Birth Order and Unwanted Fertility *

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July 13, 2019

Abstract

An extensive literature documents the effects of birth order on various individual outcomes, with later-born children faring worse than their siblings. However, the potential mechanisms behind these effects remain poorly understood. This paper leverages U.S. data on pregnancy intention to study the role of unwanted fertility in the observed birth order patterns. We document that children higher in the birth order are much more likely to be unwanted, in the sense that they were conceived at a time when the family was not planning to have additional children. Being an unwanted child is associated with negative life-cycle outcomes as it implies a disruption in parental plans for optimal human capital investment. We show that the increasing prevalence of unwantedness across birth order explains a substantial part of the documented birth order effects in education and employment. Consistent with this mechanism, we document no birth order effects in families who have more control over their own fertility.

Keywords: Birth Order, Unwanted Births, Fertility Intentions
JEL codes: J13, J22, J24

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1 Introduction

A large literature in psychology *and* economics documents clear patterns of birth order effects on various human capital and labor market outcomes. However, little is known about the potential underlying mechanisms behind these effects. Evidence suggests that later-born children receive less parental time and investment compared to their earlier-born siblings, but the sources of these differences remain poorly understood.

This paper contributes to the literature on birth order effects by proposing a novel explanation for a significant part of the observed patterns. It connects the literature on birth order effects and the literature on the negative impacts of unwanted fertility over the life cycle.[1] We show that the observed birth order effects on education and employment may reflect a disruption in parental plans for optimal investment in their children, as later born children are more likely to have been conceived when the family did not want to have an additional child.

We believe the complementary mechanism we propose to be novel as it provides distinct testable implication relative to current explanations for observed birth order effects. In particular, as we discuss below, standard theories predict that birth order effects would apply to all families irrespective of the pregnancy intention statuses of the various children in the birth order sequence. Our proposed mechanism implies that the existence or at least the size of a birth order gradient may depend on the fertility control capabilities of various families.

It is natural to expect a higher incidence of excess, unwanted children at higher parities. Few families who are still childless are likely to report that they were not planning to have children, so very few first-born children are likely to be unwanted. Some families

[1]We follow the demographic definition of an *unwanted* birth as a birth in excess of total desired fertility. The broader notion of *unintended* birth includes both unwanted and mistimed births. Given our purposes we do not consider mistimed births.

may want to have only one child, so second-born children are more likely to be unwanted. Most families desire to have only two children, so it is likely that the incidence of unwanted children among third-borns is much higher.

The very occurrence of an unwanted birth may give rise to birth order differences. While most families may wish to equalize human capital investments among their children, only families that succeed in realizing their fertility plans are able to do so. For example, those who only plan to have two children may make irreversible decisions that might only be consistent with equalization between the first two (wanted) children. Once a third (unwanted) child is born, the familys original investment plan is disrupted and it becomes ex-post suboptimal. While some re-optimization could occur, particularly by reducing planned investments or disinvesting in first- and second-born children, it might be very costly or impossible at that point to re-optimize in a way that achieves perfect equalization among all three siblings. This friction could give rise to the birth order effects we see in the data. There might be rigidities to disinvesting in existing siblings and those disinvestments might be necessary to reach perfect equalization. Moreover, the rigidity might be larger for the oldest, first-born sibling, since more time has passed and more investments have been embodied and crystalized in that child.

According to this story, if parents have made commitments that are difficult to revise upon the birth of an unwanted child, then the unwanted, higher birth order child will tend to receive less investment relative to earlier born siblings, generating a gap that contributes to the birth order effects. For example, parents may have made location, labor supply, housing, consumption, borrowing and investment decisions that would have been different had they known an additional child would be eventually born. As a result, higher order, unanticipated children may tend to do worse in terms of human capital and labor market outcomes, and this could contribute to the birth order gradient. A corollary is that birth order effects would tend to be ameliorated in families that for various reasons have more control over their own fertility.

In this paper, we first replicate earlier findings of birth order effects using both OLS and family fixed effects specifications. Next we show that these effects vanish once we focus on a subsample of families who intended to have all the children they had. We then show that the incidence of unwanted births increases significantly with birth order, and that accounting for this pattern of unwantedness flattens the birth order gradient. We also show that our birth order results are robust to alternative mechanisms, such as exposure to changes in family structure or last-born effects arising from endogenous fertility stopping rules. When we investigate sub-groups, we find that birth order effects no longer arise when we restrict our focus to families that presumably have (for religious reasons) more control over their fertility.

The data requirements to accomplish this study are somewhat demanding. We rely on longitudinal microdata from the U.S. Panel Study of Income Dynamics. The PSID data allow us to observe the adult outcomes of multiple siblings within a family along with their birth order. Of more novelty and critical for our purposes, the PSID data includes a retrospective maternal assessment about her pregnancy intention status at the time each of these siblings were conceived. We argue this information is particularly valuable for any study that aims to understand the role of birth order. Without it, one might be missing a significant element of parental decision-making.

The rest of the paper proceeds as follows. In Section 2, we discuss the two unrelated literatures that converge in this paper. In Section 3, we describe the PSID data we use in this paper. Section 4 presents our empirical methods and main findings. Section 5 provides concluding remarks.

2 Related Literature

In the two subsections below we provide a brief overview of relevant previous work in the two strands of literature that have so far developed independently of each other. First, we summarize a large literature that documents and attempts to explain birth order effects in various outcomes. Second, we summarize a smaller literature that explores the association between maternal pregnancy intention and individual outcomes later in life. To our knowledge, there is no previous work connecting these two literatures explicitly as we propose in this article.

2.1 Birth Order

A vast literature in economics explores birth order effects in various outcomes over the life cycle. Much of the literature focuses on completed education as the outcome of interest, and uses data from developed countries finding large and significant negative effects associated with a higher birth order.[2] The earliest work on birth order we are aware of in the economics literature goes back to Lindert (1977). He pointed out the importance of exploiting within family variation to ensure that no unobserved family characteristics confound the birth order patterns. He suggested that the family's time budget gets diluted across various siblings and this process tends to benefit earlier-born siblings. A modern testing of this hypothesis can be found in the influential work by Price (2008). Also seminal to this literature was work by Behrman and Taubman (1986), who explored birth order effects within the theoretical framework of Behrman et al. (1982).[3]

The literature was reinvigorated in the 2000s with the work of Black et al. (2005), who documented convincing birth order effects using a large dataset from Norway and spurred a large and still active literature in economics. Subsequent work has documented birth order effects on various outcomes, primarily focusing on school attainment and performance (Kantarevic and Mechoulan (2006), Booth and Kee (2009), De Haan (2010),

[2]The birth order literature is more limited in developing countries so little is known about how these patterns generalize to that context. For a few exceptions, see Birdsall (1979), Birdsall (1990), Behrman (1988), Horton (1988), Ejrnæs and Pörtner (2004), Edmonds (2006) and De Haan et al. (2014)

[3]See Kessler (1991) for additional early references.

