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Family size and child outcomes: Is there really no trade-off?

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Family size and child outcomes: Is there really no trade-off?*

by

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Abstract

Recent empirical work questions the negative relationship between family size and children's attainments proposed by theoretical work and supported by a large empirical literature. We use twin births as an exogenous source of variation in family size in an unusually rich dataset where it is possible to separately look at intermediate and long-run outcomes. We find little evidence of a causal effect on long-term outcomes such as years of schooling and earnings, and studies that do not take selection effects into account are likely to overstate the effects. We do, however, find a small but significant negative impact of family size on grades in compulsory and secondary school.

Keywords: Family size; Twin births; Education; Earnings
JEL-codes: J13; I20

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1 Introduction

Social scientists have for a long time been interested in how early experiences determine children's long-term welfare (e.g. Haveman and Wolfe 1995). One topic that has received much attention in the literature is the relationship between family size and the outcomes of children. Recent studies question previous findings and argue that more complex empirical strategies are needed to identify causal effects of family size. Previous research attempting to deal with the methodological problem have mainly focused on long-term outcomes such as earnings or years of schooling, with the standard result being no trade-off in the long run. In contrast, we are able to look at both long-term and intermediate outcomes (e.g. grades in compulsory and secondary school) using high quality data on entire Swedish birth cohorts. Intermediate outcomes are interesting because performance and well-being during adolescence are in themselves of interest to policy makers. Also, short-term indicators provide a supplementary and in some sense cleaner test of the quantity-quality trade-off hypothesis.

Economists' interest in the topic stems from theoretical work proposing a "quantity-quality trade-off" in parental decisions on family size (e.g. Becker and Lewis 1973, or Becker and Tomes 1976). In order to increase the quantity of children, these theories suggest, parents are forced to decrease the investments in their children, given the family budget constraint, which leads to lower "quality" of the offspring.[1]

Needless to say, the potential trade-off differs depending on economic circumstances. In developing countries with fertility rates of about six births per woman, malnutrition may be a consequence of sibship size, which could affect long-term economic outcomes. In industrialized countries with fertility rates between one and two, nutrition is in most cases not the issue. Still, parents in richer countries act under a budget constraint (at least in terms of hours available), which may decrease the resources available for each child as family size increases. Even though the effects of family size may work through different

[1] The original model considers parental investments in their children as being subject to financial constraints. The model has later been extended to take into account time constraints (Lundholm and Ohlsson 2002). Models of spillover effects have also been used to explain the observed negative relationship between family size and children's attainments (e.g. Zanjonc 1976). In short, these models suggest that adding siblings decreases the average human capital level within the family because young children do not have the same intellectual level as older family members. The hypothesis is that this will hurt the outcomes of children from large families.

mechanisms in different parts of the world, the basic theories suggest there to be universal signs of the trade-off. The empirical literature also provides strong evidence of a negative *correlation* between family size and e.g. educational attainment or earnings (e.g. Björklund et al 2004, Hanushek 1992, or Holmlund 1988). These findings and the theoretical predictions have been used as arguments for introducing policies aimed at restraining family size in several developing countries.

Still, it is not hard to come up with explanations as to why the effects may actually go in the other direction. Children may stabilize marriages or keep parents at home, which some presume to be beneficial for the upbringing of children. One could also argue that siblings act as role models or inspire each other to progress at school or in other arenas.

The net effects of family size must therefore be determined empirically, and as already mentioned, recent work questions the conclusions of the previous studies. The first objection is methodological: the observed correlation may not reflect causation. For instance, parents with preferences for small families might also be the ones who emphasize education and labor market success for their children. The second objection concerns the quality of data used: most studies are plagued by problems generated by small and often unrepresentative samples, and/or by poor child-parent match rates, making the estimates both imprecise and less reliable.

We use detailed Swedish population micro data covering entire birth cohorts 1972–87 (1,696,228 individuals) and twin births to address both of these problems. Because twin births essentially are randomly determined they provide an exogenous source of variation in family size that can be used to distinguish causation from correlation.

We have access to data from administrative records on a wide range of educational and labor market outcomes: grades in all subjects ever taken, GPA in compulsory and secondary school, transitions to higher education, highest degree attained, years of schooling, earnings, employment status, welfare dependence etc. We document effects through the educational system and then later in the labor market. Also, there is rich information on parental characteristics that makes it possible for us to directly investigate whether the effect of family size is stronger for parents with limited resources, as suggested by the seminal work by Becker and others.

Our paper is related to other studies using twin births as an instrument for family size. Rosenzweig and Wolpin (1980) were the first to employ this identification strategy. The study by Black et al. (2005) uses the twin birth strategy on extensive population micro data from Norway to investigate the effects of family size. Their findings suggest that there is no effect of family size on the amount of education completed. A similar conclusion is drawn in Angrist et al. (2006) who combine several instrumentation strategies in a study of Israeli data. The authors state that the results are “remarkably stable in showing no evidence of a quantity-quality trade-off”. [2]

The answer to the fundamental question of whether family size affects children’s outcomes must, however, still be considered open to debate. Black et al (2007a) find substantial negative effects of sibship size on young men’s IQ in Norway. Qian (2006) argues that the family size effect on school enrolment varies with birth order in China, Caceres (2006) finds inconclusive evidence on a number of outcomes in the US, whereas Rosenzweig and Zhang (2006) find negative effects on parental investments in education in China.

Similar to Black et al (2005) and Angrist et al (2006) we find no effect of family size on long-term educational attainment or labor market outcomes. The analysis also shows that one risks overstating the impact of family size unless endogeneity is handled; OLS estimations suggest a substantial correlation between sibship size and all the outcomes considered. There is, however, evidence that family size affects intermediate outcomes. For example, in families with at least three children, we find that having one more sibling lowers GPA in both compulsory school and secondary school with on average 1–2 percentiles. Taken together, the evidence presented in this paper suggests that family size only plays a minor role in determining children’s outcomes although we show that the effects on intermediate outcomes are larger in low-educated families and that the impact tends to increase with family size and with birth order.

The rest of this paper is structured as follows. In section 2 we discuss data and the institutional background concerning Sweden’s educational system and family policies. Section 3 presents our empirical strategy. Estimation results can be found in section 4. Concluding remarks are given in section 5.

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[2] Another instrument that has been used in recent studies is sibling sex composition (e.g. Lee 2006, or Conley and Glauber 2006) The argument for this approach is that parental preferences for mixed sex of their children encourage parents to have another child if their preferences are not satisfied at the latest attempt. However, the instrument has been criticized since research has shown that sex composition may have a direct effect on child outcomes (e.g. Butcher and Case 1994).

2 Data

Our data come from the IFAU database, which is based on register information from Statistics Sweden on the entire Swedish population age 16–65 during the period 1985–2004.[3] One part of the database includes annual information on standard individual characteristics (earnings, place of residence, etc). It also contains several registers with educational information, as well as a “multi-generation” register linking kids to their biological parents and thereby to their siblings.[4] Below we describe the sampling strategy and the information used.

The main sample consists of all individuals born in the years 1972–87. This means that we have information on 16 cohorts containing a total of 1,696,228 individuals. As described below, we use various subsamples of these individuals in the empirical analysis. The reason for choosing these cohorts is that we can observe their final grades in compulsory school; educational registers cover the period 1988–2004 and people typically graduate at age 16. Individuals who are not alive or not living in Sweden at age 16 are not included in the data.

We link each of these individuals to their biological parents and siblings through a unique parental identification number. We use the mother to link siblings to each other, but also connect each child to his/her biological father. In the register it is possible to observe the mother's total number of children up to and including 2004. Considering the age restrictions imposed on our sample it is likely that the observed number of children in 2004 is also the completed family size. The register contains information on year and month of birth, which makes it possible to identify twins. We also have information on the exact birth order of each child. It is important to note that the information on birth order and number of children is not conditional on having found the siblings in the other parts of the dataset (restricted to the population age 16–65 in the years 1985–2004). This information is directly recorded for each mother. Thus, we avoid the problem of poor match rates inherent in many previous studies.

For all cohorts we can observe most individuals leaving compulsory school. As we move up the education system (graduation from secondary school, uni-

[3] All registers are not available in all years, as discussed below. *Table A 1* presents all variables and which primary register they are taken from.

[4] The database also includes matched employer-employee data for all workers and workplaces, as well as detailed information on unemployment and labor market programs. These data are, however, not used in this study.

versity enrolment and graduation), we are able to observe fewer and fewer cohorts. In some applications we will therefore focus on the older cohorts in the sample. We prefer using the full set of cohorts in our main analysis when possible because this allows us to fully utilize the richness of our data. As described later, the results for the short-term outcomes are not sensitive to the cohort composition of the estimation sample. For labor market outcomes we use information from the national employment register in 2004.

