The EITC, Birth Intervals and Completed Fertility

Katherine Meckel*

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Abstract

In this paper, I consider the effects of the EITC on a new margin: birth spacing. EITC-eligible households receive a substantially increased refund upon the birth of their first child, providing stronger incentives for labor market participation. If mothers make spacing decisions by trading off their desire to limit time out of the labor market against health costs of small birth intervals, the EITC may decrease spacing. In order to test this hypothesis empirically, I use a novel identification design to identify variation in tax credits: the cutoff in first child's birthdate around the end of the new year. Using three different datasets containing child birth dates and fertility outcomes, I find that increases in tax transfers associated with a qualifying dependent decrease the interval to the next birth among low-education, single mothers but have zero or slightly negative effects on completed fertility. My findings have implications for child human capital outcomes, given recent evidence that small birth intervals deplete maternal nutrition, thought to be an important determinant of child development.

Keywords: fertility; tax and transfer system; birth spacing; Earned Income Tax Credit (EITC); regression discontinuity

*Columbia University, Department of Economics. 1022 International Affairs Building, 420 West 118th Street, New York, NY 10027. Contact e-mail: khm2110@columbia.edu. I would like to thank Mitchell Hoffman, Supreet Kaur, Ilyana Kuziemko, Neil Mehrotra, Maya Rossin-Slater, Bernard Salanie, and seminar participants at the Columbia applied micro colloquium for comments and suggestions. I am especially grateful to Wojciech Kopczuk and Janet Currie for support and guidance. Marc Montrose at the Texas Department of Health and Human Services provided help with the Texas Vital Statistics data. The project described was supported by a Graduate Research Fellowship from the National Science Foundation. The content is solely the responsibility of the author and does not necessarily represent the official views of the National Science Foundation.

1 Introduction

The Earned Income Tax Credit (EITC) is one of the largest antipoverty programs in the U.S., distributing nearly $62 billion in 2011. As such, it has attracted considerable study by social scientists and policymakers alike. Because it is designed as a wage subsidy, many of the original economics studies on the EITC focused on labor market outcomes. For example, several studies found that the EITC increases labor force participation among single mothers (Meyer and Rosenbaum, 2001; Eissa and Hoynes, 1998).

More recently, economists have expanded their focus to include "unintended" effects of the EITC on non-labor margins, including infant health (Hoynes *et al.*, 2011), auto loans (Adams *et al.*, 2009), and unemployment duration (LaLumia, 2011). In this paper, I consider the effects of the EITC on a new margin: birth spacing. EITC-eligible households receive a substantially increased refund upon the birth of their first child, providing stronger incentives for labor market participation. To the extent that mothers make spacing decisions by trading off their desire to limit time out of the labor market against health costs of small birth intervals, the EITC may decrease spacing by increasing the value of labor market participation.

In order to test this hypothesis empirically, I estimate a regression discontinuity in birth month of first child on spacing. Using three different datasets containing child birth dates and fertility outcomes, I find that increased tax savings associated with a qualifying dependent decrease spacing among low-education, single mothers and have zero or slightly negative effects on completed fertility.

This paper makes several important contributions. First, I use a novel identification design to identify variation in tax liability and credits: child birthdate. Second, I consider a relatively unexamined outcome, spacing, and present a theoretical framework of spacing decisions, which incorporates the role of information about returns. Finally, this is the first paper to look at the effects of income transfers on fertility in the U.S. using a regression discontinuity design.

This paper is organized as follows: the next section presents a Literature Review; Section 3 presents the Theoretical Framework; Section 4 describes the Data; Section 5 describes the

Empirical Methods; Section 6 describes the Results; Section 7 considers Sorting; Section 8 concludes.

2 Literature Review

Three separate strands of research are relevant to this paper: studies on birth spacing, the effects of the EITC on economic outcomes and the effects of household income on fertility. Below, I review relevant findings.

2.1 Birth Spacing

Research suggests that short birth intervals (less than 18-24 months) cause adverse birth outcomes for the younger sibling (Smits and Essed (2001), Manon Van Eijsden and Bonsel (2008), Rosenzweig and Wolpin (1988)). The mechanism is thought to be maternal nutritional depletion (Conde-Agudelo *et al.*, 2006).[1] Recent studies in economics have linked nutrition *in utero* to adult health and economic outcomes (Almond *et al.*, 2011; Almond and Currie, 2011), suggesting that the impacts of close spacing on the younger child may be long-lasting.

Further, other important parental investments may be affected by birth spacing. Powell and Steelman (1993) and Black *et al.* (2005) find that, among financially constrained families, close spacing reduces economic investments in older siblings, resulting in lower educational attainment. Similarly, Buckles and Munnich (2012), instrumenting for spacing with the presence of a miscarriage, find that close spacing is associated with lower test scores for the

[1] Conde-Agudelo *et al.* (2006) explain in greater detail: "The reasons for the association between a short interval between pregnancies and adverse perinatal outcomes are unclear. A plausible explanation is the maternal nutritional depletion hypothesis, which states that a close succession of pregnancies and periods of lactation worsen the mother's nutritional status because there is not adequate time for the mother to recover from the physiological stresses of the preceding pregnancy before she is subjected to the stresses of the next. This results in depletion of maternal nutrient stores, with the subsequent increased risk of adverse perinatal outcomes. The folate depletion hypothesis claims that maternal serum and erythrocyte concentrations of folate decrease from the fifth month of pregnancy onward and remain low for a fairly long time after delivery. Women who become pregnant before folate restoration is complete have an increased risk of folate insufficiency at the time of conception and during pregnancy. As a consequence, their offspring have higher risks of neural tube defects, intrauterine growth restriction, preterm birth, and LBW. Some investigators have attributed the higher risk of poor pregnancy outcomes to several factors associated with having short intervals, such as socioeconomic status, unstable lifestyles, failure to use health care services or inadequate use of such services, unplanned pregnancies, and other behavioral or psychological determinants. However, the fact that the birth spacing effects are not strongly attenuated when socioeconomic and maternal characteristics are controlled for suggests that the effects are not caused by these confounding factors."

older sibling. On the other hand, having children closer together could decrease total cost of childbearing if there are increasing economies of scale in childrearing (e.g., children can share clothes and toys). In general, there is little evidence on the long-term impacts of spacing.

2.2 EITC and Economic Outcomes

There is a large literature that examines the effects of the EITC on a wide variety of outcomes, including maternal labor supply, fertility, health, marriage, educational attainment, and spending patterns (see Hotz and Scholz (2003) for a thorough review). My paper adds to this literature by considering the effects of the EITC on birth spacing and completed fertility. Below, I briefly review some of the most relevant studies.

Because the size of credit is a tied to marital status and number of children, it is possible that EITC has a causal effect on these margins. Alternatively, the EITC could have a wealth effect on fertility; with more money, families can afford more children. In general, however, there is little empirical evidence that the EITC has altered marriage (Dickert-Conlin *et al.*, 2002; Eissa and Hoynes, 1998) or completed fertility (Baughman and Dickert-Conlin, 2003; Hoynes *et al.*, 2011).

Another important margin for my study is maternal labor supply, and a significant amount of research assesses the causal effects of the EITC on this outcome. For example, Eissa and Liebman (1996) and Meyer and Rosenbaum (2001) examine the impact of the EITC on the labor supply of single mothers by comparing the changes in labor supply for women with and without children before and after an expansion to the EITC in 1986. Eissa and Hoynes (1998) examine the effect of the EITC on the labor supply of married mothers with a similar methodology. These papers suggest that the EITC raises the labor supply of single mothers but reduces the labor supply of married mothers. Results are larger among women with low education levels.