Bagger et al. (2013), Hotz and Pantano (2015)), measures of cognitive ability (Conley and Glauber (2006), Black et al. (2011), Lehmann et al. (2013), Hotz and Pantano (2015), Pavan (2016)), but also exploring risky behaviors (Argys et al. (2006), Hao et al. (2008), Averett et al. (2011)), health (Black et al. (2016), Björkegren and Svaleryd (2017), Brenøe and Molitor (2018)) and delinquency (Breining et al. (2017)).

Much of the recent literature has tried to uncover and elucidate various alternative mechanisms giving rise to observed birth order effects that go beyond the above-mentioned "time dilution" theory. These hypotheses range from differential parenting strategies (Hao et al. (2008), Averett et al. (2011), Hotz and Pantano (2015)) to parental transfer behavior (De Haan (2010), Mechoulan and Wolff (2015)), direct parental preferences for birth order (Bagger et al. (2013)) and early parental investments (Lehmann et al. (2013), Pavan (2016)). Birth order effects have of course been studied in other disciplines, primarily in psychology. Early work by Belmont and Marolla (1973) provided some of the more convincing empirical evidence that spurred most of the modern research on birth order in recent decades, but some of these arguments go back to Galton (1875). Sulloway (2010) and Eckstein et al. (2010) provide surveys of the literature in psychology which focus more on how personality and non-cognitive skills vary with birth order. This is an important outcome that economists are beginning to tackle as well (Black et al. (2017)).

Overall, this literature has provided substantial evidence that earlier-born siblings tend to have higher cognitive skills and very different personality traits, go on to complete higher levels of schooling, engage less frequently in risky behaviors, earn higher wages and display a variety of positive outcomes along various dimensions later in life.

2.2 Unwanted Fertility and its Effects

The standard model of completed fertility and child quality in economics (Becker and Lewis (1973), Willis (1973) and much of the large literature that followed these seminal contributions) assumes that fertility is a perfectly controlled process, so unwanted births are not defined in that setting. Yet a growing literature, primarily in demography, has documented the prevalence of unwanted pregnancies and the life-cycle consequences associated with being the result of an unwanted pregnancy.[4] Michael and Willis (1976) extended these early economic models to incorporate imperfect fertility control and a distinction between desired and realized births. Economists have also recognized early on the importance of allowing for the effects of a dynamically changing, uncertain environment in the econometric modeling of reproductive decisions (See, for example, Barmby and Cigno (1990)).

Economists have also begun to investigate these issues empirically by exploiting direct maternal assessments of pregnancy intention (Rosenzweig and Wolpin (1993), Joyce et al. (2000), Joyce et al. (2002), Miller (2009), Lin and Pantano (2015), Lin et al. (2017)). Others have exploited natural experiments or reproductive policy changes, that allow comparison of individuals from otherwise similar cohorts that were born at times when mothers had greatly different opportunities to control their own fertility (Gruber et al. (1999), Donohue and Levitt (2001), Charles and Stephens (2006), Pop-Eleches (2006), Donohue et al. (2009), Ananat and Hungerman (2012), Ozbeklik (2014)) or at times that are thought to be auspicious for birth (Do and Phung (2010)). The balance of the literature indicates that unwanted children tend to do worse along many dimensions of adult life (education, employment, health, crime, etc.)

3 The Data

Our primary source of data is the Panel Study of Income Dynamics (PSID), a longitudinal survey of a nationally representative sample of US individuals and families with ongoing data collection since 1968. The PSID continuously collects information on individual

[4]See among others Baydar (1995), Kubička et al. (1995), Myhrman et al. (1995).

longitudinal outcomes for its initial survey respondents and their descendants. We can observe individuals and their siblings. This allows us to study the effects of birth order on outcomes later in life (e.g. completed education, employment) by comparing children of different birth order within the same families.

We construct our sample of children from the Childbirth and Adoption History File. The file contains records of childbirths and adoptions of individuals living in a PSID family at the time of the interview in any wave from 1985 through 2013. We restrict our sample to childbirth events reported by mothers and drop multiple births. We then obtain details about each child, including year of birth and birth order, as well as details about the mothers, such as their age at the time of birth, total number of births, and year of the most recent maternal report. We only include in our sample the children with mothers whose most recent report happens after she turns 35, so her total number of births is unlikely to change in the future.

Using the unique identifiers in the PSID, we combine the childbirth information with other PSID files at the individual and family levels to construct our set of control variables and to add key pieces of information for our study. First, we construct completed years of education for our sample of children. Using the PSID cross-year individual file, we only collect years of education after age 24, or when the same individual's highest years of education appears in two or more waves and consistent with the latest record, so the process of human capital accumulation is most likely to have completed. We also construct a measure of employment in adulthood. The employment variable is constructed by reverse-coding an indicator of whether the individual was unemployed at any time in 2011 and restricted to individuals who were between the ages of 24 and 50.[5]

We also obtain valuable information on pregnancy intention reported by the mothers of our sample children in the PSID. In the 1985 interview, the PSID included a ques-

[5]The indicator is not defined for those who reported being out of labor force for the entire year.

tionnaire for wives and long-term female cohabiters, allowing these women to answer for themselves some questions about their fertility history. Included in the set of questions unique to the 1985 interview is a retrospective pregnancy intention assessment. We use these pregnancy intention reports to construct our indicator defining a child as unwanted. Specifically, we define a child as unwanted if the mother reported that she was not planning to have any (more) children when she became pregnant with that child. This means that neither the children whose mother reported them as "mistimed" nor those reported as "wanted" will be considered unwanted. In particular mistimed children who were conceived "too soon", while still the result of unintended pregnancies, are not considered *unwanted* according to our definition. Parents had a plan to eventually have this child, and their decision making was conditional on that plan.[6]

Our analysis is limited by the fact that pregnancy intention questions were only fielded in the 1985 survey. We are therefore unable to evaluate pregnancy intention status for individuals born after 1985. However, this limitation is not significantly stricter than the requirement for our sample children to have completed their education by 2013. It is also worth noting that the mothers were only asked to report pregnancy intention associated with the conception of their first, last, and second to last child, which means we can only observe the complete pattern of pregnancy intentions for families with no more than three children. This limits our ability to look at larger families. Still, the available data allow us to examine birth order effects in families with two or three children. This will be sufficient to convey our main findings.

