The Effects of a Universal Child Benefit

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Abstract

I study the impact of a universal child benefit on fertility and family well-being. I exploit the unanticipated introduction of a new, sizeable, unconditional child benefit in Spain in 2007, granted to all mothers giving birth on or after July 1, 2007. The regression discontinuity-type design allows for a credible identification of the causal effects. I find that the benefit did lead to a significant increase in fertility, as intended, part of it coming from an immediate reduction in abortions. On the unintended side, I find that families who received the benefit did not increase their overall expenditure or their consumption of directly child-related goods and services. Instead, eligible mothers stayed out of the labor force significantly longer after giving birth, which in turn led to their children spending less time in formal child care and more time with their mother during their first year of life. I also find that couples who received the benefit were less likely to break up the year after having the child, although this effect was only short-term. Taken together, the results suggest that child benefits of this kind may successfully increase fertility, as well as affecting family well-being through their impact on maternal time at home and family stability.

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1. Introduction

Governments in many countries offer cash benefits to families with young children.[1] The explicit goals of these programs typically include encouraging fertility and/or improving the well-being and long-term opportunities of children. However, the success of these policies in achieving their goals, as well as any potential side-effects, has been hard to evaluate.

The main challenge in estimating the effects of a child benefit is, as usual in policy evaluation, to come up with a credible "counterfactual". What would the fertility rate have been in country X in the absence of the child benefit? What about average child outcomes? The literature has typically followed difference-in-differences strategies, where one compares families with children before and after the introduction or expansion of a child benefit, and uses other regions or non-eligible families as controls.[2] However, both kinds of control groups may suffer from comparability issues, and it is hard to rule out other sources that may be responsible for the different trajectories of the treated and control groups.

The ideal "experiment" that these research designs try to replicate would work as follows: some families would be randomly selected to receive the benefit, say at the time of the birth of a child, and then one would compare the treated and untreated families over time along the relevant dimensions.

In this paper, I exploit a natural experiment that credibly replicates a randomization of the sort described above, where mothers who give birth are "as if" randomly assigned to a

[1] As reported in Milligan (2005), families with children received some kind of benefit (through either transfers or taxes) in 28 out of the 30 OECD countries in 2002.
[2] See, for example, Milligan and Stabile (2009, 2011) for child outcomes, and Milligan (2005) or Cohen at al. (2007) for fertility effects.

treatment group (who receives a large cash benefit) or a control group (that doesn't). The source of this randomization is the sharp cut-off established for benefit eligibility. Mothers were eligible if their child was born after a certain date, and this date was not announced beforehand. This setup lends itself naturally to a regression discontinuity analysis, where the treatment effect is given by the difference in outcomes between treated and control families, arbitrarily close to the cutoff.

The natural experiment in question was generated by the introduction of a new, universal child benefit in Spain in 2007. The cash benefit, to be paid to the mother immediately after birth, was announced on July 3rd, and all mothers giving birth from July 1st on were eligible to receive it. The benefit was a one-time payment of 2,500 Euros (about $3,800), or almost 4.5 times the monthly (gross) minimum wage for a full-time worker.

The explicit goals of the benefit were to increase fertility as well as to improve the well-being of families and children. Thus, I first analyze the potential fertility effects, and then move on to a range of outcomes related to family well-being, including expenditure patterns, labor supply, family stability, and health outcomes. In order to do so, I exploit a range of independent data sets with information on the different variables of interest.

The identification strategy is quite straightforward for all the family wellbeing-related variables. I compare, say, expenditure patterns, for households who had a child right before and right after the cutoff date. These families are statistically identical in all observable dimensions, except for the fact that some received the benefit and some did not. Thus, any "jump" at the cutoff birth date observed after benefit receipt can be attributed to the policy. This type of identification strategy is typically referred to as a "regression discontinuity design" (RDD).

It has been shown in previous research that there are seasonal changes in the characteristics of women giving birth throughout the year (Buckles and Hungerman 2008). In order to incorporate these concerns, I also estimate specifications that include multiple birth years and control for "seasonality" by including calendar month of birth fixed effects, thus supplementing the RDD approach with difference-in-difference (DiD) estimates.

Estimating the fertility effects of the policy poses a tougher challenge, since conception date is not observed in any available data set (most often, not even to the mother!). I use Vital Statistics data, which include date of birth and also weeks of gestation, to construct estimated conception dates, and thus look for a discontinuity in the number of conceptions after the benefit announcement date.

Perhaps even more appealing is the argument that, if the benefit encourages fertility, it should also discourage abortions. I collect detailed abortion statistics and analyze the incidence of abortions around the date when the benefit was announced. The conceptions and abortions analysis also supplements RDD with DiD specifications (with calendar month of birth fixed-effects).

The results indicate that the child benefit was successful in increasing fertility. I find a (positive) jump in the number of conceptions right after the benefit announcement date, as well as a discrete drop in the incidence of abortions. The magnitude of the estimated effects is sizeable, suggesting that the policy increased the annual number of births by at least 5 percent.

Regarding family well-being, I find that families that received the new child benefit did not increase their overall expenditure the year following childbirth. Child-specific expenditure was also unaffected. However, mothers who received the benefit were significantly less likely to be working the year after birth, with the labor supply effect dissipating by the child's second birthday. I also find that receiving the benefit led to significantly lower expenditure on formal day-care and fewer weekly hours of day-care. The benefit-driven increase in income thus appears to have led to changes in maternal time at home and day-care use during the child's first year of life.

I also find that the parents who received the benefit were less likely to separate during the first year after childbirth, and eligible mothers reported somewhat better health. I do not find any effect on (extreme) health indicators for children.

