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Author(s): Sandra E. Black, Paul J. Devereux and Kjell G. Salvanes

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THE MORE THE MERRIER? THE EFFECT OF FAMILY SIZE AND BIRTH ORDER ON CHILDREN'S EDUCATION*

SANDRA E. BLACK
PAUL J. DEVEREUX
KJELL G. SALVANES

There is an extensive theoretical literature that postulates a trade-off between child quantity and quality within a family. However, there is little causal evidence that speaks to this theory. Using a rich data set on the entire population of Norway over an extended period of time, we examine the effects of family size and birth order on the educational attainment of children. We find a negative correlation between family size and children's education, but when we include indicators for birth order or use twin births as an instrument, family size effects become negligible. In addition, higher birth order has a significant and large negative effect on children's education. We also study adult earnings, employment, and teenage childbearing and find strong evidence for birth order effects with these outcomes, particularly among women. These findings suggest the need to revisit economic models of fertility and child "production," focusing not only on differences across families but differences within families as well.

I. INTRODUCTION

Economists have long been interested in understanding the factors that determine child outcomes. However, despite years of research, evidence on the components of the "production function" for children is still quite limited. Family environment is widely believed to be a primary component, but it is difficult to parcel this out into specific characteristics.

Among the perceived inputs in the production of child quality is family size. Greater family size may negatively affect child outcomes through resource dilution or because the average maturity level in the household is lower. One could also imagine a positive relationship between family size and child quality if children stabilize marriages or decrease the probability that both parents work outside the home.

One popular economic model is the quantity-quality model

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introduced by Becker [1960] and expanded in Becker and Lewis [1973] and Becker and Tomes [1976]. This theory was introduced to explain the observed negative correlation between family income and family size; it is often cited and is used as the basis for many macro growth models.[1] A key element of the quantity-quality model is an interaction between quantity and quality in the budget constraint that leads to rising marginal costs of quality with respect to family size; this generates a trade-off between quality and quantity.[2] *But is this trade-off real?* Casual evidence suggests that children from larger families have lower average education levels. However, is it true that having a larger family has a causal effect on the "quality" of the children? Or is it the case that families who choose to have more children are (inherently) different, and the children would have lower education regardless of family size?

This paper first attempts to isolate the causal effect of family size on children's outcomes by using data on the entire population of Norway and looking at the effect of an exogenous increase in the size of a family on children's educational attainment. Our data set includes several labor market outcomes in addition to education, covers an extended period of time, and allows us to match adult children to their parents and siblings; as a result, we are able to overcome many limitations of earlier research resulting from small sample sizes or limited information on children's outcomes after the children have left home. In addition, we have plausibly exogenous variation in family size (induced by the birth of twins) to identify the causal effect.

Like most previous studies, we find a negative correlation between family size and children's educational attainment. However, when we include indicators for birth order, the effects of family size are reduced to almost zero. These results are robust to a number of specifications, including the use of twins as an instrumental variable for family size. The evidence suggests that family size itself has little impact on the quality of each child but more likely impacts only the marginal children through the effect of birth order. The implications of these findings are quite different from the causal effect of family size on child quality implied by

1. See Becker and Barro [1988] and Doepke [2003].

2. Rosenzweig and Wolpin [1980] explicitly derive the assumptions under which an exogenous increase in family size should have a negative effect on child quality. They, like Becker and Lewis, treat quality as unvarying within a family.

the simple quality-quantity model and may suggest a reconsideration of the determinants of child outcomes.

Given that birth order effects appear to drive the observed negative relationship between family size and child education, we next turn our attention to birth order. There are a number of theories that predict birth order effects; among these are optimal stopping models, physiological differences, and dilution of parental resources when young (both financial and time). Previously, birth order effects have proved very difficult to credibly estimate due to rigorous data requirements [Blake 1989]. Our unique data set allows us to overcome these data problems; unlike the previous literature, we are able to look both across families and within families using family fixed effects models to deal with unobserved family-level heterogeneity. We find that birth order effects are strong, regardless of our estimation strategy. Moreover, the effects appear to be of similar magnitude across families of different sizes.

We augment the education results by using earnings, full-time employment status, and whether the individual had a birth as a teenager (among women) as additional outcome variables. Consistent with our earlier findings, we find little support for significant family size effects and strong evidence for birth order effects with these outcomes, particularly for women. Later-born women have lower earnings (whether employed full-time or not), are less likely to work full-time, and are more likely to have their first birth as a teenager. In contrast, while later-born men have lower full-time earnings, they are not less likely to work full-time.

The paper unfolds as follows. Section II describes our data. Section III describes the empirical literature on the effects of family size and then presents our methodology and results. Section IV describes our birth order results. Section V presents family size and birth order estimates for various subgroups of the sample. Section VI presents results for other outcomes such as earnings, employment, and teenage pregnancy. Finally, Section VII concludes.

II. Data

Our data are from matched administrative files that cover the entire population of Norway who were aged 16–74 at some point during the 1986 to 2000 interval.[3] To be included in our sample, parents must appear in this data set in at least one year, so we do not observe any parents older than 74 in 1986. The data also contain identifiers that allow us to match these individuals to their children, who also must appear in the administrative data set at least once in the 1986–2000 period.[4] We restrict our sample to children who are at least 25 years of age in 2000 to ensure that almost all have completed their education.[5] Given that we observe year of birth, we are able to construct indicators for the birth order of each child. Data on twin status are also available from Statistics Norway. We use twins to construct our instrumental variables but drop twins from our estimating samples.[6] A twin birth occurs in approximately 1.5 percent of families.

Our family size measure is completed family size. We have two sources of this information: we have data on both the total number of children born to each mother (from the Families and Demographics file from Statistics Norway) as well as a count of the number of children who are sixteen or over in the 1986–2000 interval. These numbers agree in 84 percent of cases. The primary reason for disagreement is families with children who are too young to be in the 1986–2000 data (14 percent of cases). We restrict our sample to families in which the mother and all children are alive and observed in our data at some point between 1986 and 2000. By doing this, we exclude families with children younger than sixteen in 2000. This makes it fairly certain that we have completed family size, as it is unlikely that a child of 25 who has no siblings aged less than 16 will subsequently have another sibling. Also, it allows us to accurately calculate birth order and

3. The documentation for these data is available in Møen, Salvanes, and Sørensen [2004].

4. When matching children to parents, we match using the mother's identifier, as almost all children over this period would have grown up with their mother. However, for a small proportion of children (2 percent) the father differs across children in the family, and for a larger proportion (16 percent) the father of some of the children is unknown to Statistics Norway (so the father may be different across children). We have verified that all results are robust to excluding families in which the father identifier differs across children or is missing for some children.

5. Only 4.6 percent of our sample is still in school in 2000. As a check on this censoring problem, we have reestimated all specifications using a sample of individuals aged at least 30 in 2000 and found very similar results.

6. Results were not sensitive to this exclusion. We dropped twins because of the ambiguities involved in defining birth order for twins.