The evidence on whether the EITC alters hours worked is less clear. Eissa and Liebman (1996) finding little evidence that EITC expansions altered hours worked, while Dickert *et al.* (1995), Keane and Moffitt (1998), and Meyer and Rosenbaum (2001) find small impacts of EITC expansions on hours of work.

Finally, a number of recent papers have considered the effects of the EITC on a number of margins related to fertility, including maternal health (Evans and Garthwaite, 2010), infant health (Hoynes *et al.*, 2011), and child test scores (Dahl and Lochner, 2012). Evans and Garthwaite (2010), for example, find evidence that the expansion of the EITC lowered the counts of the risky biomarkers in mothers, suggesting a reduction in maternal stress. Hoynes *et al.* (2011) find that EITC payments made while a child is in utero are associated with increased infant health outcomes, suggesting a causal impact of household income on infant health. Lastly, Dahl and Lochner (2012) find that increases in family income due to the EITC lead to improvements in child test scores.

2.3 Household Income and Childbearing

My paper also relates more generally to research on household income and childbearing decisions. Becker *et al.* (1960) modeled children as normal durable goods within the standard household consumption model. The price of children in this context consists of lifetime investments, including education and parental time. Because children are understood as normal goods, a positive shock to income should increase childbearing.

Recent studies provide evidence for the hypothesis that children are normal goods. Lindo (2010) and Amialchuk (2011), for example, find that parental job loss associated with plant closures reduces completed fertility, while Black *et al.* (2011) find that higher paternal wages increase completed fertility. An increase in parental wages raises the opportunity cost of parental time, however, potentially offsetting the wealth effect.

Another related strand of literature focuses on the relationship between fertility and the business cycle. For example, Dehejia and Muney (2004) show that infant health is countercyclical and explain this pattern using trends in fertility. They find that low-income mothers, who are more likely to have low birthweight infants, tend to reduce fertility during recessions due to liquidity constraints. High-income mothers increase fertility during recessions because their opportunity cost of time is high during booms. Dehejia and Muney (2004) as well as the other studies cited do not consider birth intervals as an outcome of interest.

3 Theoretical Framework

In the framework below, households make spacing decisions by trading off the health costs of small birth intervals against the mother's desire to limit time out of the labor market. A basic wage subsidy raises the value of time spent in the labor market, incentivizing families to space birth closely. The more complicated structure of existing wage subsidy programs, such as the EITC, may have differential effects on spacing by income level, fertility or marital status.

In the set-up below, I assume that mothers are the primary caregiver of children and children require a fixed number of hours when young. As a concrete example, children may be considered particularly "time-intensive" when less than 5, the kindergarten entrance age. Cascio (2006) finds large increases in maternal labor supply among single mothers when their children enter kindergarten, for example.

In addition, I assume that maternal time has increasing returns to scale when there are multiple young children in the household at the same time. The simplest way to model this would be to assume that two children at a time require the same time commitment as one child. Then, a mother with two children spaced one year apart faces t years of labor supply limits, whereas a mother with children spaced two years apart faces $t+1$ years of labor supply limits.

Note, in addition, that I am holding completed fertility constant in this discussion. This is for simplicity and because the variation in transfers I am able to identify is small relative to the costs associated with childbearing. I will be able to test for effects on completed fertility in the empirical section.

The model below has three periods and roughly corresponds the natural experiment used in this article. Households A and B receive a wage in period 1, w_1, and both have their first child in period 2. Upon the birth of their first child, their wages for period 2 and period 3 are realized. Household A receives w_1, w_2 and household B receives w_2, w_2, where $w_2 > w_1$. Each household then decides whether to have their second child in period 2 (small birth interval) or period 3 (larger birth interval).

Having n of children in a given period is associated with a cost $f(n) * w$, which represents the fraction of maternal wages lost, where $0 < f(n) < 1$ and $k * f(n) > f(k * n)$, implying decreasing costs. For simplicity, I choose $f(n) = (1-c)^k$, where c is a fixed constant between 0 and 1. τ denotes the health cost associated with having two children in the same period.

The table below shows the wages in each time period for two households, A and B. I now develop a solution in which A spaces and B does not space and discuss the predictions in terms of my data.

A Anticipates Refund

	Period 1	Period 2	Period 3
Household A	w1	w1	w2
Household B	w1	w2	w2

Suppose that A decides to space the births, meaning that they have the third child in period 3. The pay-off is given by:[2]

$$w_1 + w_1(1-c) + w_2(1-c)$$

If A doesn't space and has both children in period 2, the pay-off is:

$$w_1 + w_1(1-c)^2 + w_2 - \tau$$

Simplifying, A will space if the first pay-off is greater than the second pay-off, or:

$$\tau - w_1c^2 > (w_2 - w_1)c$$

Intuitively, the net cost of not spacing: the health cost minus the marginal decrease in cost through increasing returns to scale has to be greater than the net wages lost (cost of spacing).

Similarly, B receives the following if they choose to space:

$$w_1 + w_2(1-c) + w_2(1-c)$$

but gets the following if they choose not to space:

[2]For simplicity, I assume utility is linear in wealth. I discuss the implications of this further below.

$$w_1 + w_2(1-c)^2 + w_2 - \tau$$

Household B will not space if

$$w_2 c^2 > \tau$$

Intuitively, the health cost has to be smaller than the wage gain of spacing closely, which comes through increasing returns.

So the range of τ such that B is incentivized to *not* space, while A spaces, given the chosen cost function, is:

$$w_1 c^2 + (w_2 - w_1)c < \tau < w_2 c^2$$

Under this solution, the decision to have children within the same period, given c and τ, is not monotonic in the difference between w_2 and w_1. Note that for some large as well as small values of $w_2 - w_1$, both A and B will both choose not to space. Given that my research design models B's decision to space relative to A's as an increasing function of $w_2 - w_1$, my estimated effects may therefore be biased downward.[3]

The framework above assumes that households correctly anticipate their future returns. Recent evidence suggests that the majority of filers do not understand their returns, however; reliance on tax preparers is common, for example, especially among low-income filers (Chetty and Saez, 2009; Lalumia *et al.*, 2012; Kopczuk and Pop-Eleches, 2007). Further, dependent eligibility is particularly misunderstood for a household's *first* child (Lalumia *et al.*, 2012).

Incorporating limited information of dependent-related tax savings for household A changes the pay-offs. Suppose A does not correctly anticipate the increase in returns in period 3. The pay-offs from the stand-point of period 2, when the spacing decisions are made, are given below.

Now, the condition for household A to space becomes:

[3] I should not get an opposite-signed effect under this framework, as B never chooses to space for increases in $w_2 - w_1$.

A Does not Anticipate Refund

	Period 1	Period 2	Period 3
Household A	w1	w1	w1
Household B	w1	w2	w2

$$\tau > w_1 c^2$$

Note that household A is more likely to space since the *perceived* wage loss of doing so, from the standpoint of period 2, is less. Since the condition for B to not space is still the same, the resulting inequality becomes:

$$w_2 > \frac{\tau}{c^2} > w_1$$

so that the likelihood that B does not space and A spaces increases in $w_2 - w_1$.

3.1 Further Discussion on Spacing

This simplified model does not consider the possibility that the subsidy rate depends on hours worked. The EITC schedule, for example, has two "kinks" (changes in the subsidy rate), incentivizing an increase or decrease in hours for certain income ranges, as mentioned above. As I will show, it is empirically more likely for unmarried than married mothers to be located on the upward sloping part (where work is incentivized), so I would expect a larger decrease in spacing for single mothers.