There are two main contributions of the paper. First, the regression discontinuity design allows for a more credible identification of the underlying causal effects, compared with the previous literature on the effects of family or child benefits. Second, I study the (short-term) effects of the benefit on a broad set of outcomes, including those explicitly targeted by the policy as well as others suggested by economic theory and previous literature. This paints a richer picture compared with most previous papers, which tend to focus on a single outcome of interest (be it fertility, maternal labor supply, child outcomes or expenditure patterns).[3] It also allows us to think about the potential channels that may be at play in generating any long-term effects on child outcomes. Finally, the analysis is also valuable given the virtual absence of empirical studies addressing the effects of changes in income on parental investments in children (Ginja, 2010).

The paper also contributes to a broader literature that addresses the effect of exogenous increases in income on a range of individual and household outcomes. A common source of

[3] See, for example, Milligan (2005) and Cohen at al. (2007) for fertility, Dahl and Lochner (2011) and Milligan and Stabile (2011) for child outcomes, Lundberg et al. (1997) and Ward-Batts (2008) for household expenditure patterns, and Milligan and Stabile (2009) for maternal labor supply. There is also, of course, a large literature concerned with the effect of different types of public benefits on female labor supply, and some of the papers in that literature also focus on mothers or single mothers.

such an exogenous income shock exploited in the literature is lottery winning.[4] Those studies, however, suffer from the limitations that, first, lottery players may not be representative of the overall population, and second, their results may not be typical responses to other forms of unearned income (Bagues and Esteve-Volart, 2010).

The remainder of the paper is organized as follows. Section 2 introduces some additional background on the policy change that gives rise to the natural experiment. It also details the identification strategy and describes the main data sources. Section 3 discusses the results for the different sets of outcomes, and section 4 concludes.

2. Methodology and data

2.1 Institutional setting

On July 3rd, 2007, the Spanish President announced during his "State of the Nation" address that a new, universal child benefit would be introduced. The new, one-time subsidy would pay 2,500 euros (slightly over $3,800)[5] to all new mothers, starting with those giving birth on or after the announcement date. The eligibility cut-off would subsequently be moved (for practical reasons) to July 1st. The proposal became law in November,[6] and the first "baby-checks", as they were referred to in the media, were paid in late November 2007.

The magnitude of the subsidy can be appreciated by comparing it with monthly earnings. The monthly gross minimum wage for a full-time job in Spain was 570.6 Euros in 2007, and about 20% of working women earned the minimum wage or below (2007 Wage

[4] Imbens et al. (2001) look at lottery-winning effects on labor supply, earnings, consumption and savings. Lindahl (2005) and Apouey and Clark (2011) study the impact on health outcomes. Hankins and Hoekstra (2011) focus on marriage and divorce effects. Kuhn (2010) studies consumption, and Hankins et al. (2010), individual bankruptcy.

[5] In 2011 US$. Calculated using the US$-euro exchange rate from the 2nd semester of 2007 and the US rate of inflation from 2007 to 2011.

[6] Ley 35/2007 (November 15, 2007).

Structure Survey). Thus, the benefit was equivalent to 4.4 months of pay for a low-wage full-time worker. The child benefit also more than doubled median female gross monthly earnings (about 1,190 Euros, 2007 Wage Structure Survey).

The explicit goal of the new policy was twofold. As stated in the law, the benefit was meant to help parents cope with the extra expenditures associated with childbirth, while it also intended to encourage fertility, given the low prevailing birth rates in Spain and the trends in population ageing. The law also mentioned its aim to facilitate the balancing of work and family, and to maintain the living standards of low-income families.

The new benefit was universal, with no income tests, and the only requirement was to have resided legally in Spain for at least two years before giving birth. The information made available by the government regarding benefit implementation suggests practically full take-up, not surprisingly given the very low cost associated with the application (which consisted of filling out a one-page form). In 2008, the tax authorities reported paying 491,557 “baby-checks”,[7] which amounted to 95% of all births taking place in Spain during the year (including ineligibles).

2.2 Empirical strategy and data

The identification strategy relies on the fact that the policy established a sharp cutoff in birth-date for benefit elegibility, and the fact that this cutoff was announced unexpectedly. Thus, we expect families to react to the introduction of the new policy right after its announcement and immediate implementation.

There are important differences in the analysis of fertility versus the rest of the outcomes. If the policy successfully encouraged fertility, we expect to observe a sudden

[7] Memoria 2008, Agencia Tributaria (www.aeat.es).

increase in the number of conceptions (or, rather, couples trying to conceive) right after the announcement date. The empirical approach, then, is to analyze the time series of conceptions over time and look for a break around July 3, 2007, perhaps controlling for seasonality.

The second part of the analysis addresses the effect of benefit receipt on a number of family outcomes. The identification strategy here relies on comparing the behaviour of families who had a child right before versus right after the cutoff, which determined their benefit eligibility. Thus, for all outcomes other than fertility, identification is achieved via a RDD, supplemented with DiD specifications to account for potential month-of-birth effects.

Next I discuss in more detail the empirical specifications for the two sets of outcomes.

2.2.1 Fertility effects

The child benefit was introduced with the explicit goal of encouraging fertility, in a country where birth rates had been very low by international standards since the late 1990's. Because eligibility was conditional on having a child, the policy aimed to induce more families to have children (or to have more of them).

If the policy was effective, we would expect to see an increase in the number of women (couples) trying to conceive right after July 3rd, 2007. While intention to conceive is not captured in any publicly available large surveys, more couples trying to conceive should lead to a subsequent increase in the number of conceptions.

The Spanish National Statistical Institute provides micro-data on all births taking place monthly in Spain (which I will refer to as "Vital Statistics"), and these data include information on weeks of gestation at birth. Thus, we can estimate with reasonable accuracy the date of conception for the population of births in Spain, and analyze whether there was a discrete jump in the number of conceptions shortly after the introduction of the benefit.[8] To this purpose, I estimate the following equation:

(1) $$C_m = \alpha + \gamma_1 m + \gamma_2 (m \cdot post) + \beta \cdot post + \lambda X_m + \varepsilon_m \,.$$

Where C is the (natural log of) the estimated number of conceptions in month m, *post* is a binary indicator taking value 1 in all months starting July 2007, and X is the number of days in month m. The month of conception m is normalized to 0 for July 2007, and thus takes values -1 for June 2007, 1 for August 2007, etc.