TABLE I
SUMMARY STATISTICS—FULL SAMPLE

	Mean	Standard deviation
Age in 2000	38	8.5
Female	.48	.50
Education	12.2	2.4
Mother's education	9.5	2.4
Father's education	10.4	3.0
Mother's age in 2000	65	10.5
Father's age in 2000	67	10.0
Number of children	2.96	1.3
Twins in family	.015	.12
Men:		
Log earnings in 2000	12.55	.77
Log FT earnings in 2000	12.72	.45
Proportion FT in 2000	.71	.45
Women:		
Log earnings in 2000	12.05	.88
Log FT earnings in 2000	12.42	.42
Proportion FT in 2000	.46	.50
Prob (teen birth)	.13	.34

Descriptive statistics are for 1,427,100 children from 647,035 families. All children are aged at least 25 in 2000. Twins are excluded from the sample. All children and parents are aged between 16 and 74 at some point between 1986 and 2000. Earnings are in Norwegian Kroner and FT indicates working 30+ hours per week. *Source:* Matched Administrative Data from Statistics Norway.

the spacing measures we use. We also exclude a small number of families in which a birth is reported as occurring before the mother was 16 or after the mother was 49.

Educational attainment is reported annually by the educational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. The education register started in 1970; we use information from the 1970 Census for individuals who completed their education before then. Thus, the register data are used for all but the earliest cohorts of children who did not get any additional education after 1970. Census data are self-reported, but the information is considered to be very accurate; there are no spikes or changes in the education data from the early to the later cohorts. We drop the small number of children who have missing education data (0.8 percent of cases).

Table I presents summary statistics for our sample, and Table II shows the distribution of family sizes in our sample.

TABLE II
NUMBER OF CHILDREN IN THE FAMILY (BY FAMILY)

	Frequency	Percentage
1	116,000	17.9
2	264,627	40.9
3	171,943	26.9
4	63,810	9.9
5	20,308	3.1
6	6,674	1.0
7	2,251	.4
8	822	.1
9	350	.05
10+	250	.04

All children and parents are aged between 16 and 74 at some point between 1986 and 2000. *Source:* Matched Administrative Data from Statistics Norway.

About 18 percent of families have one child, 41 percent have 2, 27 percent have 3, 10 percent have 4, and about 5 percent have 5 or more.

II.A. Relevant Institutional Detail

The Norwegian educational system has long been publicly provided and free to all individuals. In 1959 the Norwegian Parliament legislated a mandatory school reform that was implemented between 1959 and 1972 with the goal of both increasing compulsory schooling (from 7 to 9 years) and to standardize access and curriculum across regions. In 1997 compulsory schooling was increased to ten years by reducing the school start age from 7 to 6. Education continues to be free through college. Levels and patterns of educational attainment in Norway are similar to those in the United States. As can be seen in Table III, about 39 percent of children in our sample complete less than twelve years of schooling, 29 percent complete exactly twelve years, and 32 percent complete more than twelve years. In Norway, many students engage in education that is vocational in nature in lieu of a more academic high school track. We treat all years of education equally in measuring education.[7]

7. See Aakvik, Salvanes, and Vaage [2004] for a useful description of the Norwegian educational system.

TABLE III
AVERAGE EDUCATION BY NUMBER OF CHILDREN IN FAMILY AND BIRTH ORDER

	Average education	Average mother's education	Average father's education	Fraction with <12 years	Fraction with 12 years	Fraction with >12 years
			Family size			
1	12.0	9.2	10.1	.44	.25	.31
2	12.4	9.9	10.8	.34	.31	.35
3	12.3	9.7	10.6	.37	.30	.33
4	12.0	9.3	10.1	.43	.29	.28
5	11.7	8.8	9.5	.49	.27	.24
6	11.4	8.5	9.1	.54	.25	.20
7	11.2	8.3	8.9	.57	.24	.19
8	11.1	8.2	8.8	.58	.24	.18
9	11.0	8.0	8.6	.59	.25	.16
10+	11.0	7.9	8.8	.59	.26	.15
			Birth order			
1	12.2	9.7	10.6	.38	.28	.34
2	12.2	9.6	10.5	.38	.30	.31
3	12.0	9.3	10.2	.40	.31	.29
4	11.9	9.0	9.7	.43	.32	.25
5	11.7	8.6	9.2	.46	.31	.22
6	11.6	8.3	8.9	.49	.31	.20
7	11.5	8.1	8.7	.51	.30	.19
8	11.6	8.0	8.6	.49	.31	.20
9	11.3	7.9	8.4	.53	.32	.15
10+	11.3	7.8	8.7	.52	.32	.15
			All			
	12.2	9.5	10.4	.39	.29	.32

Descriptive statistics are for 1,427,100 children from 647,035 families. All children are aged at least 25 in 2000. Twins are excluded from the sample. All children and parents are aged between 16 and 74 at some point between 1986 and 2000. *Source:* Matched Administrative Data from Statistics Norway.

The birth control pill was introduced in Norway in the late 1960s and so was unavailable to most cohorts of parents in the sample we study [Noack and Østby 1981]. Abortion was not legalized in Norway until 1979 and so was not relevant to any of the cohorts we study. Government-provided daycare did not begin until the 1970s and so was only available to the later cohorts of children in our sample.[8]

8. Later in the paper, we compare the relationship between family size and education for earlier and later cohorts to see whether it changed due to these institutional differences.

III. Family Size

The empirical literature on the effects of family size on child outcomes generally supports a negative relationship between family size and child "quality" (usually education), even after controlling for socioeconomic factors.[9] However, few of these findings can be interpreted as causal; family size is endogenously chosen by parents and hence may be related to other unobservable parental characteristics that affect child outcomes.

In addition to the issues of endogeneity, the literature suffers from significant data limitations. Typically the studies do not have large representative data sets and do not study outcomes of economic interest, such as completed education and earnings. Additionally, the absence of information on birth order often means that birth order effects are confounded with family size effects. While the literature is extensive, we discuss below some of the studies that attempt to deal with some or all of these problems.

Rosenzweig and Wolpin [1980], Lee [2003], and Conley [2004a] all attempt to use exogenous variation in family size to determine the causal relationship between family size and child "quality."[10] Rosenzweig and Wolpin [1980], using data from India, and Lee [2003], using data from Korea, examine the effect of increases in fertility induced by twin births and sex of the first child, respectively, on child quality. Rosenzweig and Wolpin find that increases in fertility decrease child quality, while Lee finds that, if anything, larger families result in more educational expenditures per child. However, in both cases the sample sizes are small (25 twin pairs for Rosenzweig and Wolpin, approximately 2000 families for Lee), the estimates imprecise, and any family size effect could be confounded by the omission of birth order controls.

In one of the most thorough studies to date, Conley [2004] uses U. S. Census data from 1980 and 1990 to examine the effect of family size on private school attendance and the probability a child is "held back." To identify the causal effect of family size, he uses the idea that parents who have two same-sex children are more likely to have a third child than equivalent parents with two

9. See Blake [1989] and the numerous studies cited therein.

10. There is also a literature examining the effect of family size on parental outcomes. See, for example, Bronars and Grogger [1994] and Angrist and Evans [1998].

opposite sex children.[11] Using this as his instrument, Conley finds a significant negative effect of family size on private school attendance and an insignificant positive effect on whether a child is held back; when he analyzes the effects separately for first-born children and later children, he finds the effects are significant only for later-born children. However, his work is limited by the absence of better data; because of the structure of the Census data, he only has access to intermediate outcomes that may be weak proxies for outcomes later in life, and he does not know the structure of the family for families in which some individuals do not live in the household. Also, a recent literature suggests that sex composition may have direct effects on child outcomes (Dahl and Moretti [2004], Butcher and Case [1994], Conley [2000], and Deschenes [2002] all find some evidence of sex-composition effects. However, Kaestner [1997] and Hauser and Kuo [1998] find no evidence for these effects). Such effects imply that sex composition may not be a valid instrument for family size.[12]

We take two approaches to distinguish the causal effect of family size on children's education. First, we include controls for family background characteristics and birth order to see how much of the estimated effect of family size on child education can be instead attributed to these observable factors. Our second approach implements two-stage-least squares (2SLS) using the birth of twins as a source of exogenous variation in family size.