Secondly, the discussion above assumes that utility is linear in wealth. Consider that married women are likely to have higher household income given the presence of another earner. If this translates to a lower marginal utility of consumption for married women, the costs of lost wages may be greater for unmarried women. Therefore, it may be that single women are more likely to space closely than married women.

Finally, note that what determines τ is, in fact, the household's *understanding* of health effects of spacing. Recent empirical evidence suggests that knowledge of health costs of certain behaviors, such as smoking, varies with education level (Aizer and Stroud, 2010; Currie, 2011). Given the findings above that less-educated women are more likely to space closely, it may be that knowledge of the effects of spacing varies with parental education level. If τ is lower for less-educated mothers, the likelihood of close spacing should be higher among this group.

3.2 Completed Fertility

It is conceivable that an increase in labor market incentives may also lower completed fertility by increasing maternal time-cost. This may be particularly true among mothers who prefer not to space closely. I will test for spacing and fertility effects separately below.

4 Data Description

I use three sources of data: Vital Statistics Births data from the State of Texas, Nielsen Homescan Consumer Panel, and the American Community Survey. I chose these datasets because they include variables necessary for my identification strategy: precise timing of all births linked to a given household as well as demographic information about the household.

The Texas births data, which I have for 1990-2004, is my main source of data. These data consist of the universe of birth certificates issued in the state each year. These data contain detailed information on birth outcomes, medical procedures, and maternal demographics and health, as well as geographic and time information. The most important variable for this analysis is "month and year of last live birth." These variables were originally collected to study child spacing and have not been widely used.

The variables on "month and year of last live birth" are of high quality. Among 2nd and higher parity births, information about last live birth is missing only 3.0% of the time. Importantly, missing information is likely to be rare among mothers who had their last child within the last year or two, the sample upon which I focus. In addition, I do not observe any stacking at certain months, years or month-year combinations, and last birth frequencies by month for 2nd parity roughly follow the seasonal pattern observed in 1st parity births.

I limit the TX sample to singleton births with non-missing values for parity, gestation, date of last live birth, and county or state of residence. I keep births of 2nd parity or higher only, as my empirical design depends on older sibling's birth month (so having an older sibling is necessary). In addition, I drop mothers who live outside Texas.

I calculate the date of conception for each birth record from birthdate and gestation length. I then add up all conceptions occurring within 12 months of each year/month of previous births (i.e. the birthdate of the older sibling). Now the observation level of the data is year and month of the older sibling's birth. If there are any missing "previous" year/months, I add an observation for that year/month and code the 12-month conception sum as a 0.[4]

The 12-month conception sum serves as a measure of short term fertility. I am unable to observe completed fertility in this data because I can only link each birth record to at most one previous birth (i.e. at most two siblings are linked together). In order to test whether effects on the 12-month conception sum represent changes to spacing or completed fertility, I will look at effects on 12-month conceptions among upper parities and also use the other datasets, in which I am able to approximate completed fertility. Changes to upper parities conceptions in the Texas are more likely to be indicative of changes to completed fertility than changes to lower parity.

Second, the Nielsen Homescan Consumer Panel is a dataset that tracks consumers' grocery purchases by asking consumers to scan barcodes of purchased products at home after each shopping trip. Participation is incentivized by prizes and entry into sweepstakes competitions, and households choose to remain in the panel as long as they want. The sample is a drawn from around the U.S. in such a way that it matches demographic characteristics at the national and Census Region level each year. I use data from 2004-2009, which contains 125,000 households.

Importantly for my purposes, the Nielsen data contain demographic information on each member of the household, including birthdate, age, education, and relationship to the household head. I use only these variables and drop information on purchasing. To approximate completed fertility of the household head, I count the number of biological children (no age restriction) living in the household at the time of the survey. Given that older children may exit the household, this measure will underestimate completed fertility. As explained

[4]The method used here assumes there exist previous births in that year/month. I have tried many alternate methods, including imputing year/month of previous birth using information directly from births of a lower parity. This assumes maternal characteristics do not change in between births. The results are very robust in general to these imputation methods. See Appendix C for alternative results as well as additional technical points regarding the calculation of these sums

below, I have experimented with using a number of age restrictions on children and head of household to check the robustness of my results.

I keep households in which the household head is between ages 40 and 55, in order to identify households which have likely completed fertility. In my preferred set of results, I also require that households have at least one child under age 20, in order to avoid cases in which underestimates of completed fertility are most likely. To measure spacing, I calculate the distance in months between the first and second child, the second and third child, etc.

Third, I use the American Community Survey (ACS) for the years 2005-2011. The advantages of the ACS are its large size (it is a 1-100 random sample of the population in the years I use) and individual level data on income. The drawback is that timing of children's birth is only recorded at the quarter level. Given evidence on the seasonality of birth outcomes and parental characteristics (Buckles and Hungerman, 2008), it is more difficult to draw inferences based on birth timing.

The ACS contains information previously collected in the long form of the decennial census, which includes important demographic information for all occupants of a given household. I approximate completed fertility by counting children living in the household. The large size of the ACS enables me to use a tighter age requirements (e.g. 40-45) for the household head as well as the children than is feasible with the Nielsen sample. I provide more detail below.

In all three datasets, I re-center the children's dates of birth around the end of the tax year, re-defining year of birth as starting in July (Q3) of a given year and ending in June (Q2) of the next year. To create a balanced panel of these years, I drop the first 6 months of the earliest year of oldest child's birthday and the last 6 months of the last year of oldest child's birthday. Finally, note that the Nielsen and ACS samples are at the household level, whereas the Texas sample is collapsed to counts at the previous child's year-month of birth as well as county of residence.

4.1 Simulating Household Income and Tax Measures

Households with qualifying dependents can file for several different tax benefits. These include the dependent exemption, the Earned Income Tax Credit (EITC), and the Child

Tax Credit (CTC). Table 1 gives eligibility rules, award amounts and a brief history of these benefits. Many low-income households will only benefit from the EITC since they have no tax liability.[5] I focus my discussion on low-income households and the EITC, providing more specifics on which households receive which tax benefit below.

In order to use tax savings in the analysis, I impute household income (births data) and tax liability (ACS and births data).[6] I simulate tax liability using NBER's TAXSIM program (Feenberg and Coutts, 1993). Below, I briefly outline my approach.[7]

To first impute income for the Texas births sample, I use the Census 1990 5% sample and keep women ages 18-45 living in Texas. I inflate household income forward for the years 1991-2004 using CPI from the Bureau of Labor Statistics. By holding the distribution of income constant and allowing only inflation to vary, I avoid including any endogenous responses to tax and transfer changes.[8]

I then approximate tax liability using TAXSIM in both the Census and ACS samples. More specifically, I want to calculate changes in tax liability associated with an extra dependent. To do this, I first duplicate each household observation, adding a child (dependent) to the second observation. I then calculate income tax liability by entering marital status, number of children and household income into NBER's TAXSIM program.[9] I then subtract tax liability for the first observation from the second observation; this gives me the tax gain associated with claiming an additional dependent in a given year. As a final step, I collapse the Census estimates to the following cells: parity, race, education, age, marital status and merge it to the births data.