Since the exact date of birth is not reported in the Vital Statistics data, but only the month, the analysis is performed at the monthly level. The linear term in m accounts for any smooth fertility trends, and it is allowed to change after the policy reform.[9] I also explore the inclusion of higher-order polynomials.

Coefficient β would then capture a discrete jump in the monthly number of conceptions in July 2007, and it would be positive if the benefit successfully encouraged fertility. The identifying assumption is that no other factor affected conception rates discontinuously in July 2007.

This specification presents several potential problems. First, the fact that Spanish Vital Statistics do not report exact date of birth limits our ability to detect a discontinuity right at the policy announcement date. Perhaps even more importantly, the broader our bins, the harder it becomes to rule out "seasonality effects" (systematic differences in conception rates by calendar month). In order to incorporate this concern, I also estimate specifications with calendar month of conception dummies, as shown in the next equation.

[8] For each individual birth, I estimate its likely date of conception by taking every day of the month of birth, subtracting the number of gestation weeks at birth, then adding 14 days (since the most likely date of conception is medically estimated as the birth date minus the number of gestation weeks plus 14 days). As a robustness check, I also alternatively estimate month of conception as just 9 months before the month of birth.
[9] I also estimate regressions where the trend captured by the polynomial is not allowed to change at the cutoff.

(2) $$C_m = \alpha + \gamma_1 m + \gamma_2 (m \cdot post) + \beta \cdot post + \lambda X_m + \sum_{c=2}^{12} \mu_c month_m + \varepsilon_m$$

Our coefficient of interest β would now capture any discrete jump in the number of conceptions between June and July of 2007, above and beyond the average difference in number of conceptions between June and July of the surrounding years. Of course, this specification requires that the sample includes multiple years.

A second concern is that month of conception is estimated with error. I address this issue partially by using several alternative methods for estimating conception date, but any remaining measurement error would bias our coefficients toward zero. Moreover, a third issue is that we do not expect conceptions to react immediately. The medical literature suggests that a healthy, fertile couple will typically take 3 to 6 months to conceive when actively trying. Thus, an increase in the number of couples trying to conceive would only result in additional actual conceptions gradually and with some delay.

These two concerns can be addressed by alternatively analyzing the monthly incidence of abortions, instead of the estimated number of conceptions. Month of abortion is reported accurately in the data, and the decision to terminate a pregnancy can potentially react immediately to the policy change.

The National Statistical Institute only reports the number of abortions annually. In order to obtain the data by month, I contacted the health authorities of each of the 17 Spanish regions. Nine of them, representing more than 75% of total Spanish population (and 80% of national abortions), agreed to provide the data on monthly number of abortions between 2000 and 2009. I then estimate equations 1 and 2 using the (natural log of) number of abortions as the dependent variable.

Both an increase in the number of conceptions and a reduction in the incidence of abortions would result in a higher number of births some time after the policy change. I run a final regression aimed at capturing this combined effect on fertility. Since the vast majority (about 90%) of abortions take place at less than 13 weeks of gestation, a reduction in the incidence of abortions right after July 3rd, would lead to an increase in the number of births starting as early as January 2008. On the other hand, the vast majority of pregnancies resulting in birth last 35 (gestation) weeks or more. This suggests that any additional conceptions after July 3, 2007 would lead to an increase in the number of births starting in late February of 2008, most likely in March. If we take into account that conceiving may take some time, we expect the effect on births to appear starting in January 2008 but perhaps increasing until September of 2008, if we assume it may take up to 6 months to conceive.[10]

Table 1 reports some summary statistics for the sample of conceptions and abortions by month. The full sample includes all births/abortions taking place between 2000 and 2009 (inclusive). The average national monthly number of conceptions is about 37,500, while there are about 6,000 abortions per month in the data.[11]

[10] I illustrate this overall fertility effect by estimating the following equation:

$$B_m = \alpha + \gamma_1 m + \gamma_2 m^2 + \beta_1 post_{0-5} + \beta_2 post_{6-10} + \beta_3 post_{11-15} + \beta_4 post_{16-21} + \sum_{c=2}^{12} \mu_c month_m + \varepsilon_m$$

Where B is the (natural log of) the number of births in month m, and the four "*post*" coefficients capture the increase in the number of births x months after benefit introduction. We expect the increase to show up between months 6 (January 2008) and 15 (September 2008), i.e., we expect β_2 and β_3 to be significantly positive.

[11] The data include about 80% of all abortions at the national level, so this figure needs to be scaled up in order to obtain a national estimate.

I estimate several specifications with the full sample of 120 months (10 years), and then restrict the sample progressively to include only to the months surrounding the policy change.

2.2.2 Family well-being effects

The analysis of the family well-being effects of the child benefit lends itself naturally to a (sharp) regression discontinuity approach, so that we can compare the outcomes of households who had a child right before and right after the eligibility cut-off.[12] Close enough to the threshold, treatment is "as if" randomly assigned (Lee and Lemieux, 2010).

I estimate regressions of the following form:

$$(3) \qquad Y_{im} = \alpha + \gamma_1 m + \gamma_2 (m \cdot post) + \beta \cdot post + \Pi X_{im} + \varepsilon_{im} .$$

This is very similar to equation (1), except for the individual-level subscript. Y is an outcome variable (say, household expenditure or a measure of maternal labor supply) for household i who had a child on month m. Month of birth m (the "running" variable) is again normalized to zero at the threshold (July 2007).[13] The main parameter of interest, β, captures any potential discontinuity or "jump" in Y at the cutoff. The vector X includes household-level controls, and ε is the residual.