In Table III we show the mean educational attainment and the distribution of education in the family by family size. There are two very clear patterns. First, only children have much lower education than the average child in two- or three-child families. Second, from family sizes of 2 to 10+, we see a monotonic relationship that greater family size accompanies lower average ed-

11. Goux and Maurin [2004] also use this instrument with French data and find no significant effect of family size on the probability of being held back. However, this result is difficult to interpret as they also include a variable measuring overcrowding in the home.

12. Another strategy applied in the literature is to use siblings and difference out family level fixed effects. Guo and VanWey [1999] use data from the NLSY to evaluate the impact of family size on test scores. Although they are able to replicate the OLS pattern of a negative relationship between family size and children's outcomes, the authors find little support for this relationship when they do the within-sibling or within-individual analysis. However, this strategy requires strong assumptions about parental decision-making, relies on very small samples, and is identified from very small differences in family size. Phillips [1999] provides a thoughtful critique of this work, pointing out that, though suggestive, there are a number of factors that could explain these results even if there is an effect of family size on children's outcomes.

ucational attainment. This observed negative relationship is generally found throughout the literature.[13] Table III also shows that the family size effects are present throughout the education distribution. Although in estimation we focus on years of education, we have verified that similar results are found throughout the distribution.

III.A. Regression Results

The unconditional relationship between family size and education is only suggestive; for example, it could simply represent cohort effects, as we know that family sizes have declined over time as educational attainment has increased. To better understand the relationship, we regress education of children on family size, cohort indicators (one for each year of birth), mother's cohort indicators (one for each year of birth), and a female indicator.

The estimates are reported in columns 1 and 2 of Table IV.[14] In the first column we report estimates for a linear specification of family size. The highly significant coefficient of -0.18 implies that, on average, adding one child to completed family size reduces average educational attainment of the children by just less than one-fifth of a year.[15] We get similar results when we allow a more flexible form and add indicators for family size, although we now observe the negative only child effect seen in the summary statistics; only children have a quarter of a year (.27 years) less schooling on average than children in two-child families.[16] Although it is difficult to find comparable estimates for the United States, Blake [1989], with a slightly different specification, reports coefficients of about $-.20$ for the United States.[17]

III.B. Controlling for Family Background Characteristics

In Table III we see that parental education has the same family size pattern as child education. Thus, in columns 3 and 4

13. The negative effect of being an only child is sometimes found in the literature. See, for example, Hauser and Kuo [1998].
14. All reported results are estimated at the individual level. We also tried weighting so that each family is given equal weight, thereby placing more emphasis on smaller families; the conclusions were unaffected by using these weights.
15. In this regression, as in all others in the paper, the reported standard errors allow for arbitrary correlation between errors for any two children in the same family.
16. Because very few families have more than ten children, we have placed all families with ten or more children in the same category.
17. Blake's specification includes controls for fathers SEI, farm background, age, family intactness, and father's education.

TABLE IV
EFFECT OF FAMILY SIZE ON CHILDREN'S EDUCATION

Dependent variable: Child's education	No demographic controls		With demographic controls		Demographic and birth order controls	
Number of children	−.182* (.002)		−.095* (.002)		−.013* (.002)	
2-child family		.272* (.009)		.096* (.008)		.257* (.008)
3-child family		.132* (.009)		.001* (.008)		.270* (.009)
4-child family		−.176* (.010)		−.149* (.009)		.195* (.010)
5-child family		−.481* (.014)		−.279* (.012)		.115* (.013)
6-child family		−.730* (.021)		−.394* (.018)		.034 (.019)
7-child family		−.882* (.034)		−.472* (.029)		−.018 (.031)
8-child family		−.947* (.053)		−.502* (.045)		−.039 (.046)
9-child family		−1.036* (.072)		−.522* (.065)		−.037 (.067)
10+child family		−1.198* (.084)		−.614* (.075)		−.090 (.079)
Second child					−.294* (.004)	−.342* (.004)
Third child					−.494* (.007)	−.538* (.007)
Fourth child					−.632* (.010)	−.621* (.010)
Fifth child					−.718* (.015)	−.648* (.015)
Sixth child					−.782* (.023)	−.661* (.023)
Seventh child					−.854* (.037)	−.709* (.037)
Eighth child					−.753* (.059)	−.605* (.057)
Ninth child					−.945* (.081)	−.800* (.082)
Tenth or later child					−1.131* (.116)	−.981* (.111)
N	1,427,100	1,427,100	1,427,100	1,427,100	1,427,100	1,427,100
R^2	.0465	.0498	.1949	.1954	.1989	.1999

* indicates statistical significance at the 5 percent level. Standard errors (in parentheses) allow for correlation of errors within family. All regressions include indicators for age, mother's age, and sex. Demographic controls include indicators for mother's education, father's education, and father's age. N represents number of individuals.

of Table IV, we report the analogous estimates when we add indicator variables for father's and mother's education level (one for each year of education), and father's cohort (one for each birth year).[18] Adding these controls cuts the family size effects approximately in half—the effect of the linear term is now -0.095. Note, however, that even these smaller effects are still quite large as is clear from the coefficients on the family size dummy variables (column 4)—children in five-child families have on average approximately 0.4 of a year less schooling than children in two-child families.

III.C. Controlling for Birth Order

In the estimates so far, we may be confounding the effects of family size with those of birth order. We next add nine birth order dummy variables representing second child, third child, etc. with the final dummy variable equaling one if the child is the tenth child or greater. The excluded category is first child. These results are reported in columns 5 and 6 of Table IV. The family size effects are reduced to close to zero—the coefficient on the linear term is now -0.01—with the addition of the birth order dummies. Although statistically significant, this small number suggests that family size has very little effect on educational attainment. This impression is strengthened by the small coefficients on the family size dummy variables, many of which are now statistically insignificant. This is particularly interesting given that our priors are that the family size coefficients are likely biased upwards (in absolute terms) due to the negative relationship between omitted family characteristics (such as income) and family size.[19] We have also tried estimating the regression by birth order and find small effects of family size at each birth order, suggesting again that family size effects are very weak once birth order is controlled for.

18. Information on fathers is missing for about 16 percent of the sample. Rather than drop these observations, we include a separate category of missing for father's cohort and father's education. As mentioned earlier, the results are robust to dropping cases with missing father information.

19. Unfortunately, we do not have good measures of family income for the period over which the children are growing up. However, there is substantial evidence of a negative relationship between income and family size in Norway. See Skrede [1999].

III.D. Using Twins as an Instrument for Family Size

Rosenzweig and Wolpin [1980] first discuss the idea of using twin births as unplanned and therefore exogenous variation in family size. In their model, parents have an optimal number of children. The birth of twins can vary the actual family size from the desired size, and it is this arguably exogenous variation that is used to estimate the effects of family size on child outcomes. Our general estimation strategy is as follows:

$$ED = \beta_0 + \beta_1 FAMSIZE + X\beta_2 + \varepsilon \tag{1}$$

$$FAMSIZE = \alpha_0 + \alpha_1 TWIN + X\alpha_2 + v. \tag{2}$$

In this case, *ED* is the education of the child, and *FAMSIZE* is the total number of children in the family. *X* is the full vector of control variables used in columns 5 and 6 of Table IV. Equation (2) represents the first stage of the two-stage least squares estimation, where equation (1) is the second stage.