Our instrument is a dummy variable set to unity for twin births at the nth birth (n={2,3,4}) and zero otherwise.[5] We restrict the sample to families with at least n births and study the outcomes of children born before the nth birth. Separate estimations are thus performed for kids from families with (potential) twin births at the second, third, and fourth birth respectively. We use twins only to construct the instrument and exclude all twins from the empirical analysis. The reason for not studying the outcomes of these children is that twin births are often premature resulting in e.g. low birth weight, which is known to affect children later in life (e.g. Black et al 2007b).

Parental variables can first be measured in 1985, and then annually through 2004. We measure parents' completed level of education in 1991. The reason for not using earlier years is to make sure that education is completed.[6] We use the panel nature of our data to create measures of parental "permanent" income calculated as annual earnings (measured in 1985 years prices) averaged over the observation years. This variable is defined both separately for each parent and combined as family permanent income. Having measures of permanent income is important because current income has been shown to be a poor proxy of life-time income, especially at young ages (e.g. Böhlmark and Lindquist 2006, or Haider and Solon 2006), and we believe that permanent income better captures parents' ability to invest in their children. Note, though, that we do not condition on parental earnings in the main analysis, but use it to investigate the potentially heterogeneous effects of family size and to check whether parental characteristics are related to twin births.

About 95.5 percent of the mothers are present in the data starting in 1991. For fathers, the corresponding figure is 92 percent. A parent is missing in the

[5] Triplets and quadruplets are excluded from the analysis because they constitute extremely rare and unusual events.
[6] As mentioned in section 4.2 our results are not sensitive to the inclusion of this variable.

registers if the parent was older than 65, was deceased, or had emigrated in 1991. We include these parents and control for missing data in the regressions.[7] *Table 1* displays the distribution of family sizes (number of children) for all mothers who gave birth at least once from 1972 through 1987. We see that somewhat more than half of the mothers give birth to one or two children, whereas having more than five births is quite uncommon.

Table 1 Distribution of mother's number of children

Number of children	Number of observations	Percentage	Cumulative distribution
1	126,496	12.92	12.92
2	439,308	44.86	57.78
3	278,103	28.40	86.18
4	91,912	9.39	95.57
5	27,627	2.82	98.39
6	9,383	0.96	99.35
7	3,585	0.37	99.72
8	1,535	0.16	99.88
9	666	0.07	99.95
≥ 10	602	0.05	100
Total:	979,217	100	

Table 2 gives some descriptive statistics on the children included in the estimations. The first two columns show means and standard deviations for first-born in families with two or more children. We see that the average child in this sample has about 13 years of schooling, and that as much as 90 percent has a high school degree. The university enrolment rate of 47 percent further signals that this is not a completely representative sample of Swedish youth.[8] Educational attainment is relatively high, which is not so surprising given that first-born typically perform better than other children (see e.g. Black et al 2005). This is also clear when we compare the three samples. All measures of educational attainment decrease as we go from sample (i) to (iii): GPAs are lower, fewer graduate from high school and go on to university, and the total amount of schooling is lower in samples where family size and average birth order is higher. Similar patterns are also visible for labor market outcomes. Not

[7] Note that we have complete information on demographic characteristics for all parents and children (e.g. number of children and year of birth) from the multi-generation register. Thus, missing data is only an issue for the information on parents' socioeconomic status.

[8] Further details on our measures of educational attainment are given below in the description of the institutional background and in Table A.1 presenting the contents of the variables.

surprisingly, the mothers of many children are also less educated on average, which also seems to be true for the fathers.

Table 2 Summary statistics for samples used in the analysis

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
	(1) Mean	(2) Std. dev.	(3) Mean	(4) Std. dev.	(5) Mean	(6) Std. dev.
Individual characteristics						
GPA compulsory school	51.70	28.66	47.94	28.92	42.20	28.86
Graduated sec. school (72–84)	.90	.30	.87	.33	.82	.39
GPA secondary school (72–84)	51.70	28.99	49.75	29.02	46.34	29.10
Years of schooling (72–79)	12.90	2.10	12.63	2.12	12.19	2.13
Enrolled in university (72–79)	.47	.50	.42	.49	.34	.47
Welfare dependence (72–79)	.06	.23	.07	.25	.10	.30
log(earnings)	7.20	1.16	7.14	1.17	7.06	1.21
Non-employed (72–79)	.21	.41	.23	.42	.26	.44
Female	.49	.50	.48	.50	.49	.50
Age (in 2004)	24.64	4.74	24.29	4.66	24.04	4.59
Mother's characteristics						
Age (in 2004)	49.31	5.71	49.25	5.64	49.37	5.93
Education: Compulsory school	.21	.41	.25	.43	.34	.47
High school ≤ 2 years	.41	.49	.40	.49	.39	.49
High school >2 years	.09	.29	.08	.27	.07	.25
University ≤ 2 years	.16	.37	.15	.36	.11	.32
University >2 years	.12	.33	.12	.32	.09	.29
Father's characteristics						
Age (in 2004)	52.25	6.11	52.24	6.05	52.54	6.41
Education: Compulsory school	.27	.44	.29	.45	.34	.47
High school ≤ 2 years	.33	.47	.33	.47	.34	.47
High school >2 years	.14	.35	.13	.33	.11	.32
University ≤ 2 years	.11	.32	.10	.31	.09	.28
University >2 years	.15	.36	.15	.36	.12	.33
Family permanent income (in 1985 years prices)	193,116	101,059	179,181	100,893	150,664	95,891
Observations 72–87 cohorts		568,701		484,747		206,984
Observations 72–84 cohorts		457,827		384,831		162,608
Observations 72–79 cohorts		291,467		232,495		93,463
Pr(twins at nth birth)		.009		.010		.010

Notes: Summary statistics is based on the full sample (1972–87 cohorts) if not indicated in parenthesis. Summary statistics for parents' education and income is conditional on having found the parent in the employment register. For a description of the variables, see *Table A 1*.

2.1 Institutional background

2.1.1 Sweden's educational system

This brief description of the Swedish schooling system draws primarily on Björklund et al (2005). We refer to that publication for further details on the education system in general, and for information on the reforms that took place in the 1990s.[9] For the cohorts considered here, practically everybody started their nine years of compulsory education at age 7, and followed a common curriculum determined by the central government. After the 9th grade, a vast majority moved on to upper-secondary education. In the mid-1980s, the transition rate was about 80 percent, but grew to as much as 97–98 percent in the mid-1990s (Landell, Gustafsson and Grannas 2000). The transition is still, however, voluntary, and also includes a choice between a number of vocational training programs on the one hand, and on the other a collection of programs preparing for further studies. Over time, the vocational programs have been reformed so to give eligibility for pursuing higher education. This involved a gradual change from two-year to three-year programs (which was the length of the preparatory programs throughout the observation period). In practice, however, university enrolment is still low after completion of the vocational programs. Furthermore, the possibility of "correcting" one's choice by adding grades for specific subjects was present for all the cohorts considered here.

After finishing upper-secondary school—typically at age 19—an increasing number of youth move on to college/university, although many times not immediately following graduation. Swedish universities are with few exceptions public, and there is a centralized admission system. There is of course heterogeneity in terms of the length of the university studies, both because programs differ and because students take additional programs/courses to a varying extent. A typical program leading to a Master's degree lasts 4–5 years.

Let us now describe the grading systems in compulsory and secondary school in somewhat more detail. Those in our sample who finished school in 1996 or before were given grades on a scale from 1 to 5, where 5 was the highest. These grades were "relative" so that the national average for each cohort was to

[9] Note that throughout the empirical analysis we include cohort fixed effects to capture effects of changes in the educational system (as well as other variations over time).

be 3.0.[10] The GPA used here is simply the mean of the individual's grades, rounded to one decimal. Since nobody has an average below 1, we have 40 steps in the GPA for these years.

After 1996 the grading system was replaced by an "absolute" scale, where each subject could give one of the following points: 0 (fail), 10 (pass), 15 (pass with distinction), or 20 (pass with special distinction). The compulsory school GPA is then computed as the sum of the best 16 grades. The maximum score in the compulsory school GPA is 320, and the lowest score observed is 0. There are 80 steps in the observed distribution of the compulsory school GPA. The secondary school GPA weights the subjects by the length of the courses taken, so that a long course affects the GPA more than a short course.

There are obviously institutional differences in the grading system over time, and there has also been a debate on increasing grade inflation in the new system. To make cohorts comparable we therefore: (i) use the by-cohort percentile ranking of the individual grade; (ii) include cohort dummies in all estimations.

