Birth order effects are weaker and less consistent for larger families (perhaps because of smaller sample sizes).

Rodgers et al. (2000) analyzed intact families because the within-family analysis automatically controls for between-family differences, such as mother's IQ, education, income, and other familywide characteristics that might influence a child's IQ. However, there are also within-family processes and conditions that can influence a child's IQ, most notably parenting differences from one child to another. Blake's (1981) resource dilution theory and Zajonc's (1976) confluence model both assert that children in larger families receive lower quality parenting (less attention, instruction, affection, etc.), thereby causing negative family size and birth order effects. Rodgers et al. did not test this theory directly, even though the NLSY study collects parenting data that allow a direct test of the dilution theory.

For example, the NLSY study administers the Home Observation of Measurement of the Environment inventory of parenting behaviors, which consists of cognitive stimulation and emotional support scales. Within-family analysis shows clear birth order effects for cognitive stimulation and emotional support that parallel the results for the PPVT. That is, children in larger families and later-born children receive less cognitive stimulation and emotional support than children in smaller families and earlier-born children. These results are clearly consistent with the resource dilution theory and, in fact, offer a direct explanation for the birth order and family size effects shown for the PPVT verbal ability scores.

Third and perhaps most puzzling is the absence of any multivariate analysis. Rodgers et al.'s (2000) conclusion that a mother's IQ rather than family size determines a child's IQ is based on a single bivariate relationship between the number of children and the average mother's IQ (see Rodgers et al., 2000, Figure 5), which they said shows a correlation of −.99. However, this is an aggregate correlation, not an individual relationship, and a causal inference is not warranted.

The proper way to resolve the relative contributions of these and other factors to children's ability is to conduct multiple regression analyses. Indeed, recent comprehensive analyses of the NLSY data used regression analysis to study the influence of mother's age, family poverty, and race on children's ability test scores (Moore, Morrison, & Greene, 1997; Phillips, Brooks-Gunn, Duncan, Klebanov, & Crane, 1998; Smith, Brooks-Gunn, & Klebanov, 1997). Moore et al. (1997) and Phillips et al. (1998) included birth order in some of their regression models, where it was highly significant. None of these studies were cited in Rodgers et al.'s (2000) article.

A regression analysis can establish the relative importance of each of these factors, including birth order, family size, and mother's IQ. Although regression does not establish causation, it is the best way to estimate the relative contribution of each family characteristic, controlling for all other characteristics. By controlling for most of the known causal influences on a child's ability, researchers can also take advantage of the full number of tested children.

In my own regression analyses, birth order has significant effects on math, reading, and verbal ability after controlling for the list of family and parenting characteristics above; family size has significant effects on reading and verbal ability. The effect of mother's IQ has significant effects on all three test scores, but the effect is reduced by about one half after controlling for other family characteristics. The parenting variables of cognitive stimulation and emotional support have the second strongest effects on test scores after mother's IQ. In other words, a number of family and parenting characteristics contribute significantly to a child's academic ability, and the importance of mother's IQ is diminished when these other influences are taken into account.

In conclusion, Rodgers et al. (2000) applied very simple analytic techniques to a very complex question, leading to unwarranted conclusions about family size and intelligence. Loss of cases, omission of an important ability test, and failure to apply multivariate techniques are the biggest problems in their article. Multivariate analyses, described here and published elsewhere, reveal that both birth order and family size have modest impacts on a child's intelligence after controlling for a host of other family variables, including mother's IQ, as do a number of other family characteristics.

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Birth Order Debate Resolved?

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Rodgers, Cleveland, van den Oord, and Rowe (June 2000) claimed that the "apparent relation between birth order and intelligence has been a methodological illusion" (p. 599). The illusion, according to Rodgers et al., has been produced by analyses based on aggregate data that "are so filled with potential selection (and other) biases as to be virtually useless in addressing birth order effects on intelligence" (p. 602). To support their claim, Rodgers et al. offered a number of data sets, most with null birth order effects.

Comparisons among siblings clearly could be superior to cross-sectional data (Zajonc, 2001, this issue). However, as Rodgers et al. (2000) properly noted (pp. 601, 605, 610), researchers must, among others things, observe time variations in the age of testing when making such comparisons. If siblings are administered an intellectual performance test at the same time, their ages will necessarily be different, and the confluence model (see, e.g., Zajonc, 1983, pp. 475–480; Zajonc & Bargh, 1980, p. 360; Zajonc, Markus, & Markus, 1979, pp. 1328–1338; Zajonc & Mullally, 1997, pp. 690–692) clearly predicts birth order and family size effects that are age specific. In particular, the two terms of the difference equation of the confluence model—the intellectual environment (α_t) and the teaching function

(λ_t)—contribute differentially to the growth of intellectual maturity, the first negatively and the second positively.