The *TWIN* indicator is equal to 1 if the nth birth is a multiple birth and equal to 0 if the nth birth is a singleton. We restrict the sample to families with at least n births and study the outcomes of children born before the nth birth.[20] In practice, we estimate the specification for values of n between 2 and 4. By restricting the sample to families with at least n births, we make sure that, on average, preferences over family size are the same in the families with twins at the nth birth and those with singleton births. In addition, we avoid the problem that families with more births are more likely to have at least one twin birth. By restricting the sample to children born before birth n, we avoid selection problems that arise because families who choose to have another child after a twin birth may differ from families who choose to have another child after a singleton birth. This also allows us to avoid the problem that a twin birth both increases family size and shifts downwards the birth order of children born after the twins.[21]

20. Due to their small sample size, Rosenzweig and Wolpin [1980] use the ratio of the number of twin births to the total number of births of the mother as their instrument for completed family size. This approach is problematic, as the denominator is, at least partly, a choice variable for the mother. Thus, their instrument is still likely to be correlated with preferences of the parents over number of children. Our methodology avoids this problem.

21. Rosenzweig and Wolpin [1980] use outcomes of all children, and so their estimates suffer from this problem. In a later paper [Rosenzweig and Wolpin 2000] they recommend using whether or not the first born child is a twin as an

III.E. Validity of Twins Instrument

In order for our IV estimates to be consistent, it must be that the instrument is uncorrelated with the error term in equation (1). One concern is that the occurrence of a twin birth may not be random and may be related to unobservable family background characteristics. By definition, this is untestable, but we do examine whether the probability of twins is related to observed characteristics such as mother's and father's education by estimating linear probability models of the probability of a twin birth at each parity using the full set of control variables. F-tests indicate that the hypothesis that the coefficients on mother's education are jointly zero and that the coefficients on father's education are jointly zero cannot be rejected at even the 10 percent significance level. Given the enormous sample sizes, these results strongly suggest that twinning probabilities are not related to parents' education.[22]

Although we include controls for year-of-birth of both mother and child (and, hence implicitly, age of mother at birth controls), these relate to the age of the child under study and not to the age of the child from the potential twin birth. It is well-established that twin probabilities increase with maternal age at birth [Jacobsen, Pearce, and Rosenbloom 1999; Bronars and Grogger 1994]. As a check, we have included the age of the mother at the time of the potential twin birth as a control; this has very little effect on our estimates.

Another concern is that the birth of twins may have a direct effect on sibling outcomes beyond just increasing family size. Although inherently untestable, we did examine one possible mechanism through which twins might affect outcomes of earlier children: spacing. We found that, in families without twins, early children tended to have lower education if the two immediately following siblings are more closely spaced together. If this result can be extrapolated to the case of twins, in which the space is zero, it implies that the effect of a twin birth is both to increase family size and to adversely affect prior children through spacing. Thus, the 2SLS estimates of the effects of family size are probably

instrument in this type of context. This approach would also suffer from the confluence of family size and birth order effects.

22. Given the youngest children in our sample were born in 1975, modern fertility drugs which make multiple births more likely are not relevant to our sample.

biased toward finding negative effects of family size itself. More generally, to the extent that twins are not excludable and have a negative direct effect on the educational attainment of the other children in the family (perhaps because they are more likely to be in poor health), our estimates of the effect of family size using the twins instrument will be biased toward finding larger negative effects.

III.F. Results Using Twins

The 2SLS estimates are presented in Table V, along with the first-stage coefficients and the OLS estimates using the same sample. The first stage is very strong and suggests that a twin birth increases completed family size by about 0.7 to 0.8. As expected, twins at higher parity have a larger effect on family size, presumably because they are more likely to push families above their optimal number of children. The t-statistics from the first stage are typically around 60, indicating that there are no concerns about weak instruments in this application.

The 2SLS estimate of the effect on the first child of changes in family size induced by the second birth being a twin birth is 0.038 (0.047). This implies no large adverse effects of increased family size on educational outcomes (the lower bound of the 95 percent confidence interval is $-.055$). The equivalent estimate for families that have at least three births is -0.016 (0.044), and for families that have at least four births it is -0.024 (0.059). The lower bound of the 95 percent confidence intervals for these estimates are $-.101$, and $-.140$, respectively. Taken together, these three estimates are all less negative than the OLS estimates, and the first is estimated precisely enough to rule out large negative effects of family size on education. These results are consistent with the evidence in Table IV that family size has a negligible effect on children's outcomes once one controls for birth order.

III.G. Using Same Sex as an Instrument for Family Size

As described earlier, there is some question whether sex composition of siblings has an independent effect on children's outcomes. However, for completeness, we describe results using this instrument here. We study the outcomes of the first two children and use as the instrument whether or not these two children are the same sex. The first stage is strong—a

TABLE V
EFFECT OF FAMILY SIZE ON EDUCATION OF CHILDREN USING PRESENCE OF TWIN BIRTH AS AN INSTRUMENT LOOKING AT CHILDREN BORN BEFORE POTENTIAL TWIN BIRTH

	OLS (Twins sample)	First stage	Second stage	N
Instrument: Twin at second birth		.676*		525,952
(Sample: First child in families with 2 or more births)		(.013)		
Number of children in family	−.060*		.038	
	(.003)		(.047)	
Instrument: Twin at third birth		.755*		500,406
(Sample: First and second children in families with 3 or more births)		(.012)		
Number of children in family	−.076*		−.016	
	(.004)		(.044)	
Second child	−.314*		−.324*	
	(.006)		(.010)	
Instrument: Twin at fourth birth		.817*		259,349
(Sample: First, second, and third children in families with 4 or more births)		(.016)		
Number of children in family	−.059*		−.024	
	(.006)		(.059)	
Second child	−.305*		−.310*	
	(.010)		(.013)	
Third child	−.531*		−.542*	
	(.013)		(.023)	

* indicates statistical significance at the 5 percent level. Standard errors (in parentheses) allow for correlation of errors within family. All regressions include indicators for age, mother's age, mother's education, father's education, father's age, and sex. N represents number of individuals.

coefficient of 0.086 with a standard error of 0.002. The second stage estimate is 0.28 (0.06), implying that increased family size leads to significantly higher educational outcomes for children. We do not find the magnitude of this estimate credible and suspect that there are independent positive effects on outcomes of having a sibling of the same sex. Future research of our own will examine the direct effects of family sex composition on children's outcomes.