Looking at the GPA variables in *Table 2* we see that, on average, the percentile rank of the compulsory school GPA for a first-born child in families with two or more children is about 51 while being as low as 42 for children in families with at least four children. There is a similar pattern for secondary school GPA.

2.1.2 Family policy in Sweden

One could argue that Sweden is not the first place to look for trade-off effects on children. The welfare state encompasses a number of measures to assist children and their parents; from health care, via child care, to financial aid (see Björklund 2006 for details). Health care is free for all children, and until school start kids attend regular check-ups to monitor health and the development of physical and psychological skills. There is also a (more or less) mandatory vaccination program. Schools then take over the responsibility for following the children during their adolescence.

Public child care was rapidly expanded during the 1970s. Compulsory pre-school from age 6 had been implemented nationally by the late 1970s. An increasing majority of the children attend child care at a much younger age

[10] In practice, the national average may vary slightly across cohorts since grades were not synchronized.

than 6; local governments are obliged to provide care to cover the time the parents spend on market work, job search or studies. Child care is heavily subsidized, and the fees are means-tested. Dismissal due to pregnancy, delivery or marriage has been illegal sine 1939, and since 1979 parents have the right to reduce work hours to 75 percent. There is also a flat rate child allowance, which is not means-tested. The amount has been changed over the years, and since 1982 there is a bigger allowance for the third child and beyond.

In 1975 Sweden introduced legalized abortion. This could be a concern for our study. If there are selective abortions due to twin pregnancies, the instrument may be invalid.[11] However, selective abortions are extremely rare in Sweden, and it was probably even more uncommon in the late 1970s and 1980s, which is when most of the siblings of the sampled individuals were born.[12] Therefore we do not believe that selective abortions constitute a problem for our analysis.

Another concern is the use of fertility treatments, which can increase the probability of twin births. If parents consider this possibility in decisions on whether to use the drug or not, there may be a selection problem in twin births similar to that in family size in general. Frequent use of fertility drugs and assisted conceptions is a quite recent phenomenon. For example, the first successful assisted conception in Sweden took place in 1982. For many of the cohorts considered it was not an issue at all, for others it is likely to be of minor importance. As will be discussed below, the results do not change if we only look at the earlier cohorts.

3 Empirical strategy

The aim of this paper is to study the effects of family size on educational and labor market outcomes. We use the incidence of twin births as an exogenous source of variation in fertility. More specifically, we follow previous work (Black et al 2005, Angrist et al 2006) and study the older siblings, meaning that we compare e.g. first-born from families where the second birth was a twin

[11] A selective abortion is defined as one where the pregnancy is wanted and the motive for having an abortion is that the fetal is believed to have some unwanted characteristics. This is opposed to a general abortion where the motive is not the fetus but rather the pregnancy in itself.

[12] In 1999, 31,000 abortions were performed, out of which only 375 were classified as selective. Virtually all of these were performed due to illnesses or defects of the fetus.

birth to first-born from families where the second birth was a singleton. The advantage of this approach is that we avoid the potential problem that parents who choose to have another child after the occurrence of the twin birth possibly represents a selected sample. Also, restricting the sample to families with at least n births ensures that, ex ante, preferences for family size in families experiencing a twin birth or a singleton at the nth birth are the same.

To see the problems associated with estimating the causal effect of family size on child outcomes, consider the following regression model

$$Y_i = \gamma_0 + S_i\gamma_1 + \mathbf{P_i}'\boldsymbol{\gamma}_2 + \mathbf{X_i}'\boldsymbol{\gamma}_3 + u_i \tag{1}$$

where Y_i is some measure of human capital indexed for individual *i*; S_i denotes family size; $\mathbf{P_i}$ is a vector of parental characteristics; $\mathbf{X_i}$ is a vector of individual characteristics; u_i is an individual specific error term. Equation (1) represents the standard model that has been used in previous literature (see e.g. Guo and VanWey 1999). Typically, these studies conclude that family size is negatively related to several outcomes (education, earnings, teen pregnancies etc).

The main concern with this model is that family size may be correlated with the error term, i.e. $\mathrm{E}[S_i u_i] \neq 0$. This is the case if parents make their decision on family size based on unobserved characteristics that also affect their children's outcomes. For instance, parents with low resources in some (unobserved) dimension might choose to have large families and also invest less in their children. If a negative shock, like unemployment, increases the likelihood of having another child (to feel needed or to qualify for economic benefits) and at the same time affects the outcomes of the children, we have a similar problem. Correlation between family size and the error term will render $\hat{\gamma}_1^{OLS}$ inconsistent.

Another potential source of bias is from simultaneity. Parents might adjust their perceptions of the optimal number of children depending on the quality of previous children. If their last child is of high quality, parents may feel no need to have another child, and vice versa (Behrman and Taubman 1986). One can also imagine an opposite situation where parents have babies until they find that they are unable to devote as much resources to the last one as they wish; Black et al (2005) interpret their finding of a "last child" effect in this way.

To deal with these problems our strategy is to use twin births as an instrument for family size. Given that twin births are determined by nature—and unrelated to parental characteristics—our estimates are arguably free from bias originating from omitted variables and simultaneity.

The first-stage in our 2SLS model can be written as

$$S_i = \pi_0 + T_i\pi_1 + \mathbf{P_i}'\pi_2 + \mathbf{X_i}'\pi_3 + v_i \tag{2}$$

The instrument denoted by T_i is a dummy variable set to unity for the nth birth being twin and zero otherwise.

Of course, for this approach to make sense, twin births must be correlated with family size, i.e. $\mathrm{E}[T_i S_i] \neq 0$. Furthermore, the standard exclusion restriction must hold: the instrument must not have an independent effect on the outcome, and must not be correlated with any unobserved factors affecting the outcome. Our data allow us to shed some light on this last issue for a sub sample of parents.

Table 3 reports results from linear probability models where we regress the instrument on parental characteristics. To make sure that education is completed, and thus not affected by twin births, we here measure education in 1985 and restrict the sample to parents born before 1961. We study parents who experienced their nth birth later than 1985. It is well-known that the probability of twinning increases with the mother’s age; this is confirmed in a separate analysis (available upon request), which emphasizes the need to control for the mother’s age when giving birth in the regressions.

As can be seen in *Table 3*, parental socioeconomic status is not found to be correlated with the instrument. This is expected, since twin births are essentially randomly determined. The fact that observed characteristics are not related to the probability of having a twin birth supports the assumption that neither are there unobserved characteristics influencing this probability.[13]

[13] Remember that unobserved variables affecting twin births are only a problem if they are also related to the outcome variable, and if this correlation is not captured by the covariates included in the model.

Table 3 Correlation between twin births and parental characteristics

Instrument:	(1) Pr(Twins at 2nd birth)		(2) Pr(Twins at 3rd birth)		(3) Pr(Twins at 4th birth)	
	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
Mother's characteristics						
log(permanent income)	–.0005	.0004	–.0007	.0004	–.0006	.0006
Compulsory school	Ref.		Ref.		Ref.	
High school ≤ 2 years	–.0015	.0011	–.0002	.0011	–.0020	.0016
High school >2 years	–.0025	.0014	–.0015	.0015	.0000	.0027
University ≤ 2 years	–.0013	.0013	.0021	.0014	–.0033	.0020
University >2 years	–.0016	.0015	.0002	.0016	–.0017	.0026
Father's characteristics						
log(permanent income)	.0002	.0003	–.0005	.0004	–.0003	.0005
Compulsory school	Ref.		Ref.		Ref.	
High school ≤ 2 years	–.0007	.0010	–.0015	.0011	–.0008	.0017
High school >2 years	–.0009	.0012	–.0019	.0014	–.0035	.0020
University ≤ 2 years	–.0002	.0013	–.0015	.0015	.0003	.0024
University >2 years	–.0012	.0012	–.0013	.0014	.0008	.0022
Number of observations		105,022		90,129		33,854

Notes: The table reports estimates, together with robust standard errors, from regressions of dummies for twin births (at the nth birth) on parental characteristics. Each column represents a separate regression. Education is measured in 1985. The sample restricted to parents born before 1961 who experienced their nth birth (conditional on having at least n children) later than 1985. All regressions include fixed effects for birth cohort, year of the potential twin birth, and missing value on education. For definitions of the variables, see *Table A 1*. Value of F-statistic [p-value] corresponding to the null hypothesis that the coefficients on mother's {father's} characteristics are jointly equal to zero: column (1) 1.13 [0.34] {0.29 [0.92]}; column (2) 1.83 [0.11] {0.98 [0.43]}; column (3) 1.05 [0.39] {0.78 [0.57]}.