Table 1 shows the calculated yearly tax gain of claiming an additional dependent on one's tax returns in both the 1990 Census and the ACS 2005-2011. The figures are broken down by education and marital status of the household head. Note that the estimates are not very comparable across datasets due to several important differences in sample construction.[10]

[5] Although the CTC is partially refundable now, it was non-refundable before 2001

[6] I do not simulate tax liability for Nielsen households due to sample size.

[7] More detail is available in Appendix D.

[8] This is a standard technique used in public finance literature. See Gruber and Saez (2002), for example.

[9] I assume married women file jointly; unmarried, childless women file as singles; and unmarried women with children file as head of household.

[10] The differences between the two samples used to make this table are as follows: the Census contains data from 1990, inflated forward to 2004, while the ACS contains data from 2005-2011. Estimates are not

From Table 1, it is clear that an additional qualifying dependent can generate substantial tax savings, with estimates ranging from 622.97 to 2741.89 per year and 0.0656 to 0.1630 of yearly income. Returns are higher for unmarried than married, and (as a fraction of income) for low-education than high-education households. For single-headed households with less than a high school education, for example, an additional dependent generates from 0.0836 (Census) to 0.1630 (ACS) of yearly households income.

Low-education households are likely to qualify for the EITC. Figure 1, which shows the EITC parameters for 2012, demonstrates that tax savings in the EITC are not distributed as lump sums, but as function of labor supply. This design creates varying incentives depending on one's taxable income, as discussed above. In the ACS sample for 2011 with tax simulations, single household heads with less than a high school diploma and at least one child have median income 15595 (75th is 26900), implying that at least half of the distribution will be located approximately on the upward sloping part of the 2012 schedule. For married households with less than a high school diploma and at least one child have median wages of 35400 (25th is 21600), implying that most of these households will be located on the downward sloping part.

4.2 Empirical Evidence on Spacing from Births Data

In this section, I examine the extent to which households in my dataset space births closely. A substantial fraction of households conceive within 12 months of their previous birth. In the Texas 1990-2004 sample, 19.19% of 2nd parity births are conceived within 1 year of the previous birth. In the Nielsen sample, 25% of households conceive their second child within 24 months of the first child. It appears a significant fraction of households space births closely.[11]

adjusted for inflation. The ACS data contains all households with non-missing education and marital status, whereas the Census contains women aged 18-45 only, as I need to match estimates from the Census to mothers in the births data. The Census consists only of the state of Texas, which has no state income tax, exemptions or credits.

[11] One factor mitigating conception in the postpartum period is that women can experience temporary infertility if they exclusively breastfeed (effects last up to 6 months, which is smaller than my 12 month conception window). Only around 10% of women are exclusively breastfeeding at 6 months in the U.S. from 2003-2005, however. Sources: www.plannedparenthood.org/health-topics/birth-control/breastfeeding-4219.htm and www.cdc.gov/breastfeeding/data/nis_data/ In addition, there are 6 remaining months of my 12 month time period for those couples who are exclusively breastfeeding to conceive.

5 Empirical Design

I empirically test my hypotheses using a regression discontinuity in birthdate of previous child. The cut-off is January or Quarter 1. The outcomes considered are space between previous and current child or total fertility.

5.1 Texas Births Sample

The regression specification I use with the Texas births data is the following:

$$C_{ymg} = \alpha + \beta receivebenefits_{ym} + \gamma f(month) + \alpha_t + \delta_s + \rho_g + \nu_g y + \epsilon_{ymg} \tag{1}$$

where C is the sum of conceptions over the next 12 months; m denotes month of the last live birth (i.e. the older sibling's birth month); y denotes the year, re-defined around January; and g denotes location of mother's residence (state or county). $month$ is equal to the difference between month of last live birth and January of the filing year (treated households have a last birth before Jan.);[12] $receivebenefits_{ym}$ indicates month of last live birth is December of the filing year or earlier. α_y are tax year fixed effects, δ_s are season of last birth fixed effects,[13] ρ_g are location fixed effects, and $\nu_g * y$ are location specific linear time trends. Standard errors are clustered on the relevant geographic level (state or county). I replace C_{ymg} with $\ln\{\max\{C_{ymg}, 0\}\}$ in several regressions, as this form deals 0 values of C_{ymg} without distorting the form of the desired log function for non-zero values.

This regression discontinuity design (RD) identifies a causal effect by comparing similar households on either side of the cut-off (January, coded as 0). The RD assumptions are compromised if households sort around the cut-off. The timing of previous births may be manipulated through c-section and induction of labor. I address this in detail in the last section.

I then interact the RD coefficient with simulated tax gain of an additional qualifying dependent. This further tests my hypotheses by adding an additional source of variation

[12] Subscript m is equivalent to $month$

[13] Codes for seasons (winter, spring, summer, fall) are as follows: 1 = December, January, February, 2 = March, April, May, 3=June, July, August, 4= September, October, November.

to Equation 1, helping to mitigate concerns that my results may be driven by other factors that vary across the New Year.

$$C_{ymgep} = \alpha + \beta receivebenefits_{ym} * gain_{yep} + \phi receivebenefits_{ym} + \sigma familyincome_{ygep}$$
$$\psi gain_{yep} + \gamma f(month) + \Theta' EducXPar + \alpha_y + \delta_s + \rho_g + \nu_g y + \epsilon_{ymgep} \tag{2}$$

where y, m, and g stand for year, month and geography as above and e and p stand for education and parity subsets. $gain_{yep}$ is the simulated tax benefit of claiming an additional dependent for given values of tax year and month of previous child's birth as well as education of mother and parity of previous child. $familyincome$ is the simulated measure of household income from the Census. $EducXPar$ is a vector of interactions between education and parity. I control for education, parity and family income as controls to adjust for differences in socioeconomic status in my overall estimates of fertility.[14]

5.2 Nielsen Sample

The Texas births regressions will be informative about short term fertility, and I use the Nielsen sample to understand whether these effects reflect changes to spacing or completed fertility. I estimate the following equation:

$$\ln(SP12_h) = \alpha + \beta receivebenefits_{ym} + \gamma f(month) + \Theta' Controls_h + \alpha_y + \delta_s + \rho_g + \nu_g y + \epsilon_h \tag{3}$$

where the outcome is the log of months between the first and second child in the household; m and y denotes month and year of the oldest child's birth (re-centered around January) and g stands for state of residence of the household and h is household id. I include as controls: indicators for race, marital status and Hispanic ethnicity of the head, age of head and spouse, education of head and spouse and the panel year the household is observed last. Standard errors are clustered on the state level.

[14] In unreported results from estimating Equation 1, I find that the fertility-income response varies importantly across education and parity subsets. In addition, I want to take advantage of income variation by education and tax savings by parity.

I then estimate Equation 4 to test whether there are effects on completed fertility:

$$TC_h = \alpha + \beta receivebenefits_{ym} + \gamma f(month) + \Theta' Controls_h + \alpha_y + \delta_s + \rho_g + \nu_g y + \epsilon_h \quad (4)$$

where TC is the total children in the household; m and y denotes month and year of the oldest child's birth (re-centered around January) and g stands for state of residence of the household and h is household id. I include as controls: indicators for race, marital status and Hispanic ethnicity of the head, age of head and spouse, education of head and spouse and the panel year the household is observed last. Standard errors are clustered on the state level.

Note that the sample used for Equation 3 consists of households with at least two children, whereas the sample used for Equation 4 includes all households with at least one child.

