186), except the validity of academic ability is less than the reliability assumed by Olneck (p. 172) on the basis of published estimates. One might expect to find a lower value in our model because we treated measured mental ability as an indicator of a construct that also comprises academic achievement per se. The reliability of brothers’ educational attainments is slightly less than that estimated by Olneck, and the reliability of the proxy report of education (ZEDPR) is as high as that of the self-report (ZED). Except for the correlation between a man’s reports of his mother’s and father’s schooling, there is no evidence of a correlation between errors in reports of the same variable by the same respondent in the Kalamazoo survey itself. However, there remains the puzzlingly inconsistent set of correlations between errors in academic achievement (EDQ and ZEDQ) and errors in the reports of parental status.\(^{21}\)

\(^{21}\) In retrospect, we believe these errors are patterned, namely, that the correlations occur only between errors in measured academic achievement and
Table 7 displays the fit of selected versions of the model of Figure 3 in the two Kalamazoo subsamples. For each model, we report a brief description, the fit statistic ($L^2$), the dfs, and the bic statistic. As reported in line A, the initial version of the structural model yields $L^2 = 145.96$ with 103 dfs. There are 4 more dfs for error in the structural model because we estimated the measurement model by deleting $\eta_6$ from the setup of Figure 3 and freeing all the elements in the $\Psi$ matrix; that is, we ignored the restrictions in the structural model and permitted all the latent constructs to be freely correlated. The additional 4 dfs come from the two proportionality constraints of the MIMIC model within each subsample.

In line B, we report the fit of a model in which 26 constraints are added for consistency across subsamples within ages; that is, no age differences in parameters of the structural model are eliminated, but all differences between primary and secondary respondents within age are eliminated. In the $B$ matrix the constraints are

\begin{align}
\beta_{6,2}^{(1)} &= \beta_{6,2}^{(2)}, \\
\beta_{6,3}^{(1)} &= \beta_{6,3}^{(2)}, \\
\text{and } \beta_{6,4}^{(1)} &= \beta_{6,4}^{(2)},
\end{align}

which pertain to effects of parental statuses on the common family factor, and

\begin{align}
\beta_{7,1}^{(1)} &= \beta_{7,1}^{(2)}, \\
\beta_{7,5}^{(1)} &= \beta_{7,5}^{(2)}, \\
\beta_{7,6}^{(1)} &= \beta_{7,6}^{(2)}, \\
\beta_{7,7}^{(1)} &= \beta_{7,7}^{(2)}, \\
\text{and } \beta_{7,8}^{(1)} &= \beta_{7,8}^{(2)},
\end{align}

which pertain to age-specific effects of academic ability, of the common family factor, or of brother's educational attainment on educational attainment. Within $\Psi$ we introduce the following constraints:

\begin{align}
\psi_{1,1}^{(1)} &= \psi_{5,2}^{(2)}, \\
\psi_{5,5}^{(1)} &= \psi_{5,2}^{(1)}, \\
\psi_{1,2}^{(1)} &= \psi_{2,2}^{(2)}, \\
\psi_{5,2}^{(1)} &= \psi_{5,3}^{(2)}, \\
\text{and } \psi_{4,4}^{(1)} &= \psi_{4,4}^{(2)},
\end{align}

which pertain to variances of the exogenous variables, and

\begin{align}
\psi_{2,1}^{(1)} &= \psi_{5,2}^{(2)}, \\
\psi_{5,2}^{(1)} &= \psi_{5,2}^{(1)}, \\
\psi_{5,3}^{(1)} &= \psi_{5,3}^{(2)}, \\
\psi_{4,4}^{(1)} &= \psi_{4,4}^{(2)}, \\
\psi_{5,4}^{(1)} &= \psi_{5,4}^{(2)}, \\
\psi_{5,5}^{(1)} &= \psi_{5,5}^{(2)}, \\
\psi_{5,6}^{(1)} &= \psi_{5,6}^{(2)}, \\
\text{and } \psi_{4,3}^{(1)} &= \psi_{4,3}^{(2)},
\end{align}

which pertain to covariances among the exogenous variables, and

\begin{align}
\psi_{6,6}^{(1)} &= \psi_{6,6}^{(2)}, \\
\psi_{7,7}^{(1)} &= \psi_{7,7}^{(2)},
\end{align}

which pertain to variances of the endogenous variables. In this model, the test statistic increases only by $L^2 = 31.88$ with 26 df, which is not statistically significant, and the bic statistic decreases to $-569.46$; thus, in the remainder of the analysis, we disregard the distinction between primary and secondary respondents and look only at age differences in parameters of the model.

