Sibship Size and Intellectual Development: Is the Relationship Causal?
Author(s): Guang Guo and Leah K. VanWey
Reviewed work(s):
Source: *American Sociological Review*, Vol. 64, No. 2 (Apr., 1999), pp. 169–187
Published by: American Sociological Association
Stable URL: http://www.jstor.org/stable/2657524
Accessed: 21/01/2013 21:29

SIBSHIP SIZE AND INTELLECTUAL DEVELOPMENT: IS THE RELATIONSHIP CAUSAL?*

Guang Guo
University of North Carolina-Chapel Hill

Leah K. VanWey
University of North Carolina-Chapel Hill

Previous research has consistently found a negative statistical relationship between sibship size and children's intellectual development. Two explanations have been offered for this finding. The prevailing explanation is that the relationship is causal, suggesting that limiting family size would lead to more intelligent children. A second explanation maintains that the relationship is spurious—that one or more undetermined factors correlated with family size are causally related to intellectual development. Using data on children from the National Longitudinal Survey of Youth, we reexamine the issue using change models. These change models allow us to control for such unmeasured effects as family intellectual climate, family value system, and family genetic heritage. We begin by replicating in these data the negative statistical relationship between three cognitive measures and sibship size. We then apply the change models to siblings measured at two points in time and to repeated measures of the same individuals. By considering sibship size as an individual trait that changes over time, we control for effects that are shared across siblings and over time. When these shared effects are controlled, the negative relationship between sibship size and intellectual development disappears, casting doubt on the causal interpretation of the negative relationship conventionally found.

The effects of sibship structure including sibship size or family size,[1] birth order, and birth spacing on intellectual development have attracted scholarly interest at least since the late 1800s, when Galton (1874) noted the intellectual advantages of the eldest-born child. Among the various aspects of sibship structure, only sibship size consistently has been found to be negatively associated with children's intellectual achieve-

ment or, more generally, "child quality" (Blake 1981, 1985, 1989; Downey 1995; Featherman and Hauser 1978; Powell and Steelman 1993; Steelman 1985; Steelman and Powell 1989).

Many leading scholars have offered advice to parents and would-be parents. In an article titled "Dumber by the Dozen" in *Psychology Today,* Zajonc (1975), the chief architect of the highly influential confluence model, wrote,

> If the intellectual growth of your children is important to you, the model predicts that you should have no more than two (children) . . . because the larger the family, the lower the overall level of intellectual functioning. (P. 43)

Blake (1981), in her presidential address to the Population Association of America titled "Family Size and the Quality of Children," concluded,

> If people believe that they can trade off child quantity for child quality they are, indeed, on the right track. (P. 433)

[1] "Sibship size" and "family size" are used interchangeably throughout this article: Both terms refer to the number of children in the family.

In the same address, Blake further concluded that family size is a more important factor than socioeconomic status, because by the time a family is formed, family socioeconomic status has been largely determined whereas final family size is still a decision variable. A couple can more easily influence the quality of their children through family-size decisions than by trying to change their socioeconomic status. All this advice rests on the premise that the effect of family size on child quality is causal. If the effect is not causal, couples only reducing the desired number of children will not see improved quality in their children.

To date, almost all the evidence for the relationship between sibship size and child quality comes from conventional regression analysis using cross-sectional data, and the causal interpretation of the statistical relationship has been frequently questioned. The alternative interpretation is that the statistically found negative relationship between sibship size and child quality is spurious, induced by another factor or combination of other factors highly correlated with sibship size, such as family socioeconomic status (Anastasi 1956; Blake 1989; Ernst and Angst 1983; Steelman 1985), family genetic heritage (Anastasi 1956; Blake 1989; Grotevant, Scarr, and Weinberg 1977; Steelman 1985), or the general intellectual climate of the home (McCall 1984).

In this study, we test the alternative interpretation of the effect of sibship size on child's intellectual development through sibling analysis and analysis of repeated measures of the same individuals. Both analyses are variations of change models or fixed-effects models. Change models enable us to control permanent family effects including family socioeconomic status (SES), family genetic makeup, and intellectual atmosphere in the home by "differencing them out." Thus, we can determine if, and how much, the sibship-size effect is confounded by other family influences that are difficult or impossible to control in conventional regression analysis.

The data are from the National Longitudinal Survey of Youth (NLSY). Our first step is to replicate the usual finding using the conventional regression approach; then we apply the change models. The change models take advantage of two features of the NLSY: (1) Cognitive development of the same individual is measured at least twice—in 1986 and 1992; and (2) the sibship size relevant to each individual changes between 1986 and 1992.

In this analysis, we measure child quality by intellectual development, which is in turn measured by three standardized cognitive tests administered in the NLSY. Cognitive test scores are the most commonly used dependent variables in sibship studies (Cicirelli 1978; Downey 1995; Steelman 1985). More recently, sociologists considered other measures of child quality. Blake (1981, 1989) frequently measured child quality by total years of education in her own empirical work, but she based her conclusions on both her own work and previous research on the relation of sibsize to cognitive ability. Steelman and Powell (1989, 1991) studied the effect of sibship size on parental financial support of college education. Because we measure child quality by intellectual development, our work only speaks directly to previous research using measures of cognitive development.

CONTENDING EXPLANATIONS

Most researchers on the topic agree that there exists a negative statistical relationship between sibship size and intellectual development (Blake 1989; Downey 1995; Steelman 1985). This finding holds in studies using data from the United States (Blau and Duncan 1967; Featherman and Hauser 1978) and also in large-scale studies of school children in France (Institut National d'Études Démographiques 1973), Scotland (Scottish Council for Research in Education 1949), and England (Douglas 1964; Eysenck and Cookson 1970; Marjoribanks 1974). Two theoretical models have been developed to interpret the effect of sibship size on child quality as causal. Most researchers, however, acknowledge the possibility of a spurious effect caused by one or more factors other than sibship size.

The Confluence Model

The confluence model was developed to explain not only the relationship between sib-

ship size and intellectual development but also that between birth spacing/birth order and intellectual development (Zajonc and Markus 1975). Zajonc and Markus theorized that a child's intellectual development is a function of two factors: family intellectual environment and a child-to-child teaching benefit. Family intellectual environment, measured by the average intellectual level of all members of the family, deteriorates with the birth of each child as a result of the low intellectual level of young children. This part of the theory explains the negative link among sibship size/birth spacing and intellectual development. The theory also argues that teaching a younger sibling is beneficial to intellectual development. Last-born children are at a disadvantage because they have no younger siblings to teach. In spite of its elegance and appeal, most studies failed to find empirical support for the confluence model (Berbaum and Moreland 1980; Galbraith 1982; Retherford and Sewell 1991; Rodgers 1984).

The Resource Dilution Model

The resource dilution model focuses on the effect of sibship size alone (Blake 1981, 1989). A child's intellectual development depends on family resources. The more children in a family, the more the resources are divided, the fewer resources each child would enjoy, and therefore the lower the quality of the children. Blake (1981) described three categories of family resources. First, parents provide "types of home, necessities of life, cultural objects (like books, pictures, music, and so on)" (p. 422). Second, parents provide "specific chances to engage the outside world" or specific chances "to get to do things" (p. 422). Finally, parents provide "personal attention, intervention, and teaching" (p. 422). Using data from the National Education Longitudinal Study of 1988, Downey (1995) analyzed data on family size, parental resources, and educational performance and found that the effect of family size is mediated through parental resources.

The resource dilution theory appears to offer a particularly appealing explanation for the effect of sibsize on parental financing of higher education (Steelman and Powell 1989, 1991), but for child intellectual growth

we must proceed with caution. A negative effect of sibship size on intellectual growth is typically obtained in regression analyses based on cross-sectional data, in which sibship size and intellectual growth are measured at about the same time. There is one potential problem with the interpretation of results thus obtained via the resource dilution theory. If sibship size affects intellectual growth, it must do so in a dynamic process. A family acquires children gradually over a number of years and children continually undergo intellectual development. Intellectual development may be more sensitive to environmental influences such as family size at some ages than at others.