5.3 ACS Sample

Finally, I use the ACS to provide a second test of completed fertility.

$$\begin{aligned} TC_h = \alpha + \beta receivebenefits_{yq} * \ln total_h + \phi receivebenefits_{yq} + \psi \ln total_h + \\ \gamma f(quarter) + \Theta' Controls_h + \alpha_y + \delta_s + \rho_g + \nu_g y + \epsilon_h \end{aligned} \quad (5)$$

where TC is the total children in the household; q and y denotes quarter and year of the oldest child's birth (re-centered around the first quarter) and g stands for state of residence of the household and h is household id. I include as controls: indicators for race, sex, marital status, Hispanic ethnicity and education of the head and the survey year. Standard errors are clustered on the state level. Because quarter of birth is a very rough measure, I do not estimate spacing regressions with the ACS.

5.4 Graphical Evidence

Figure 3 plots raw sums of 12-month conceptions against previous child's birth month, by mother's education. Figure 3 shows a clear jump downward at January in the full sample, meaning lower short-term fertility among mothers not receiving tax benefits. This pattern is replicated in the sample of births to mothers with less than a high school education, but there is no effect in the high-education sample.

5.5 Regression Evidence

5.6 Texas Births Results

Tables 3 presents estimates of β from Eqn. 1. A positive β means more conceptions among households with qualifying dependents. I include as the control function a linear trend in month of previous birth, allowing the slope to vary on either side of the discontinuity. This functional form seems to best fit the graphical evidence in Figure 1.[15]

Table 3 shows that households with a qualifying dependent are 3.19-11.06% more likely to conceive another child in the 12 months following the previous child's birth. The coefficients are larger and more precise for low-education mothers, at 5.92-12.70%. This follows my predictions above.

Next, I estimate Equation 2, which interacts the RD with variation in tax transfers (Tables 4 and 5). A positive β means more conceptions among households with qualifying dependents *and* higher transfers. I use the following forms of the outcome and transfer amount, respectively: level-level and log-level. Level-level provides information about direction and precision of estimates, but the magnitudes are hard to compare across subsamples without knowing additional information about means, etc. I therefore use log-level as my preferred specification.[16]

Note also that putting the outcome in logs will reduce the precision of the coefficient estimates in this case. This is due to the fact that the average cell size is small (4.3 conceptions). Percentage changes off a low base have high variance by construction, especially when the range is restricted to integers. I think of the level-level results as more indicative of precision and the log-level results as more indicative of magnitude.

Table 4 and Table 5 present estimates of β, ϕ and ψ from Equation 2 for different subsamples of mothers by education. Like in Table 3, there is no evidence of a fertility effect for high-education mothers. For low-education mothers, the level-level specifications produce

[15] Appendix E shows that the regression results are highly consistent across variations of $f(month)$.

[16] Another option is log-log, which would give the effects of a 1% benefit amount. I prefer to generate the effects of 1$ given variance in benefit amounts across subsamples.

positive and significant coefficients, while the log-level results are positive and imprecise. The magnitude of the log-level regressions for the low-education sample suggest that 1$ increase in yearly transfers increases the relative likelihood that households will conceive in the following 12 months by 1-2%. Recall from the discussion above that the percentage of households which conceive within 12 months of their previous birth is 19.19%.

Table 5 divides up the sample of low-education mothers by marital status. For unmarried mothers with a high school diploma or less, both specifications (level-level and log-level) produce positive and highly significant results. The log-level coefficients are again about 1-2%. Recall from the discussion above that there are reasons to expect stronger results for unmarried versus married mothers in the low-education sample. This prediction is partially born out here–at the lowest bandwidth the log-level result is imprecise for married mothers only. I will continue to test this part of my hypotheses.

5.7 Nielsen Results

I next present results from estimating Equations 3 and 4 using Nielsen data (Tables 6 and 7). This allows me to test (a) whether I can replicate my results using births data on short-term fertility and (b) whether such changes in short-term fertility represent movements to spacing or completed fertility. Because of the relatively small size of the dataset, Nielsen coefficients are estimating on the largest bandwidth in first child's birth month (July-June).

The negative coefficients in Table 6 mean that families whose first child is born before the end of the tax year tend to space their second child more closely. The coefficient on the RD is larger for households in which the head has a high school diploma or less. In the subsample of low-education households in which the head is unmarried, the coefficient is much larger and highly significant. It implies that for these households, an expansion in tax benefits decreases spacing by 56%.[17]

These results indicate that the births results are at least partly driven by a reduction in spacing, and that these effects are concentrated among low-income, single-headed households.

[17]That the coefficient in the <=HS sample is less coefficient and much smaller may be explained by the facts that all married heads are male whereas most unmarried heads are female (72.6%), and education is measured with respect to the head. The trade-offs discussed in the theoretical framework, between labor supply and spacing, apply primarily to mothers, so education of the head is imprecise measure of mother's labor market incentives.

Table 7 tests whether there are effects on completed fertility, proxied by total children in a given household, as well. The results shown indicate that completed fertility may be low for families who receive tax benefits associated with their first child earlier. The coefficient in the full sample indicates a 2% decrease in completed fertility, significant at 10%. The largest effect (a 7% decrease) is for the low-education, single moms, but it is not precisely estimated.

As discussed above, it may be that receiving a higher EITC one year earlier causes lower fertility overall. To the extent that the higher EITC incentivizes labor force participation one year earlier, mothers who choose not to space closely may have fewer children overall. In the next section, I test the robustness of the results on completed fertility using the ACS.

5.8 ACS Results

Recall that the ACS has the benefits of being a very large survey with household level data on income. This means that I can use precise variation in income. The main drawback is that household birth dates are only recorded at the quarter-year level, which means I run the risk of conflating effects with those of seasonality, as mentioned above.

Table 8 presents results of estimating equation 5 on the sample of household heads aged 40-55.[18] There appears to be no effect on completed fertility. The coefficients on β are inconsistently signed and very imprecise. These results are in line with previous research showing no effects of the EITC on subsequent fertility (e.g. Hoynes *et al.* (2011)).

6 Shifting Births Across the Tax Year

A couple of papers ask whether potential tax savings motivate households to shift births back from the first week in January to the last week in December through elective procedures such as c-section or labor induction (Dickert-Conlin and Chandra (1999); Lalumia *et al.* (2012)). This behavior could potentially compromise my results to the extent that it causes stacking in my running variable, month of previous birth, at the end of the tax year.

Dickert-Conlin and Chandra (1999) consider a sample of 170 births and find that a 10 percent increase in child-related tax benefits (from \$401 to \$441 in 1996 dollars) increases

[18]Results are consistently imprecise across all 5, 10, 15, 20 and 25 year subsets of ages 35-65.

the probability of a December birth by 1.4 percentage points (from 51.6% to 53%). They predict that the implementation of a $500 child tax credit would cause late-December births to account for 65.5% of births in the two-week window. A $500 child tax credit was enacted in 1997, but aggregate birth records indicate that December's share of all births in the two-week window held stead at around 51%.[19]

In addition, two recent studies have cast doubt on these original findings. Maghakian and Schulkind (2011) estimate a much smaller effect of taxes on birth timing using aggregated birth certificate data and imputed tax values from the Census. Lalumia *et al.* (2012) use the universe of tax returns from 2001 and 2010 and also find small effects, that an additional $1,000 of tax savings is associated with a 1% increase. The authors conclude: "Our results cast doubt on the hypothesis that, over the last decade, large numbers of parents have strategically shifted the timing of childbirth in response to tax incentives."

Further, the tax benefits may not offset of the cost of the elective procedures needed to shift births.[20] In addition, shifting of births has not been observed for other, comparably large financial margins for which there is a birthdate cut-off, such as kindergarten entrance, which generates childcare savings (Dickert-Conlin and Elder, 2010).