In the model of line B, we find $\beta_{8,7}^{(1)} = \beta_{7,8}^{(2)} = .310$ with a standard error of .106; thus, the effect of the older brother's educational attainment on the younger brother's attainment is highly significant statistically. On the other hand, we find $\beta_{8,7}^{(1)} = \beta_{7,8}^{(2)} = .013$ with a standard error of .130, which is obviously not statistically significant. This finding would appear to support Benin and Johnson's contention that the predominant flow of influence is from older to younger brothers. However, this finding is not entirely reliable. In line C of
Table 6. Selected Parameters of Final Measurement Model: Educational Attainments of Kalamazoo Brothers

<table>
<thead>
<tr>
<th>Parameter</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
<th>11</th>
<th>12</th>
<th>13</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \Theta^* )</td>
<td>0.44</td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>2</td>
<td>0.22</td>
<td>-2.05</td>
<td>-0.14</td>
<td>2.39</td>
<td>0.37</td>
<td>2.53</td>
<td>1.67</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<td></td>
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<tr>
<td>3</td>
<td>-0.30</td>
<td>1.13</td>
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<tr>
<td>4</td>
<td>-0.02</td>
<td></td>
<td>1.19</td>
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<tr>
<td>5</td>
<td>0.36</td>
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<td></td>
<td>2.45</td>
<td></td>
<td>5.45</td>
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</tr>
<tr>
<td>6</td>
<td>0.05</td>
<td></td>
<td></td>
<td></td>
<td>2.73</td>
<td></td>
<td>2.13</td>
<td></td>
<td></td>
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<tr>
<td>7</td>
<td>0.36</td>
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<td></td>
<td>0.62</td>
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<td>2.48</td>
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<tr>
<td>8</td>
<td>0.25</td>
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<td></td>
<td></td>
<td></td>
<td>2.14</td>
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<td>9</td>
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<td></td>
<td></td>
<td>0.52</td>
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<tr>
<td>10</td>
<td></td>
<td></td>
<td>-0.15</td>
<td>0.20</td>
<td>0.07</td>
<td>-0.31</td>
<td>-0.03</td>
<td>-0.15</td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Reliability ( (\Theta^*_j) )</td>
<td>0.79</td>
<td>0.85</td>
<td>0.78</td>
<td>0.75</td>
<td>0.77</td>
<td>0.77</td>
<td>0.71</td>
<td>0.76</td>
<td>0.81</td>
<td>0.91</td>
<td>0.87</td>
<td>0.90</td>
<td>0.90</td>
</tr>
</tbody>
</table>

* Diagonal entries are elements of \( \Theta^* \), that is, error variances. Below-diagonal entries are correlations between errors in variables; above-diagonal entries are \( t \)-statistics of corresponding error correlations. Variable identifications are 1 = IQ, 2 = EDQ, 3 = POPDUNC, 4 = ZPOPDUNC, 5 = POPED, 6 = ZPOPED, 7 = MOMED, 8 = ZMOMED, 9 = ZIQ, 10 = ZEDQ, 11 = ED, 12 = ZED, 13 = ZEDPR. See Table 4 for descriptions of the variables. Estimates and standard errors are based on the final structural model (line F of Table 7). There is one additional parameter of the measurement model, \( \lambda_{21} = \lambda_{105} = 0.865 \) with a standard error of 0.029.