The use of retrospective assessments of pregnancy intention is controversial, as many (see, for example, Westoff and Ryder (1977) and Rosenzweig and Wolpin (1993)) fear that these reports could be contaminated by ex-post rationalization and other selective recall problems. However, work by Schoen et al. (1999) and Joyce et al. (2002) specifically

[6]Children who result from mistimed pregnancies, particularly when these occur before marriage, may also have negative effects on outcomes later in life. See for example Nguyen (2018)

addresses these questions and suggest that these retrospective reports about pregnancy intention tend to be valid.[7] We present summary statistics for our analysis sample in Table 1.

Table 1: Summary Statistics

	Mean	Standard Deviation	Min	Max
3-Child Family	0.52	0.50	0	1
First-Born	0.46	0.50	0	1
Second-Born	0.39	0.49	0	1
Third-Born	0.15	0.36	0	1
Unwanted	0.16	0.36	0	1
Completed Years of Education	13.71	2.15	1	17
Employed in 2011	0.86	0.34	0	1
Self Reported Good Health before 17	0.83	0.37	0	1
Male	0.51	0.50	0	1
Age	40.24	12.34	18	93
Mother's Age at Childbirth	24.78	5.23	13	48
Mother's Education in 1985	12.49	2.45	5	18
White	0.44	0.50	0	1
Black	0.19	0.39	0	1
Hispanic	0.02	0.14	0	1
Other Race	0.35	0.48	0	1
Observations	5499			

Note: Sample includes children from families with 2 or 3 children, with non-missing values of the outcome variable. For some variables like mother's education and the indicator for pregnancy intention associated with each child, the effective sample size is smaller as observations with missing values are excluded from the summary statistics. Mother's education is collected as of 1985, the year in which the retrospective pregnancy history was collected.

As can be seen in the table, our sample comes from families with two or three children so the incidence of third-born children in the sample is smaller than that of either first-

[7] Joyce et al. (2002) find that prospective and retrospective reports of pregnancy intention provide the same estimate of the effects of being an unintended child on various prenatal outcomes once they control for selective pregnancy recognition using an IV procedure. Further, they show that the extent of unwanted fertility was the same regardless of whether the assessment was during pregnancy or after birth. They show this for a subsample of women for whom pregnancy intention was assessed both prospectively (during pregnancy) and retrospectively (after birth).

born or second-born children.[8] The average number of years of completed education in the sample is about 14. The sample is primarily composed of white and black children with a small number of Hispanics and children of other races. The average age of the children in our sample is 40 as of 2013. By 2013 the children in our sample have all grown up and are all well into their adult years. About 16 percent of these children are unwanted as defined above. However, as we will now show, this masks substantial variation across birth order.

The pattern of unwantedness by birth order for subsamples of children from families with two or three children is presented in Table 2. In families with two children, 11% of first-borns are unwanted. The rate of unwantedness increases to 15% for second-born children.

Families with three children show the same pattern of increasing prevalence of unwanted pregnancy with birth order. Notably though, 37% of third-born children are reported as unwanted by their mothers, a substantial increase relative to first and second birth order. As discussed earlier, we are unable to document unwantedness status for all children in families of more than three children. In addition, a much smaller number of families in PSID has four or more children. For these two reasons, we limit our analysis to families with two or three children. [9]

It is of interest to explore whether the pattern we identify for our PSID children born before 1985 holds also for more recent cohorts. Families who had all of their children

[8]In principle, since we are looking at families with two and three children, the number of first-born and second-born children should be the same. In practice however, our number of second-born children is slightly smaller than the number of first-borns because they are more likely to have missing information on our outcome of interest, completed education.

[9]However, it is of interest to explore whether the pattern of increasing prevalence of unwanted children across birth order holds in 4-child families. Since we only know whether the first, last or second to last child in a family was unwanted, we can't tell whether a second-born child in a four-child family was unwanted. But we can still look at first-, third- and fourth-born children in those families. Consistent with the patterns in Table 2 we find that in four-child families the incidence of unwanted children grows from 16% among first-borns, to 27% among third-borns to a whopping 53% among fourth-borns.

Table 2: Patterns of Unwanted Fertility by Birth Order

	(1) 2-Child Families	(2) 3-Child Families
Birth Order = 1	0.11	0.13
	[0.09,0.12]	[0.11,0.15]
Birth Order = 2	0.15	0.16
	[0.13,0.17]	[0.14,0.19]
Birth Order = 3		0.37
		[0.33,0.40]
Observations	2361	2524

Note: 95% confidence intervals in brackets. Sample: PSID children from families with two or three children whose maternal pregnancy intention status at conception is not missing.

more recently may have been in better position to plan their fertility and not exceed their desired family size. To explore this we rely on data from various recent waves of the National Survey of Family Growth (NSFG) as reported in Child Trends (2013). The NSFG is a repeated cross-sectional survey and, as such, can't be used to explore birth order effects in adult outcomes like completed schooling. However, the NSFG includes a valuable retrospective assessment of a woman's fertility history that can be used to explore how pregnancy intention varies with birth order. Table 3 combines information from our PSID sample with reports based on the NSFG.

As can be seen in the table, there is still a substantial increase in the prevalence of unwantedness as we move across the birth order in more recent years, particularly for those who are third-born or have an even higher birth order. The incidence of unwanted children among third-borns is somewhat smaller in the more recent cohorts that can be reported about in the NSFG waves. This is because our PSID sample of children draws from earlier cohorts where the opportunities to prevent unwanted births were more limited. In particular, at the time in which many of the children in these earlier cohorts were conceived, abortion was not yet legal, and oral contraceptives were not yet widely

Table 3: Percentage of Unwanted Children by Birth Order PSID and NSFG

	1985	2002	2006-10
Birth Order = 1	11.8	8.5	8.8
Birth Order = 2	15.6	11.3	11.3
Birth Order $\geq$ 3	36.6	26.6	23.0
Source	PSID	NSFG	NSFG

Notes: The table report the percentage of children who are retrospectively assessed as unwanted by their mothers at the time of interview. A child is defined as unwanted if the mother reports that when the child was conceived, she was not planning to have any (more) children. Neither at that time, nor in the future. The retrospective information collected in 1985 is based on the 1985 PSID wave and its Childbirth and Adoption History File. The 1985 sample is limited to families with two or three children. It does not include one-child families, or any birth order higher than three. The information based on retrospective information collected in 2002 and 2006-10 comes from a Child Trends (2013) report based on corresponding waves from the National Survey of Family Growth.

available. Lin and Pantano (2015) document a decrease in the prevalence of unintended births following legalization of abortion. Bailey (2010) points out the large increase in the share of families with fewer than three children following the "contraceptive revolution" of the 1960's. Presumably much of that change was accomplished by the avoidance of what would have otherwise been unwanted *third-born* children. However, as the NSFG numbers show, while in the more recent years the opportunities to prevent or terminate unwanted pregnancies are more widely available, it remains the case that the prevalence of unwanted births increases with birth order and it is particularly high for third- and higher-born children.