IV. Birth Order

Given that birth order effects appear to drive the observed negative relationship between family size and child education, we next turn our attention to birth order. Blake [1989] describes some of the factors that make empirical estimation of birth order effects difficult. First, it is necessary to fully control for family size, or one will confound family size and birth order effects. Second, the presence of cohort effects in educational attainment will tend to bias results to the extent that later born children are in different cohorts from earlier born children. Thus, one needs to have multiple cohorts for each birth order and include unrestricted cohort effects. Third, it is important to include cohort effects for the parents, as, conditional on child cohort, the parents of first-borns are likely to be younger than parents of third or fourth born children.[23]

Studies of the effects of birth order on education have been limited by the absence of the large representative data sets necessary to thoroughly address these issues. For example, Behrman and Taubman [1986] use data on only about 1000 individuals from the National Academy of Science/National Research Council twin sample and their adult offspring. Hanushek [1992] uses a small sample of low-income black families collected from the Gary Income Maintenance Experiment. Hauser and Sewell [1985] use the Wisconsin Longitudinal Study; their data are less than ideal as, by definition, the survey respondent has completed high school. Hauser and Sewell find no evidence for birth order effects; Behrman and Taubman find some evidence that later children have lower education, and Hanushek observes a U-shaped pattern of achievement by birth order for large families, with oldest and youngest children having higher education than middle children (although it is not clear that there are any statistically significant differences). However, all these studies estimate birth order effects quite imprecisely and, due to small samples, do not

23. She also suggests that estimated birth order effects could be biased if spacing has an independent effect on outcomes and spacing differs on average by birth order. (Also see Powell and Steelman [1993].) To address this issue, we created three variables (1) the number of children born within one year of the person, (2) the number of children born two or three years apart from the person, and (3) the number of children born four or five years apart from the person. Adding these variables to the regression had no appreciable effect on the estimated birth order effects. These specifications should be treated with some caution as spacing may be optimally chosen by families and, hence, is an endogenous variable.

include the full set of family size indicators, cohort indicators, and parental cohort indicators we use in this paper.

More recently, Iacovou [2001] uses the British National Child Development Study (NCDS) and finds that later born children have poorer educational outcomes than earlier born. While this is a very thorough study, it does suffer from some weaknesses. First, the sample size is small (about 18,000 initially), and there is much attrition over time (about 50 percent) so estimates are imprecise and may be subject to attrition bias. Second, all children in the sample are born the same week so, conditional on mother's cohort, birth order is strongly correlated with age at first birth and it is difficult to tease out separate effects of these two variables.[24]

The empirical literature on the effect of birth order on children's outcomes is quite extensive; despite this, however, there have been no strong conclusions due to data and methodological limitations.[25] Because of our large data set on the population of Norway over an extended period of time, we are able to overcome most of the limitations of the prior literature. Also, unlike the previous literature, we use family fixed effects models in addition to OLS. Family fixed effects allow us to estimate effects of birth order *within* families, thereby differencing out any family-specific characteristics that are affecting all children.

IV.A. Birth Order Results

The average education level and distribution of education by birth order are listed in Table III; there is a clear pattern of declining education for higher birth orders. However, as with the case of family size effects, these summary statistics can be misleading in that we are not controlling for family size, cohort effects, or any other demographic characteristics that may be influencing these statistics. As a result, we estimate the relationship between birth order and educational attainment in a regression framework, using the same set of control variables as in the family size analysis.

In column 1 of Table VI we present estimates for the full sample, including a full set of family size dummies (presented in

24. She does not control for age at first birth. Age at first birth is implicitly controlled for in our family fixed effects specifications. We have also added it as a control in our OLS models, and doing so had little effect.

25. There is also an extensive literature in psychology and sociology on the effects of birth order on personality. See Conley [2004b] for a summary.

TABLE VI
EFFECT OF BIRTH ORDER ON CHILDREN'S EDUCATION: ESTIMATED BY FAMILY SIZE

	All families[a]	Two-child family	Three-child family	Four-child family	Five-child family	Six-child family	Seven-child family	Eight-child family	Nine-child family	Ten+ child family
Second child	−.342*	−.378*	−.318*	−.327*	−.278*	−.256*	−.247*	−.449*	−.284*	−.350*
	(.004)	(.007)	(.007)	(.012)	(.020)	(.035)	(.063)	(.104)	(.172)	(.174)
Third child	−.538*		−.610*	−.558*	−.488*	−.493*	−.460*	−.632*	−.595*	−.626*
	(.007)		(.011)	(.015)	(.024)	(.040)	(.072)	(.115)	(.189)	(.208)
Fourth child	−.621*			−.768*	−.646*	−.659*	−.610*	−.695*	−.480*	−.877*
	(.010)			(.021)	(.030)	(.047)	(.081)	(.134)	(.199)	(.226)
Fifth child	−.648*				−.815*	−.701*	−.704*	−.841*	−.662*	−.936*
	(.015)				(.040)	(.059)	(.097)	(.150)	(.222)	(.245)
Sixth child	−.661*					−.872*	−.742*	−.813*	−.638*	−1.042*
	(.023)					(.073)	(.117)	(.171)	(.257)	(.262)
Seventh child	−.709*						−.873*	−1.024*	−.696*	−1.241*
	(.037)						(.141)	(.203)	(.271)	(.287)
Eighth child	−.605*							−1.013*	−.460	−1.445*
	(.057)							(.236)	(.319)	(.314)
Ninth child	−.800*								−.816*	−1.569*
	(.082)								(.373)	(.343)
Tenth or later child	−.981*									−1.919*
	(.111)									(.399)
N	1,427,100	478,957	449,799	227,697	92,203	36,645	14,412	6,074	2,878	2,435

* indicates statistical significance at the 5 percent level. Standard errors (in parentheses) allow for correlation of errors within family. Each column represents a separate regression. All regressions include indicators for age, mother's age, mother's education, father's education, father's age, and sex.

a. In this specification, indicator variables for family size are also included in the regression. N represents number of individuals.

Table IV, column 6). Relative to the first child, we observe a steady decline in child's education by birth order. The large magnitude of the birth order effects relative to the family size effects is clear in Figure I.

Each subsequent column in Table VI represents a separate regression for a particular family size. If we look across row one, we can see the effect of being a second child (omitted category is first child) is large and negative for all family sizes. This is particularly striking, given that earlier work found somewhat different effects for different family sizes [Hanushek 1992].[26] As in column 1, we find a monotonic decline in average education as birth order increases. It is interesting to note that, in addition to the monotonic decline in educational attainment by birth order, we also observe a negative "last child" effect. This "last child" effect could be consistent with an optimal stopping model in which parents continue to have children until they have a "poor quality" child, at which point parents may opt to discontinue childbearing.[27] However, this model cannot explain the monotonic decline we observe in educational attainment of the earlier children; for example, it cannot explain the large magnitudes of the second child effect in families with more than two children.[28]

Table VII then presents the results with family fixed effects included. These estimates are almost identical to those without family fixed effects, suggesting that the estimated birth order effects do not reflect omitted family characteristics.[29]

V. Heterogeneous Effects of Family Size and Birth Order

In Table VIII we test the sensitivity of our results to various stratifications of our sample. In columns 1 and 2 of Table VIII, we first break the sample by sex. As one can see, the results are quite

26. In his work, Hanushek also focuses on the effect of school quality by including such measures in the estimation; although we do not have school quality measures, we have tried adding municipality effects (schools are organized at the municipality level) and municipality-year effects to control for geographical and temporal differences in school quality; the inclusion of these variables does not affect our family size or birth order conclusions.

27. There is some evidence that early behaviors are reasonable predictors for later outcomes. (See Currie and Stabile [2004].)

28. This "last child" effect is also consistent with Zajonc's [1976] hypothesis that last children suffer from having nobody to teach. Also, last-born children may be more likely to have been unplanned children.

29. We have added controls for family size at ages two and five, and this had little impact on the estimated birth order effects. This suggests that birth order is not just proxying for family size when young.