The second potential problem is harder to disregard: having younger siblings who are twins may affect you through other ways than the mere increase in family size. Some studies have shown evidence of a correlation between birth-spacing and children's attainments (e.g. Petterson Lidbom and Skogman Thoursie 2007). If this is the case, then twin births potentially affect older siblings through its effect on spacing. Also, twins have lower average birth weight, and may therefore require more of the family's resources than other kids (Rosenzweig and Zhang 2006). One way to investigate whether variation in family size given by multiple births is equivalent to variation coming from other sources is to ask whether there are effects beyond the increase immediately caused by the twin birth. *Figure A 1* provides an indication on the existence of such effects: e.g. the probability of having four children is higher among mothers experiencing twin births at the second birth. As noted by Angrist et al (2006), this could be explained by the fact that a twin birth effectively increases the available time for child-bearing.

What is, however, appealing about the twin strategy is that the reduced form—i.e. the impact of a twin birth on the outcomes of older siblings—is in itself interesting to estimate since it carries some policy relevance. If older siblings are affected, policy makers may want devote special attention to older siblings in families who for some reason have one more child than planned, or who have younger children with extra needs.

4 Results

In this section we present the results from our empirical analysis of the impact of family size on child outcomes. Section 4.1 presents the main results using twin births as an instrument for family size. Section 4.2 provides results from robustness checks. In section 4.3 we study differential effects. Last, in section 4.4 we use sibling sex composition as an alternative instrument for family size and compare with our previous results.

Before proceeding to the analysis of the *causal impact* of family size, let us look at *Figure 1* showing *correlations* between sibship size and educational and labor market outcomes. The graphs are based on regressions of the respective outcome variable on a set of dummies for the number of children in the family. The reference group is children from one-child families. The differences in outcomes are quite small when the number of children is in the order of 2–3. We can see a small positive effect for children from two-child families but no effect for children from three-child families. For larger families there is however a sharp decline in the average outcomes. Kids with four brothers and/or sisters have as much as ten percentiles lower GPA in compulsory school, a full year less of schooling, and earn about 15 percentage points less compared to single kids.

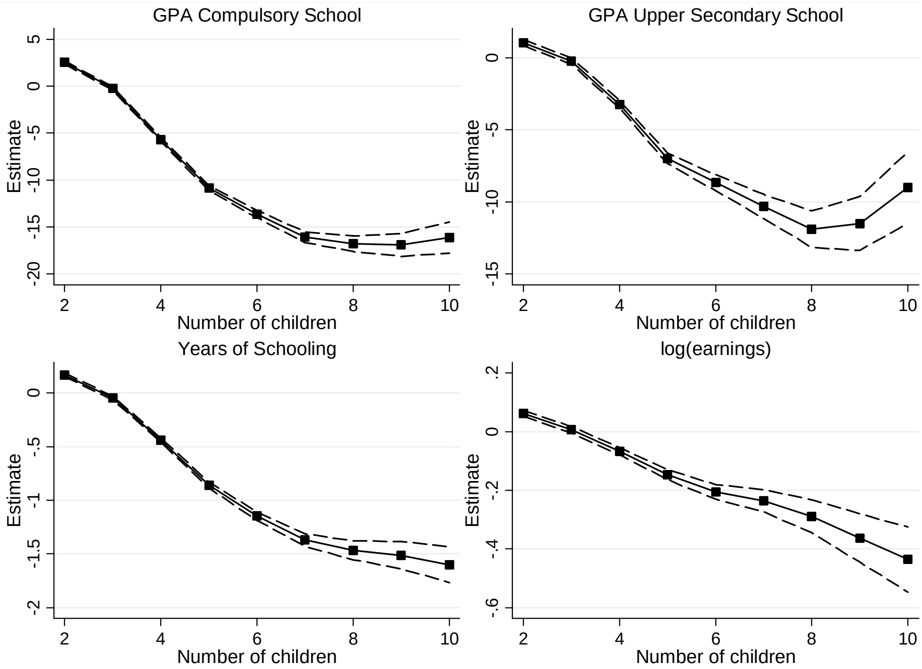

Figure 1 Correlation between family size (number of children) and various education and labor market outcomes

Notes: The graphs are based on regressions of the respective individual outcome variable on a set of dummies for the number of children in the family. Dashed lines represent 95% confidence intervals. No other covariates are included in the regressions. The omitted category is children from one-child families.

These findings are very much in line with the results presented in Black et al (2005), who also demonstrate that much/all of the correlations are due to the performance of the younger children in large families, i.e. a birth order effect.[14] Our point here, however, is to demonstrate that it is very easy to jump to conclusions regarding the effects of family size, considering the substantial differences visible in the data. As discussed above, however, several papers have found that this pattern may not reflect a causal relationship. In the following sections, we shed some further light on this issue.

[14] It is worth noting that the "effects of sibship size" consider the impact on a given individual. Provided that there are birth order effects, increases in family size means a change in *average* child quality in the family, even though the outcomes of the individuals are unaffected.

4.1 The baseline results

As described above, our point of departure is to use twin births as an exogenous shock affecting family size. We have already the discussed the first condition for this approach to be useful: the exogeneity (randomness) of twin births with respect to the potential outcomes of the older siblings. The second condition is of course that twin births affect family size, i.e. that the first stage regressions of the 2SLS models have explanatory power. As is evident from *Table 4*, this is clearly the case. Having a twin birth at the second birth increases family size by about 0.75 children. For twin births at higher birth-orders, the effects are even bigger. One could imagine different mechanisms behind this effect. Obviously, for many parents having twins at the second, third or fourth birth directly means one more child than planned. If there are other parents whose preferences are not so much concerning the number of children, but rather on having children during a sequence of years, these parents may still opt to have kids after the twin birth, even if this results in a larger offspring than what they originally planned for.

To judge the strength of the instrument *Table 4* displays values of an F-statistic corresponding to the null hypothesis that the coefficient on the instrument in the first stage regression is zero. As seen in *Table 4*, the F-statistic takes on values in the order of 886–7,510, suggesting that weak instruments are not a concern.[15]

[15] These values are considerably larger than the values suggested by Staiger and Stock (1997) as being the lower limits that ensures that weak instruments cause no major problem.

Table 4 Family size explained by twin births

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
Outcome	Estimate	F-statistic	Estimate	F-statistic	Estimate	F-statistic
GPA comp. school	.752	7,510	.811	5,883	.804	1,867
Graduated sec. school	.771	5,104	.855	4,268	.792	1,258
GPA sec. school	.768	5,383	.845	5,465	.784	1,851
Enrolled in university	.779	3,458	.872	2,598	.778	886
Years of schooling	.780	3,419	.871	2,629	.780	870
Unemployed	.779	3,419	.872	2,597	.780	872
log(earnings)	.778	3,242	.866	2,794	.777	950
Welfare	.779	3,458	.872	2,598	.778	886

Notes: The table displays first stage estimates by outcome. The F-statistic corresponds to the null hypothesis that the coefficient on the instrument (twin births) is zero. The sample consists of children born 1972–87. For the outcomes: *GPA secondary school* and *Graduated secondary school* the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

We now turn to the baseline estimations. *Table 5* presents results for an array of outcomes, using different models for different samples. Note that each cell in the table represents a unique regression. The models include fixed effects for birth order[16], gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth (i.e. the potential twin birth), parental education (5 levels), and for missing parental data. Given the number of estimates included in the table we do not show the coefficients for other covariates—full results are available upon request. Let us, though, mention that these estimates show an expected and stable pattern: females perform better than males in school, highly educated parents mean better outcomes, and higher birth order implies worse outcomes.

The first row of results in panel A is for GPA in compulsory school. As we go down the table, the dependent variables become more long-term, ending in

[16] While birth order effects are indeed interesting, we choose to focus solely on family size in the presentation. One reason for this is that there appears to be less uncertainty regarding the effects of birth order (e.g. Black et al 2005, or Booth and Kee 2005), another is too avoid an exceedingly long paper.

the panel B using labor market outcomes in 2004. There are three samples used in this analysis, all constructed in a similar way: we study effects on the n–1 first siblings in families with at least n births, using twin births at the nth birth as an instrument for family size. In other words: In the first sample we include first-born in families with at least two children, where the instrument is whether the second birth was a twin birth or not. For each sample there are three sets of estimates: OLS, Reduced form (RF) and 2SLS. In the OLS models we simply include family size among the regressors. These estimates are not to be interpreted as causal, but it is worth noting that they do not necessarily capture the same type of selection effects as the estimates underlying the results presented in *Figure 1*. The reason is that we now focus on a sample that is much more homogenous in terms of family size, and thereby presumably also regarding e.g. parental preferences. In the reduced form models, the twin births dummy is included directly among the regressors, and in the 2SLS models it is used as an instrument for family size. Provided that the underlying assumptions hold, these two models capture a causal link between the regressors and the dependent variables.