4 Methods and Results

To examine the relationship between birth order and completed years of schooling we consider the following model in the same mold as Kantarevic and Mechoulan (2006) and others in the birth order literature:

$$Y_{ih} = \alpha_1 + \alpha_2 1\left[BO_{ih} = 2\right] + \alpha_3 1\left[BO_{ih} = 3\right] + \beta X_{ih} + \varepsilon_i \quad (1)$$

where Y_{ih} denotes completed years of education of child i in family h,$1\left[BO_{ih} = k\right]$ is an indicator variable that equals 1 whenever child i has the k^{th} birth order in family h. X_{ih} is a vector of control variables that accounts for observed characteristics of child i and/or family h. Note that first-borns correspond to the omitted category.

We begin by replicating the results reported in Kantarevic and Mechoulan (2006), who also use the PSID data. Table 4 presents our results. We are successful at replicating their main findings. The results are presented along six columns.

The first three specifications do not include controls for X_{hi}, whereas the last three do. In both cases the first specification, in columns (1) and (4), pools families with 2 and 3 children (controlling for family size) and the following two specifications consider models separately for a subsample of families with 2 children and a subsample of families with 3 children. Standard errors are clustered at the family level in all of our specifications.

As can be seen in column 1, in the simplest specification without controls, second- and third-born children tend to complete 0.09 and 0.25 fewer years of education, respectively. Once we control for the child's sex and race as well as family size and maternal characteristics such as mother's age at birth and mother's education we find that the birth order effects are accentuated. The negative coefficients on the indicators for second and third born children are now larger in magnitude (-0.31 and -0.44) and more statistically significant. Estimates for control variables in columns 4-6 have the expected sign: males and children in black families tend to have completed less schooling. Family size has overall a negative and statistically significant effect, with those growing up in families with three children having on average 0.20 fewer years of schooling than those in 2-child families. Controlling for family size is particularly important when pooling children from

Table 4: Birth Order and Education - OLS

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families	(4) 2- and 3-Child Families	(5) 2-Child Families	(6) 3-Child Families
Birth Order = 2	-0.09*	-0.13*	-0.03	-0.31***	-0.33***	-0.29***
	(0.05)	(0.07)	(0.08)	(0.06)	(0.08)	(0.08)
Birth Order = 3	-0.25***		0.02	-0.44***		-0.45***
	(0.08)		(0.09)	(0.10)		(0.12)
Age				0.05***	0.04	0.06**
				(0.02)	(0.02)	(0.02)
Age Squared				-0.00**	-0.00	-0.00*
				(0.00)	(0.00)	(0.00)
Male				-0.46***	-0.51***	-0.41***
				(0.05)	(0.07)	(0.07)
Black				-0.42***	-0.41***	-0.41***
				(0.08)	(0.11)	(0.12)
Hispanic				-0.16	-0.29	-0.09
				(0.25)	(0.41)	(0.31)
Other Race				0.11	-0.08	0.22
				(0.37)	(0.49)	(0.55)
Family of 3 Children				-0.20***		
				(0.06)		
Constant	13.78***	13.98***	13.51***	7.99***	8.53***	7.19***
	(0.04)	(0.06)	(0.07)	(1.00)	(1.37)	(1.41)
Observations	5499	2652	2847	5499	2652	2847
Mean Dependent Variable	13.71	13.92	13.50	13.71	13.92	13.50

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates in columns (4)-(6) include indicators for various levels of maternal education, as well as and indicator for whether maternal education information is missing, maternal age at childbirth and dummy variables indicating various age categories. Dependent variable measures completed years of education.

families of different sizes because, by construction, a higher birth order is only feasible in larger families.

While some observed characteristics of the mother and the family can be controlled for, it is always possible that there are additional unobservable characteristics that could confound the effects of birth order. To tackle this issue we exploit information on siblings of different birth order within the same families. To do so we follow Kantarevic and Mechoulan (2006) and explore a family fixed effects specification:

$$Y_{ih} = \alpha_1 + \alpha_2 1\left[BO_{ih} = 2\right] + \alpha_3 1\left[BO_{ih} = 3\right] + \beta X_i + \lambda_h + \varepsilon_{ih} \qquad (2)$$

where the only differences with respect to the model in (1) is that X_i now is a vector of control variables that only accounts for observed characteristics of child i within a family h as the family characteristics, both observed and unobserved are absorbed into the family fixed effect λ_h. Results of estimating the model in (2) are presented in Table 5.

Table 5: Birth Order and Education - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.24***	-0.16	-0.30***
	(0.08)	(0.13)	(0.10)
Birth Order = 3	-0.39***		-0.50***
	(0.15)		(0.18)
Male	-0.52***	-0.72***	-0.40***
	(0.06)	(0.10)	(0.08)
Constant	12.42***	14.00***	11.55***
	(1.29)	(1.69)	(1.77)
Observations	5499	2652	2847
Mean Dependent Variable	13.71	13.92	13.50

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable measures completed years of education.

They show that the birth order patterns are robust to controlling for family characteristics that are common across siblings within a given family. Controlling for family fixed effects and thus using only within family variation produces estimated birth order effects that remain sizable and significant, especially among three-child families. In particular, the pooled specification still shows significant reductions of 0.24 and 0.39 years of schooling for second and third-born children relative to their own first-born siblings.

4.1 Imperfect Fertility Control

Our main hypothesis is that families who have the ability to perfectly control their fertility are better able to equalize outcomes among their offspring. We conjecture that families with more imperfect fertility control are less likely to accomplish such equalization. This could be because unwanted children in excess of the family's target level of desired fertility are not "budgeted for" in the family's investment plan. It is possible for the family to re-optimize upon the birth of an unwanted child by re-allocating resources from elder siblings to the new unwanted child. But such re-allocations are unlikely to accomplish a perfect equalization.

To investigate this possibility we next explore how results in Table 5 change when we focus on families for which there is evidence of perfect fertility control and families for which there is not. To implement this we create an indicator $Unwanted_{hi}$ which equals one whenever child i in family h was the result of a pregnancy that was retrospectively assessed as unwanted by the mother. A child is defined as unwanted in the sense described in Section 3. We then define $W_h = 1$ if all children in family h are reported as wanted, and we set $W_h = 0$ otherwise. Panels A and B of Table 6 present the results in the two subsamples ($W_h = 1$ and $W_h = 0$). We first re-estimate the family fixed effects specification in (2) for a subsample of families where all children are reported as wanted ($W_h = 1$). These are families who planned to have all of the children they ended up having, and therefore faced no disruption in their optimal child investment allocation process. These families represent 50% of all the families whose children we use in the full sample. Panel A of Table 6 shows the results.

