(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;

however, we do not agree that this decline represents a strong effect. (It corresponds to a correlation of about $r = -.16$; Cohen [1988] would refer to this as a small effect size.) We note also that the NLSY included an additional intelligence measure, the Digit Span, that Armor neglected to criticize us for not using. We analyzed the data from this test, and they did not show a within-family relationship with birth order. Therefore, the results are anomalous. The birth order effect identified by Armor seems to derive from whatever part of the PPVT does not overlap with either the PIAT or the Digit Span.

We also noted a certain age pattern that challenges the internal validity of Armor's (2001) PPVT findings. Our replication of Armor's analysis showed that PPVT scores increased systematically with age (a feature not characteristics of the PIAT or the Digit Span scores). Older children—who had lower birth orders on average—usually took the test several times and had more of a chance for this advantage to accrue. Younger children—who had higher average birth orders—took the test fewer times, in some cases only once. Whether this effect is due to a problem with the norming sample, to practice effects (i.e., participants learning how to take the test, a particular problem for vocabulary tests), or to some other process is open to further investigation. In any case, the birth order effect that Armor found may simply measure this inflationary effect of age or multiple test taking. Indeed, when we partialed age out of the relationship between birth order and the PPVT, the effect size dropped substantially.

We have one final comment on Armor's (2001) modeling criticism: Contrary to his report, we did run a number of regression analyses and reported several. However, in many cases, a good graph shows more than a regression analysis. Armor's contention that the "proper way to resolve the relative contributions of these and other factors to children's ability is to conduct multiple regression analyses" (p. 522) has its own problems. His assertion assumes perfect measurement of the independent variables, assumes away problems with multicolinearity, and ignores the bias and precision problems that occur when the model is (necessarily) misspecified. Regression is a panacea? We have never believed this to be true. Rather, it is simply a useful tool with well-known strengths and weaknesses.

We have only a few comments in response to Michalski and Shackelford's (2001) comment. Their effort to explain Rodgers et al.'s (2000) null results as being due to "an increased ability of parents to invest in later-born offspring" (Michalski & Shackelford, 2001, p. 520) strikes us as the type of awkward post hoc explanation exhibited by those who are certain of birth order effects and will not let data patterns (or the lack thereof) stand in their way. Their comment that a "within-family model does not account for within-family change over time" (p. 520) leaves us puzzled; such a model certainly could be defined if the data were rich enough in longitudinal structure to justify its development. They brought our focus on intelligence into their own interest in personality by noting that "intelligence may be used by siblings to develop personalities that best utilize their niche" (p. 521). This intriguing suggestion could motivate future research on birth order and personality.

Little in these comments challenges Rodgers et al.'s (2000) basic position. Cross-sectional data are clearly suspect as evidence for within-family trends in intelligence. Models using within-family processes to explain the apparent patterns in cross-sectional data were built on the wrong foundation. So far, when we have looked inside families and directly compared the intelligence scores of siblings, there has seldom been anything to model.

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