Table 7, we report the fit of a model that differs from line B only by introducing the constraint \( \beta_{78} = \beta_{87} \) in both groups. This constraint increases the test statistic by 3.25 with 1 df, which is statistically significant at the .05 level, but not at the .01 level in a one-tailed test. The pooled estimate of cross-sibling influence is \( \beta_{76} = \beta_{87} = 0.166 \) with a standard error of .081. Thus, the data appear to support a finding of reciprocal influence in the educational attainments of brothers, but sampling variability may be too great to sustain a finding about the predominant direction of cross-sibling influence.

In the preceding analysis, we did not condition our test of equality in the reciprocal effects of brothers’ educational attainments on any other equalities between OBs and YBs in parameters of the structural model; thus, we also tried to select forward from a highly constrained structural model. We estimate a model of complete equality between OBs and YBs in the parameters of the structural model, and then we relax the assumption of equal reciprocal effects in educational attainment. The fit of these two models is reported in lines 4 and 5 of Table 7. Relative to the model of line C, seven additional constraints are needed to impose complete equality between OBs and YBs in parameters of the structural model. These constraints can be written without reference to the group structure of the data:

\[
\begin{align*}
\beta_{76} &= \beta_{86} = 1, \\
\beta_{71} &= \beta_{85}, \\
\psi_{11} &= \psi_{55}, \\
\psi_{21} &= \psi_{52}, \\
\psi_{31} &= \psi_{53}, \\
\psi_{41} &= \psi_{54}, \\
\text{and } \psi_{77} &= \psi_{88}. 
\end{align*}
\] (15)

These additional constraints lead to a nonsig-
significant deterioration in fit; comparing the models of lines 3 and 4, we find $L^2 = 12.01$ with 7 $df$. In the model of line D, the $bic$ statistic declines to $-600.54$, which is preferable to that of any earlier model. One interesting constraint in this set is that of the common family factor; recall that in the Nebraska data, this slope was about 70 percent as large for younger as for older siblings. In the Kalamazoo data, the point estimate was similar, $\hat{\beta}_{.6}^{(1)} = \hat{\beta}_{.6}^{(2)} = .686$; however, the standard error of this estimate was .255, which implies that the point estimate of the slope is not significantly different from unity.22

Relative to the model of line D, we can eliminate the restriction $\beta_{87} = \beta_{78}$ to test the hypothesis of equality in reciprocal effects of educational attainment. The fit of this model is shown in line E, for which $L^2$ declines only by 1.71 with 1 $df$; thus, by forward selection, we find still less evidence that the OB has a larger effect on the YB than the YB has on the OB. In this case, the difference between the unconstrained estimates of the reciprocal effects is also less than in the case of backward selection; the estimates are $\hat{\beta}_{87} = .211$ with a standard error of .092 and $\hat{\beta}_{78} = .111$ with a standard error of .084.23

Finally, drawing on our finding in the Nebraska data that the effects of mother’s and father’s educational attainments were not significantly different from one another, we add a similar constraint in the model of line F. The fit changes by $L^2 = 2.00$ with 1 $df$, so we confirm this finding in the Kalamazoo data.

The parameter estimates of the final structural model are reported in Table 8. In the Kalamazoo data, parental status and academic ability account for 90 percent of the variance in the common family education factor. This proportion may be compared with about 54 percent of the common variance explained by a vector of social background variables among male cohorts in the 1973 OCG survey (Hauser and Featherman 1976, p. 117), with 36 percent in the NASIS data, and with 53 percent in the Lincoln data. Evidently, the addition of academic ability to the model substantially increases the share of common family influence that can be explained. The common factor, in turn, accounts for 60 percent of the variance in individual educational attainment; the latter estimate is similar to those of Hauser and Featherman in the OCG cohorts. However, we note that in our estimates for the Nebraska data, the share of common variance in educational attainment of brothers may be lower: 67 and 49 percent among OBs and YBs in the NASIS data and 44 and 31 percent among OBs and YBs in the Lincoln data.