The OLS estimates consistently show a negative correlation between sibship size and outcomes: grades are lower, transitions to higher education less frequent, years of schooling fewer, non-employment more prevalent, earnings lower and welfare dependence more common. To get to the causal estimates, assume for now that the only reason that a twin birth influences the outcomes of older siblings is that it increases family size, which says that the 2SLS estimates are the ones to focus on. It then seems that the effects are larger when the twin birth occurs at higher parities (samples ii and iii). For these samples, we find that one more sibling lowers compulsory as well as secondary school GPA by about 1–2 percentiles.[17] We do not find any effects of family size in sample (i). Family size does not seem to affect either the probabilities of enrolling in university or graduating from high school, nor does it seem to affect years of schooling. However, there is a slight tendency that the effect is stronger in sample (iii) (although the estimates are not statistically significant). Furthermore, there is no evidence of an impact on the probability of non-employment, on welfare dependence, or on earnings.

[17] It is somewhat notable that the OLS estimates suggest a smaller impact on GPA than the 2SLS estimates do. Black et al (2007a) find a similar pattern for IQ among Norwegian men.

Despite the very well-established theory on the impact of family size, one could argue that there are also other things going on in the families experiencing twin births. There is little doubt that *one* consequence of having twins is that family size increases. But it also means closer spacing of the offspring, which could mean harder restrictions on the families' resources, but also potentially economies of scale in e.g. homework assistance. Twins are also different in the sense that they can be expected to generate—but also divert—attention. In other words: it is quite possible that there are several mechanisms at work here, all of which reflect circumstances during childhood. Believers in this hypothesis would argue that the reduced form estimates are the ones illuminating the causal effect of interest. As is clear from *Table 5*, the impression does not differ very much whether we look at the reduced form or at the 2SLS estimates. Typically, the 2SLS estimates for the GPA outcomes are slightly larger in magnitude than the reduced form estimates. A high degree of similarity is also expected given the strong first stage estimates.

Table 5 OLS, Reduced Form (RF), and 2SLS estimates of the relationship between family size and child outcomes

Sample:	(i) First child in families with at least two births			(ii) First two children in families with at least three births			(iii) First three children in families with at least four births		
	(1) OLS	(2) RF	(3) 2SLS	(4) OLS	(5) RF	(6) 2SLS	(7) OLS	(8) RF	(9) 2SLS
Panel A. Educational outcomes									
GPA compulsory school	–1.314	–.043	–.058	–1.205	–.963	–1.187	–.884	–1.258	–1.563
	(.043)	(.345)	(.458)	(.055)	(.343)	(.479)	(.087)	(.647)	(.803)
Graduated secondary school	–.020	.007	.009	–.023	–.001	–.001	–.018	–.015	–.019
	(.001)	(.004)	(.006)	(.001)	(.005)	(.006)	(.001)	(.010)	(.013)
GPA secondary school	–.606	.445	.580	–.885	–1.208	–1.429	–.459	–1.798	–2.295
	(.058)	(.458)	(.596)	(.077)	(.504)	(.595)	(.120)	(.855)	(1.089)
Enrolled in university	–.013	.004	.005	–.016	–.003	–.003	–.012	–.014	–.018
	(.001)	(.009)	(.011)	(.001)	(.009)	(.011)	(.002)	(.014)	(.019)
Years of schooling	–.113	.017	.022	–.132	–.038	–.043	–.100	–.033	–.042
	(.004)	(.038)	(.048)	(.006)	(.042)	(.048)	(.009)	(.065)	(.083)
Panel B. Labor market outcomes									
Non-employment	.019	–.005	–.007	.023	.000	.000	.019	–.026	–.033
	(.001)	(.009)	(.010)	(.001)	(.009)	(.010)	(.002)	(.014)	(.018)
log(earnings)	–.049	.034	.044	–.056	–.022	–.025	–.039	.016	.021
	(.003)	(.024)	(.030)	(.004)	(.026)	(.030)	(.006)	(.041)	(.053)
Welfare dependence	.016	.001	.001	.020	.004	.005	.018	–.001	–.001
	(.001)	(.005)	(.006)	(.001)	(.005)	(.006)	(.002)	(.010)	(.013)

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions. The sample consists of children born 1972–87. For the outcomes: *GPA secondary school* and *Graduated secondary school* the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. The instrument is a dummy for twin births at the nth birth. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

4.2 Robustness checks

We have performed a number of robustness checks to investigate whether our results are sensitive to changes in sample composition or to the choice of covariates. Due to the large number of estimates involved in this exercise we do not report the results but provide a discussion of the most important findings.[18]

There is some evidence in the literature that the probability of having twin births differs across ethnicities (Myrianthopoulos 1970). This might be a concern since ethnicity is likely to be correlated with the error term in the outcome equations. To deal with this issue we re-estimated our models including fixed effects for mothers' region of birth (27 strata aggregated by Statistics Sweden). The estimates are not sensitive to this inclusion.[19]

To investigate whether our results for the GPA outcomes are driven by the later cohorts (cf. Black et al 2007a), and out of concern that the increasing use of fertility treatments may influence our estimates, we restricted the sample to children born 1972–79. Of course, twinning may occur later but our finding that the estimates are stable makes it less likely that our results are driven by the introduction of fertility treatments.[20]

Previous we showed that parental characteristics are uncorrelated to the probability of having twins. One can however never be too sure that omitted variables are not causing bias. If observed variables are at least equally as important as unobserved variables dropping the former can provide information on whether or not the estimates are likely to be driven by confounders. We examined this by estimating models where we dropped the covariates for parental education. We found that the estimates are practically invariant, which strengthens our belief that omitted variables are not a problem.

Because we observe family size in 2004 it is possible that our estimates are not capturing the impact of twin births on *completed* family size. To examine this we imposed the restriction that the mothers must be at least 40 years old in 2004 (very few mothers in previous cohorts have children after this age). This

[18] All estimates are available upon request.

[19] One interesting variation would be to estimate separate models for children with foreign-born mothers. However, the number of twin births would be too low get reasonable precision in the estimates.

[20] One would expect that the use of fertility treatment causes a positive bias to the estimates since parents who have twins because of fertility treatment are likely to be better prepared and more planning than other parents.

restriction does not affect our estimates. Also, excluding very large families (>6 children) does not change our estimates.

4.2.1 Alternative intermediate outcomes

Since *Table 5* suggests that there are some effects on GPA, we also experimented with some other intermediate outcomes. The first two rows of results in *Table 6* show estimates for the probability of delaying graduation from compulsory and upper secondary school respectively. Arguably, delayed graduation is an indication of rather severe problems, and it is not so surprising that we see no impact from family size. Somewhat in contrast, the third row suggests a tendency to negative effects of sibship size on the probability to graduate from a preparatory high school program (as opposed to a vocational). Now, the effect is only statistically significant in sample (i) (where we, admittedly, found no impact on grades), but the point estimate is very similar in sample (iii).

Furthermore, the table shows that the GPA estimates do not change when the percentile ranking is performed by high school program and year (as opposed to year only in *Table 5*). One could also ask whether the impact on grades is bigger in certain parts of the grade distribution. *Table A 5* contains estimates for family size effects on the probability of being above the 1st, 2nd, and 3rd quartile in the GPA distributions. The point estimates are only marginally significant for sample (iii). If we are willing to interpret them anyway, it seems that the impact is larger in relative terms in the upper half of the grade distribution.[21]

If the proposition of parental investments per child being affected by the number of children is correct, one could argue that we would expect bigger grade effects in subjects where parental efforts—e.g. homework assistance—are more likely to matter.

Table 7 presents estimates for grades in specific (groups of) subjects, which give some support to this idea. Family size appears to have no impact on performance in sports, but there are indications on effects on grades in Swedish, science and social science in samples (ii) and (iii).

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[21] Remember that sample mean is approximately 0.75, 0.50 and 0.25 in the Q_1, Q_2 and Q_3 estimations respectively. The relative magnitude of the point estimates is thus higher in the latter specifications.