The reason for using month of birth as the running variable rather than exact date of birth is that we do not observe exact dates of birth in any of the data sets.[14] This specification is estimated for a range of outcome variables. First I explore whether benefit

[12] For recent articles on regression discontinuity design and its applications in economics, see Lee and Lemieux (2010), Imbens and Lemieux (2008) and van der Klaauw (2008).

[13] Again, the equation includes a linear term in m that is allowed to change at the cutoff, but I also explore higher-order polynomials.

[14] Even if we were able to observe exact dates of birth, the limited number of observations would not allow us to look much closer to the threshold. In the largest of the data sets that I use, I observe about 450 births per month.

receipt translated into changes in household expenditure patterns (section 3.2.1). Then I analyze the potential labor supply effects (section 3.2.2). I also explore whether the benefit affected child care use (3.2.3), family stability and in particular parental separation (section 3.2.4), and finally I analyze the impact on maternal and child health (section 3.2.5).

In the main specification, Y is observed in 2008, or about 12 months after birth (on average) for children born at the cut-off date. The main sample includes households with children born between 9 months before and 9 months after the policy change (i.e. between October 2006 and March 2008)[15], so that the children are between 0 and 2 years of age when observed.[16] In additional specifications, I vary the number of months around the threshold included in the sample, up to only 2 months on either side of the cutoff.

The identifying assumption is that no other factor affected families with children born after June 30, 2007 discontinuously.[17] I do allow for a smooth trend (a polynomial) in month of birth, which we expect will be important since all mothers are observed at the same point in time (2008), so that earlier births necessarily imply children who are older.

There are two checks that should be performed in order to confirm the validity of the RDD approach (Lee and Lemieux, 2010). First, we should observe no discontinuity in the number of births around the threshold. Since date of birth determined benefit eligibility, the program generated an incentive to postpone birth to after the cut-off date. In our setting, such shifting is highly unlikely, given that the benefit was announced three days after the threshold date. In any case, we run regressions such as (1) and (2) where the dependent

[15] The main specification includes only children born up to 9 months after the benefit announcement in order to minimize the likelihood of including births induced by the policy change ("selection effect").
[16] Technically I would need to exclude mothers with less than 2 years of legal residence in Spain from the sample (ineligibles). Legal residence status, however, is not observed in the data. Alternative regressions are estimated that exclude recent immigrants from the sample.
[17] No other policy changes in 2007 or thereafter applied differentially to children born before and after July 1, 2007. Note that the cutoff birthdate that determines the year when a child starts school (or pre-school) in Spain is December 31.

variable is the total number of births by month, and show that there was no discontinuity in number of births around July 1, 2007 (see section 3.2 for results).

A second check is to compare household characteristics around the threshold. If the treatment is "as if" randomly assigned, we should observe no significant differences between treated and control families. I thus estimate regressions such as (4) where the dependent variables are a range of household characteristics (age and education level of the parents, immigrant status, etc). The results are reported in section 3.2.

As in the fertility analysis, and particularly since the running variable is discrete (month of birth), there remains a concern that an RDD analysis could capture seasonality effects, i.e. any potential systematic differences between June and July births that are unrelated to the benefit. I address this concern by additionally running difference-in-difference specifications, where I include multiple birth-years in the analysis and include calendar month of birth fixed-effects. Thus, a discontinuity observed between June and July 2007 births would only be interpreted as a treatment effect if it was larger than the average June-July difference in other surrounding years.

I perform two different DiD exercises. In the first, shown in column 7 of tables 4 to 8, all households are interviewed in 2008, and the sample is composed of families with children born between 2005 and 2008 (both included). Therefore, all families are observed at the same point in time, and the children's ages range between 0 and 3. The second DiD specification, shown in column 7 of tables 4 to 7, also includes observations from families with children born between 2005 and 2008, but now I merge survey data from 2006 to 2009, so that families are always interviewed the year following childbirth (in 2008 for 2007 births, but in 2009 for 2008 births, etc). Thus, in this second specification, the children are observed at approximately the same age (about 12 months), but in different calendar years.

Different data sets are used for each set of dependent variables. The expenditure analysis uses Household Budget Survey (HBS) data. Table 1 shows that households in the sample spent on average 30,500 euros in 2008, including almost 5,000 in directly child-related goods or services. The labor supply and parental separation analysis uses data from the larger Labor Force Survey (LFS). When interviewed in 2008, table 1 shows that 42% of mothers in the sample reported working the previous week (54% were employed, indicating that 12% were on leave from their job). Also, 3.4% of the mothers were separated or divorced.

3. Results

3.1 Fertility

The fertility results are summarized in table 2 and figure 1. As described in section 2.2.1, first I look for a discontinuity in the number of conceptions around the benefit announcement date, using Vital Statistics data and estimating month of conception by combining information on month of birth and gestation weeks at birth. The first panel of figure 1 shows the estimated monthly number of conceptions (that resulted in birth) between 2005 and 2009. Lines are fitted separately for months before and after July 2007. Visually, it appears as if there might have been an increase in the number of conceptions in July 2007. This is confirmed by the regression results, described next.

Table 2 reports the results from estimating equations (1) and (2), in eight different specifications. The dependent variable is the natural log of the (estimated) number of conceptions per month. The first column uses the full sample of births between 2000 and

2009. It controls for a third-order polynomial in month of birth, as well as the number of days in each month. The result suggests that conceptions increased significantly, by about 5.6 log-points, in July 2007. Columns 2 to 5 restrict the sample to months closer and closer to the cutoff. Column 2 includes five years of data (and drops the cubic term on the polynomial), and estimates a 7 percent increase in conceptions. Columns 3 to 5 include only 12, 9 and 3 months before and after the cutoff, respectively. The estimated effects vary between 5 and 8.5 percent.

Finally, columns 6 to 8 report the results from difference-in-difference specifications that include calendar month dummies. These specifications remove any potential seasonality effects that are unrelated to the policy change. The estimated effect remains significant and around 5-6 percent in magnitude.