In the final model, we estimate $\hat{\beta}_{87} = \hat{\beta}_{78} = .174$ with a standard error of .081. Thus, each increase of a year in a man’s schooling leads directly to an increase of about one-sixth of a year of schooling for his brother. When we take account of the causal loop specified in the model (see Duncan 1975), we find that the total effect of one brother’s schooling on the other’s is thus $\hat{\beta}_{78}(1 - .174^2) = .179$, which appears to be substantially larger than the effect of either parent’s educational attainment.24 Of course, parental status is also represented in this model by father’s occupational status. In any event, although the estimated reciprocal effect is subject to substantial sampling variability, the finding is surely strong and important enough to justify the replication and extension of the present model to include additional antecedents of educational attainment.

---

22 In fact, the loadings of schooling on the common family factor scarcely differ between older and younger brothers in the unrestricted reduced-form equations, so we do not think that the Kalamazoo data provide any substantial support for our suggestion that families have smaller effects on younger than on older offspring.

23 The simultaneous equation model does not have much statistical power to detect differences between the effect of older on younger brother and of younger on older brother (Matsueda and Bielby 1986). For example, if we are prepared to reject the null hypothesis with $p = .05$, then the statistical power is only .27 when we test the null hypothesis of equal reciprocal effects against the alternative of the unconstrained estimates of reciprocal effects.

24 Because the variances of schooling among older and younger brothers are the same by construction, the estimated reciprocal effects are the same when the variables are expressed in raw or standardized form. That is, in the case of the reciprocal effects, the structural coefficients are also standardized coefficients.
Table 8. Estimated Parameters of the Structural Model of Sibling Resemblance and Cross-sibling Effects on Educational Attainment: Kalamazoo Brothers

<table>
<thead>
<tr>
<th>Independent Variable</th>
<th>( \eta_6 )</th>
<th>( \eta_7 )</th>
<th>( \eta_8 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \eta_1 ) = older brother's</td>
<td></td>
<td>.866</td>
<td></td>
</tr>
<tr>
<td>academic ability</td>
<td></td>
<td>(.082)</td>
<td></td>
</tr>
<tr>
<td>( \eta_2 ) = father's occupational</td>
<td>.156</td>
<td></td>
<td></td>
</tr>
<tr>
<td>status</td>
<td>(.062)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \eta_3 ) = father's educational</td>
<td>.091</td>
<td></td>
<td></td>
</tr>
<tr>
<td>attainment</td>
<td>(.025)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \eta_4 ) = mother's educational</td>
<td>.091</td>
<td></td>
<td></td>
</tr>
<tr>
<td>attainment</td>
<td>(.025)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \eta_5 ) = younger brother's</td>
<td></td>
<td>.866</td>
<td></td>
</tr>
<tr>
<td>academic ability</td>
<td></td>
<td>(.082)</td>
<td></td>
</tr>
<tr>
<td>( \eta_6 ) = common family</td>
<td></td>
<td>1.000*</td>
<td>1.000*</td>
</tr>
<tr>
<td>education factor</td>
<td></td>
<td>(.082)</td>
<td>(.082)</td>
</tr>
<tr>
<td>( \eta_7 ) = older brother's</td>
<td>.174</td>
<td></td>
<td></td>
</tr>
<tr>
<td>educational attainment</td>
<td>(.081)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \eta_8 ) = younger brother's</td>
<td>.174</td>
<td></td>
<td></td>
</tr>
<tr>
<td>educational attainment</td>
<td>(.081)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \psi_{ij} ) = disturbance variance</td>
<td>.051</td>
<td>2.482</td>
<td>2.482</td>
</tr>
<tr>
<td></td>
<td>(.479)</td>
<td>(.466)</td>
<td>(.466)</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>.902</td>
<td>.606</td>
<td>.606</td>
</tr>
</tbody>
</table>

a Values in parentheses are standard errors. Fixed coefficients, for which there are not standard errors, are indicated by "*".

DISCUSSION

With regard to both major issues in the present analysis—the existence of cross-sibling effects and their predominant direction—there are obvious needs and opportunities for additional research. The indirect evidence of mutual influence marshaled by Benin and Johnson appears also in our extension of their analysis, yet we find less evidence in the Nebraska data that cross-sibling effects are larger among male siblings. Like the Benin-Johnson data for Nebraska, the Kalamazoo data appear to confirm a process of mutual influence between brothers in the attainment of schooling, but they provide evidence that is little more than suggestive with regard to a predominant flow of influence from older to younger siblings.