Table 6: Birth Order and Education in Families with and without Perfect Fertility Control - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Panel A: Families *with* Evidence of Perfect Fertility Control			
Birth Order = 2	0.06	0.11	-0.03
	(0.14)	(0.20)	(0.19)
Birth Order = 3	-0.02		-0.10
	(0.25)		(0.32)
Observations	1904	1139	765
Mean Dependent Variable	14.25	14.39	14.04
Panel B: Families *without* Evidence of Perfect Fertility Control			
Birth Order = 2	-0.46***	-0.46***	-0.44***
	(0.10)	(0.16)	(0.13)
Birth Order = 3	-0.64***		-0.72***
	(0.18)		(0.22)
Observations	3595	1513	2082
Mean Dependent Variable	13.42	13.57	13.31
Panel C: Panel B minus *A* Differences [p-value]			
Birth Order = 2	-0.52***	-0.57**	-0.40*
	[0.002]	[0.027]	[0.078]
Birth Order = 3	-0.63**		-0.61
	[0.044]		[0.114]

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable measures completed years of education.

As can be seen in the table, the birth order effects we documented in Table 5 essentially disappear. The coefficients on second- and third-born children are now much smaller and, in some cases, even positive for these families who had no unwanted children. Moreover, none of the birth order effects in Panel A are statistically significant.

On the other hand, Panel B of Table 6 shows that families without evidence of perfect fertility control ($W_h = 0$) retain sizable and significant birth order effects.[10]

Panel C presents tests of statistical significance for the differences between panels A and B. In the pooled specification, the differences are statistically significant at 1% for the second child and 5% for the third child. The differences for the second child are also significant even in the smaller subsamples of 2- and 3-children families. These substantial differences provide our first line of evidence showing that birth order effects are somehow linked to families' fertility control.

While the differences in the birth order gradient are striking, we exercise caution when interpreting these results. It is possible that unobserved family heterogeneity could be driving our findings. That would be the case if some family unobservable is correlated with both, imperfect fertility control and the existence of birth order effects. Families who are good at avoiding unwanted births are different (in observed and likely unobserved ways) from other families who are less successful regarding fertility planning. Therefore we do not claim to attach a causal interpretation to these results. For example, it could be that families good at contracepting are particularly averse to inequality in outcomes and thus try harder to avoid a large birth order gradient among their offspring. If that's the case one would see more inequality in outcomes among the children of families who contracept poorly. This is something to bear in mind as the family fixed effects in our child level models do not necessarily account for this problem.

[10]These are families for which we identify at least one unwanted child or families for which information for pregnancy status is missing for at least one child.

4.2 Accounting for Pregnancy Intention in Estimation of Birth Order Effects

The family fixed effects specification has become standard when looking at birth order effects, but it only controls for unobserved factors that are common across siblings within a family. We have already controlled for maternal age at birth, but there might be other characteristics that are typically unobserved, vary across siblings, and are correlated with both birth order and later life outcomes like completed years of schooling. One such factor we do get to observe is the pregnancy intention status corresponding to the conception of each child in the family. As documented in Section 3, higher birth order children are more likely to be the result of an unwanted pregnancy. Given the large differences observed in panels A and B of Table 6 it is natural to explore how birth order effects change once we account for this child specific factor, often unobserved in various datasets that are used to document birth order effects.

Before exploring how accounting for pregnancy intention affects birth order effects on completed education in the full sample, we provide a more systematic examination of how the chance of being unwanted rises with birth order. While the results in Table 2 are quite suggestive, we first investigate whether those results are robust to controlling for the same set of observable characteristics X. To do so, we re-estimate the model in (1) using our indicator that denotes whether the individual was the result of an unwanted pregnancy as dependent variable. The results are presented in Table 7.

As we can see, the pattern of increasing unwantedness as we move higher in the birth order is robust to controlling for observable characteristics. Moreover, as can be seen in Table 8 the results are robust to further controlling for family fixed effects, particularly for third born children.

Having established a clear pattern of increasing prevalence of unwanted children higher on the birth sequence, we explore the implications of pregnancy intention across

Table 7: Birth Order and the Probability of Being Unwanted - OLS

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families	(4) 2- and 3-Child Families	(5) 2-Child Families	(6) 3-Child Families
Birth Order = 2	0.04***	0.04***	0.03**	0.07***	0.08***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Birth Order = 3	0.25***		0.23***	0.28***		0.28***
	(0.02)		(0.02)	(0.02)		(0.02)
Age				0.00	-0.01**	0.01***
				(0.00)	(0.00)	(0.00)
Age Squared				0.00	0.00**	-0.00***
				(0.00)	(0.00)	(0.00)
Male				-0.02**	-0.02*	-0.03*
				(0.01)	(0.01)	(0.02)
Black				0.20***	0.19***	0.21***
				(0.02)	(0.02)	(0.02)
Hispanic				0.10***	0.04	0.14***
				(0.03)	(0.04)	(0.05)
Other Race				0.02	0.00	0.03
				(0.05)	(0.06)	(0.08)
Family of 3 Children				-0.02		
				(0.01)		
Constant	0.12***	0.11***	0.13***	0.68***	1.01***	0.29
	(0.01)	(0.01)	(0.01)	(0.22)	(0.28)	(0.31)
Observations	4885	2361	2524	4885	2361	2524
Mean Dependent Variable	0.17	0.13	0.21	0.17	0.13	0.21

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include an indicator for individuals whose mother's education information in 1985 is missing, maternal age at childbirth and dummy variables indicating various age categories. Dependent variable equals 1 if an individual is the result of an unwanted pregnancy, zero otherwise.

birth order for the estimation of birth order effects in education. To that end we include our key measure characterizing each child within a family as wanted or unwanted in our models for completed education. Our objective is to explore if and how the pattern of birth order effects on education changes once we control for the maternal pregnancy intention indicators corresponding to each child. Table 9 presents results from family fixed effects specifications that add the child-level $Unwanted_{hi}$ indicator to the model in (2).

The results show that the birth order gradient becomes less pronounced once we account for indicators characterizing maternal pregnancy intention at the time these children were conceived.

Table 8: Birth Order and the Probability of Being Unwanted - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	0.06***	0.04*	0.06***
	(0.02)	(0.02)	(0.02)
Birth Order = 3	0.24***		0.27***
	(0.03)		(0.04)
Male	-0.00	-0.02	0.01
	(0.01)	(0.02)	(0.02)
Constant	0.84***	0.68**	1.01***
	(0.24)	(0.31)	(0.36)
Observations	4885	2361	2524
Mean Dependent Variable	0.17	0.13	0.21

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable equals 1 if individual is the result of an unwanted pregnancy, zero otherwise.