If sibship size does dilute family resources and affect intellectual growth, the sibship size that dilutes resources will not be the sibship size measured in the cross-sectional data and used in the regression analysis. Not all siblings would influence the index child's intellectual development at any particular time. To test the resource dilution model, we must account for changes in sibship size during a child's intellectual growth. This argument is consistent with the emphasis on determining causal direction and the use of longitudinal and intact family data in many articles that tested the confluence model (Galbraith 1982; Olneck and Bills 1979; Retherford and Sewell 1991; Rodgers 1984). This argument also provides a theoretical basis for our empirical approach of regressing *changes* in intellectual development on *changes* in sibship size.

Sibship Size as a Spurious Effect

From the beginning, scientists studying the relationship between intellectual development and sibship size realized that the apparent association could be spurious. Various possible factors causally affecting both intellectual development and sibship size have been identified. The actual causal relationship may well involve a combination of two or more of these factors.

Some researchers suspect that family SES, not sibship size, is the actual causal factor (Anastasi 1956; Ernst and Angst 1983; Kennett and Cropley 1970; Steelman 1985). If parents of lower SES are more likely to have large families and SES is not ad-

equately adjusted for, a statistical effect of sibship size on intellectual development will emerge even if the causal factor is SES.

Genetic factors also have been suggested as possible causal variables behind the relationship between sibship size and intellectual development (Anastasi 1956; Blake 1989; Grotevant et al. 1977; Steelman 1985). It is well known that parental cognitive ability tends to be correlated with offspring's cognitive ability. If less able parents tend to have more offspring because of poor family planning or some other reason, research would reveal a negative relationship between children's cognitive test scores and sibship size even if the relationship is not causal. In a volunteer sample of 120 natural and 104 adoptive families in Minnesota, Scarr and Weinberg (1979) found the usual correlation of IQ with sibship size in natural children, but IQ in the adopted children was not correlated with the number of children in the family. This finding seems to indicate that the correlation relies on genetic relatedness among children in the sample and suggests that the estimated sibship effect may actually be an effect of genetic heritage.

Some potentially confounding factors are less tangible. McCall (1984) described the intellectual climate of the home. The climate is assumed to have an equal effect on all children within a family. Factors characterizing the climate include parental encouragement of intellectual activities, opportunities for enriching experiences, and so forth. Couples who choose to have large families may have value systems (Steelman 1985) or normative expectations (Rankin, Gaite, and Heiry 1979) that differ from those of couples who choose to have small families. Parents who prefer small families may value academic success, for example. If any of these is the case, the causal factor for intellectual growth would not be sibship size, but home intellectual climate, value systems, or normative expectations, which affect both fertility and intellectual development.

The standard strategy for rejecting these noncausal competing explanations is to take them into account as far as possible in conventional regression analysis. However, some potential confounding factors such as family income and family genetic influences are difficult to measure. Others, such as home intellectual climate, value systems concerning academic success, and normative expectations, may never be adequately measured. As a result, we must look beyond conventional regression analysis for persuasive evidence for the causal explanation.

THE METHODS

Conventional Regression Analysis

Regression analysis of cross-sectional data is the most frequently used nonexperimental approach. It may be presented as

$$id_i = \beta_0 + \beta_1 fs_i + \beta_2 \mathbf{CH}_i + \beta_3 \mathbf{FAM}_i + \beta_4 \mathbf{NEI}_i + \varepsilon_i, \tag{1}$$

where intellectual development (id_i) for child i is a function of family size (fs_i) controlling child characteristics ($\mathbf{CH}_i$), family characteristics ($\mathbf{FAM}_i$), and other environmental characteristics such as neighborhood characteristics ($\mathbf{NEI}_i$). The lower-case variables such as id_i are scalar variables whereas the upper-case variables such as $\mathbf{FAM}_i$ are vector variables indicating that more than one family characteristic are imbedded in $\mathbf{FAM}_i$. Both the confluence theory and the resource dilution theory predict a negative and significant effect of sibship size from equation 1.

The potential problem with this approach is that the estimated effect of sibship size could be spurious if equation 1 fails to include even one child, family, or neighborhood variable that is causally related to intellectual development and correlated with sibship size. For this reason, we consider other approaches to test for the robustness of the results obtained from conventional regression analysis.

Change Models

Change models (or fixed effects models) constitute one promising solution to the problem of omitted variables (Liker, Augustyniak, and Duncan 1985). Change models allow control for unobserved variables whose effects are shared by clusters of observations. In the present analysis, change models can control for such unobserved familial variables as family intellectual climate as long as the effects of these variables do

not change over time and are shared by all family members. The exact "amount" of unobserved familial effect controlled is determined by the extent to which the effect is shared. We apply two variations of change models: sibling analysis and analysis of repeated measures of the same individuals.

Sibling analysis and twin analysis were used to study the effect of education on income (Ashenfelter and Krueger 1994; Gorseline 1932; Griliches 1979). The central concern of such studies is that the better educated tend to be more able and that the more able tend to earn higher incomes. The effect of education would be exaggerated if innate ability were not adequately controlled. Innate ability is exceedingly difficult to measure in conventional regression analysis; sibling analysis and twin analysis can control whatever ability is shared among siblings or between twins without measuring it explicitly.

Using change models and longitudinal data for 62 less-developed countries, Firebaugh and Beck (1994) were able to control unmeasured enduring national characteristics. They found large and robust positive effects of economic growth on national welfare and found no negative effects of Third World's dependence on foreign investment and trade. These results contrast sharply with those from more conventional dependency studies, which typically regress the difference between the two outcome variables for the two time periods on the original set of independent variables. Such dependency studies often found that economic growth harms rather than benefits the living standards of the masses.

Sister-pair analysis caused a major reconsideration of the socioeconomic costs of teenage childbearing (Geronimus and Korenman 1992, 1994; Hoffman, Foster, and Furstenberg 1993a, 1993b). Conventional analysis generally found substantial socioeconomic costs for young mothers. The difficulty with this conclusion is that teenage births are much more common in socioeconomically disadvantaged families. Geronimus and Korenman (1992) argued that measures of family background used in conventional analysis might have been insufficient. Using sister-pair analysis, they concluded that previous estimates based on con-

ventional analysis had exaggerated the negative consequences of teenage childbearing.

Sibling models have been used to study the effect of birth order on intellectual development as tests of the confluence model (Olneck and Bills 1979; Retherford and Sewell 1991). Once the unmeasured effects shared by sibling pairs were adjusted for, no significant effects of birth order were found, providing evidence against the empirical validity of the confluence model.

To our knowledge, sibling analysis has not been applied to the study of the sibship-size effect. Sibship size is traditionally regarded as a family trait, meaning that the value of this variable is the same for all children of a family. A typical change model such as a sibling model would difference out all family variables including sibship size. The consequence is that the effect of sibship size would not be estimated. In our study, using longitudinal data, we treat sibship size as an individual trait that changes as a child ages. Later, we will show that sibship size can change significantly between two measures of intellectual development on the same individual or between two measures of intellectual development for a pair of siblings.

Sibling analysis. Sibling models require data consisting of sibling pairs or sibling clusters. The promise of sibling data lies in the fact that siblings share a variety of hard-to-observe influences. Siblings share substantial amounts of environmental influences: They have similar access to family economic resources, are exposed to a similar family intellectual climate, and grow up under the influences of the same parents, similar friends, similar neighborhoods, and similar schools. Sibling data have been used in previous studies because siblings on average share one half of their genes (Plomin, DeFries, and McClearn 1980). When sibling data are used to study the effect of education on income, the genetic relatedness among full siblings enables researchers to control for about one half of the variance in innate ability.