These results suggest that the child benefit may have been successful in encouraging fertility via increasing the number of conceptions. I supplement the analysis of conceptions with a look into the incidence of abortions, which may potentially have reacted more sharply to the introduction of the benefit.

The bottom panel of figure 1 displays the monthly number of abortions between 2005 and 2009, with separate linear fits for the periods before and after the benefit. Although there is an overall positive trend, there may have been a drop in the incidence of abortions around July 2007.

Regression results from estimating equations (1) and (2) are reported in the second row of table 2. The eight specifications are the same as for the number of conceptions, although the dependent variable is now the natural log of the monthly number of abortions. The RDD specifications (columns 1 to 5) indicate a large drop in the number of abortions exactly after the introduction of the benefit. The magnitudes range between 12 and 23 percent. However, the results with calendar month dummies (reported in columns 6 to 8) suggest that seasonality might be an important driver of the monthly number of abortions. Once the month fixed-effects are included, the magnitude is reduced to about 5 percent.

Combined, the results presented in this section suggest that the child benefit may have reduced the incidence of abortions and encouraged new conceptions, both resulting in additional births. My estimates indicate that the benefit may have led to an overall increase in the number of births of about 6 percent.[18]

3.2 Family well-being

Next I report the results from the analysis of the effects of the child benefit on a range of outcomes related to family well-being. This section relies on comparing families who had children shortly before and after the introduction of the benefit, in a regression-discontinuity analysis that is also supplemented by difference-in-difference specifications.

Before the results, it is useful to report a number of validity checks in support of the RDD approach. First I confirm that there was no discontinuity in the number of births around the cutoff date, as would be the case if families could adjust the date of birth as a response to the benefit. In 2007, there were about 41,000 births a month in Spain. There were more births in July (42,810) than in June (40,210), but there is on more day in July than in June. Thus, I estimate equations (1) and (2) with the natural log of the monthly number of births as a dependent variable. The results of eight different specifications (five RDD, three DiD) are presented in table A1.[19] There is no evidence of a significant jump in the number of births around July 1, 2007. The last column, which includes 30 months before and 30 after the threshold, a second-order polynomial in *m*, and calendar month of

[18] The results table for the overall effect on number of births is available upon request from the author.
[19] Note that the specifications in table A1 are parallel to those in table 2.

birth dummies, suggests a small (0.8 log-points) effect, but the coefficient is far from statistically significant.[20]

We also need to check that control and treatment groups do not differ in their observable covariates, which would cast doubt on the "as if" randomization around the threshold. We thus estimate regressions such as (3) with different household characteristics as dependent variables:

(4) $$X_{im} = \alpha + \gamma_1 m + \gamma_2 (m \cdot post) + \beta \cdot post + \varepsilon_{im},$$

In particular, I check for balance in age, educational attainment, marital status and immigrant status of the mother and the father, as well as parity of the child. I do so using all available data sets (mainly Vital Statistics, Household Budget Survey and Labor Force Survey). Results are reported in figure 2 and table 3.

Figure 2 shows monthly averages for four maternal characteristics around the threshold, using Vital Statistics data, and thus the population of births in Spain around the introduction of the benefit. I also show separate linear fits for the data before and after the cutoff. There is little variation in average maternal age by month, with mothers being on average 31 years of age in 2007 and no noticeable jump in July 2007. There is a clear upward trend in the fraction of mothers who are foreign-born (almost 20% in 2007), but again there is no visual evidence of a discontinuity at the threshold. There is also no obvious jump at the cutoff date in the fraction of mothers who are married or the proportion with a high-skill job. Of course, the visual evidence needs to be complemented with regression analysis.

[20] The regressions in table A1 can also be thought of as a placebo test for the fertility results in section 3.1.

Table 3 reports the results of estimating equation (4) with Household Budget Survey and Labor Force Survey data, for ten different covariates. Five different specifications are reported. The first one includes births 9 months before and after the cutoff, and later columns restrict the data to births closer and closer to the threshold, so that in column 5 I only include children born between 2 months before and 2 months after July 1, 2007.

Mother characteristics appear to be fairly balanced around the threshold. In the HBS regressions (first panel of table 3), none of the coefficients are statistically significant at the 90% confidence level. There is no significant discontinuity in age, education or immigrant status of the mother or father, or in the parity of the child. Control and treated mothers are similar in their observable covariates, as expected. In the LFS sample (second panel), control and treated families are similar in most of the covariates. However, treated fathers appear slightly younger (between 0.6 and 0.9 years), while mothers have lower high school graduation rates (between 6 and 10 percentage points). The age jump is not large, but the discontinuity in the education level of the mothers, most likely a chance occurrence due to sampling since it does not come up in the other data sets, suggests that we should control for education in all our LFS specifications.

The remainder of this section reports the results for the different sets of outcome variables related to family well-being. The results are presented in tables 4 through 7. Note that all tables have the same structure, reporting the results from the same eight specifications, for the different outcomes and estimated from the different data sources.

3.2.1 Household expenditure

The first set of results analyzes whether benefit receipt translated into changes in expenditure patterns. The permanent income model would predict no effect of an unexpected, transitory increase in income on household consumption. However, this prediction could fail to hold if families face liquidity constraints. I compare total annual expenditure as reported in 2008 for families who had a child shortly before and after July 1, 2007. As dependent variables, I look at total expenditure, durable goods expenditure, and child-related expenditure.[21] The results are summarized in figure 3 and table 4.

In brief, I find no evidence that benefit receipt translated into higher average expenditure the year following childbirth. This is also true if I look at the subset of goods and services that are directly related to the children, and if I restrict attention to durable goods (plausibly more affected by potential liquidity constraints).

The first panel of figure 3 shows average annual expenditure, by month of birth of the child. Average expenditure for families who had a child in 2007 was about 30,000 euros, and there is no perceptible discontinuity around July 2007. This is confirmed by the regression analysis. The first two rows of table 4 show the results when using total expenditure in euros or in logs as the dependent variable, for six RDD specifications and two DiD ones (with calendar month of birth dummies). Coefficients are mostly negative and never statistically significant. Families who received the benefit did not increase their expenditure, on average.