It is also probable that there are other important factors that determine birth timing around the end of the year. For example, physicians may wish to avoid being the hospital during the New Year's holiday, and certain types of physicians may have more leverage. By far the largest change in births between the last week in December and the first week in January is from 1999-2000, when presumably both physicians and patients wish to avoid the emergency room.[21].

I directly test for stacking in births in December versus January. Figures A1-A5 in show that there is no pattern of an increase of December over January birth sums (Lalumia *et al.* (2012) also note this). I conclude that stacking in my running variable is not a serious threat to my empirical design.

[19] Source of these figures: Lalumia *et al.* (2012)

[20] Lalumia *et al.* (2012) write: "Data from the Healthcare Cost and Utilization Project carried out by the Agency for Healthcare Research and Quality indicate that, in 2010, the mean charge for vaginal delivery was $10,166 while the mean charge for a cesarean delivery was $17,052. Data are available at http://hcupnet.ahrq.gov. Naturally, insurance can shield a patient from paying this cost difference out-of-pocket."

[21] Source: Wingender (2009), Figure 2

7 Conclusion

In this paper, I consider the effects of the EITC on a new margin: birth spacing. EITC-eligible households receive a substantially increased refund upon the birth of their first child, providing stronger incentives for labor market participation. To the extent that mothers make spacing decisions by trading off their desire to limit time out of the labor market against health costs of small birth intervals, the EITC may decrease spacing. I argue that this effect may be particularly strong in light of evidence that households do not correctly anticipate their refunds. Using three different datasets containing child birth dates and fertility outcomes, I find that increased tax savings associated with a qualifying dependent decrease spacing among low-education, single mothers and have zero or slightly negative effects on completed fertility. My findings are important given recent evidence that small birth intervals deplete maternal nutrition, thought to be an important determinant of child human capital outcomes.

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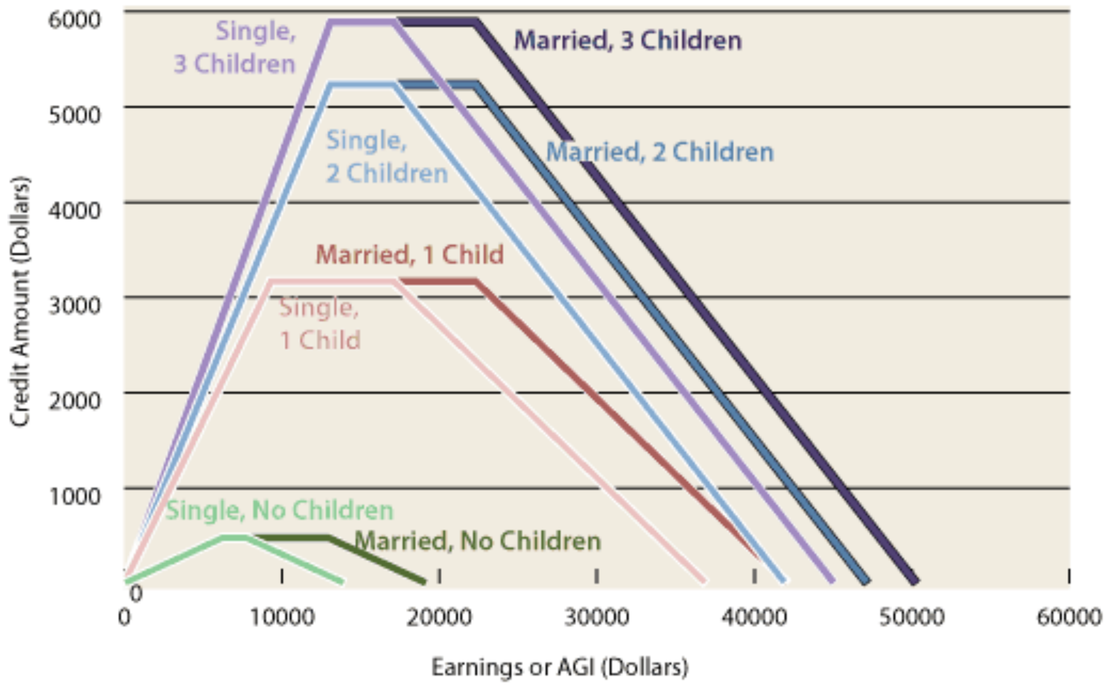
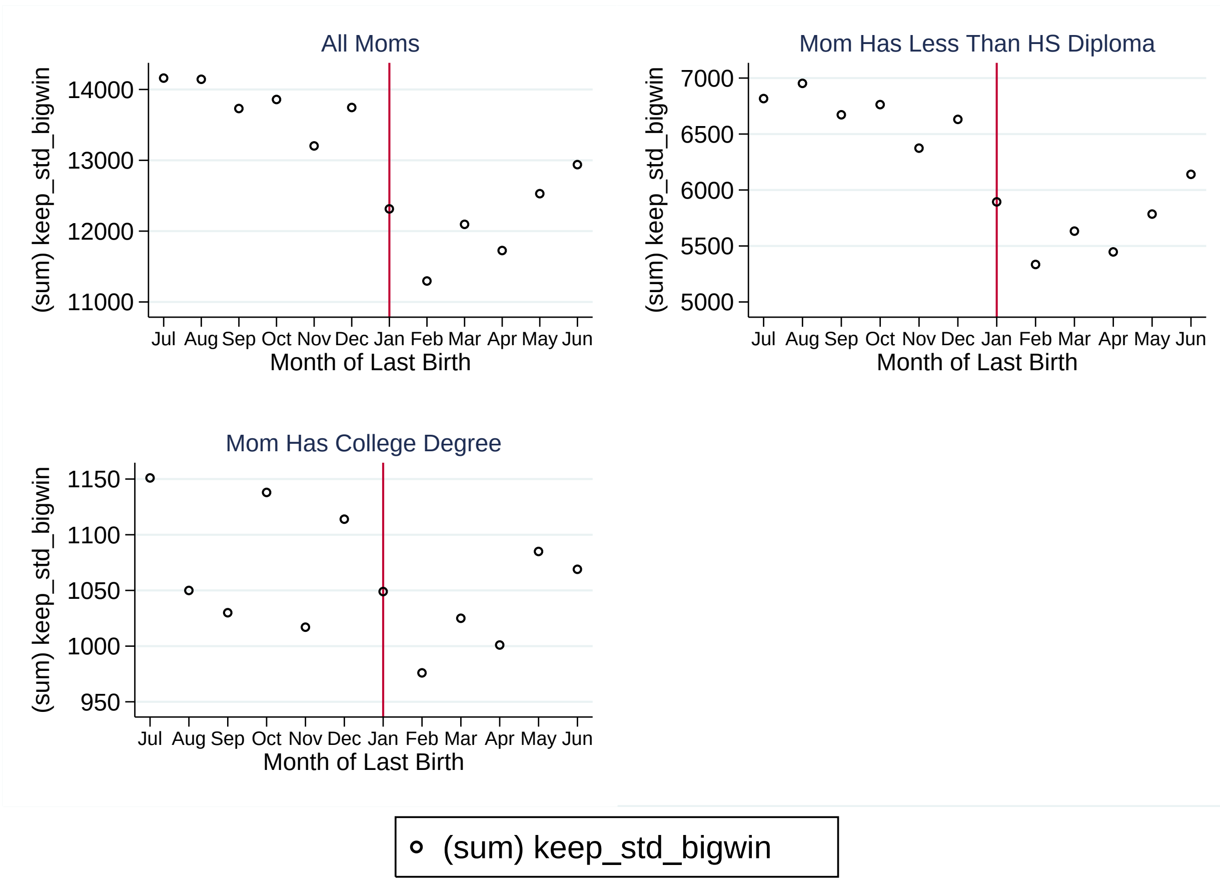
Figure 1. Earned Income Tax Credit by Number of Children and Filing Status, 2012



Source: 2012 EITC parameters taken from
http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=36

Figure 2: Tax Transfers and Fertility: Texas 1990-2004



Notes: Sample is drawn from the universe of conceptions of parity 2, 3, and 4 linked to a birth certificate issued in Texas from 1990-2004. Any observations with missing information on gestation or date of previous birth are dropped. Total conception counts are plotted against month of the previous birth. Total conceptions are counted either over the 12 months following the previous birth. Note the relevant filing year begins in January on the graph. See text for additional details.