For example in the pooled specification in column 1, the effects for second- and third-born children change from -0.24 and -0.39 in Table 5 to -0.14 and -0.23 in Table 9, both large percent-wise reductions in magnitude. Moreover, using a 1000-resamplings bootstrap, we found that the birth order effect differences in the pooled sample of two-and three-child families across Tables 5 and 9 are both statistically significant at the 10% level. Further, we now see that only higher birth order children in three-child families have a significantly negative effect, but the magnitudes (-0.25 for second-born and -0.37 for third-born) are much smaller than those in Table 5 (-0.30 for second-born and -0.50 for third-born). Indeed, for third-born children in three-child families the difference across tables is, again, statistically significant at the 10% level.

Table 9: Birth Order and Education Accounting for Unwantedness - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.14	-0.01	-0.25**
	(0.09)	(0.14)	(0.12)
Birth Order = 3	-0.23		-0.37*
	(0.16)		(0.19)
Unwanted	0.05	-0.01	0.06
	(0.13)	(0.22)	(0.16)
Male	-0.53***	-0.71***	-0.41***
	(0.06)	(0.10)	(0.08)
Constant	12.33***	13.96***	11.45***
	(1.30)	(1.71)	(1.79)
Observations	5499	2652	2847
Mean Dependent Variable	13.71	13.92	13.50

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. For children with missing information on their maternal pregnancy status at conception we include an indicator which equals to one whenever the pregnancy intention information is missing and equals zero otherwise. Further we interact this indicator with the birth order indicators.

Our focus here is not to estimate the impact of being an unwanted child, but rather account for differential pregnancy intention rates across birth order in the estimation of birth order effects. Further we aim to explore how different the birth order gradient is across families with and without imperfect fertility control. However, it is worth noting that the $Unwanted_{hi}$ indicator is not statistically different from zero in the family fixed effect specifications of Table 9. This is consistent with work by Joyce et al. (2000) who find similar results using the National Longitudinal Survey of Youth, albeit looking at outcomes earlier in life. This is not surprising because, as pointed out by Rosenzweig and Wolpin (1993), the family fixed effects specification will tend to provide a biased estimate of the effects of being unwanted. This is because the birth of an unwanted child will likely have an effect on existing siblings as parents re-optimize (i.e. reduce) their allocations toward those siblings as they cope with the arrival of the unwanted child. While our main approach takes as given the idea that unwanted children do worse, neither our OLS (because of omitted variable bias) nor our family fixed effect (because of spillover on existing siblings) strategies are well suited to test that hypothesis in a causal sense. The true effect is probably somewhere in between.

Taken together, the attenuation of the birth order gradient once we account for unwanted births, coupled with the absence of birth order effects in families where all children are wanted, suggests that pregnancy intention might be an important consideration when assessing the effects of birth order on various outcomes.

4.3 Birth Order Effect Heterogeneity in Groups with More or Less Imperfect Fertility Control

In this subsection we explore how birth order effects vary among groups with differential fertility control. We follow Lin and Pantano (2015) and use information on maternal religion to classify our children into two groups. First, we create a group whose mothers report a religion affiliation that tends to be more strongly against the use of abortion. We denote this as the "pro-life" subsample. We group a second set of children whose mothers report either no religion affiliation or an affiliation that has less stringent attitudes towards abortion. We denote this as the "pro-choice" subsample. We use the same criteria as in Lin and Pantano (2015) to classify these religions into this binary indicator of attitudes toward abortion.[11] To be sure, within each religion there will be those who adhere more strictly to their religion's stance and those who will align less strongly. The indicator is not meant to classify the exact attitude of a particular mother, but rather provide an indicator of her *likely* ability to terminate unwanted pregnancies. We expect the birth order effects to be stronger in the "pro-life" sub-sample as mothers in this sub-sample are less likely to use abortion to terminate unwanted pregnancies.

Table 10: Birth Order and Education - Family Fixed Effects (Pro-Life)

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.26**	-0.15	-0.32**
	(0.10)	(0.17)	(0.13)
Birth Order = 3	-0.40**		-0.51**
	(0.19)		(0.23)
Male	-0.38***	-0.62***	-0.24**
	(0.08)	(0.13)	(0.10)
Constant	13.57***	15.18***	12.81***
	(1.62)	(2.20)	(2.15)
Observations	3556	1673	1883
Mean Dependent Variable	13.62	13.80	13.47

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable measures completed years of education. Subsample of individuals from families with maternal religion *more* strongly associated with a pro-life stance on abortion.

On the other hand we expect the birth order effects to be milder in the "pro-choice" sub-sample as these families are more likely to use abortion to prevent unwanted births.

[11]We follow the religion taxonomy in Evans (2002) and classify the following religions as having a more strict attitude against abortion: Roman Catholic, Protestant, other Protestant, other Non-Christian, Latter Day Saints, Mormon, Jehovahs Witnesses, Greek/Russian/Eastern Orthodox, Lutheran, Christian, Christian Science, Seventh Day Adventist, Pentecostal, Jewish, Amish, and Mennonite. Mothers reporting these religions are more likely to be pro-life and less likely to use abortion to terminate unwanted pregnancies. We then classify Baptists, Episcopalians, Methodists, Presbyterians and Unitarians along with Agnostics and Atheists as having a less strict attitude towards abortion.

As a result, the prevalence of unwanted third-born children, relative to first-born children may not be as high for these "pro-choice" families. We re-estimate the family fixed effects specification in (2) in the sub-samples of children grouped according to these different maternal religious affiliations. Tables 10 and 11 present the results.

Table 11: Birth Order and Education - Family Fixed Effects (Pro-Choice)

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.11	0.03	-0.20
	(0.14)	(0.23)	(0.18)
Birth Order = 3	-0.21		-0.33
	(0.26)		(0.30)
Male	-0.71***	-0.90***	-0.61***
	(0.11)	(0.18)	(0.14)
Constant	10.64***	11.81***	9.79***
	(2.42)	(3.10)	(3.01)
Observations	1943	979	964
Mean Dependent Variable	13.85	14.13	13.57

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable measures completed years of education. Subsample of individuals from families with maternal religion *less* strongly associated with a pro-life stance on abortion.

The birth order effects are quite strong in the "pro-life" subsample. Indeed they are as strong as those reported in Table 5. For example, in the pooled specification, on average, second- and third-born siblings complete 0.26 and 0.40 fewer years of education than their first-born sibling within the same family.

On the other hand, there are no apparent birth order effects in the "pro-choice" subsample as reported in Table 11. As can be seen in this table, while the point estimates are still mostly negative, all of them are smaller in magnitude and none of them is statistically significant. However, while the differences in the birth order estimates across the two groups are sizable, we are unable to reject the hypothesis that the effects in Tables 10 and 11 are the same.