There is a subtle and important difference between our use of sibling data and how sibling data are traditionally used. Instead of controlling for the genetic effects shared among siblings as in a traditional approach, we control for parental genetic effects. It is

Table 1. Levels of Control for Selected Unobserved Variables in the Conventional Regression Analysis, Sibling Analysis, and Analysis of Repeated Measures, and Expected Family-Size Effects

Unobserved Variables	Conventional Regression Analysis	Sibling Analysis	Analysis of Repeated Measures
Variables Controlled			
Family environment	None	Substantial	Most
Family genetics	None	100%[a]	100%
Child-specific	None	None	Most
Family environment × Family genetics × Child-specific	None	None	Most
Expected Magnitude of Family-Size Effect		Weakening effect	

[a] We control here for parental genetic influence rather than for the proportion of genes shared among siblings. Full siblings share 100 percent of their parents' genetic influence, whereas they traditionally are thought of as sharing 50 percent of their own genes.

parental genetic effects rather than genetic effects shared among siblings that may be correlated with family size. Since full siblings have the same parents, and thus the same genetic influences at the parental level, the percentage of genetic influences controlled for at the parental level should be 100 instead of 50. By the same argument, half siblings share about 50 percent of the genetic influences at the parental level because half siblings share only one biological parent. In Table 1, we summarize the magnitude of various unobserved effects controlled in conventional regression analysis, full-sibling analysis, and analysis of the repeated measures of the same individuals. As more potentially confounding effects are controlled, we expect the negative effect of sibship size to weaken if not to disappear altogether.

Sibling analysis controls for all effects shared between the two members of a sibling pair by cancelling them out. Suppose the conventional analysis (equation 1) holds for sibling A at time t_1 and sibling B at time t_2 in a sibling pair:

$$id_{At_1} = \beta_0 + \beta_1 fs_{At_1} + \beta_2 \mathbf{CH}_{At_1}$$
$$+ \beta_3 \mathbf{FAM} + \beta_4 \mathbf{NEI} + \varepsilon_{At_1}, \quad (2)$$

$$id_{Bt_2} = \beta_0 + \beta_1 fs_{Bt_2} + \beta_2 \mathbf{CH}_{Bt_2}$$
$$+ \beta_3 \mathbf{FAM} + \beta_4 \mathbf{NEI} + \varepsilon_{Bt_2}, \quad (3)$$

where neither **FAM** nor **NEI** has a subscript, implying that the siblings in the same family share the same time-invariant family and

neighborhood influences. A sibling model can be constructed by subtracting equation 2 from equation 3:

$$id_{Bt_2} - id_{At_1} = \beta_1\left(fs_{Bt_2} - fs_{At_1}\right)$$
$$+ \beta_2\left(\mathbf{CH}_{Bt_2} - \mathbf{CH}_{At_1}\right)$$
$$+ \left(\varepsilon_{Bt_2} - \varepsilon_{At_1}\right), \quad (4)$$

where the family and neighborhood influences cancel out. Note that equations 2 and 3 must be established for different times to allow sibship size to change and that the three β_1s in equations 2, 3, and 4 are equivalent. Now we can obtain an estimate of the effect of sibship size (β_1) from the regression of changes in intellectual development on changes in sibship size and other child characteristics without having to measure all the shared effects, familial or otherwise. This estimate is free of biases caused by family or neighborhood influences shared by the two members of a sibling pair.

Repeated measures analysis. Analysis of repeated measures of the same individuals represents an extreme form of change models. The repeated measures of the same child resemble the measures from a sibling cluster. The difference is that the repeated measures are subject to much more similar environmental influences than are the measures from a sibling cluster. Most of the familial and other environmental influences on a child at different time points would remain largely unchanged (Table 1). The potential genetic influence at the parental level shared

by the repeated measures of the same individual is the same as that shared by full siblings (100 percent).

Unlike sibling analysis, analysis of the repeated measures control all child characteristics that remain stable over time such as individual ability and perseverance. Also controlled are the interactions between familial and child characteristics because these interaction effects tend to be shared by all the measures of the same individual. In conventional regression analysis, some of these interactions may contribute to a spurious relationship between sibship size and child intellectual development. For instance, the possible interaction between family SES and child ability would be partially responsible for the spurious relationship if high-SES parents tend to have fewer children, if high-SES parents with more resources are more likely to make a greater investment in brighter children, and if SES is not taken into consideration adequately.

To construct a model of repeated measures of the same individuals, suppose the conventional analysis (equation 1) holds for child i at both t_1 and t_2,

$$id_{t_1} = \beta_0 + \beta_1 fs_{t_1} + \beta_2 \mathbf{CH} + \beta_3 \mathbf{FAM}$$
$$+ \beta_3 \mathbf{NEI} + \beta_5 Age_{t_1} + \varepsilon_{t_1}, \qquad (5)$$

$$id_{t_2} = \beta_0 + \beta_1 fs_{t_2} + \beta_2 \mathbf{CH} + \beta_3 \mathbf{FAM}$$
$$+ \beta_4 \mathbf{NEI} + \beta_5 Age_{t_2} + \varepsilon_{t_2}, \qquad (6)$$

Note that no subscript is present for **CH**, **FAM**, and **NEI**, reflecting the observation that repeated measures of the same individuals are under similar child-specific, familial, and neighborhood influences. Taking the differences between the two time points at which the intellectual development of the same child is measured yields:

$$id_{t_2} - id_{t_1} = \beta_1 \left(fs_{t_2} - fs_{t_1} \right)$$
$$+ \beta_5 \left(Age_{t_2} - Age_{t_1} \right)$$
$$+ \left(\varepsilon_{t_2} - \varepsilon_{t_1} \right), \qquad (7)$$

where all child-specific characteristics except age cancel out. The model requires that there is enough variation in family size (fs) between t_1 and t_2. Because this analysis of repeated measures controls for more poten-

tially confounding effects, we expect that the strength of the estimated sibship effect from the analysis of repeated measures will be the weakest among all the models considered in Table 1.

DATA

We use data from the National Longitudinal Survey of Youth (NLSY). Our research design requires (1) that intellectual development is measured at least twice over time on the same individual or on a pair of siblings with one measure on each sibling, (2) that the time span between the two measures is wide enough to allow change in sibship size to occur, and (3) that the two measures are comparable. The NLSY contains a large number of children for whom intellectual development was repeatedly assessed in 1986, 1988, 1990, and 1992. Because of the household sampling design of the survey, the NLSY sample contains a large number of sibling pairs.

Although the NLSY sample includes many related children, the exact nature of the kinship link is sometimes ambiguous. For instance, an apparent sibling pair can be a full- or half-sibling pair. The shared genetic and environmental effects vary widely across these two types of pairs. For instance, unlike full siblings, half siblings only spend part of their lives in the same family. Half siblings' intellectual development is subject to only some of the same familial influences. While they share some environmental influences from shared family culture, friends, schools, and neighborhoods, the shared influences must be less than those full siblings are subject to. How much less is unclear and depends on how long the half siblings lived together.

Fortunately, linking algorithms that classify the NLSY respondent pairs into appropriate kinship categories were first developed for the 1986 and 1988 NLSY-children data (Rodgers, Rowe, and May 1994) and then were updated for the 1990 and 1992 data (Charng and Baydar 1996). To determine sibling status, children were first assigned to sibling pairs. For each pair and for each year, the index child's report of the residence of his or her father was compared to the sibling's report of his or her father's residence. The consistency of father's residence

within and across years was used to determine whether a sibling pair consisted of full siblings, half siblings, probable full siblings, or probable half siblings. We use Charng and Baydar's (1996) update of the linking algorithm to distinguish between full-sibling and half-sibling pairs.

Full-sibling and half-sibling pairs seem to have grown up in conspicuously different environments in our NLSY sample. The average household income for half siblings is about 64 percent of that for full siblings in both 1986 and 1992. The proportion of never-married women among mothers of half siblings is more than three times as high as that of full siblings. Mothers of half siblings scored much lower on the AFQT (a cognitive test) and experienced much higher marital instability. The samples of half siblings by racial group are sometimes unacceptably small (30 or 40). For these reasons, we decided to exclude half siblings from the sibling analysis.[2]

MEASURES

We measure children's intellectual development by three cognitive tests: (1) The Peabody Picture Vocabulary Test–Revised (PPVT) "measures an individual's receptive (hearing) vocabulary for Standard American English and provides, at the same time, an estimate of verbal ability or scholastic aptitude" (Dunn and Dunn 1981). This assessment is administered to children aged three and older. (2) The Reading Recognition Assessment of the Peabody Individual Achievement Test (PIAT-R) measures word recognition and pronunciation. Children are asked to read a word silently and then to say it aloud.