The next two rows of table 4 (and the second panel of figure 3) show the results for child-related expenditures. The results suggest that families that received the benefit did not subsequently spend more on child-related goods or services. The same holds for durable goods (final two rows of table 4).[22]

[21] See appendix for the definition of durable goods and child-related expenditure.
[22] I also estimate separate regressions for different subgroups of the population (by marital status of the mother, education level of mother and father, and age of the parents). I only find a (borderline) positive

The available evidence suggests that families did not, on average, increase expenditures as a result of receiving the benefit.[23] There are several explanations for this finding. First, a one-time payment does not increase permanent income by much, so the permanent income theory would predict a small impact on consumption. But there is a second possible explanation: that an increase in unearned income may have reduced other sources of household income via a reduction in labor supply, thus compensating the initial increase. The next subsection analyzes the labor supply effect of the child benefit.

3.2.2 Maternal labor supply

Next I analyze whether benefit eligibility affected household labor supply. A static model of labor supply would predict that an increase in unearned income leads to a reduction in household labor supply, as long as leisure (or, in this case, "home time") is a normal good. On the other hand, a dynamic model would predict a small, if any, contemporaneous effect of a one-time transfer. This prediction would however fail to hold in the presence of credit constraints, in which case the static model might be a better depiction of reality.

The main results are presented in figure 4 and table 5. The main dependent variable in table 5 (first row) is a binary indicator that takes value 1 if a mother was working when interviewed in 2008, when her child was (on average) 12 months of age. A second dependent variable takes value 1 if the woman was employed, even if she may have been on temporary leave from her job.[24]

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significant effect on overall expenditure for families with low-educated fathers. Results are available upon request.

[23] I also run separate regressions for each expenditure item available in the data separately. The results do not show significant increases in expenditure on any specific items more than would be expected by chance (since there are almost 50 different expenditure categories).

[24] Parallel specifications are estimated for fathers' labor supply, with no significant effects found. Regression results are available upon request.

The six different RDD specifications suggest that mothers who received the benefit were 4 to 6 percentage points less likely to be working in 2008 compared with ineligible mothers. The magnitude remains in the same range in the first of the DiD specifications, but is substantially reduced in the second (column 8), although the effect remains statistically significant.

The result is illustrated graphically in figure 4 (first panel). About 48 percent of women who had a child in June 2007 were back to work when interviewed in 2008, compared with 43 percent of July 2007 mothers. The DiD results suggest that this difference cannot be explained by seasonality, since the jump is significant even when compared with the same calendar months in the surrounding years.

Parallel regressions are estimated using 2009 LFS data, when the children were on average 24 months old (see table A2 in appendix).[25] As shown in the second panel of figure 4, mothers who received the benefit were no less likely to be working two years after childbirth, suggesting that the labor supply effect of the benefit was only short-term.

These results suggest that mothers may have used the benefit to "buy" time at home during the first few months of their child's life.[26] If this is true, then we should observe differences in childcare use between eligible and ineligible families. We turn to this analysis in the next subsection.

3.2.3 Childcare use

Childcare use by families can be measured with expenditure data from the HBS. In addition, the EU-SILC (Survey of Income and Living Conditions) reports hours spent by

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[25] Note that specification 8 is absent from table A2. The reason is that HBS and LFS data for 2010 were not available at the time of writing, so that specification 8 could not be estimated.

[26] I also estimate separate regressions for different subgroups of the population. The labor supply effects are more pronounced for single mothers and low-educated mothers. Results are available upon request.

children in the household in different forms of childcare. Although this data set contains a very low number of observations per month of birth, I use it to supplement the main results.

Figure 5 and table 6 present the main findings with HBS data. The HBS reports separately expenditures in three different forms of external daycare: private daycare centers, public infant care centers, and nannies or babysitters. I estimate the same eight specifications for each category of expenditure in levels (first three rows of table 6). In addition, I create three binary variables that indicate whether a family spent any positive amount in each type of childcare during the year (last three rows).

The regressions suggest that families receiving the benefit may have spent significantly less in private daycare during the first year of the child's life. The regressions in levels suggest that the magnitude of this effect was between 100 and 200 euros, for an average of 300 (although only two of the eight specifications in levels show statistical significance above 90%). Perhaps more convincing are the results in the last row, showing that receiving the benefit decreased the fraction of families using private daycare by 4 to 12 percentage points.

These results are illustrated in figure 5. We observe a drop in average expenditure in private daycare from about 400 to about 200 euros at the benefit cutoff (first panel). The second panel shows a drop in the fraction of families with positive expenditure in private daycare from about 40 to about 30 percent.

Receiving the benefit did not appear to affect expenditure on official care centers (neither in levels not in the fraction of families that use them at all). This is unsurprising since these public institutions are not very flexible and their supply is quite restricted.[27]

[27] Families apply for public daycare, which is heavily subsidized, months in advance. Demand is much higher than supply, and part-time hours are usually not offered.

Few families use nannies, and again there are no significant changes in levels nor the proportion of users, with very unstable coefficients across specifications.

These results obtained from HBS data are confirmed by the analysis of hours in different forms of daycare using EU-SILC data (see appendix table A3). These results suggest that families that received the benefit were 4 to 6 percentage points less likely to use private daycare during the first year of the child's life.

Taken together, the results in the last three sections suggest that families reacted to the child benefit by having the mother stay at home longer after childbirth, thus reducing her labor supply and earnings the year following birth, and also reducing the use of external forms of childcare during that time.

The following two sections explore additional outcomes that may provide a fuller picture of the overall well-being effects of the benefit (in the short term): family stability and health.