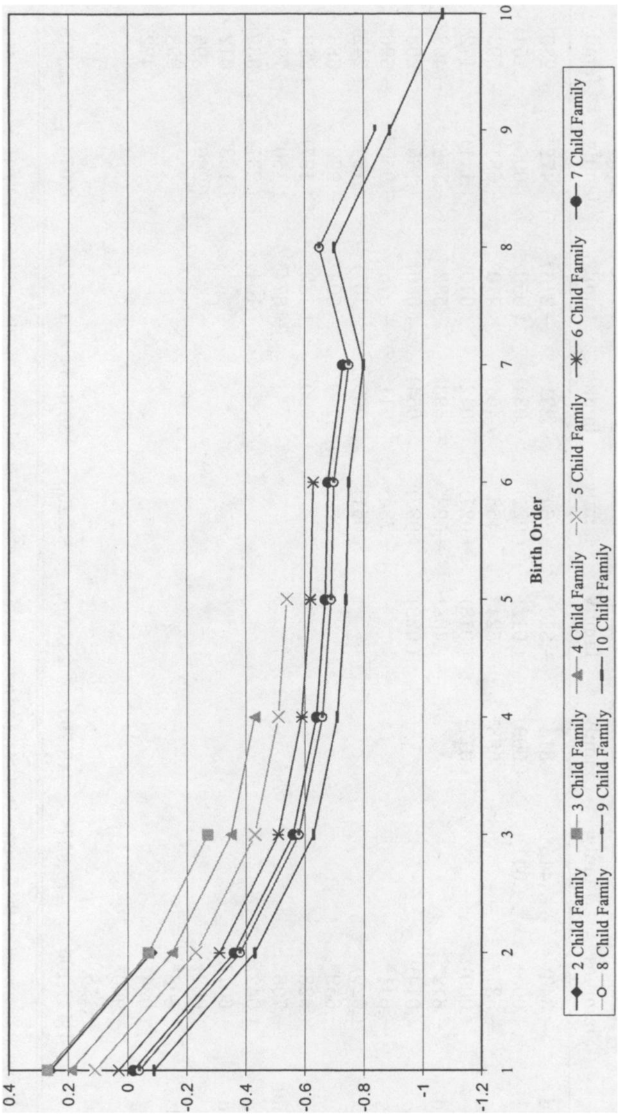

FIGURE I
Effect of Family Size and Birth Order on Educational Attainment Relative to the Educational Attainment of a Child in a Single-Child Family (Predicted Values from Table III, column 6)

TABLE VII
EFFECT OF BIRTH ORDER ON CHILDREN'S EDUCATION: ESTIMATED BY FAMILY SIZE WITH FAMILY FIXED EFFECTS

	All families	Two-child family	Three-child family	Four-child family	Five-child family	Six-child family	Seven-child family	Eight-child family	Nine-child family	Ten+ child family
Second child	−.340*	−.415*	−.315*	−.313*	−.264*	−.231*	−.216*	−.472*	−.280	−.366*
	(.005)	(.011)	(.009)	(.012)	(.021)	(.034)	(.059)	(.096)	(.151)	(.170)
Third child	−.531*		−.598*	−.524*	−.458*	−.451*	−.410*	−.681*	−.593*	−.666*
	(.009)		(.017)	(.018)	(.027)	(.042)	(.070)	(.111)	(.172)	(.188)
Fourth child	−.612*			−.708*	−.592*	−.593*	−.532*	−.778*	−.498*	−.918*
	(.014)			(.028)	(.037)	(.054)	(.086)	(.135)	(.203)	(.214)
Fifth child	−.641*				−.734*	−.611*	−.575*	−.950*	−.696*	−.967*
	(.020)				(.051)	(.069)	(.107)	(.164)	(.242)	(.246)
Sixth child	−.659*					−.749*	−.604*	−.939*	−.686*	−1.081*
	(.028)					(.090)	(.132)	(.197)	(.286)	(.284)
Seventh child	−.706*						−.674*	−1.160*	−.791*	−1.293*
	(.042)						(.164)	(.237)	(.339)	(.328)
Eighth child	−.607*							−1.153*	−.612	−1.532*
	(.062)							(.286)	(.396)	(.368)
Ninth child	−.808*								−.998*	−1.647*
	(.094)								(.472)	(.416)
Tenth or	−.940*									−1.982*
later child	(.118)									(.483)
N	1,427,100	478,957	449,799	227,697	92,203	36,645	14,412	6,074	2,878	2,435

* indicates statistical significance at the 5 percent level. Standard errors are in parentheses. Each column represents a separate regression. All regressions include indicators for age and sex. N represents number of individuals.

TABLE VIII
FAMILY SIZE AND BIRTH ORDER EFFECTS BY GENDER, MOTHER'S EDUCATION, COHORT, AND FAMILY STRUCTURE

	Males	Females	Mother's education ≤12	Mother's education >12	Mother born before 1935	Mother born after 1935	Intact families
			Family size				
OLS	−.093*	−0.95*	−.099*	−.021*	−.083*	−.120*	−.116*
	(.002)	(.002)	(.002)	(.007)	(.003)	(.003)	(.003)
OLS with birth order	−.025*	.002	−.015*	.071*	−.016*	−.011*	−.032*
	(.003)	(.003)	(.002)	(.008)	(.003)	(.003)	(.003)
IV—twin at 2nd birth	.031	.054	.044	−.089	.100	−.007	.016
	(.066)	(.078)	(.052)	(.122)	(.077)	(.058)	(.062)
IV—twin at 3rd birth	−.031	−.001	−.042	.098	.018	−.051	−.066
	(.060)	(.057)	(.047)	(.121)	(.069)	(.054)	(.055)
IV—twin at 4th birth	−.079	−.036	−.012	−.169	−.065	.022	.001
	(.077)	(.078)	(.062)	(.203)	(.085)	(.077)	(.074)
			Birth order				
Second child	−.301*	−.382*	−.329*	−.293*	−.389*	−.305*	−.311*
	(.006)	(.006)	(.006)	(.019)	(.008)	(.007)	(.007)
Third child	−.462*	−.615*	−.504*	−.523*	−.601*	−.476*	−.500*
	(.009)	(.010)	(.010)	(.036)	(.014)	(.014)	(.012)
Fourth child	−.513*	−.732*	−.583*	−.626*	−.691*	−.549*	−.587*
	(.014)	(.014)	(.014)	(.056)	(.020)	(.021)	(.018)
Fifth child	−.538*	−.766*	−.620*	−.667*	−.725*	−.579*	−.625*
	(.022)	(.022)	(.020)	(.088)	(.026)	(.031)	(.026)
Sixth child	−.527*	−.803*	−.646*	−.679*	−.745*	−.603*	−.657*
	(.034)	(.035)	(.028)	(.149)	(.036)	(.049)	(.036)
Seventh child	−.528*	−.907*	−.687*	−.874*	−.818*	−.588*	−.728*
	(.052)	(.057)	(.042)	(.264)	(.051)	(.084)	(.054)
Eighth child	−.380*	−.871*	−.599*	−1.046*	−.711*	−.536*	−.664*
	(.087)	(.085)	(.063)	(.395)	(.074)	(.135)	(.078)
Ninth child	−.669*	−.945*	−.825*	−1.136	−.921*	−.715*	−.812*
	(.109)	(.131)	(.096)	(.702)	(.110)	(.226)	(.120)
Tenth child	−.912*	−1.058*	−.914*	−2.378*	−1.099*	−.426	−1.239*
	(.151)	(.169)	(.119)	(.934)	(.136)	(.313)	(.142)