---

[2] We have excluded the military sample that does not include any siblings. The original national sample of the NLSY included a total of about 10,000 youths aged 14 to 21 as of January 1, 1979, with oversamples of African Americans, Hispanics, and economically disadvantaged whites. It is well known that the NLSY tends to overrepresent children born to younger, less educated, and minority mothers. To reduce possible bias in the sample, in all regression models and whenever appropriate we control for characteristics overrepresented in the sample such as maternal age at child's birth, race, mother's educational attainment, and mother's cognitive ability.

This and the PIAT-M test are given to all children aged five or older. (3) The mathematics assessment of the Peabody Individual Achievement Test (PIAT-M) measures a child's achievement in mathematics as commonly taught in American schools. The materials covered range from recognizing numerals to measuring advanced concepts in geometry and trigonometry. Usually, a child examines the problem and then chooses one of four listed answers.

THE RESULTS

Changes in Sibship Size over Time

Figure 1 shows the distribution of change in sibship size between the 1986 measure and the 1992 measure of an individual, and the distribution of change in sibship size between a sibling pair—one sibling is evaluated in 1986 and the other in 1992. Between 40 and 55 percent of the individuals who contributed repeated measures to the analysis experienced no change in sibship size between 1986 and 1992. The other 45 to 60 percent experienced an increase of one, two, or more siblings during the period, with the exact percentage dependent upon the particular cognitive measure and race. The change in sibship size for sibling pairs tends to be greater than that for repeated measures. Between 35 and 45 percent of sibling pairs experienced no change in sibship size between 1986 and 1992. The other 55 and 65 percent of sibling pairs experienced an increase of one, two, or more siblings during the period.

The 1986–1992 period is one of high fertility for the mothers of the children in the NLSY. The mothers were aged 21 to 28 in 1986 and 27 to 34 in 1992. About three-quarters of the fertility in the 1992 synthetic cohort in the United States fall in the age range of 21 to 34 (U.S. Department of Health and Human Services 1996). Since fertility rates in the United States have been fairly stable over the last several decades, similar fertility rates can be used to describe the cohort in our study. The changes in sibship size between 1986 and 1992 are, therefore, important changes, especially considering our theoretical argument that a preferred test of the resource dilution model is a dynamic analysis relating changes in sibship size to changes in
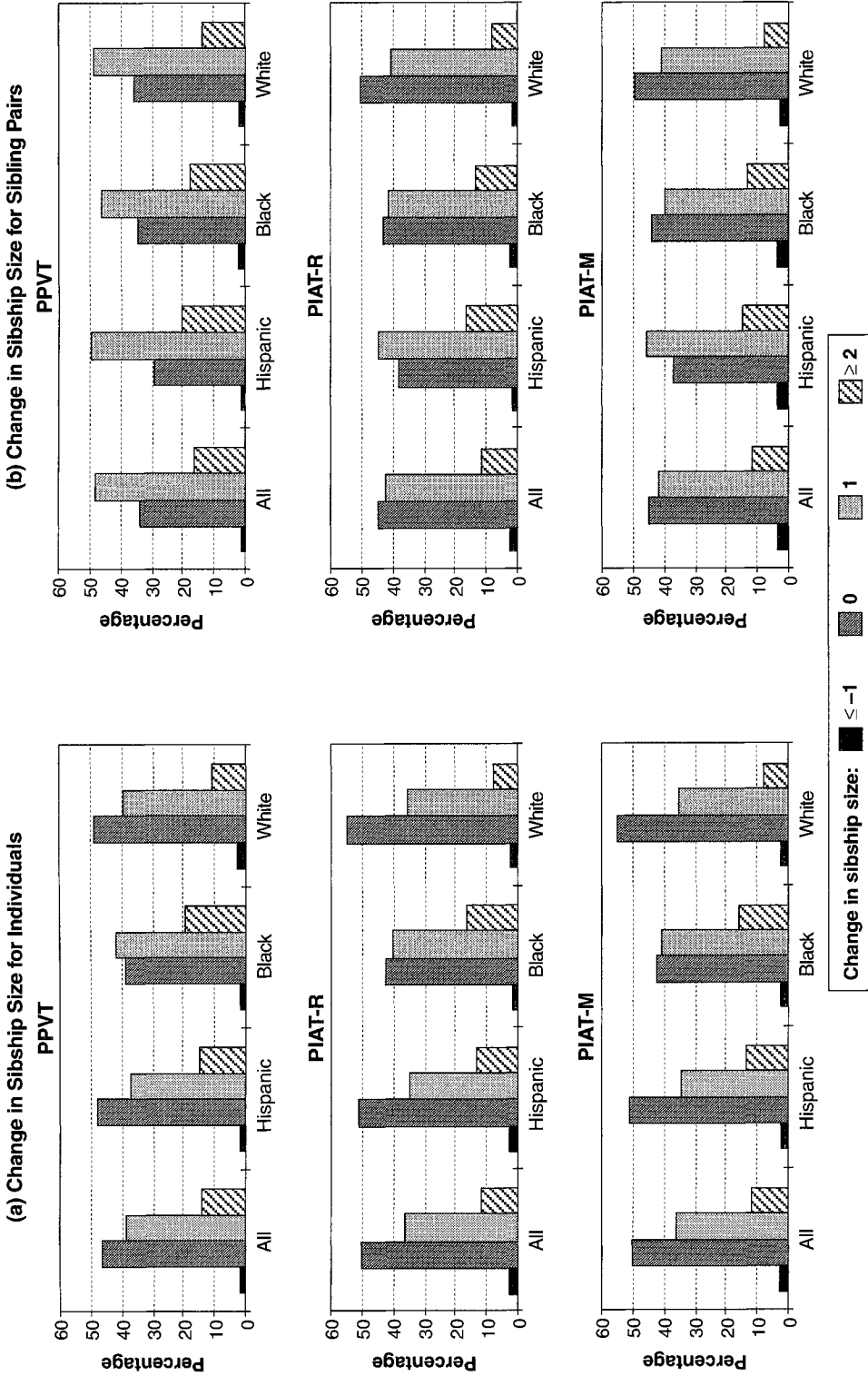
Figure 1. Frequency Distributions of Change in Sibship Size from 1986 to 1992 for Individuals and between Two Siblings in a Sibling Pair: Individual and Sibling-Pair Samples from the NLSY

Table 2. Means and Standard Deviations for Control Variables, and Coefficients from the Conventional Regression Model Testing for the Effect of Sibship Size on Child's PPVT Score: NLSY, 1992

	Individual Sample (All Races)				Sibling Sample (All Races)			
	(1) Sample Characteristics		(2) Conventional Regression Model		(3) Sample Characteristics		(4) Conventional Regression Model	
Variable	Mean	S.D.	Beta	S.E.(b)	Mean	S.D.	Beta	S.E.(b)
Child's PPVT score, 1992	90.42	16.89	—	—	91.25	16.61	—	—
Sibship size, 1989–1992[a]	2.65	1.13	-1.92^{***}	(.34)	2.63	.84	-1.42^{**}	(.55)
Child's Characteristics								
Gender (female = 1)	.50	.50	$-.05$	(.68)	.49	.50	.20	(.87)
Birth order	1.48	.75	-1.49^{**}	(.56)	1.30	.62	-2.08^{**}	(.77)
Child's age, 1992 (in months)	148.32	26.85	$-.04^*$	(.02)	15.40	27.47	$-.04^*$	(.02)
Family Characteristics								
Family income, 1989–1992 (in $1,000s)[a]	23.26	15.86	$.07^*$	(.03)	24.36	15.87	.04	(.04)
Mother's education, 1992	11.65	2.01	.21	(.21)	11.73	1.94	.23	(.27)
Family Structure:								
Never-married, 1989–1992[a]	.19	.39	-1.37	(1.31)	.16	.36	-1.64	(1.73)
Divorced, 1989–1992[a]	.24	.39	1.25	(1.26)	.22	.38	$-.03$	(1.66)
Father in the household, 1989–1992[a]	.46	.45	3.05^{**}	(1.07)	.50	.45	2.35	(1.37)
Mother's age, 1992 (in years)	31.94	2.10	.08	(.20)	31.95	2.10	.20	(.26)
Mother's AFQT score, 1979	565.35	208.70	$.03^{***}$	(.002)	577.56	206.78	$.03^{***}$	(.002)
Environmental Characteristics								
Urban, 1989–1992[a]	.78	.40	-1.95^*	(.86)	.79	.40	-2.28^*	(1.11)
South, 1989–1992[a]	.45	.49	-4.07^{***}	(.72)	.43	.49	-4.36^{***}	(.92)
Number of cases	1,702		1,627		1,048		995	

[a] Based on average characteristics during the time period indicated.