3.2.4 Family stability

The literature in child development and sociology suggests that household income may affect the level of stress and conflict in the family, which could in turn affect child outcomes.[28] In particular, an exogenous increase in unearned income just after childbirth may result in lower levels of stress for the parents. I test this hypothesis in two ways. First, in this section I analyze whether receiving the benefit had an impact on the likelihood of the parents separating or divorcing shortly after the birth of the child. In the next section, I study potential health effects of the benefit for both mothers and children, which may in part result from the same channel of lower family conflict.

[28] See, for example, child development paper Yeung, Linver, and Brooks-Gunn (2002).

Table 7 summarizes the parental separation results. The dependent variable is a binary indicator that takes value 1 if the mother was separated or divorced when interviewed in 2008 (LFS data). The RDD specifications (columns 1 to 6) show that mothers who received the benefit were 2 to 5 percentage points less likely to be separated a year after giving birth, and the estimated effect is strongly significant. The DiD specifications confirm this finding, although the estimated magnitude of the effect drops to between 1 and 2 percentage points (still sizeable when compared with the average separation rate of 3.4%).

Thus, it appears that the benefit may have lowered separation rates for parents. However, table A2 in the appendix shows that this effect was only short-term: we observe no drop in separation probabilities when parents are interviewed in 2009, when the children born at the cutoff were on average 24 months old. This suggests that any effects on family conflict of the benefit were only temporary, which is unsurprising given the one-time nature of the subsidy.

3.2.5 Health outcomes

Finally, this section analyzes the potential health consequences of the benefit, both for the mother and for the children. The main reason for including health outcomes is that health, unlike labor supply or even marital breakup, can be unequivocally linked to well-being. However, data availability is much more limited. The only data set that includes health variables for the adults in the household is the EU-SILC, but the number of observations is very low. Child health variables are not available in any of the available surveys. Thus, for children we are limited to studying extreme health outcomes, namely child mortality as reported in Vital Statistics (official records of all deaths in Spain).

Table 8 reports the results for child mortality. The main dependent variable is the mortality rate (by age 24 months) per 1,000 children born in a given month around the cutoff (first row). Since any health effects are unlikely to materialize just after birth (given that the benefit was only received a few weeks after birth), I also report results that exclude deaths taking place during the first month of the baby's life (second row), or during the first two months (third row).

If the subsidy had any positive health consequences, we would expect a drop in mortality rates as a result. I do not find any evidence to support this conclusion. Coefficients are mostly non-significant, and signs are positive in almost all specifications. Thus, I conclude that the benefit had no effect on extreme health outcomes for children, on average.

Table A4 reports the results on maternal health using data from EU-SILC (2008). There are two binary dependent variables. The first takes value 1 if the mother reported bad or very bad health. The second indicated mothers who reported that their daily activities had been hampered by some health problem during the previous 6 months. The results are very imprecise, in part due to the small sample sizes, but there is some evidence that mothers who received the benefit were less likely to report poor health the year following childbirth.

4. Conclusions

This paper analyzes the effects of a 2,500-Euro, universal child benefit introduced in Spain in 2007. I find evidence suggesting that the subsidy may have been successful in increasing fertility. My estimates indicate that births increased by about 6 percent as a result of the new policy.

Regarding the effect on recipient families, the results suggest that the benefit induced no significant change in overall household expenditure or child-related expenditure the year following child birth. I do find a significant effect on maternal labor supply and, most likely as a result, child care arrangements during the child's first year of life. When children born at the cutoff date were about 12 months of age, eligible mothers were 2 to 4 percentage points less likely to be working, compared with control mothers. Consistent with this labor supply response, children born after the threshold were significantly less likely to be in formal day care during their first year of life.

I also find that parents who received the benefit were less likely to separate during the first year after childbirth, and eligible mothers reported somewhat better health. I do not find any effect on (extreme) health indicators for children.

I conclude that the main effect of the child benefit on parental investments in children was an increase in maternal care time during the child's first year, with no significant change in the consumption of child-related goods or services. Together with the parental separation results, this may well have an impact on child well-being.[29] Recent research suggests that maternal employment during a child's first year(s) of life may have detrimental effects on cognitive development and health.[30] Also, a recent study by Carneiro et al. (2010) finds that an extension of maternity leave in Norway had positive long-term effects on children's educational attainment.[31]

[29] Milligan and Stabile (2011) found that increases in child benefits in Canada were associated with higher test scores and improved child health. Our results suggest that increased maternal time at home may be one factor contributing to these effects.

[30] See Baum (2003), Berger et al. (2005), Bernal (2008), Bernal and Keane (2010), Blau and Grossberg (1992), James-Burdumy (2005), Ruhm (2000, 2004).

[31] Although other studies have found no effect of maternity leave expansions on long-term child outcomes in other countries (see, for instance, Dustmann and Schonberg, 2009 for Germany).

As an aside, the Spanish child benefit was removed in May 2010 (in effect for births starting January 2011), as part of broader budget cuts. It will be interesting to see if the repeal of the benefit reverses the effects observed after its introduction.

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Appendix: Expenditure categories

I construct ***child-related expenditure*** by adding up all 4-digit items that are child-specific. There are 14 such items: baby food and drinks (1194), children or baby clothes (3123), children or baby shoes (3213), large furniture, including cribs, play-pens, high-chairs and other baby furniture (5111), kitchen utensils (non-electric) and other household articles, including baby bottles (5413), domestic service, including nannies and baby-sitters (5621, 5622), toys, games, hobbies and small musical instruments (9311), books, excluding textbooks (9511), paper and painting products, including pens, crayons, paint, chalk etc (9541), official infant education centers (for children ages 0 to 3) (10111), personal hygiene non-electric products, including soap, lotion, diapers, etc (12122), other baby products, including strollers, baby carriers, car seats, pacifiers, etc (12222), and center-based child services excluding schools (day care centers) (12312).