Table 6 OLS and 2SLS estimates of the relationship between family size and alternative intermediate (instrument: twin births)

Sample:	(i) First child in families with at least two births			(ii) First two children in families with at least three births			(iii) First three children in families with at least four births		
	(1) OLS	(2) 2SLS	(3) Mean	(4) OLS	(5) 2SLS	(6) Mean	(7) OLS	(8) 2SLS	(9) Mean
Delayed compulsory school	.010	–.004	.035	.013	.007	.042	.010	–.005	.064
	(.000)	(.003)		(.001)	(.004)		(.001)	(.006)	
Delayed sec. school	.014	.003	.122	.016	.004	.129	.013	.002	.154
	(.001)	(.007)		(.001)	(.007)		(.002)	(.013)	
Preparatory program	–.017	–.021	.573	–.014	–.002	.518	–.009	–.022	.452
	(.001)	(.010)		(.001)	(.010)		(.002)	(.018)	
GPA by year×program	–.525	.861	51.21	–.831	–1.319	50.23	–.401	–2.190	47.92
	(.061)	(.623)		(.081)	(.623)		(.125)	(1.140)	

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions. For the outcome: *Delayed compulsory school* the sample consists of children born 1972–87. For remaining outcomes the sample is restricted to children born 1972–84. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

Table 7 2SLS estimates of the effect of family size on grades in single subjects/group of subjects in compulsory school using twin births as an instrument for family size

Sample:	(i) First child in families with at least two births	(ii) First two children in families with at least three births	(iii) First three children in families with at least four births
Swedish	–.337	–.700	–1.773
	(.429)	(.429)	(.705)
Science	–.079	–1.411	–1.850
	(.469)	(.470)	(.810)
Social science	–.167	–.630	–1.218
	(.460)	(.456)	(.801)
Sports	.325	.073	–.656
	(.473)	(.478)	(.865)

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions. The sample consists of children born 1972–87. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data. The dependent variable is the percentile rank of the (mean of the) grade(s) of the respective subject(s). Subjects included in *Science* are: physics, chemistry, biology, technology. Subjects included in *Social science* are: social science, history, geography, religion. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

4.3 Differential effects

Having established that our estimates are robust to various sensitivity checks we now proceed by analyzing the impact of family size for various sub samples. We start by stratifying the sample according to gender and parental socio-economic characteristics and continue with birth order effects.

Table 8 displays the results for the GPAs; the (mostly insignificant) results for other outcomes are shown in *Table 2–Table 4*. There is a tendency to bigger effects for boys than for girls for the GPA outcomes. A stronger indication on differential effects appears when the samples are stratified according to parental education. The category "Academic parents" is defined as having at least one parent with at least three years of upper secondary education, which is equivalent to attending a theoretical/preparatory upper secondary program. The point estimates in samples (ii) and (iii) are clearly larger for individuals with low-educated parents. [22]

Parental education may be important because it enables parents to assist their children at school. It may also serve as a proxy for financial resources. The next set of results investigates this possibility further, by splitting the samples by the individual's position in the family permanent income distribution.[23] No clear pattern emerges from this exercise; the estimates are in most cases not significantly different from each other. It therefore seems as if parental education matters due to other channels than providing higher income. This is perhaps not so surprising considering Sweden's compressed earnings distribution and extensive welfare state.

Table 5 suggests that the effects are larger in the samples with larger family size; remember that we find nothing for first-born in the sample consisting of families with at least two children. This raises the question of whether the effects vary with birth order, addressed in *Table 9*. Even though there is not an entirely clear-cut pattern, one can discern a tendency for the effects to be stronger at higher birth orders. GPA tends to be affected for all children in

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[22] Note that we have taken a conservative approach in these regressions and excluded all individuals with *any* parent having missing information on education to avoid misclassification errors. This leads to samples with somewhat fewer observations than those used in the main analysis. An alternative strategy is to classify parents with missing data as non-academic; the estimates from the two approaches are very similar.

[23] Because most databases do not contain information on income for an extended time-period previous work has been unable to look at this question in detail. We restrict the sample to children with both parents present in the data (starting in 1985) and each parent having positive earnings.

samples (ii) and (iii), but for 2nd and 3rd children there are also traces of an impact on the probability to graduate from high school, enroll in university, and on years of schooling.[24]

Table 8 2SLS estimates of the effect of family size on grades in different subpopulations using twin births as an instrument for family size

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
	GPA comp.	GPA sec.	GPA comp.	GPA sec.	GPA comp.	GPA sec.
Estimate as in *Table 5*	–.058	.580	–1.187	–1.429	–1.563	–2.295
	(.458)	(.596)	(.479)	(.595)	(.803)	(1.089)
By gender						
Girl	–.120	–.122	–1.206	–.552	–.691	–1.118
	(.648)	(.815)	(.633)	(.818)	(1.164)	(1.531)
Boy	.083	1.420	–.769	–2.270	–2.201	–3.240
	(.648)	(.870)	(.598)	(.815)	(.945)	(1.399)
By parents' education						
Academic parents	.015	1.366	–.474	–.936	–1.340	–.817
	(.635)	(.806)	(.668)	(.815)	(1.308)	(1.743)
Non-academic parents	–.091	–.324	–1.789	–2.243	–2.102	–3.530
	(.704)	(.921)	(.663)	(.931)	(1.042)	(1.393)
By position in family permanent income distribution						
Lower third	–.181	1.175	–1.278	–2.082	–1.979	.248
	(.880)	(1.224)	(.856)	(1.234)	(1.599)	(2.436)
Middle third	–.272	1.023	–.392	–.636	–1.251	–4.567
	(.825)	(1.033)	(.800)	(1.075)	(1.279)	(1.733)
Upper third	.643	.157	–.915	–1.496	–1.270	–1.767
	(.702)	(.919)	(.710)	(.877)	(1.277)	(1.654)

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions. The sample consists of children born 1972–87. For the outcome: *GPA secondary school* the sample is restricted to children born 1972–84. All regressions include fixed effects for birth order, gender (where appropriate), the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data (where appropriate). For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

[24] Sample size likely contributes to the large standard errors, making some of the estimates insignificant and others only marginally significant.

Table 9 2SLS estimates of the effect of family size on child outcomes by birth order using twin births as an instrument for family size

Outcome:	GPA comp.	GPA sec.	Grad. sec.	Enrolled univ.	Yrs school.	Non-emp.	log (earn.)	Welf. dep.
Families with at least three births								
Estimate as in ***Table 5***	–1.187	–1.429	–.001	–.003	–.043	.000	–.025	.005
	(.479)	(.595)	(.006)	(.011)	(.048)	(.010)	(.030)	(.006)
1st child	–.777	–1.369	.007	.008	.016	–.015	–.061	.009
	(.595)	(.775)	(.008)	(.014)	(.062)	(.013)	(.043)	(.009)
2nd child	–1.586	–1.452	–.009	–.014	–.106	.017	.014	.001
	(.599)	(.793)	(.009)	(.014)	(.061)	(.014)	(.040)	(.008)
Families with at least four births								
Estimate as in *Table 5*	–1.187	–1.429	–.001	–.003	–.043	.000	–.025	.005
	(.479)	(.595)	(.006)	(.011)	(.048)	(.010)	(.030)	(.006)
1st child	–1.183	–2.229	.013	.002	.102	–.004	–.069	–.012
	(1.087)	(1.508)	(.018)	(.026)	(.117)	(.029)	(.087)	(.019)
2nd child	–1.325	–.289	–.039	–.022	–.238	–.025	.035	.025
	(1.139)	(1.703)	(.020)	(.029)	(.138)	(.030)	(.090)	(.022)
3rd child	–2.093	–4.366	–.034	–.040	.009	–.080	.111	–.022
	(1.207)	(1.756)	(.022)	(.032)	(.141)	(.031)	(.096)	(.019)

Notes: Each cell represents the 2SLS coefficient on the *Number of children* variable in unique regressions. The sample consists of children born 1972–87. For the outcomes: *GPA secondary school* and *Graduated secondary school* the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels), and for missing parental data. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

4.4 Using sibling sex composition as an alternative instrument for family size

This paper uses twin births to address the endogeneity problem associated with estimating the relationship between family size and child outcomes. As previously mentioned, sibling sex composition represents an alternative instrument. The idea is that parental preferences for mixed sex of their children encourage parents to have another child if all previous children are of the same sex (e.g. Lee 2006, or Conley and Glauber 2006). Because a child's sex is randomly determined the argument is that gender composition can be used to distinguish causation from correlation. However, for this approach some caveats are warranted. First, several papers have found that sibling sex composition may influence both children (e.g. Butcher and Case 1994) and parents (e.g. Dahl and Moretti 2004, or Johansson 2007) along various socioeconomic dimensions. These findings suggest that sibling sex composition may be an invalid instrument since it may have a direct effect on children's outcomes. Second, if the effect of family size differs across individuals the two instruments (twin births and sex composition) are likely to identify different parameters. While the twin instrument identifies the treatment effect on the non-treated (Angrist et al 2006), i.e. the average effect of having e.g. a third child in families who only chooses to have two children, the sex composition instrument identifies the effect of family size for a very special group of individuals: children whose parents are induced to have another child because previous children were of the same sex (e.g. Angrist and Imbens 1995).