$^*p < .05$ $^{**}p < .01$ $^{***}p < .001$ (two-tailed tests)

intellectual development rather than the conventional static regression of intellectual development on eventual family size.

The Effect of Sibship Size on Intellectual Development

Table 2 (column 1 and 3) shows the means and standard deviations of the variables used in the conventional regression analysis for both the individual and sibling samples. As

an example, Table 2 also presents the full set of coefficients from the conventional regression model testing the effect of sibship size on child's PPVT score for all races, for both the individual sample (column 2) and the sibling sample (column 4). Subsequent tables, which present results from both the conventional and change models for all measures of cognitive development and for each racial/ethnic group, show only the sibship size coefficient.

Results from individual samples. An indi-
vidual sample is composed of all children that
contribute two measures on a specified out-
come. Thus, as indicated in Table 2, the PPVT
sample contains 1,627 individuals who have
a valid standardized score on the PPVT for
both 1986 and 1992. Children who were not
living with their mother in both 1986 and
1992 were excluded. An individual sample is
analyzed twice—once by the conventional re-
gression analysis (results presented in Tables
2 and 3) and once by the analysis of repeated
measures (results presented in Table 3). We
performed the two analyses on the same ba-
sic sample so that the comparison of the re-
sults from the two analyses would not be
complicated by the differences between the
two samples. Differences in sample size be-
tween the two analyses are due to missing
values on the independent variables. For in-
stance, the PPVT individual sample consists
of 1,702 cases, all of which are used in the
repeated measures analysis; 75 cases are ex-
cluded in the conventional analysis because
more independent variables are included.[3]

The conventional regression analysis at-
tempts to replicate the negative effect of sib-
ship size on intellectual development using
cross-sectional data and the method of ordi-
nary least squares. Sibship size is measured
as the average number of children living in
the household over 1989–1992.

The analysis controls for various indi-
vidual, family, and other environmental char-
acteristics as specified in equation 1. At the
individual level, we control for child's *gen-
der*, *birth order*, and *child's age* in months at
the 1992 assessment date. At the family
level, we control for *family income*, *mother's
education*, *family structure*, *maternal age,*
and *mother's AFQT score*. *Family income* is
measured by the average of the total family
income over 1989–1992, which should more
effectively capture the permanent income of
a family than a single-year measure.[4]

---

[3] Other ways of drawing samples for the con-
ventional regression analysis are possible. We
could have drawn a sample that maximizes the
sample size for a conventional analysis, but this
sample would be much larger than and not com-
parable to any available sample for a change-
model analysis.

[4] Supplemental analyses were conducted sub-
stituting a 10-year average of income for the 4-

Mother's education is the highest grade com-
pleted by the mother at the 1992 interview.
For each year, *family structure* is measured
by a three-category dummy variable: *never-
married*, *divorced*, and *married or widowed*,
with married or widowed as the reference
category. An alternative measure of family
structure is a dummy variable indicating
whether father is in the household. Our mea-
sure of family structure averages this yearly
dummy variable over the 1989–1992 period.

Other environmental variables controlled
are *urban/rural residence* and *residence in
the South*. Both variables were constructed by
averaging dummy variables coded 1 for ur-
ban residence and 1 for residence in the South
over the 1989–1992 period. Analyses that dis-
tinguish central city residence from other ur-
ban residence and suburban residence did not
lead to substantive changes in the magnitude
or direction of the sibship-size effect.

Table 3 shows the effects of sibship size on
cognitive development in the individual
samples estimated by the conventional re-
gression model and the analysis of repeated
measures. The analysis was performed sepa-
rately for each racial group and each cogni-
tive measure. The first three columns in Table
3 present the effects of sibship size from the
12 conventional OLS regression models. The
estimated coefficients for the control vari-
ables are generally consistent with previous
literature and follow the pattern shown in
Table 2 (column 2) for the PPVT scores. The
resultes in Table 3 indicate that the estimated
effect of sibship size varies considerably by
cognitive outcome. For PPVT, the effect of
sibship size is negative, comparable in size to
those estimated in previous work, and statis-
tically significant by the usual standards. This
result holds in a sample that combines all ra-
cial groups as well as in samples that contain
only whites, blacks, or Hispanics. The com-
bined sample yields a coefficient of –1.92,
suggesting that each additional sibling in the
household would lead to a decrease of about
2 points on the PPVT test.

For PIAT-R, a significant negative effect of
sibship size is estimated in all regressions ex-
cept the one for the black sample. The sib-

---

year average and substituting a measure of pov-
erty for income. The results regarding the effect
of sibship size remained substantively unchanged.

Table 3. **Coefficients from Conventional and Repeated Measures Models Regressing Selected Measures of Intellectual Development on Sibship Size, by Race: Individual Sample from the NLSY, 1992 and 1986**

Cognitive Measure[a]/ Race	Conventional Regression Model (1992)[b]			Repeated Measures Model (1992 and 1986)		
	Coef.	S.E.	Number of Cases	Coef.	S.E.	Number of Cases
All Races						
PPVT	-1.92^{***}	(.34)	1,627	.38	(.50)	1,702
PIAT-R	-1.47^{***}	(.40)	1,064	.31	(.55)	1,106
PIAT-M	$-.52$	(.33)	1,057	1.37^{*}	(.54)	1,100
White						
PPVT	-1.80^{***}	(.55)	626	$-.21$	(.78)	649
PIAT-R	-1.73^{*}	(.70)	373	.88	(1.08)	382
PIAT-M	$-.87$	(.58)	369	1.49	(1.05)	378
Black						
PPVT	-1.51^{**}	(.53)	663	.40	(.75)	693
PIAT-R	$-.71$	(.63)	475	$-.10$	(.76)	493
PIAT-M	$-.12$	(.51)	472	.74	(.77)	491
Hispanic						
PPVT	-2.08^{*}	(.85)	338	.33	(1.18)	360
PIAT-R	-2.18^{*}	(.89)	216	.48	(1.14)	231
PIAT-M	$-.03$	(.77)	216	2.44^{*}	(1.09)	231

Note: Numbers in parentheses are standard errors.

[a] Cognitive measures are the PPVT (Peabody Picture Vocabulary Test–Revised), PIAT-R (Peabody Individual Achievement Test–Reading), and PIAT-M (Peabody Individual Achievement Test–Math).

[b] Apart from estimating the effect of family size, the conventional regression models control for gender, birth order, child's age at the time of the cognitive assessment, family income, mother's education, family structure, maternal age, mother's cognitive ability, urban/rural residence, and region of the country.

$^{*}p < .05$ $^{**}p < .01$ $^{***}p < .001$ (two-tailed tests)

ship-size effect in the black sample is negative, but not significant at the $p < .05$ level. For PIAT-M, the combined sample and the white sample produce a negative effect of considerable size, but none of the regressions yields a significant effect. The absence of a sibship-size effect for PIAT-M is in agreement with previous work, which has often found that the sibship-size effect was more pronounced on verbal than nonverbal tests (Blake 1989; Mascie-Taylor 1980; Mercy and Steelman 1982; Nisbet and Entwisle 1967). It was reasoned that verbal development more than mathematics ability depends on social context and requires social interactions with other people.