Another, less central issue in the analysis also deserves further attention. There are indications in the NASIS, Lincoln, and Dutch data, though not in the German or Kalamazoo data, that the effect of parental statuses on younger offspring may be less than that among older offspring. We think there should be further tests of this finding; it may indicate a birth-order effect that is quite different from the additive effects that are usually investigated. Why should parental status matter less for younger children? Do parents use more status-linked resources in the upbringing of older children? Are parents less concerned with the maintenance of status among younger children? Or is this evidence that cohort trends in the effects of social background on educational attainment, like those in mean levels of schooling (Hauser and Sewell 1985), occur within the family, as well as among families?

Our findings about measurement error in the Kalamazoo data reinforce the evidence from earlier studies that proxy reports of status variables by adult offspring about one another are just about as accurate as are self-reports. However, both types of reports are subject to response variability, and there appears to be more response variability in proxy reports of parents' educational attainments than in those of adult offspring. Thus, it would appear wise to continue efforts to collect data on siblings (and other survey-based measurements of social stratification) using research designs that generate independent multiple measurements of status variables.

Given the level of sampling variability in both the Nebraska and Kalamazoo data, it will be useful to replicate the present analysis among Wisconsin siblings and other available

25 Compare the diagonal elements of Table 6 pertaining to the statuses of parents and offspring.
samples of siblings. For example, in data from the Wisconsin Longitudinal Study, it will be possible to estimate models like that of Figure 3 for mixed sex pairs ordered by age, as well as among brothers. It will also be useful to add more social background variables to the models and to incorporate explicit measurements of adolescent aspiration. As was noted earlier, these elaborations should make it possible to test assumptions of the simultaneous equation model used in the present analysis.

Finally, in future research, we may want to ask whether mutual influence among siblings may also be a source of resemblance in postschooling success. For example, do men or women find ways to improve the employment or occupational chances of their siblings or to provide opportunities for better earnings? As this outline for future research becomes longer, we ought not to forget that there may be more direct ways of learning about cross-sibling influence: Better models and methods may help us to understand the sources of familial resemblance, but we ought also to ask people about their relationships with their siblings.

REFERENCES


26 These might include the samples of siblings from the original Parnes NLS samples, those from the 1979 Ohio State–National Opinion Research Center sample of American youths, and samples of siblings drawn from High School and Beyond.
Robert M. Hauser, Ph.D., is Vilas Research Professor and Samuel A. Stouffer Professor, Department of Sociology, and Director, Center for Demography and Ecology, University of Wisconsin–Madison. His main fields of interest are social stratification, social statistics, education, and research methods. He is currently working on further studies of sibling resemblance in educational attainment, on trends in social mobility, and on the role of genetic and environmental factors in the determination of individual differences.


SIBLING RESEMBLANCE

mobility in the United States, and on trends in college aspirations and enrollment of blacks and whites. He recently completed a study of pay equity among nonfaculty professionals in a large university.

Raymond Sin-Kwok Wong, MA, is a Ph.D. dissertator, Department of Sociology, University of Wisconsin–Madison. His main fields of interest are social stratification, comparative sociology, quantitative methods, sociology of economic change, and demography. He is now working on his dissertation on comparative social mobility in six countries to understand cross-national and cross-temporal stability and variability in mobility patterns.

ERRATA

Please note the following two errors that appeared in Brian Powell and Lala Steelman, “The Liability of Having Brothers: Paying for College and the Sex Composition of the Family” (April 1989 issue). In both cases, the term “sex composition” is used instead of “sex:”

1. Page 145, first paragraph, first sentence should read:
   “At first glance, it may appear that the absence of a main effect of sex, except for savings and the degree of self-support, concurrent with the presence of a strong negative association between the number of brothers and parental contribution, is counterintuitive.”

2. Page 145, first paragraph, seventh sentence should read:
   “Thus, daughters and sons who attend college or at least survive the first few years of college may both receive financial support; the result is no significant effect of sex on the dependent variables used in our study.”