success either by adjusting their behaviors to the local environment or by manipulating their environment to facilitate their strengths. This definition of intelligence parallels the logic that Sulloway (1996) proposed regarding later-born disadvantage and the personality characteristics that later-borns adopt to increase parental investment. It is possible that, given the methodological issue at hand, it may not be intelligence per se that covaries with birth order but the ways in which intelligence is used. Intelligence may be used by siblings to develop personalities that best utilize their niche. Perhaps this is why within-family designs yield differences in personality but not intelligence as a function of birth order. Within-family designs may identify only the absence of a relationship between birth order and intelligence. Further research could profitably address (a) the ways in which intelligence is used in personality and (b) confounds not previously included in either between-family or within-family studies of birth order.

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DOI: 10.1037//0003-066X.56.6-7.521

On Family Size and Intelligence

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Because of methodological flaws and incomplete data, Rodgers, Cleveland, van den Oord, and Rowe's (June 2000) article is not the last word on family size and IQ. It not only failed to settle the effect of birth order on IQ, it overstated the importance of mothers' IQs at the expense of family size and other family influences. More specifically, their analysis omitted nearly half of the tested children in the sample they studied; they neglected a cognitive ability test (the Peabody Picture Vocabulary Test [PPVT]) that showed significant birth order effects on a much larger sample; and most puzzling, they failed to conduct multivariate analyses, which led to the unwarranted conclusion that "large families do not make low-IQ children in modern U.S. society" (p. 599) and implied that mothers' IQs are the main cause of both large families and low-IQ children. These conclusions led to a misleading headline in *USA Today*: "Mom's IQ, Not Family Size, Key to Kids' Smarts" (2000).

Rodgers et al.'s (2000) article stressed that most existing studies use cross-sectional data, which hamper rigorous inferences about the family effects of birth order on IQ. Their solution was to use the National Longitudinal Survey of Youth (NLSY) data, which has a number of advantages. However, a longitudinal study does not guarantee rigorous causal inferences from a simple bivariate table. Unlike a randomized experiment, longitudinal studies are still subject to selection effects and to the causal ambiguity of bivariate relationships, which can mask the effects of uncontrolled variables. The only way to untangle these complex relationships is to do a multivariate analysis, and Rodgers et al. did not do this.

First, Rodgers et al.'s (2000) primary NLSY analysis was based on 1,311 families and 2,566 children who had complete achievement test data for all siblings as of 1990 or 1992 (see Rodgers et al., 2000, Table 1 and Figure 4). That is, the analysis was based on those families that had test scores for all of their children in those two years (called *intact families*). Excluded were families with children who were too young or too old for the achievement tests in those two years (ages 5 to 15), children who were tested in 1986, 1988, or 1994 but not in 1990 or 1992, and children who were in the proper age range but who were not tested for other reasons. In any assessment year, the math test was not administered to about 10% of the children, and an even higher percentage were not given the reading test (especially among those ages 5 to 7 years). Rodgers et al. replicated their results on a second sample of families using 1994 and 1996 data, but it is likely that many of these families were also included in the first sample, so it is not actually an independent replication sample. Breaking up the sample of children in this way eliminated families that might have had complete data if all available test scores between 1986 and 1996 had been utilized.

These losses of data could have biased the intact family subsample by excluding (a) mothers who had children very early and whose older children were beyond the testing age in 1990 or 1992, (b) mothers who delayed having children and whose younger children were below the testing age, and (c) families who had one or more children who did not take the achievement tests. The rate of omitted families became higher as families became larger because there was a higher probability that at least one child did not have test scores. Thus, Rodgers et al. (2000) analyzed only 233 three-child families, 56 four-child families, and 14 five-child families, when in fact there were over 800 three-child, 280 four-child, and 70 five-child families with achievement scores for some of their children. Altogether, as of the 1994 assessment, the NLSY had over 3,200 families and 6,100 children with math scores in at least one assessment and had about 3,000 families and 5,200 children with at least one reading score.

Second, it is unclear why Rodgers et al. (2000) did not examine the PPVT in their analysis. This test is especially important in preserving NLSY cases because it was administered to all children ages 3 years and over, thereby generating the largest number of intact families for a within-family analysis. The PPVT is probably more fittingly described as an IQ test than either the math or the reading achievement tests.

With the PPVT, the relationship between verbal ability, number of children, and birth order can be examined for 2,447 intact families with test scores for all of their children between 1986 and 1994 (similar to the analysis shown in Table 1 in Rodgers et al.'s [2000] article). This simple bivariate analysis yields a strong birth order effect of 3 points for firstborn versus second-born children and about 2 points for second-born versus third-born children.

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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DOI: 10.1037//0003-066X.56.6-7.522

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