The estimated effects of sibship size from the analysis of repeated measures contrast sharply with those from the conventional analysis (columns 4–6, Table 3). None of the 12 analyses of repeated measures yields a significant negative sibship-size effect. Compared with the conventional analysis, the coefficients are much closer to zero or much more positive. Two significant coefficients occur, but these are *positive* effects, meaning that each additional sibling would *increase* the index child's math score. This finding is the opposite from the usual negative effect of sibship size.[5]

Results from the sibling samples. The conventional regression, full-sibling analy-

---

[5] Appendix A presents the estimates from the difference models of PPVT, PIAT-M, and PIAT-R that include other time-varying covariates.

Table 4. Coefficients from Conventional Model, Sibling Analysis, and Repeated Measures Model Regressing Selected Measures of Intellectual Development on Sibship Size, by Race: Sibling Sample from the NLSY, 1992 and 1986

Cognitive Measure/ Race	Conventional Regression Model (1992)			Sibling Analysis			Repeated Measures Model		
	Coef.	S.E.	Number of Cases	Coef.	S.E.	Number of Cases	Coef.	S.E.	Number of Cases
All Races									
PPVT	−1.42**	(.55)	995	−.32	(1.15)	594	.70	(.64)	1,048
PIAT-R	−1.55**	(.57)	671	−1.64	(1.11)	436	−.33	(.72)	702
PIAT-M	−.93+	(.49)	666	−.61	(1.04)	431	.83	(.69)	694
White									
PPVT	−1.49	(.98)	396	−.98	(1.76)	272	−.76	(.98)	415
PIAT-R	−1.90*	(.94)	250	−3.03	(2.00)	189	−.34	(1.40)	256
PIAT-M	−.51	(.71)	249	1.08	(1.76)	185	−.04	(1.29)	254
Black									
PPVT	−1.37+	(.80)	390	.07	(2.03)	172	1.54	(1.00)	407
PIAT-R	−1.36	(.91)	288	−1.02	(1.90)	141	−1.06	(1.02)	301
PIAT-M	−1.07	(.74)	285	−3.75+	(1.98)	140	.57	(1.03)	296
Hispanic									
PPVT	−.69	(1.26)	209	−.28	(2.33)	150	.15	(1.52)	226
PIAT-R	−1.37	(1.24)	133	−1.67	(1.99)	106	.76	(1.39)	145
PIAT-M	−.59	(1.26)	132	1.13	(1.64)	106	2.10	(1.38)	144

Notes: Numbers in parentheses are standard errors. For the conventional regression model, estimation of the standard errors adjusts for the correlation between members of a sibling pair using a procedure originally developed by Huber (1967). See Table 2, footnote a, for cognitive measures. See Table 2, footnote b, for the variables controlled in the conventional regression model.

+$p < .05$ (one-tailed tests)

*$p < .05$ **$p < .01$ ***$p < .001$ (two-tailed tests)

sis, and the analysis of repeated measures were performed on sibling samples for each racial group and each cognitive measure (Table 4). To construct the sibling sample, we first grouped the data into family or sibling clusters. We then selected all families in which the oldest sibling has a valid score on a given cognitive measure in 1986 and the youngest sibling has a valid score on the same cognitive measure in 1992, thus composing a sample of sibling pairs, each of which included the oldest and youngest children from a family. This sample of sibling pairs was then divided into full siblings and half siblings based on the kinship linking algorithms developed by Rodgers et al. (1994) and Charng and Baydar (1996). Full-sibling pairs and probable full-sibling pairs formed the sibling samples. The sibling pairs in a sibling sample were separated into individual children to create a sample for conventional

and repeated measures analysis that will be compared with the sibling analysis. Some of the individual children are then excluded from the conventional or repeated measures analysis due to missing values on either explanatory variables or outcome measures at one of the time points.

In the first three columns of Table 4, we present the effects of sibship size from the conventional analysis based on the sibling samples. The similarity between these effects and those based on the individual samples is apparent although the sibling-sample effects tend to be weaker. One possible reason for the weaker effects is that the individual samples are much larger than the corresponding sibling samples (1,627 vs. 995 for all-race/PPVT). Regressions for the verbal tests are more likely to yield a statistically significant negative effect of sibship size. Larger samples combining all racial groups also are

more likely to yield a significant effect. All coefficients are negative, but those for the math test tend to be smaller than those for the verbal tests. We should avoid reading too much into the negativity of a single nonsignificant coefficient or a single significant coefficient, but as a whole, the negativity of all or nearly all coefficients may well represent a pattern.

The second three columns of Table 4 show the results of the sibling analysis, using samples of sibling pairs for each outcome. For all racial groups and all outcomes, the estimated effects of sibship size on intellectual development are no longer significant at the $p < .05$ level. The majority of the estimates are less negative than those from the conventional analysis. There are a few exceptions, but the general pattern that emerges when one moves from the conventional to the sibling analysis confirms our expectations as specified in Table 1.

The last three columns of Table 4 present the results of the analysis of repeated measures of individuals using the sibling pair samples. These results continue to follow our expected pattern. As in Table 3, none of the estimates are statistically significant. The majority are more positive than the estimates from the sibling analysis of a comparable sample. Some of the estimates, especially those for PIAT-M, suggest a possibility of a positive effect of sibship size. Moving from left to right across the three types of models in Table 4, we generally find the expected weakening of the effect of sibship size on cognitive development. The overall pattern displayed in Table 4 is consistent with the pattern expected in Table 1.

DISCUSSION AND CONCLUSION

Our objective has been to examine the causal interpretation of the negative effect of family size on children's intellectual development. Our analysis has provided evidence against this widely held interpretation.

We reviewed previous work that questions the causal interpretation of the frequently found negative effect of family size. We argue that an appropriate test of the resource dilution model should make use of longitudinal data and a dynamic modeling approach. We have reexamined the interpretation using

sibling analysis and analysis of repeated measures of the same individuals.

While the conventional analysis has generally replicated the longstanding result of a negative sibship-size effect, the sibling analyses and the analyses of repeated measures have failed to yield such an effect. In other words, after implicitly controlling for the additional family and other environmental effects, genetic effects at the family level, child-specific effects, and the interactions between child and family effects, sibship size no longer has a negative effect on children's intellectual development, suggesting that the effect found from the conventional analysis may be due to one or more of these additional effects rather than to family size. As we compare the conventional analysis with the sibling analysis and then with the analysis of repeated measures, the negative sibship-size effect weakens as more potential confounding effects are controlled. This pattern agrees with the theoretical expectations specified in Table 1. The negative sibship-size effect essentially disappears in the sibling analysis, suggesting that the additional controls for environmental and genetic effects in the sibling analysis are enough to account for the sibship-size effect estimated in the conventional analysis. Last, we should point to the evidence for the possibility of a positive sibship-size effect on children's mathematics skills. In evaluating our results, we have tried to rely on the patterns indicated by the results from all analyses based on all samples rather on the result from a single regression analysis.

Because the sibship-size effect disappears in the sibling analysis, there must be a time-invariant family influence or a collection of time-invariant family influences that are correlated with sibship size, omitted in the conventional analysis, and controlled in the sibling analysis. We have reasoned in our discussion of Table 1 that the source of this influence or set of influences is family environment, family genetic heritage, or a mixture of both. We believe that an argument can be made that the most likely confounding influence is home intellectual environment determined by the orientation of the parents toward knowledge and learning. Parents who believe in the value of knowledge tend to pass this onto their children. Knowledge in

this context does not mean only school or book knowledge. It includes more experiential forms of knowledge encountered in school or everyday life. Children growing up in a favorable home intellectual environment are more likely to develop a desire for knowledge. This includes a desire to read both fiction and nonfiction and to learn about other people's lives and experiences. It also includes a desire to do things, tinkering with electronics or cars, and experimenting with chemistry sets or cake recipes. Parents who are oriented toward learning and knowledge may view larger families as less compatible with the type of home environment they want to create. They may have fewer children on average. This argument can be tested explicitly when good measures on home intellectual environment are available.