4.4 Alternative Mechanisms

We have found compelling evidence of the important role played by pregnancy intention in generating birth order effects. We now test two alternative mechanisms that could give rise to birth order effects in our sample.

First, it has been documented that family structure may affect children's education outcomes (Ermisch and Francesconi (2001)). Then it is possible that children higher in the birth sequence could be more affected by changes in family structure relative to earlier born siblings and this could affect their educational achievement. As a result, it has become standard in the literature to test whether birth order effects hold in a subsample of intact families (see for example Black et al. (2005), Hotz and Pantano (2015)). Second, it is possible that endogenous fertility stopping rules could be the reason behind our birth order effects. We show that the birth order effects we identify in this sample are robust to these two possibilities. To test whether our results in Table 5 reflect just differential exposure to changes in family structure we re-estimate our main fixed effects specification in (2) but in a subsample of intact families.

To construct our subsample of intact families we link our data with the PSID Marriage History File, which contains information on the mothers marriage events collected retrospectively in the 1985 through 2013 waves. We use this data to create a sample of intact families within our main sample by keeping a family only when the mothers first marriage stayed intact by the year her last child turned 24, and when the mothers most recent marriage report was collected after her last child turned 24.

By applying these sample restrictions, we focus on a subsample of individuals whose family structure was relatively stable through the completion of their education. Results are presented in Table 12.

As we can see in the table, sizable birth order effects are still present in this subsample, and remain statistically significant despite the smaller sample size. This suggest that the birth order effects we find are not driven by differential exposure to disruptions in family structure. Later-born siblings have lower educational attainment for reasons other than their higher likelihood of growing up in a broken home.

Table 12: Birth Order and Education within Intact Families - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.31**	-0.23	-0.38**
	(0.13)	(0.19)	(0.18)
Birth Order = 3	-0.62***		-0.69**
	(0.23)		(0.29)
Male	-0.57***	-0.80***	-0.39***
	(0.10)	(0.15)	(0.14)
Constant	10.67***	11.98***	10.21***
	(1.58)	(2.20)	(2.28)
Observations	1933	1004	929
Mean Dependent Variable	14.49	14.68	14.30

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable is completed years of education. Subsample of individuals from "intact" families as defined in Section 4.4.

Next, we investigate whether the birth order effects we document are driven by endogenous fertility stoppage. When a newborn child is particularly unhealthy, it is possible that parents may not have additional children, to ensure they have the time and resources to care for the unhealthy child. If this is the case, the last born would tend to be particularly unhealthy and this could affect cognitive development and, ultimately, educational attainment. Note that later-born siblings tend to engage more in risky behaviors in their late teen and early twenties and this could lead to reduced health in adulthood, but would not necessarily provide evidence consistent with the rationale behind an endogenous fertility rule. Still, in our preliminary explorations we found no birth order effects in measures of adult health. To investigate this further, we deemed more appropriate to use a measure of health earlier in life. We re-estimated our main fixed effects specification in (2) but using as dependent variable a measure of health during childhood and adolescence.

This presents a challenge as the individuals in our sample grew up during years in which the PSID was not yet systematically collecting information on health. Fortunately, in more recent years PSID asked heads of households and wives to provide a summary retrospective assessment about their own health status earlier in their lives (health status before they turned 17 years old). This measure could be more plausibly related to the type of health issues that could lead an individual's parents to stop their fertility. Results are presented in Table 13. As can be seen in this table we find no significant birth order effects in health status during childhood and adolescence. These findings ameliorate concerns that our results could be driven by endogenous last-born effects.

Table 13: Birth Order and Good Health Before Age 17 - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.02	-0.01	-0.04
	(0.02)	(0.04)	(0.03)
Birth Order = 3	-0.05		-0.06
	(0.04)		(0.06)
Male	0.03	0.02	0.03
	(0.02)	(0.03)	(0.03)
Constant	1.26***	1.64***	1.04**
	(0.33)	(0.44)	(0.46)
Observations	3763	1868	1895
Mean Dependent Variable	0.82	0.83	0.82

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates include maternal age at childbirth and dummy variables indicating various age categories. Dependent variable is a binary indicator for a retrospective self-assessment of own health status earlier in life (before age 17). "Very Good" and "Excellent" health equal 1, zero otherwise.

4.5 Employment Outcomes

In this subsection we explore how birth order is associated with adult employment and whether pregnancy intention plays a similar role. The construction of our measure of employment in adulthood is described in Section 3. Our findings are broadly similar to the reported effects on completed education.

Tables A.1-A.7 in the Appendix present the results. Table A.1 presents the basic OLS estimates and Table A.2 reports the basic family fixed effects estimates. These tables show that there is a reduction in the probability of adult employment for second- and third-born children relative to first-born children. In the family fixed effects specification, the decline in employment is particularly strong and statistically significant for later-born siblings in three-child families, with declines of 9 and 13 percentage points. Tables A.3 and A.4 show that, again, the effects are very different in families with and without evidence of perfect fertility control.[12] Also, once we control for pregnancy intention status as in Table A.5, the birth order gradient in employment is attenuated relative to that in Table A.2. Further, in line with our education findings, column (3) in Tables A.6 and A.7 show that the reported decline in employment is statistically significant in the pro-life sample but not in the pro-choice sample.

5 Conclusions

In this paper we connect two disjoint literatures to shed more light on a novel mechanism that can give rise to birth order effects. The sources of birth order effects have puzzled economists and social scientists for decades. We document that, as one might expect, the prevalence of unwanted children increases with birth order, particularly when moving from second- to third-born children. We then replicate earlier findings related to birth

[12] In the pooled specification, the effects for the third child are statistically significantly different from each other across the two tables.

order effects on completed education for the United States using data from PSID and a research design that exploits within-family variation. We go on to show that these birth order effects are reduced once we account for the differential pregnancy intention status of children born into different birth order. Moreover, we show that birth order effects no longer arise once we focus on a subsample of children from families who had no unwanted children or on families with religion background with less stringent attitudes towards the use of abortion. We conclude that the increasing prevalence of unwanted children at higher parities could be an important mechanism behind the well-documented birth order effects.

We show that our results are robust to alternative hypotheses that have been proposed in the literature on birth order effects. In particular, we show that our results hold in intact families and that they are not likely the result of endogenous last-born effects arising from fertility stopping after the birth of an unhealthy child. We also investigate adult employment and find similar patterns of attenuation of negative birth order effects once we factor in pregnancy intention.

It is possible that families might be averse to inequality in outcomes among their offspring, and they may try to equalize these outcomes as a result. Yet, families that avoid unwanted births and do not exceed their target desired fertility seem to be in better position to equalize outcomes among their offspring. While further research is necessary to directly test this mechanism, our findings are consistent with it.