Table 1: Tax Transfers Available to Households with Qualifying Dependents

Name	Refundable?	Amount	Dep. Age	Eligibility	Brief History
Dependent Exemption	No	$3,800	0-18	Households must file income taxes to be eligible.	Created in 1913, the dependent exemption was originally worth $3,000, or $66,367 in 2011$, compared with only $3,700 in 2011. By some calculations, it was the largest federal tax expenditure on children until the end of the 1960s. It subsequently declined greatly in value, as it was unadjusted for inflation until 1984.
Earned Income Tax Credit (EITC)	Yes	Max credit increases by $2,694 with first child; by $2,067 with second child; by $655 for the third child	0-19	The income limit is $50,270. The credit varies by income, family size, filing status and other factors.	The EITC began in 1975 as a small program and expanded dramatically through three tax acts: the 1986 Tax Reform Act (TRA86) and the Omnibus Reconciliation Acts of 1990 and 1993 (OBRA90, OBRA93). Today, these refunds can be quite large; for example, among families with two or more children,the average credit in 2008 was $2,563.
Child Tax Credit (CTC)	Partially	$1,000	0-17	Full credit is available for houseolds with incomes under $110,000. Partial credit for those with incomes from $110,000 to $130,000	The CTC was created in 1997 and worth $400 per child. Expanded in 2001 and 2009, it is now worth $1,000 per child.

Eligibility rules, descriptions and amounts are given as of tax year 2012. Rules pertaining to yearly income limits, qualifying age ranges and household size measure these outcomes as of December 31st of the previous year (the tax year). This table excludes an additional tax benefit available to households with dependents: the Dependent and Child Care Credit. The age range for dependents is not measured at the end of the year for this benefit, so its effects are no identified in my research design. For more information on precise eligibility rules, exceptions, benefit amounts and their history, please see: `http://www.taxpolicycenter.org/briefing-book/key-elements/family/index.cfm` and `http://www.irs.gov/uac/A-"Qualifying-Child"`

Table 2: Tax Savings with Add'l Qualifying Dependent

		Census Sample		ACS Sample	
Parental Education	Married	Gain	Gain / Income	Gain	Gain / Income
< HS Diploma	Yes	622.97	0.0278	1859.30	0.0650
	No	1073.16	0.0836	2387.26	0.1630
HS Diploma	Yes	780.79	0.0230	1689.26	0.0395
	No	1376.05	0.0747	2507.74	0.1230
Some College	Yes	836.46	0.0201	1693.69	0.0314
	No	1631.95	0.0778	2741.89	0.1120
College Plus	Yes	887.62	0.0156	1367.00	0.0179
	No	1767.51	0.0676	2632.80	0.0656

"Gain" refers to the estimated tax savings associated with claiming an additional dependent on one's tax returns. Tax information is simulated using NBER's TAXSIM model, entering information on income, marital status and number of children. "Income" refers to household income measured in the respective dataset. "Parental Education" refers to maternal education (Census sample) or education of the head of household (ACS). The Census sample is the 1990 5% public-use sample, including only female residents of the state of Texas aged 18-45, duplicated for the years 1991-2004 (income is inflated forward). The subsetting of the Census sample is done to match the sample to the Texas births data, in which information on mother is available. Observations in which information on education, marital status, income or children. The ACS sample consists of public use files for 2005-2011, at the household level, dropping observations with missing information on education, marital status, income, or children. Dollar amounts are not adjusted for inflation.

Table 3: Short Term Fertility, Texas Births Sample

		Concep'ns in 12 mos. after Prev. Birth		
Bandwidth of Prev. Births:				
Oct. to April	Eligible	0.0319+	0.0592**	-0.0538
		(0.0187)	(0.0277)	(0.0487)
	No. Obs	3500	3500	3500
	R2	0.2128	0.1207	0.0888
Sept. to May	Eligible	0.0915***	0.1092***	0.0433
		(0.0159)	(0.0222)	(0.0331)
	No. Obs	4500	4500	4500
	R2	0.1885	0.1042	0.0831
July to June	Eligible	0.1106***	0.1270***	0.0656**
		(0.0160)	(0.0222)	(0.0317)
	No. Obs	6000	6000	6000
	R2	0.1852	0.0966	0.0768
Mom's Education		All	LTHS	College+

Each set of four cells is from a separate regression and contains an estimate of β from Equation 1, its standard error, the sample size and R^2. The data is collapsed to the month-year-county of residence level (month-year of previous birth). The dependent variable is $\max(\ln C, 1)$, where C is the sum of conceptions over the 12 months following a given month/year of previous birth and for a given county. All specifications controls for county, season, year and month fixed effects as well as county specific linear year trends. Specifications include a linear control function in month of previous birth which is interacted with the RD cut-off. Standard errors are clustered at the county group level. The data sample consists of all singleton births with non-missing parity, gestation, date of last live birth and county of residence. + significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Short Term Fertility, Texas Births Sample

		Conceptions in 12 months after Previous Birth					
Bandwidth of Prev. Births:							
Oct. to April	Eligible*Gain	0.3064***	0.0056	0.1079	0.0034	0.4265***	0.0121
		(0.1045)	(0.0126)	(0.1206)	(0.0206)	(0.1433)	(0.0163)
	Eligible	-0.1053	0.0174	-0.0104	0.0033	-0.1118	0.0259
		(0.0892)	(0.0150)	(0.1143)	(0.0227)	(0.1151)	(0.0220)
	Gain	1.4069+	0.1285**	6.1912**	0.5896***	1.3101**	-0.0037
		(0.7844)	(0.0513)	(2.6328)	(0.1452)	(0.5554)	(0.0346)
	No. Obs	37134	37134	18292	18292	18842	18842
	R2	0.3023	0.4901	0.2741	0.3249	0.2830	0.3915
July to June	Eligible*Gain	0.3614***	0.0126	0.1795	0.0115	0.4675***	0.0173
		(0.0935)	(0.0084)	(0.1138)	(0.0171)	(0.1092)	(0.0104)
	Eligible	0.1905***	0.0588***	0.0874	0.0270	0.3550***	0.0856***
		(0.0582)	(0.0107)	(0.0728)	(0.0174)	(0.1015)	(0.0167)
	Gain	1.5067+	0.1368***	6.2956**	0.5941***	1.4322**	0.0072
		(0.8173)	(0.0510)	(2.6529)	(0.1441)	(0.6190)	(0.0289)
	No. Obs	58382	58382	28778	28778	29604	29604
	R2	0.3021	0.4924	0.2767	0.3271	0.2825	0.3905
Specification		Level-Level	Log-Level	Level-Level	Log-Level	Level-Level	Log-Level
Mom's Education		All		>=Some Coll.		<=HS	