Our results seem generally straightforward and consistent, but how about our approach? How should we evaluate our approach? Do change models always "wash out" the effects estimated from conventional analysis? Although change models rarely have been used in sociology, economists have a long tradition of using such models to study the effects of education on income (Ashenfelter and Krueger 1994; Gorseline 1932; Griliches 1979). In most cases, sibling and twin analyses reduce, but do not eliminate, the effects of education on income estimated from conventional analysis. The results are more mixed from sister analysis of the socioeconomic consequences of teenage childbearing (Geronimus and Korenman 1992, 1994; Hoffman, Foster, and Furstenberg 1993a, 1993b): Sister analysis sometimes reduces and sometimes eliminates the estimated consequences of early childbearing.

Our results were obtained under the assumption that all effects specified in Table 1 are time-fixed or do not change over time. Another characteristic of our analysis is that only shared effects are controlled in the change models. However, some family and other environmental effects *do* change over time, and all environmental effects are not shared. These qualifications suggest that our findings hold with less than perfect control for potentially confounding variables. To test the importance of these time-varying influences, we estimated change models that ex-

plicitly measure and control for time-varying variables such as family income, mother's education, and family structure (some results are shown in Appendix A). The main results remain unchanged.

Because of the overrepresentation of young mothers in the NLSY, the proper control for maternal age is an important issue in every study using the NLSY children. Our change models address this issue by differencing out the effect of maternal age. This control for maternal age is as effective as the extent to which the effect of maternal age is constant across siblings or across repeated measures. Ultimately, however, our findings should be replicated in samples more representative of the U.S. population and in populations in other parts of the world.

The generalizability of our results is restricted by the amount of sibsize changes captured in the NLSY during the six years between 1986 and 1992. Few families could increase the number of children by more than a few over the six years. Consequently, the generalization of our results to situations in which families experience large increases in sibship size will have to wait for replications using data that capture substantially more sibsize changes. Promising data for such replications may come from longitudinal studies of intellectual growth in high-fertility developing countries or from longitudinal studies of intellectual growth that span considerably more than six years in low-fertility developed countries.

Although a study spanning more than six years would help, spans much longer than six years may not be necessary. The development of cognitive ability may be vulnerable to environmental influences only under a certain age, beyond which cognitive ability may not be affected by sibship size. For instance, when a child's cognitive development is first assessed at age six, the maximum number of years we follow her or him may not need to exceed 10. In other words, cognitive ability may not be affected by sibship size after age 16. This example illustrates our argument that long-term studies of 15 to 20 years that record all the births in large families may not be necessary for testing the resource dilution theory. For this reason, the moderate span of six years in our study may have captured some of the most important elements in the

relationship between family size and intellectual growth.

While we agree that our conclusions are restricted by the limited amount of sibship-size changes captured in our study, the absence of sibship-size effects seems primarily due to our theoretical and empirical approach rather than to an inadequate amount of variation in fertility. Using the same NLSY data and the same outcome variables (PPVT and PIAT-R), Parcel and Menaghan (1994a) found that the 1986 PPVT is significantly predicted by whether additional children had been born during the child's first three years of life, and that the 1988 PIAT-R is significantly predicted by the additional number of children born between 1986 and 1988 (1994b). The sibship-size changes in these two studies amount to only about one-half to one-third of the fertility changes in our study. The crucial difference seems to be that while Parcel and Menaghan used a semidifference model, we used a difference model. A semidifference model in this case is similar to the conventional analysis because the additional number of children born between 1986 and 1988 is likely to be correlated with family size.

Our work casts doubt on the standard causal interpretation of the effect of family size on cognitive development and on the validity of the suggestion that everything else being equal, limiting the number of children in a family leads to more intelligent children. Yet, the findings from conventional analysis are still valuable in their own right. Family size can still be used to predict children's cognitive development. Sometimes it is unnecessary to establish causality to predict. Even if the real causes are not observed, as long as we observe family size and as long as family size is correlated with the causes, we will be able to make good predictions.

Downey's (1995) work illustrates another important use of traditional analysis. Downey assumes that the family-size effect is causal, and he proceeds to study the factors that mediate the effect of family size. Even if the effect of family size is not causal, the mediating factors *can* be causal. These mediating factors are caused by something other than family size, and these mediating factors may in turn cause shifts in children's intellectual development. So even if we don't know exactly what family size represents, we do know it is something detrimental, and we know that the detrimental factor is mediated by other factors. Policy intervention can choose to focus on the mediating factors without knowing what family size really represents.

However, the situation is dramatically different for individual couples planning to limit family size to achieve more intelligent children. If family size does not *causally* affect children's intellectual development, couples who manipulate family size are not manipulating the real causes of intellectual development. In such a case, limiting family size will not lead to higher quality children. Our systematic examination of sibling pairs and repeated measures of the same individuals via the change models has shown that limiting family size does not lead to children with a higher level of intellectual development.

Guang Guo *is Assistant Professor of Sociology at the University of North Carolina at Chapel Hill. His recent work includes the timing of the poverty effect on children's intellectual development, the mechanisms mediating the effects of poverty on children's intellectual development, the environmental effects on the realization of genetic potential for intellectual development, genetic basis for friendship selection, and the mixed models for behavior genetic analysis.*

Leah K. VanWey *is a Ph.D. student in the Department of Sociology at the University of North Carolina at Chapel Hill. Her interests include quantitative research methods, household decision-making, and migration.*

Appendix A. Coefficients from Repeated Measures Analysis Showing the Effects of Sibship Size and Other Time-Varying Covariates on Change in Child's Scores on the PPVT, PIAT-R, and PIAT-M, by Race: Individual Samples, NLSY, 1992 and 1986

Time-Varying Covariates	All Races	White	Hispanic	Black
Peabody Picture Vocabulary Test (PPVT)				
Difference in: Sibship size	.361	−.153	1.073	.244
Child's age (in months)	.036***	.016	.054**	.048***

(Appendix A continued from previous page)

Time-Varying Covariates		All Races	White	Hispanic	Black
Peobody Picture Vocabulary Test (PPVT) (Continued)					
Difference in:	Family income (in $1,000s)	.050	−.022	.171[*]	.107[*]
	Family income (in $1,000s)	.050	−.022	.171[*]	.107[*]
	Mother's education	.571	.956	.329	.260
	Never-married	−.954	−4.086	6.605	−.899
	Divorced	1.991	.701	5.388[*]	1.593
Number of cases		1,657	641	349	667
R^2		.060	.019	.130	.091
Peabody Individual Achievement Test–Reading (PIAT-R)					
Difference in:	Sibship size	.481	.749	.903	−.057
	Child's age (in months)	−.062[***]	−.040[**]	−.043[*]	−.088[***]
	Family income (in $1,000s)	.047	.003	.125	.019
	Mother's education	1.559[**]	1.059	2.647[**]	.376
	Never-married	4.358[**]	−1.158	1.118	3.820
	Divorced	−.305	−2.675	.880	.857
Number of cases		1,076	378	222	476
R^2		.113	.047	.068	.239
Peabody Individual Achievement Test–Math (PIAT-M)					
Difference in:	Sibship size	1.405[*]	1.903	3.143[**]	.474
	Child's age (in months)	−.034[***]	−.038[**]	−.046[**]	−.027[*]
	Family income (in $1,000s)	.0089	−.030	.077	.038
	Mother's education	.937	1.694[*]	1.995[*]	−1.912
	Never-married	1.606	4.916	5.067	.707
	Divorced	−.265	−.571	−.357	.784
Number of cases		1,070	374	222	474
R^2		.030	.044	.073	.034

Note: Difference scores are constructed by subtracting the 1986 value of a variable from the 1992 value of that variable.

[*]$p < .05$ [**]$p < .01$ [***]$p < .001$ (two-tailed tests)

REFERENCES

Anastasi, Anne. 1956. "Intelligence and Family Size." *Psychological Bulletin* 53:187–209.

Ashenfelter, Orley and Alan Krueger. 1994. "Estimating the Returns to Schooling Using a New Sample of Twins." *American Economic Review* 84:1157–73.

Berbaum, Michael L. and Richard D. Moreland. 1980. "Intellectual Development within the Family: A New Application of the Confluence Model." *Developmental Psychology* 16:506–15.