As the number of siblings increases, the intellectual environment declines in its relative quality. The teaching function, however, whereby the older children serve as tutors to the younger ones, mitigates the negative effects of the expanding family. Two or three years after firstborns gain a sibling, they can assume tutorial functions—functions that benefit the tutor as much as the tutee. The accumulation of these effects, however, has a different trajectory. The benefits of teaching grow less rapidly than the disadvantages of increasing sibships. The confluence model, therefore, predicts a negative influence or no influence of birth order (lower scores for high birth ranks) for children less than age 11 ±2 years and predicts a positive influence of birth order (higher scores for high ranks) for older children. These predictions have been confirmed by a variety of data sets (Zajonc, 1983; Zajonc & Bargh, 1980; Zajonc et al., 1979; Zajonc & Mullally, 1997) for birth order effects and family size effects (see, e.g., Zajonc et al., 1979, Figures 1, 3, and 4). Yet, after noting that birth order effects are age specific, Rodgers et al., in support of their claim, offered data for populations right at the crossover age where the two terms of the confluence process cancel each other out. At the minimum, they should have looked at older (and also younger) populations. I have analyzed 50 studies (Zajonc, 1983) for the crossover effect, and there was not one study population over 13 years of age that had a negative birth order effect.

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Birth Order and Intelligence: Together Again for the Last Time?

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Does birth order reliably contribute to variance in intelligence? Researchers using cross-sectional data versus within-family data have arrived at very different conclusions in attempting to answer this question. Rodgers, Cleveland, van den Oord, and Rowe (June 2000) proposed a resolution. In the cross-section, birth order—a within-family indicator—can indirectly measure hundreds of between-family differences in variables like socioeconomic status, parental education, parental IQ, nutrition, quality of the home environment, and quality of schooling. If one or many between-family factors actually cause the relationship between birth order and intelligence in the cross-section, then we would expect the relationship to go away when within-family data are evaluated. And so it does in a number of different data sources reviewed in Rodgers et al.'s article. Zajonc (2001, this issue), Armor (2001, this issue), and Michalski and Shackelford (2001, this issue) have responded to our proposed resolution. The comments by Zajonc and Armor provided fundamental criticisms of our study, whereas Michalski and Shackelford's comment extended the methodological point in our study to assess the relationship between birth order and personality. None do any ultimate damage to the methodological resolution proposed in Rodgers et al.'s article.

Zajonc (2001) criticized Rodgers et al.'s (2000) empirical analyses for failing to properly account for age. He suggested that we used "data for populations right at the crossover age where the two terms of the confluence process cancel each other out" (Zajonc, 2001, p. 523). Actually, we anticipated that concern. Because Zajonc had previously identified the crossover point as occurring around age 11 (Zajonc & Mullally, 1997), we broke one of our samples into two parts, on the basis of whether all children in the family were age 11 or younger (mean age 8.3 years) or whether some were age 12 or older (mean age 11.2 years). We also developed an additional sample in which every single sibling was age 12 or older (with an average age of 14.2 years). In each case, birth order and intelligence had a random relationship. It is difficult to argue over the modeling details when there is no relationship between birth order and intelligence to model in the first place. In addition, these random patterns were similar to those found in a number of other within-family studies that used data from both children and adults. Finally, we note that Zajonc can hardly interpret his reported analysis of 50 studies (Zajonc, 1983, 2001) as suggestive of within-family effects, given that virtually all of those were based on cross-sectional data.

Armor (2001) analyzed the Peabody Picture Vocabulary Test (PPVT) data from the National Longitudinal Survey of Youth (NLSY) and found a different within-family result than Rodgers et al. (2000) did using NLSY Peabody Individual Achievement Test (PIAT) scores. There were many aspects of our study that Armor did not like, including our dependent variable, our selected testing times, our sample sizes, our analytic methods, and (we suspect this one drives all of the others) our conclusions. Why did we choose to use the PIAT composite, constructed from PIAT Math, Reading Recognition, and Reading Comprehension subscales? It seemed to provide the best measure of general intelligence in the NLSY. Why did we average only two scores from two testing ages? We wanted to be able to use samples with different age structures to help account for the types of concerns that Zajonc (2001) raised. Why were our sample sizes smaller than the ones that Armor reported? They were smaller because he averaged scores from five PPVT administrations from 1986 to 1994. If we believed in the confluence model, we would hasten to point out to Armor that averaging over eight years of IQ scores can wash out potential effects of interest. From our side, we note that most of Armor's criticisms have more to do with external validity than internal validity; the latter was the primary goal of our study.

We should give some attention to Armor's (2001) empirical analysis. He found an average decline of about 2.5 PPVT points between children of consecutive birth orders. We replicated his PPVT findings;