Each set of cells is from a separate regression and contains estimates of β, ϕ, and ψ from Equation 2, their standard errors, the sample size and R^2. *Gain* is divided by 1000 in Level-Level regressions. "Bandwidth" is given in terms of months of birth of the previous child. The data is collapsed to the month-year-county of residence-maternal education-parity level. The dependent variable is a form of C, the sum of conceptions over the 12 months following a given month/year of previous birth and for a given county-education-parity cell. "Level-level" indicates both C and *Gain* are entered in levels, where C is transformed using the truncated log function: $Max(C, log(1))$, whereas "Level-Log" indicates that C is entered in levels and *Gain* is entered in log form. All specifications controls for county, season, year and month fixed effects, all parity*education interactions as well as county specific linear year trends. Specifications include a linear control function in month of previous birth which is interacted with the RD cut-off. Standard errors are clustered at the county level. The data sample consists of all singleton births with non-missing parity, maternal gestation, date of last live birth, and county of residence.+ significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Short Term Fertility, Texas Births Sample

		Conceptions in 12 months after Previous Birth			
Bandwidth of Prev. Births:					
Oct. to April	Eligible*Gain	0.1156**	0.0204**	0.2375**	0.0191
		(0.0466)	(0.0085)	(0.1098)	(0.0198)
	Eligible	-0.0662	-0.0001	0.0080	0.0218
		(0.0582)	(0.0134)	(0.0805)	(0.0221)
	Gain	0.5755***	0.0534+	-0.1344	-0.0622
		(0.1379)	(0.0303)	(0.2979)	(0.0484)
	No. Obs	18038	18038	18751	18751
	R2	0.2121	0.2145	0.2687	0.3382
July to June	Eligible*Gain	0.0908**	0.0161**	0.3133***	0.0400***
		(0.0401)	(0.0072)	(0.0732)	(0.0135)
	Eligible	0.1745***	0.0392***	0.2387***	0.0555***
		(0.0546)	(0.0129)	(0.0776)	(0.0164)
	Gain	0.7371***	0.0846***	-0.0840	-0.0554
		(0.1580)	(0.0262)	(0.2827)	(0.0426)
	No. Obs	28353	28353	29467	29467
	R2	0.2111	0.2143	0.2679	0.3372
Specification		Level-Level	Log-Level	Level-Level	Log-Level
Mom's Education		<=HS & Unmarried		<=HS & Married	

Each set of cells is from a separate regression and contains estimates of β, ϕ, and ψ from Equation 2, their standard errors, the sample size and R^2. *Gain* is divided by 1000 in Level-Level regressions. "Bandwidth" is given in terms of months of birth of the previous child. The data is collapsed to the month-year-county of residence-maternal education-parity level. The dependent variable is a form of C, the sum of conceptions over the 12 months following a given month/year of previous birth and for a given county-education-parity cell. "Level-level" indicates both C and *Gain* are entered in levels, where C is transformed using the truncated log function: $Max(C, log(1))$, whereas "Level-Log" indicates that C is entered in levels and *Gain* is entered in log form. All specifications controls for county, season, year and month fixed effects, all parity*education interactions as well as county specific linear year trends. . Specifications include a linear control function in month of previous birth which is interacted with the RD cut-off. Standard errors are clustered at the county level. The data sample consists of all singleton births with non-missing parity, maternal gestation, date of last live birth, and county of residence.+ significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Spacing, Nielsen Sample

		Log(Months Between Births 1 & 2)			
Bandwidth of Prev. Births:					
July to June	Eligible	-0.0515+	-0.0786	-0.5652**	-0.0339
		(0.0265)	(0.0672)	(0.2571)	(0.0345)
	No. Obs	11049	3119	380	7930
	R2	0.0874	0.0941	0.3496	0.0949
Mom's Education		All	<=HS	<=HS, Single	>=Some Coll.

Each set of four cells is from a separate regression and contains estimates of β from Equation 3, standard errors, the sample size and R^2. The outcome is the number of months between the first and second of a household's birth, logged. "Bandwidth" is given in terms of months of birth of the previous child. The sample consists of all households in the Nielsen data, 2004-2009, in which the head is between ages 40-55, there are at least two children in the household and one of the children is under 20. The level of observation in the sample is the household unit. All specifications control for state, season, year and month fixed effects, as well as state specific linear year trends. In addition, the following are included as controls: indicators for race, marital status and Hispanic ethnicity of the head, age of head and spouse, indicators for education of head and spouse (where not subsetted by education) and the panel year the household is observed last. Specifications include a linear control function in month of previous birth which is interacted with the RD cut-off. Standard errors are clustered on the state level. + significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Completed Fertility, Nielsen Sample

		Total Children in Household			
Bandwidth of Prev. Births					
-6 to 5	Eligible	-0.0289+	-0.0336	-0.0720	-0.0268
		(0.0156)	(0.0293)	(0.0732)	(0.0192)
	No. Obs	17939	5274	840	12665
	R2	0.1887	0.1828	0.2000	0.1960
Mom's Education		All	<=HS	<=HS, Single	>= Some Coll.

Each set of four cells is from a separate regression and contains estimates of β from Equation 4, standard errors, the sample size and R^2. The outcome is the total number of children living in the household. "Bandwidth" is given in terms of months of birth of the previous child. The sample consists of all households in the Nielsen data, 2004-2009, in which the head is between ages 40-55, there is at least one child is under 20 in the household. The level of observation in the sample is the household unit. All specifications control for state, season, year and month fixed effects, as well as state specific linear year trends. In addition, the following are included as controls: indicators for race, marital status and Hispanic ethnicity of the head, age of head and spouse, indicators for education of head and spouse (where not subsetted by education) and the panel year the household is observed last. Specifications include a linear control function in month of previous birth which is interacted with the RD cut-off. Standard errors are clustered on the state level. + significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Completed Fertility, ACS Sample

		Total Children in Household			
Bandwidth of Prev. Births :					
Q3 to Q1	Eligible*Gain	-0.0001	0.0008	0.0001	-0.0006
		(0.0009)	(0.0020)	(0.0032)	(0.0011)
	Eligible	0.0210+	0.0159	0.0078	0.0240+
		(0.0108)	(0.0216)	(0.0325)	(0.0125)
	Gain	-0.0095***	-0.0288***	-0.0264***	0.0008
		(0.0016)	(0.0032)	(0.0030)	(0.0017)
	No. Obs	896175	151973	42839	643040
	R2	0.1627	0.1454	0.0889	0.1712
Mom's Education		All	<=HS	<=HS, Single	>= Some Coll.

Each set of four cells is from a separate regression and contains estimates of β from Equation 5, standard errors, the sample size and R^2. The outcome is the total number of children living in the household. The sample consists of all households in the ACS, 2005-2011, in which the head is between ages 40-55 and all children in the household are under age 20. The level of observation in the sample is the household unit. All specifications control for state, season, year and quarter fixed effects, as well as state specific linear year trends. In addition, the following are included as controls: indicators for race, marital status, sex, age and Hispanic ethnicity of the head and the survey year.Specifications include a linear control function in quarter of previous birth. Year and quarter of first child's birth are re-centered around Q1, as described in the text. Standard errors are clustered on the state level. + significant at 10%; ** significant at 5%; *** significant at 1%