Taken together, our results suggest novel avenues for future research on birth order effects. It would be interesting to see whether birth order effects are stronger in countries with more imperfect fertility control. Similarly, it might be interesting to explore whether, within countries, birth order effects are stronger during periods where the ability to avoid unwanted births is more limited. By the same token, one would expect birth order effects to be more pronounced, everything else equal, among groups that for cultural reasons are less prone to utilize various forms of fertility control.

Compliance with Ethical Standards

Funding: Wanchuan Lin acknowledges research support from the National Natural Science Foundation of China (Grant No.71573004) and the Key Laboratory of Mathematical Economics and Quantitative Finance (Peking University), Ministry of Education. Shuqiao Sun was supported by the George Katona Economic Behavior Research Award, funded by the Institute for Social Research at the University of Michigan. **Conflict of Interest:** The authors declare that they have no conflict of interest.

Appendix

Table A.1: Birth Order and Employment - OLS

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families	(4) 2- and 3-Child Families	(5) 2-Child Families	(6) 3-Child Families
Birth Order = 2	-0.02	-0.01	-0.03	-0.03*	-0.02	-0.04*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Birth Order = 3	-0.04*		-0.04	-0.06**		-0.07**
	(0.02)		(0.03)	(0.03)		(0.03)
Age				0.03**	0.03	0.03*
				(0.01)	(0.02)	(0.02)
Age Squared				-0.00**	-0.00	-0.00
				(0.00)	(0.00)	(0.00)
Male				0.01	0.01	0.02
				(0.01)	(0.02)	(0.02)
Black				-0.12***	-0.12***	-0.11***
				(0.02)	(0.03)	(0.03)
Hispanic				-0.02	0.01	-0.04
				(0.05)	(0.06)	(0.07)
Other Race				0.05	0.11	-0.08
				(0.07)	(0.08)	(0.10)
Family of 3 Children				0.00		
				(0.02)		
Constant	0.87***	0.87***	0.87***	0.44	0.27	0.65
	(0.01)	(0.01)	(0.02)	(0.35)	(0.50)	(0.52)
Observations	2273	1169	1104	2273	1169	1104
Mean Dependent Variable	0.86	0.87	0.84	0.86	0.87	0.84

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Covariates in columns (4)-(6) include indicators for various levels of maternal education, as well as an indicator for whether maternal education information is missing. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011.

Table A.2: Birth Order and Employment - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.04	0.00	-0.09**
	(0.03)	(0.04)	(0.04)
Birth Order = 3	-0.05		-0.13**
	(0.05)		(0.06)
Male	0.01	-0.00	0.03
	(0.02)	(0.03)	(0.03)
Age	0.02	-0.02	0.05
	(0.02)	(0.04)	(0.03)
Age Squared	-0.00	0.00	-0.00*
	(0.00)	(0.00)	(0.00)
Constant	0.50	0.98	0.25
	(0.49)	(0.81)	(0.61)
Observations	2273	1169	1104
Mean Dependent Variable	0.86	0.87	0.84

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011.

Table A.3: Birth Order and Employment in Families *with* Evidence of Perfect Fertility Control - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	0.01	0.07	-0.05
	(0.04)	(0.06)	(0.04)
Birth Order = 3	0.06		-0.08
	(0.08)		(0.08)
Male	0.02	0.01	0.05
	(0.03)	(0.04)	(0.05)
Age	0.04	0.06	0.01
	(0.04)	(0.06)	(0.07)
Age Squared	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)
Constant	-0.13	-0.92	0.97
	(0.89)	(1.25)	(1.32)
Observations	924	615	309
Mean Dependent Variable	0.90	0.91	0.89

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011.

Table A.4: Birth Order and Employment in families *without* Evidence of Perfect Fertility Control - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.08*	-0.07	-0.11**
	(0.04)	(0.07)	(0.05)
Birth Order = 3	-0.13*		-0.15*
	(0.07)		(0.09)
Male	0.01	-0.03	0.02
	(0.03)	(0.06)	(0.04)
Age	0.03	-0.07	0.06
	(0.03)	(0.05)	(0.04)
Age Squared	-0.00	0.00	-0.00*
	(0.00)	(0.00)	(0.00)
Constant	0.59	2.32**	0.06
	(0.61)	(1.00)	(0.73)
Observations	1349	554	795
Mean Dependent Variable	0.83	0.82	0.83

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011.

Table A.5: Birth Order and Employment Accounting for Unwantedness - Family Fixed Effects

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.04	-0.00	-0.07*
	(0.03)	(0.04)	(0.04)
Birth Order = 3	-0.02		-0.09
	(0.05)		(0.07)
Unwanted	0.06	0.13	0.04
	(0.05)	(0.09)	(0.07)
Male	0.01	0.00	0.02
	(0.02)	(0.03)	(0.03)
Age	-0.01	-0.01	0.01
	(0.03)	(0.04)	(0.04)
Age Squared	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)
Constant	0.99*	0.71	1.05
	(0.53)	(0.86)	(0.68)
Observations	2273	1169	1104
Mean Dependent Variable	0.86	0.87	0.84

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011.

Table A.6: Birth Order and Employment - Family Fixed Effects (Pro-Life)

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.05	0.02	-0.11**
	(0.04)	(0.06)	(0.05)
Birth Order = 3	-0.06		-0.17**
	(0.07)		(0.08)
Male	-0.01	-0.03	0.01
	(0.03)	(0.05)	(0.04)
Age	0.02	-0.04	0.05
	(0.03)	(0.05)	(0.05)
Age Squared	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)
Constant	0.56	1.30	0.36
	(0.66)	(1.04)	(0.84)
Observations	1312	656	656
Mean Dependent Variable	0.86	0.86	0.87

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011. Subsample of individuals from families with maternal religion *more* strongly associated with a pro-life stance on abortion.

Table A.7: Birth Order and Employment - Family Fixed Effects (Pro-Choice)

	(1) 2- and 3-Child Families	(2) 2-Child Families	(3) 3-Child Families
Birth Order = 2	-0.04	0.01	-0.08
	(0.05)	(0.07)	(0.06)
Birth Order = 3	-0.02		-0.08
	(0.08)		(0.12)
Male	0.02	0.03	0.01
	(0.03)	(0.04)	(0.05)
Age	0.03	0.04	0.04
	(0.05)	(0.07)	(0.06)
Age Squared	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)
Constant	0.23	0.02	0.25
	(0.98)	(1.51)	(1.29)
Observations	961	513	448
Mean Dependent Variable	0.84	0.87	0.81

Robust standard errors in parentheses, clustered at the family level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category is first-born child. Age refers to age the child (now an adult) in the year 2011. Dependent Variable measures employment in 2011. Subsample of individuals from families with maternal religion *less* strongly associated with a pro-life stance on abortion.

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