With these limitations in mind it can however be useful to compare the results for the two different approaches. We use a similar specification as in Angrist et al (2006) and look at the outcomes of the first two children in families with at least two births and the outcomes of the first three children in families with at least three births. We specify the first stage relationship between sibling sex composition and family size for each of the two samples as follows

$$S_i = \delta_0 + \sum_{j=1}^{2} B_{ij}\delta_{1j} + Samesex_i\delta_2 + \mathbf{P_i}'\boldsymbol{\delta_3} + \mathbf{X_i}'\boldsymbol{\delta_4} + e_i \tag{3a}$$

$$S_i = \rho_0 + \sum_{j=1}^{3} B_{ij}\rho_{j1} + \sum_{k=B,G} U_{ik}\rho_{k2} + Samesex_i\rho_3 + \mathbf{P_i}'\boldsymbol{\rho_4} + \mathbf{X_i}'\boldsymbol{\rho_5} + f_i \tag{3b}$$

where B_{ij} is a dummy for the *j*th child being boy; U_{ik} is a dummy for the sex of the first two children (*k*=B,G); The instrument, $Samesex_i$, is a dummy for the first two and three children having the same sex in the two samples respectively.[25] We include the same set of covariates as in *Table 5* except for that we do not control for mother's age at the nth birth.[26] We exclude all twins in these samples.

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[25] We have experimented extensively using different instruments and sample compositions, e.g. by allowing separate effects of having all boys or all girls. The results are not sensitive to these changes.

[26] The results are not sensitive to whether or not we control for mother's age at the nth birth.

Table 10 displays the results. Columns (1), (3), and (5) contain the first stage estimates where family size is regressed on the instrument. We can see that the relationship between sibling sex composition and fertility is considerably weaker compared to the twin births instrument. On average, having several children of the same sex increases family size with between 0.07–0.08 children. These coefficients are estimated with high precision and are very similar to those found in Angrist et al (2006). Turning to the 2SLS estimates, presented in columns (2) and (4), we find that little evidence of an effect. There is a significant *positive* effect on grades in the first sample.[27] Note also the negative impact on years of schooling and enrolment in university, which is close to being significant on the 5 percent level.

Although the sex composition instrument can be criticized for being potentially invalid and/or for identifying the effect for a very special group of individuals, in a qualitative sense the estimates for the two instruments leads to similar conclusions: both instruments suggest that the causal effect of family size on child outcomes is very small, if existing.

[27] Black et al (2005) report a positive estimate on years of schooling from using the sex composition as an instrument for family size.

Table 10 Relationship between family size and child outcomes using sibling sex composition as an instrument for family size

Sample:	(i) First two children in families with at least two births		(i) First three children in families with at least three births	
	(1) First stage	(2) 2SLS	(3) First stage	(4) 2SLS
Panel A. Educational outcomes				
GPA compulsory school	.076	3.544	.081	–.202
	(.002)	(.716)	(.004)	(1.240)
Graduated secondary school	.075	–.008	.081	–.016
	(.002)	(.009)	(.004)	(.016)
GPA secondary school	.077	3.389	.074	–1.745
	(.002)	(.873)	(.004)	(1.692)
Enrolled in university	.076	–.000	.076	–.053
	(.003)	(.016)	(.005)	(.030)
Years of schooling	.076	.006	.076	–.241
	(.003)	(.070)	(.005)	(.130)
Panel B. Labor market outcomes				
Non-employment	.076	–.007	.076	–.021
	(.003)	(.014)	(.005)	(.027)
log(earnings)	.077	–.024	.076	–.010
	(.003)	(.041)	(.005)	(.079)
Welfare dependence	.076	.006	.076	–.001
	(.003)	(.008)	(.005)	(.016)

Notes: Each cell represents a coefficient from a unique regression. The instrument is a dummy variable set to unity for the first two and three children being of same sex in columns (i) and (ii) respectively. Columns (1), (3), and (5) provide estimates of the relationship between sex composition family size. Columns (2), (4), and (5), contain 2SLS estimates of the effect of family size on each outcome. The sample consists of children born 1972–87. For the outcomes: *GPA secondary school* and *Graduated secondary school* the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for subject's sex, birth order, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and missing parental data,. Column (ii) also controls for the first two children being boys or girls. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

5 Concluding remarks

This paper investigates the effect of family size on children's educational and labor market outcomes in Sweden. As in other countries there is a strong correlation between family size and intermediate as well as long run individual outcomes. To deal with the potential influence of confounders we instrument family size using the incidence of twin births. Similar to other recent studies (Black et al 2005, Angrist et al 2006), we find that much of the correlations present for long-term outcomes (such as years of schooling and earnings) do not have a causal interpretation. There is, however, evidence that family size affects intermediate outcomes. For example, in families with at least three children, we find that having one more sibling lowers GPA in both compulsory school and secondary school with on average 1–2 percentiles.

Taken together, the evidence presented in this paper suggests that family size only plays a minor role in determining children's outcomes although we show that the effects on intermediate outcomes are larger in low-educated families and that the impact tends to increase with family size and with birth order. These findings may be taken as suggestive evidence that the effect of family size on intermediate outcomes may be larger in developing countries.

Our data are very rich, both in terms of the number of observations and concerning the variety of outcome variables available. Since the effects appear to be relatively small and only present for certain types of outcomes, detecting them may require large datasets of high quality. Our results are roughly in line with the results on young men's IQ presented in Black et al (2007a).

Do our results make sense? The idea of parents having constrained resources for each child when the family becomes big enough is plausible, and it seems strange that it would have *no* impact on the children. On the other hand, the period during which children require the most attention is relatively short. One possible interpretation of the findings is therefore that while an unplanned increase in family size may imply restrictions that affect the older siblings negatively at some point during adolescence (causing lower grades), there is still time for parents, children and society to correct this behavior so that there are no clear long-term traces of family size.

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Appendix

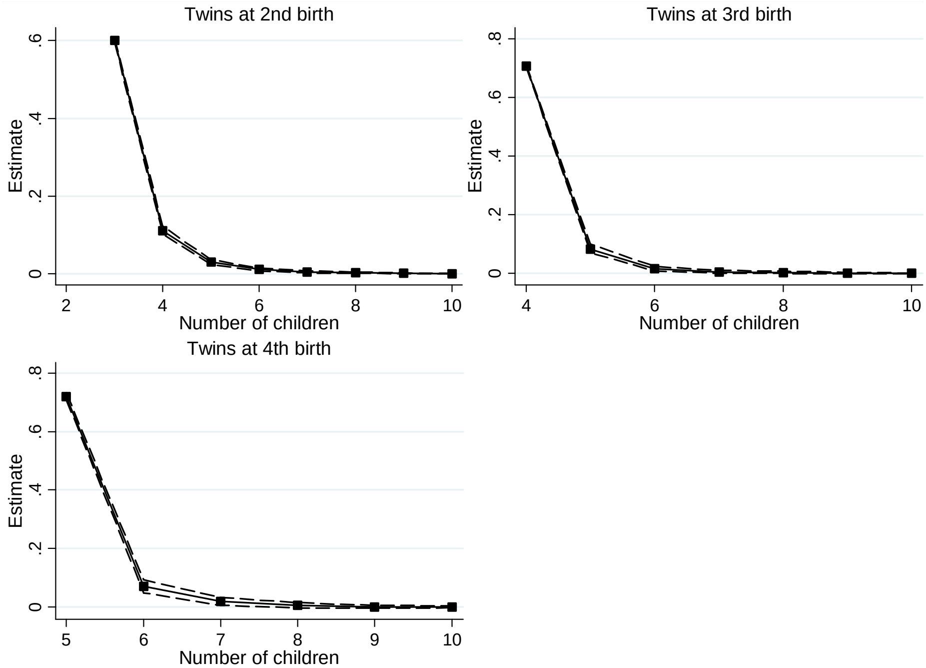

Figure A 1 The relationship between twin births at different parities and the number of children.

Notes: Each coefficient represents an estimate of the effect of twin births at the nth birth on having at least n+j children from separate regressions. The sample consists of children born 1972–87. Regressions include the same covariates as in *Table 5*.