Blake, Judith. 1981. "Family Size and the Quality of Children." *Demography* 18:421–42.

———. 1985. "Number of Siblings and Educational Mobility." *American Sociological Review* 50:84–94.

———. 1989. *Family Size and Achievement.* Vol. 3, *Studies in Demography.* Berkeley, CA: University of California Press.

Blau, Peter M. and Otis Dudley Duncan with Andrea Tyree. 1967. *The American Occupational Structure.* New York: Wiley.

Charng, Hong-Wen and Nazli Baydar. 1996. "1992 Children of the NLSY Kinship Links." Health and Population Research Center, Battelle–HARC, Seattle, WA. Unpublished document.

Cicirelli, Victor G. 1978. "The Relationship of Sibling Structure to Intellectual Abilities and Achievement." *Review of Educational Research* 48:365–79.

Douglas, James William Bruce. 1964. *The Home and the School; A Study of Ability and Attainment in the Primary School.* London, England: Macgibbon and Kee.

Downey, Douglas B. 1995. "When Bigger Is Not

Better: Family Size, Parental Resources, and Children's Educational Performance." *American Sociological Review* 60:746–61.

Dunn, Lloyd M. and Leota M. Dunn. 1981. *PPVT-R Manual*. Circle Pines, MN: American Guidance Service.

Ernst, Cécile and Jules Angst. 1983. *Birth Order: Its Influence and Personality*. Berlin, Germany: Springer-Verlag.

Eysenck, Hans Jurgen and D. Cookson. 1970. "Personality in Primary School Children: III. Family Background." *British Journal of Educational Psychology* 40:117–31.

Featherman, David L. and Robert M. Hauser. 1978. *Opportunity and Change*. New York: Academic Press.

Firebaugh, Glenn and Frank D. Beck. 1994. "Does Economic Growth Benefit the Masses? Growth, Dependence, and Welfare in the Third World." *American Sociological Review* 59: 631–53.

Galbraith, Richard C. 1982. "Sibling Spacing and Intellectual Development: A Closer Look at the Confluence Model." *Developmental Psychology* 18:151–74.

Galton, Sir Francis. 1874. *English Men of Science: Nature and Nurture*. London, England: MacMillan.

Geronimus, Arline T. and Sanders Korenman. 1992. "The Socioeconomic Consequences of Teen Childbearing Reconsidered." *Quarterly Journal of Economics* 107:1187–1214.

———. 1994. "Does Young Maternal Age Adversely Affect Child Development? Evidence from Cousin Comparisons in the United States." *Population and Development Review* 20:585–609.

Gorseline, Donald W. 1932. *The Effect of Schooling Upon Income*. Bloomington, IN: Indiana University Press.

Griliches, Zvi. 1979. "Sibling Models and Data in Economics: Beginning of a Survey." *Journal of Political Economy* 87:S37–64.

Grotevant, Harold D., Sandra Scarr, and Richard A. Weinberg. 1977. "Intellectual Development in Family Constellations with Adopted and Natural Children: A Test of the Zajonc and Markus Model." *Child Development* 48:1699–1703.

Hoffman Saul D., E. Michael Foster, and Frank F. Furstenberg Jr. 1993a. "Reevaluating the Costs of Teenage Childbearing." *Demography* 30:1–13.

———. 1993b. "Reevaluating the Costs of Teenage Childbearing: Response to Geronimus and Korenman." *Demography* 30:291–96.

Huber, P. J. 1967. "The Behavior of Maximum Likelihood Estimates under Non-Standard Conditions." Pp. 221–33 in *Proceedings of the Fifth Berkeley Symposium in Mathematical Statistics and Probability*. Berkeley, CA: University of California Press.

Institut National d'Etudes Demographiques. 1973. *Enquête National sur le Niveau Intellectuel des Enfants d'Age Scolaire* (National Report on the Intellectual Development of School-Age Children). Cahier no. 64. Paris, France: Presses Universitaires de France.

Kennett, Keith F. and Arthur J. Cropley. 1970. "Intelligence, Family Size and Socio-Economic Status." *Journal of Biosocial Science*. 2:227–36.

Liker, Jeffrey K., Sue Augustyniak, and Greg J. Duncan. 1985. "Panel Data and Models of Change: A Comparison of First Difference and Conventional Two-Wave Models." *Social Science Research* 14:80–101.

Mascie-Taylor, C. G. Nicholas. 1980. "Family Size, Birth Order and IQ Components: A Survey of a Cambridge Suburb." *Journal of Biosocial Science* 12:309–12.

Marjoribanks, Kevin. 1974. "Academic Achievement: Family Size and Social Class Correlates." In *Environments for Learning*, edited by K. Marjoribanks. Windsor, England: NFER Publishing.

McCall, Robert B. 1984. "Developmental Changes in Mental Performance: The Effect of the Birth of a Sibling." *Child Development* 55:1317–21.

Mercy, James A. and Lala Carr Steelman. 1982. "Familial Influences on the Intellectual Attainment of Children." *American Sociological Review* 47:532–42.

Nisbet, J. D. and N. J. Entwistle. 1967. "Intelligence and Family Size: 1949–1965." *British Journal of Educational Psychology* 37:188–93.

Olneck, Michael R. and David B. Bills. 1979. "Family Configuration and Achievement: Effects of Birth Order and Family Size in a Sample of Brothers." *Social Psychology Quarterly* 42:135–48.

Parcel, Toby L. and Elizabeth G. Menaghan. 1994a. *Parents' Jobs and Children's Lives*. New York: Aldine De Gruyter.

———. 1994b. "Early Parental Work, Family Social Capital, and Early Childhood Outcomes." *American Journal of Sociology* 99:972–1009.

Plomin, Robert, John C. DeFries, and Gerald E. McClearn. 1980. *Behavioral Genetics: A Primer*. San Francisco, CA: Freeman.

Powell, Brian and Lala Carr Steelman 1993. "The Educational Benefits of Being Spaced Out: Sibship Density and Educational Progress." *American Sociological Review* 58:367–81.

Rankin, Richard J., A. J. Gaite, and Thomas Heiry. 1979. "Cultural Modification of Effects of Family Size on Intelligence." *Psychological Reports* 45:391–97.

Retherford, Robert D. and William H. Sewell.

1991. "Birth Order and Intelligence: Further Tests of the Confluence Model." *American Sociological Review* 56:141–58.

Rodgers, Joseph L. 1984. "Confluence Effects: Not Here, Not Now!" *Developmental Psychology* 20:321–31.

Rodgers, Joseph L., Dand C. Rowe, and Kim May 1994. "DF Analysis of NLSY IQ/Achievement Data: Nonshared Environmental Influences." *Intelligence* 19:157–77.

Scarr, Sandra and Richard A. Weinberg. 1979. "Intellectual Similarities in Adoptive and Biologically Related Families of Adolescents." Department of Psychology, University of Virginia, Charlottesville, VA. Unpublished manuscript.

Scottish Council for Research in Education, Mental Survey Committee. 1949. *The Trend of Scottish Intelligence. A Comparison of the 1947 and 1932 Surveys of the Intelligence of Eleven-Year-Old Pupils.* London, England: University of London Press.

Steelman, Lala Carr. 1985. "A Tale of Two Variables: A Review of the Intellectual Consequences of Sibship Size and Birth Order." *Review of Educational Research* 55:353–86.

Steelman, Lala Carr and Brian Powell 1989. "Acquiring Capital for College: The Constraints of Family Configuration." *American Sociological Review* 54:844–55.

———. 1991. "Sponsoring the Next Generation: Parental Willingness to Pay for Higher Education." *American Sociological Review* 56:1505–29.

U.S. Department of Health and Human Services. 1996. *Vital Statistics of the United States.* Vol. 1, *Natality.* Hyattsville, MD: Centers for Disease Control and Prevention and National Center for Health Statistics.

Zajonc, Robert B. and Gregory B. Markus. 1975. "Birth Order and Intellectual Development." *Psychological Review* 82:74–88.

Zajonc, Robert B. 1975. "Dumber by the Dozen." *Psychology Today* 8(8):37–43.