* indicates statistical significance at the 5 percent level. All family size regressions include indicators for age, sex, mother's age, mother's education, father's education, and father's age. The third row presents the coefficient on family size for the first child for the sample of families with at least two children, using twins at second birth as an instrument; the fourth row is the effect of family size on the first two children conditional on having at least three children using twins at third birth as an instrument; and the fifth row is the effect of family size on the first three children conditional on having at least four children using twins at fourth birth as an instrument. The IV estimates include a full set of birth order controls. The birth order estimates come from family fixed effects specifications that include age indicators plus a sex indicator except for columns 1 and 2, where birth order effects are from specifications that include the controls listed above plus a full set of family size indicators. N represents number of individuals. Sample sizes are 744,292 for column 1, 682,808 for columns 2, 1,251,988 for column 3, 162,291 for column 4, 709,700 for column 5, 717,400 for column 6, and 772,985 for column 7.

similar for men and women; although statistically different from zero, OLS family size effects (presented in the first row; these estimates include controls for family background) become close to zero in magnitude when birth order is controlled for (row 2). When we estimate the family size coefficients using the twin instrument, we get small but imprecisely estimated coefficients for both men and women. (The third row presents the coefficient on family size for the first child for the sample of families with at least two children using twins at second birth as an instrument; the fourth row is the effect of family size on the first two children conditional on having at least three children using twins at third birth as an instrument, and the fifth row is the effect of family size on the first three children conditional on having at least four children using twins at fourth birth as an instrument.) While, overall, the effects of family size for women seem smaller than those for men, in both cases we see small family size effects. In contrast, birth order effects are larger for women than for men.[30]

We also stratify our sample by mother's educational attainment. (See columns 3 and 4 in Table VIII.) This may be a relevant break if financial constraints are driving the observed patterns; families with better educated mothers may be less financially constrained than those with lower educated mothers. We find that the magnitude of the birth order effects does not differ much across education groups; if anything, birth order effects are stronger among individuals with more educated mothers, which runs counter to the expected results if financial constraints were driving the results.

We next compare the effects of family size and birth order for earlier cohorts relative to later cohorts. Later cohorts had the benefit of more effective birth control and, as a result, parents may have exercised more control over completed family size. When we stratify our sample based on mother's cohort (those born before 1935 versus those born after), we find similar effects of family size and birth order for both samples. (See Table VIII, columns 5 and 6.) While birth order effects are slightly smaller in

30. In these summary results (and in Table IX), we report birth order effects from family fixed effects specifications except in the case where we split the sample by sex. In this case, because we are missing family members in each sample, using fixed effects induces selection effects (for example, the only two-child families used in the female sample are families with all girls), and provides rather imprecise estimates (because so many families are omitted). When we do the male/female split, the birth order effects come from the OLS model with the full set of demographic controls and the full set of family size indicators.

the younger sample relative to the older sample, they are still quite large and significant.

It may be the case that birth order is proxying for family structure. Because we do not observe marital status of parents at each age, it may be that younger children are more likely to be from broken homes, and children from broken homes have lower educational attainment. To test this, we examine the effect of family size and birth order on educational attainment for a subset of children whose parents were still married in 1987 (the first year we have information on marital status); this includes approximately 60 percent of our sample.[31] The results are presented in column 7 of Table VIII. The family size results are quite consistent with the results obtained using the full sample; significant OLS coefficients, much smaller when we include birth order, and no statistically significant effects using IV. While not reported in the table, it is interesting to note that the negative only child effect observed in the full sample disappears when we use the intact family subsample, suggesting that this result is in fact being driven by family structure. Birth order effects are almost exactly the same as in the full sample, implying that these effects do not result from marital breakdown.

VI. Other Outcomes

Education is only one measure of human capital; we next examine the effect of family size and birth order on labor market outcomes such as earnings and the probability of working full time.[32] In addition, teenage motherhood has been associated with many long-term economic and health disadvantages such as lower education, less work experience and lower wages, welfare dependence, lower birth weights, higher rates of infant mortality, and higher rates of participation in crime [Ellwood 1988; Jencks

31. Note that the youngest children in our sample would have been twelve at this time. We also tested the sensitivity of these results to looking only at children who were at least 30 in 2000; in this case, the youngest children would have been 17 at the point we are measuring family structure. The results are entirely insensitive to this age cutoff.

32. There is some previous research using labor market outcomes; for example, Kessler [1991] uses data from the NLSY to examine the effects of birth order and family size on wages and employment status. He finds that neither birth order nor childhood family size significantly influences the level or growth rate of wages. Also, Behrman and Taubman [1986] do not find evidence for birth order effects on earnings. Both of these papers suffer from very small sample sizes (generally fewer than 1000 observations).

1989; Hoffman, Foster, and Furstenberg 1993]. Therefore, we also study it as an outcome variable.

VI.A. Labor Market Outcomes

We examine the effects of family size and birth order on the following labor market outcomes: the earnings of all labor market participants, the earnings of full-time employees only, and the probability of being a full-time employee. Descriptive statistics for these variables are included in Table I. Earnings are measured as total pension-qualifying earnings; they are not topcoded and include all labor income of the individual. For the purposes of studying earnings and employment, we restrict attention to individuals aged between 30 and 59 who are not full-time students. In this group, approximately 90 percent of both men and women have positive earnings. Given this high level of participation, our first outcome is log(earnings) conditional on having nonzero earnings. Since the results for this variable encompass the effects on both wage rates and hours worked, we also separately study the earnings of individuals who have a strong attachment to the labor market and work full-time (defined as 30+ hours per week).[33] As seen in Table I, about 71 percent of men and 46 percent of women are employed full-time in 2000. Our third outcome variable is whether or not the individual is employed full-time (as defined above). We have chosen this as our employment outcome as most individuals participate to some extent and, particularly for women, the major distinction is between full-time and part-time work.

To maximize efficiency, we use all observations on individuals in the 1986–2000 panel, provided that they are aged between 30 and 59. Because we have people from many different cohorts, individuals are in the panel for different sets of years and at different ages. Therefore, as before, we control for cohort effects. Also, we augment the previous specification by adding indicator variables for the panel year. This takes account of cyclical effects on earnings, etc. Some individuals are present in more periods than others and, hence, have greater weight in estimation. We

33. To identify this group, we use the fact that our data set identifies individuals who are employed and working full-time at one particular point in the year (in the second quarter in the years 1986–1995, and in the fourth quarter thereafter). An individual is labeled as employed if currently working with a firm, on temporary layoff, on up to two weeks of sickness absence, or on maternity leave.

have verified that if we weight each individual equally in estimation, we get similar but less precisely estimated coefficients. None of our conclusions change. Also, the robust standard errors take account of the fact that we have repeated observations on individuals.

Summary results for earnings are presented in Table IX. Because of the sizable differences in labor market experiences of men and women, we estimate separate regressions by gender. The first three columns present the results for men, and the second three present the results for women. Interestingly, the earnings results are quite consistent with the results obtained using education as the outcome. The OLS family size effects become much smaller when birth order controls are introduced. Also, the family size estimates using the twins instruments are always statistically insignificant, always less negative than OLS, and generally have a positive sign. As with education, women seem to be more affected by birth order; among women, the inclusion of the birth order effects in the OLS regressions reduces the family size coefficient by more than half, and the birth order effects are quite large. While the effects are significant for men, they are much smaller in magnitude. However, the differences between men and women are much smaller when the sample is restricted to full-time employees. To get a sense of the magnitude of these earnings effects, we estimated the return to education for the full-time sample using the birth order indicators as instruments for education (including family size indicators in addition to the usual controls); when we do this, we get an implied return of approximately .05 for men and .07 for women, suggesting that much of the birth order effects on earnings is likely working through education.[34]

We study the probability of working full-time in columns 3 and 6 of Table IX. For computational reasons, we use linear probability models and do the IV estimation using 2SLS.[35] We find pronounced birth order effects for women (later-borns are less likely to work full-time), and the addition of birth order effects reduces the OLS family size estimate approximately in half. However, for men the family size coefficient is approximately the same with and without birth order effects, and the

34. Despite the significant birth order effects on earnings, they have a negligible effect on the within-family earnings variance.