Table A 1 Variable definitions (Statistics Sweden register in parentheses)

Variable	Definition
GPA compulsory school	The percentile rank of compulsory school GPA (computed by year of graduation) (Grade 9 student register)
GPA secondary school	The percentile rank of upper secondary school GPA (computed by year of graduation) (Register of high school graduates)
Graduated secondary school	Indicator variable = 1 if completed upper secondary school no later than 2004; 0 otherwise (Employment register)
Enrolled in university	Indicator variable = 1 if enrolled in university no later than 2004; 0 otherwise (University register)
Years of schooling	Completed level of education translated into years of schooling according to the International Standard Classification of Education 1997 (ISCED97) (Employment register)
Welfare dependence	Indicator variable = 1 for the incidence of welfare in 2004; 0 otherwise (LOUISE)
log(earnings)	The natural logarithm of (annual) labor related income in 2004 (including self-employment) measured in hundreds of SEK (Employment register)
Non-employed	Indicator variable = 1 for not employment status “not employed” on November 1, 2004 (Employment register)
Delayed compulsory (secondary) school	Indicator variable = 1 if graduated after age 16 (19); 0 otherwise (Register of high school graduates)
Preparatory program	Indicator variable = 1 if attended a theoretical/preparatory program in upper secondary school; 0 otherwise (Register of high school graduates)
Female	Indicator variable = 1 if female; 0 otherwise (Multi-generation register)
Parental characteristics	
Number of children	Mother’s recorded number of children (Multi-generation register)
Education	Indicator variable = 1 for highest completed level of education; 0 otherwise (5 levels: compulsory school, high school ≤ 2 years, high school > 2 years, university ≤ 2 years, university > 2 years) (Employment register)
Permanent income	Annual labor related income (including self-employment) measured in 1985 prices and averaged over observation years. (Employment register)

Table A 2 2SLS estimates of the effect of family size on child outcomes for first child in families with at least two births in different subpopulations

Outcome:	GPA comp.	GPA sec.	Grad. sec.	Enrolled univ.	Years of school.	Non-emp.	log(earn.)	Welf. dep.
Estimate as in ***Table 5***	–.058	.580	.009	.005	.022	–.007	.044	.001
	(.458)	(.596)	(.006)	(.011)	(.048)	(.010)	(.030)	(.006)
By gender								
Girl	–.120	–.122	.003	.010	–.011	.013	.049	.000
	(.648)	(.815)	(.007)	(.016)	(.066)	(.015)	(.044)	(.008)
Boy	.083	1.420	.015	.001	.061	–.027	.030	.002
	(.648)	(.870)	(.008)	(.017)	(.071)	(.014)	(.041)	(.009)
By parents' education								
Academic parents	.015	1.366	.015	.005	.106	–.002	.026	–.009
	(.635)	(.806)	(.005)	(.016)	(.067))	(.014)	(.041)	(.007)
Non-academic parents	-.091	–.324	.010	.009	–.056	–.011	.050	.014
	(.704)	(.921)	(.010)	(.017)	(.072	(.015)	(.046)	(.010)
Family income distribution								
Lower third	–.181	1.175	.024	.030	.105	–.005	.041	.015
	(.880)	(1.224)	(.013)	(.021)	(.086)	(.020)	(.059)	(.013)
Middle third	–.272	1.023	.013	.011	.057	–.020	.135	–.003
	(.825)	(1.033)	(.009)	(.022)	(.091)	(.018)	(.052)	(.009)
Upper third	.643	.157	-.002	–.015	–.039	–.005	–.010	–.010
	(.702)	(.919)	(.007)	(.018)	(.078)	(.015)	(.047)	(.008)

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions. The sample consists of children born 1972–87. For the outcomes: *GPA secondary school* and *Graduated secondary school* the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for birth order, gender (where appropriate), the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data (where appropriate). For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

Table A 3 2SLS estimates of the effect of family size on child outcomes for first two children in families with at least three births in different subpopulations

Outcome:	GPA comp.	GPA sec.	Grad. sec.	Enrolled univ.	Years of school.	Non-emp.	log(earn.)	Welf. dep.
Estimate as in ***Table 5***	–1.187	–1.429	–.001	–.003	–.043	.000	–.025	.005
	(.479)	(.595)	(.006)	(.011)	(.048)	(.010)	(.030)	(.006)
By gender								
Girl	–1.206	–.552	–.005	–.008	–.045	.015	–.058	–.001
	(.633)	(.818)	(.009)	(.016)	(.069)	(.015)	(.047)	(.009)
Boy	–.769	–2.270	.002	.000	–.046	–.013	.003	.010
	(.598)	(.815)	(.009)	(.014)	(.062)	(.013)	(.036)	(.009)
By parents' education								
Academic parents	–.474	–.936	–.002	–.000	–.042	–.005	–.010	–.002
	(.668)	(.815)	(.007)	(.017)	(.071)	(.014)	(.041)	(.007)
Non-academic parents	–1.789	–2.243	–.002	–.011	–.079	.008	–.032	.013
	(.663)	(.931)	(.011)	(.015)	(.068)	(.014)	(.044)	(.010)
Family income distribution								
Lower third	–1.278	–2.082	–.011	–.005	–.068	–.003	–.032	.008
	(.856)	(1.234)	(.014)	(.020)	(.088)	(.020)	(.058)	(.014)
Middle third	–.392	–.636	.009	.012	.001	.007	–.033	–.003
	(.800)	(1.075)	(.010)	(.021)	(.084)	(.018)	(.056)	(.009)
Upper third	–.915	–1.496	.006	–.009	–.035	–.008	–.002	.005
	(.710)	(.877)	(.008)	(.016)	(.077)	(.015)	(.045)	(.009)

Notes: Each cell represents the coefficient on the Number of children variable in unique regressions. The sample consists of children born 1972–87. For the outcomes: GPA secondary school and Graduated secondary school the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for birth order, gender (where appropriate), the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data (where appropriate). For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

Table A 4 2SLS estimates of the effect of family size on child outcomes for first three children in families with at least four births in different subpopulations

Outcome:	GPA comp.	GPA sec.	Grad. sec.	Enrolled univ.	Years of school	Non-emp.	log (earn.)	Welf. dep.
Estimate as in ***Table 5***	–1.563	–2.295	–.019	–.018	–.042	–.033	.021	–.001
	(.803)	(1.089)	(.013)	(.019)	(.083)	(.018)	(.053)	(.013)
By gender								
Girl	–.691	–1.118	–.034	–.045	–.146	–.048	.041	–.017
	(1.164)	(1.531)	(.019)	(.027)	(.122)	(.027)	(.086)	(.018)
Boy	–2.201	–3.240	–.004	.007	.071	–.021	.003	.014
	(.945)	(1.399)	(.016)	(.024)	(.107)	(.023)	(.066)	(.017)
By parents' education								
Academic parents	–1.340	–.817	–.014	.028	.035	–.016	–.047	.026
	(1.308)	(1.743)	(.015)	(.033)	(.137)	(.027)	(.085)	(.017)
Non-academic parents	–2.102	–3.530	–.032	–.051	–.074	–.044	.057	–.029
	(1.042)	(1.393)	(.019)	(.021)	(.106)	(.025)	(.068)	(.018)
Family income distribution								
Lower third	–1.979	.248	–.043	–.024	.024	–.100	–.004	–.014
	(1.599)	(2.436)	(.029)	(.037)	(.190)	(.043)	(.125)	(.032)
Middle third	–1.251	–4.567	.001	–.033	–.023	–.002	.054	–.011
	(1.279)	(1.733)	(.019)	(.031)	(.127)	(.029)	(.075)	(.017)
Upper third	–1.270	–1.767	–.016	–.004	–.082	–.020	.013	.015
	(1.277)	(1.654)	(.017)	(.029)	(.122)	(.027)	(.084)	(.020)

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions. The sample consists of children born 1972–87. For the outcomes: *GPA secondary school* and *Graduated secondary school* the sample is restricted to children born 1972–84. For remaining outcomes the sample is restricted to children born 1972–79. All regressions include fixed effects for birth order, gender (where appropriate), the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data (where appropriate). For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

Table A 5 2SLS estimates of the effect of family size on grades, by place in the grade distribution

Sample:	(i) First child in families with at least two births		(ii) First two children in families with at least three births		(iii) First three children in families with at least four births	
Level of schooling:	Compulsory school	Secondary school	Compulsory school	Secondary school	Compulsory school	Secondary school
Pr(GPA above Q1)	–.012	–.001	–.008	–.012	–.025	–.034
	(.007)	(.008)	(.008)	(.008)	(.013)	(.015)
Pr(GPA above Q2)	.001	.006	–.011	–.006	–.029	–.032
	(.008)	(.009)	(.008)	(.009)	(.013)	(.015)
Pr(GPA above Q3)	.005	.009	–.008	–.005	–.021	–.020
	(.007)	(.008)	(.007)	(.008)	(.011)	(.013)

Notes: Each cell represents the coefficient on the *Number of children* variable in unique regressions using the probability of scoring above the jth quartile (j={1,2,3}) in the percentile ranked GPA distribution as outcome variable. For compulsory school, the sample consists of children born 1972–87. For secondary school, the sample is restricted to children born 1972–84. All regressions include fixed effects for birth order, gender, the individual's and his/her parents' birth cohorts, mothers' age at the nth birth, parental education (5 levels) and for missing parental data. For definitions of the variables, see *Table A 1*. Standard errors robust for within family correlation are reported in parentheses.

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