35. The heteroskedasticity that results from the linear probability model is accounted for by our robust standard errors.

TABLE IX
FAMILY SIZE AND BIRTH ORDER EFFECTS

	Men			Women			
	Log (earnings)	Log (earnings) given full-time	Prob work full-time	Log (earnings)	Log (earnings) given full-time	Prob work full-time	Prob of teen birth
			Family size				
OLS	−.015*	−.010*	−.040*	−.020*	−.011*	−.010*	.016*
	(.001)	(.0004)	(.0004)	(.001)	(.0005)	(.0004)	(.0005)
OLS with birth order	−.012*	−.006*	−.011*	−.010*	−.010*	−.006*	.010*
	(.001)	(.0005)	(.0005)	(.001)	(.001)	(.001)	(.001)
IV—twin at 2nd birth	−.003	−.005	−.005	.030	.015	.021	−.018
	(.020)	(.014)	(.013)	(.026)	(.016)	(.015)	(.013)
IV—twin at 3rd birth	.013	.019	−.010	.013	.005	.013	−.005
	(.018)	(.012)	(.012)	(.022)	(.014)	(.012)	(.011)
IV—twin at 4th birth	.022	.002	−.003	.013	−.006	.024	.026
	(.020)	(.014)	(.013)	(.026)	(.016)	(.015)	(.015)
			Birth order				
Second child	−.012*	−.018*	.0016	−.042*	−.027*	−.021*	.022*
	(.002)	(.001)	(.0013)	(.003)	(.002)	(.001)	(.001)
Third child	−.028*	−.031*	−.001	−.066*	−.040*	−.031*	.040*
	(.003)	(.002)	(.002)	(.004)	(.002)	(.002)	(.002)
Fourth child	−.031*	−.038*	.001	−.088*	−.052*	−.035*	.050*
	(.004)	(.003)	(.003)	(.006)	(.004)	(.003)	(.003)
Fifth child	−.030*	−.040*	.010*	−.091*	−.057*	−.038*	.056*
	(.007)	(.004)	(.005)	(.009)	(.006)	(.005)	(.006)
Sixth child	−.025*	−.038*	.018*	−.077*	−.048*	−.038*	.054*
	(.012)	(.007)	(.008)	(.015)	(.009)	(.008)	(.009)
Seventh child	−.013	−.036*	.028*	−.111*	−.091*	−.056*	.084*
	(.020)	(.012)	(.012)	(.025)	(.018)	(.013)	(.017)
Eighth child	−.033	−.051*	−.008	−.114*	−.109*	−.041*	.064*
	(.034)	(.019)	(.020)	(.041)	(.031)	(.019)	(.025)
Ninth child	−.011	−.025	.039	−.144*	−.080	−.105*	.066
	(.047)	(.022)	(.029)	(.074)	(.049)	(.029)	(.042)
Tenth child	−.017	−.085	.044	−.212	−.208*	−.035	.050
	(.053)	(.043)	(.039)	(.087)	(.056)	(.041)	(.054)

* indicates statistical significance at the 5 percent level. All family size regressions include indicators for age, sex, mother's age, mother's education, father's education, and father's age. The third row presents the coefficient on family size for the first child for the sample of families with at least two children, using twins at second birth as an instrument; the fourth row is the effect of family size on the first two children conditional on having at least three children using twins at third birth as an instrument; and the fifth row is the effect of family size on the first three children conditional on having at least four children using twins at fourth birth as an instrument. The IV estimates include a full set of birth order controls. The birth order estimates come from specifications that include the controls listed above plus a full set of family size indicators. The sample sizes differ between columns. In column 1 there are 5,503,423 observations on 590,879 men; in column 2 there are 4,125,280 observations on 525,225 men, in column 3 there are 5,900,902 observations on 604,703 men, in column 4 there are 4,532,190 observations on 528,137 women, in column 5 there are 2,080,870 observations on 368,887 women, in column 6 there are 5,154,202 observations on 547,819 women, and in column 7 there are 378,534 women with one observation per person.

only significant birth order effect is that fifth, sixth, and seventh children are more likely to work full-time than earlier born children.

VI.B. Probability of Having a Teen Birth

We present the effects of family size and birth order on the probability of having a teen birth in Table IX, column 7. We restrict the sample to women aged at least 36 in 2000 and denote a teen birth if they have a child aged at least sixteen in 2000 who was born before the woman was aged twenty.[36] The results are quite consistent with the earlier results for education; a positive OLS effect of family size on teenage pregnancy that becomes significantly smaller when birth order effects are included. Once family size is instrumented for with the twins indicator, the family size effect becomes statistically insignificant. On the other hand, there are large birth order effects, with a move from being the first child to the fourth child increasing the probability of having a child as a teenager by five percentage points (the average probability of having a teen birth is .13).

VII. Conclusions

In this paper we examine the effect of family size on child education using both exogenous variation in family size induced by twin births as well as extensive controls for not only parent and child cohort effects and parental education, but also birth order effects. We find evidence that there is little if any family size effect on child education; this is true when we estimate the relationship with controls for birth order or instrument family size with twin births.

Given that family sizes continue to decline in developed countries, these results suggest that children may not necessarily be better off than if their family had been larger. Our results imply that, though average child outcomes may improve, there may be little effect on first-born children.

In contrast, we find very large and robust effects of birth order on child education. To get a sense of the magnitude of these effects, the difference in educational attainment between the first

36. This sample restriction is required because to know whether a woman had a teen birth we need to observe both mother's and child's ages, so both must appear in the administrative data.

child and the fifth child in a five-child family is roughly equal to the difference between Black and White educational attainment calculated from the 2000 census. We augment the education results by using earnings, whether full-time employed, and whether had a birth as a teenager as additional outcome variables. We also find strong evidence for birth order effects with these other outcomes, particularly for women. Later born women have lower earnings (whether employed full-time or not), are less likely to work full-time, and are more likely to have their first birth as a teenager. In contrast, while later born men have lower full-time earnings, they are not less likely to work full-time.

These sizable birth order effects have potential methodological implications: researchers using sibling fixed effects models to study economic outcomes may obtain biased estimates unless they take account of birth order effects in estimation.

One important issue remains unresolved: what is causing the birth order effects we observe in the data? Our findings are consistent with optimal stopping being a small part of the explanation. Also, the large birth order effects found for highly educated mothers, allied with the weak evidence for family size effects, suggest that financial constraints may not be that important. Although a number of other theories (including time constraints, endowment effects, and parental preferences) have been proposed in the literature, we are quite limited in our ability to distinguish between these models. Finding relevant explanations will have important implications for models of household allocation (which can predict both compensatory or reinforcing behavior on the part of parents and also depend on differences in endowments and preferences) and models of child development.

Our findings so far are quite provocative; if, in fact, there is no independent trade-off between family size and child quality, perhaps we need to revisit models of fertility and reconsider what should be included in the "production function" of children. What other explanations will generate the patterns we observe in the data? Clearly, our results suggest a need for more work in this area.

University of California, Los Angeles, IZA, and National Bureau of Economic Research
University of California, Los Angeles, and IZA
Norwegian School of Economics and Business Administration, Statistics Norway, and IZA

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