Durable goods are defined to include: furniture, household appliances, silverware, glassware and chinaware, tools and accessories, vehicles (cars, motorcycles, bicycles), phone and fax equipment, audio and visual equipment and accessories (sound systems, photographic equipment, television and video equipment, computers, etc), other durable goods related to leisure (boats, horses, musical instruments, sports equipment, etc), books, jewellery and watches, bags and suitcases, and other personal products (pipes, umbrellas, sunglasses, etc).

Table 1. Descriptive statistics

i) Vital Statistics				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Monthly n. of conceptions	37458	3609	24690	44375
Post-June 2007 dummy	0,175	0,382	0	1
Month of conception	-39,5	34,8	-99	20

Note: The sample includes all births in Spain between 2000 and 2009 (both included). The number of observations is 120.

ii) Abortions statistics				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Monthly n. of abortions	6009	1309	3858	8695
Post-June 2007 dummy	0,2417	0,4299	0	1
Month of abortion	-30,5	34,78	-90	29

Note: The sample includes all abortions in 9 of the 17 Spanish regions between 2000 and 2009 (both included). The number of observations is 120.

iii) Household Budget Survey (2008)				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Total expenditure	30507	17721	3785	182173
Child-related expenditure	4778	4275	0	28744
Durable expenditure	5654	8740	0	68091
Daycare expenditure	306	848	0	6571
Post-June 2007 dummy	0,487	0,500	0	1
Month of birth	-0,780	5,056	-9	8
Age of mother	32,62	5,14	16	49
Mother some secondary	0,234	0,424	0	1
Mother high school graduate	0,326	0,469	0	1
Mother college grad.	0,321	0,467	0	1
Mother immigrant	0,165	0,371	0	1
Not first-born	0,539	0,499	0	1

Note: The sample includes all households interviewed in 2008 who had a baby between October 2006 and March 2008 (both included). The number of observations is 958.

Labor Force Survey (2008)				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Worked last week	0,422	0,494	0	1
Currently employed	0,541	0,498	0	1
Separated or divorced	0,034	0,180	0	1
Post-June 2007 dummy	0,477	0,500	0	1
Month of birth	-0,815	5	-9	8
Age of mother	32,38	5,25	16	50
Mother some secondary	0,236	0,425	0	1
Mother high school grad.	0,343	0,475	0	1
Mother college grad.	0,297	0,457	0	1
Mother immigrant	0,170	0,376	0	1
Not first-born	0,523	0,500	0	1

Note: The sample includes all households interviewed in 2008 who had a baby between October 2006 and March 2008 (both included). The number of observations is 8691.

Table 2. Fertility results

	RDD 10 years		RDD 5 years		RDD 12-12m		RDD 9-9m		RDD 3-3m		DiD 10 years		DiD 7 years		DiD 5 years
	1		2		3		4		5		6		7		8
Conceptions	0,0558	**	0,0714	***	0,0852	**	0,075	**	0,0503		0,0489	***	0,059	***	0,0555
	(0,0222)		(0,0247)		(0,0359)		(0,0285)		(0,0314)		(0,0107)		(0,0093)		(0,0110)
Abortions	-0,150	***	-0,1724	***	-0,2323	**	-0,2125	***	-0,1159	*	-0,0549		-0,0503	*	-0,0516
	(0,0447)		(0,0517)		(0,0843)		(0,0643)		(0,0405)		(0,0335)		(0,0279)		(0,0361)
Years included	2000-09		2005-09		2006-08		2006-08		2007		2000-09		2003-09		2005-09
N (number of months)	120		60		24		18		6		120		96		60
Linear trend in m	Y		Y		Y		Y		N		Y		Y		Y
Quadratic trend in m	Y		Y		Y		N		N		Y		Y		Y
Cubic term in m	Y		N		N		N		N		Y		N		N
N. days of the month	Y		Y		Y		Y		Y		Y		Y		Y
Calendar month dummies	N		N		N		N		N		Y		Y		Y

(* p<0.1 ** p<0.05 *** p<0.01)

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable in the first row is the natural log of the monthly number of conceptions in Spain (estimated from Vital Statistics on births). The dependent variable in the second row is the natural log of the monthly number of abortions in 9 out of the 17 Spanish regions.

Table 3. Balance in covariates

	Household Budget Survey (2008)					Labor Force Survey (2008)				
	1	2	3	4	5	1	2	3	4	5
Age of the mother	0.359	0.095	-0.024	-0.044	-0.407	-0.307	-0.283	-0.292	-0.692***	-0.164
	(1.029)	(0.814)	(0.997)	(0.597)	(0.708)	(0.342)	(0.269)	(0.332)	(0.189)	(0.227)
Age of the father	0.117	0.120	-0.415	0.465	-0.324	-0.430	-0.405	-0.728**	-0.912***	-0.605***
	(1.165)	(0.912)	(1.129)	(0.679)	(0.769)	(0.353)	(0.274)	(0.344)	(0.191)	(0.233)
Mother secondary	0.044	0.045	0.071	-0.010	-0.005	0.009	0.013	0.050*	0.024	0.040**
	(0.079)	(0.063)	(0.077)	(0.047)	(0.052)	(0.028)	(0.022)	(0.028)	(0.015)	(0.019)
Mother high school gr.	-0.003	-0.055	-0.043	0.010	0.001	-0.092***	-0.077***	-0.097***	-0.061***	-0.066***
	(0.095)	(0.074)	(0.092)	(0.053)	(0.063)	(0.031)	(0.025)	(0.031)	(0.017)	(0.021)
Mother college grad.	-0.026	0.012	0.018	0.011	0.012	0.063**	0.047*	0.062**	0.041**	0.029
	(0.093)	(0.073)	(0.090)	(0.052)	(0.062)	(0.030)	(0.024)	(0.030)	(0.017)	(0.020)
Father secondary	0.127	0.084	0.118	0.044	0.041	-0.015	-0.023	0.002	-0.001	-0.012
	(0.086)	(0.069)	(0.085)	(0.050)	(0.058)	(0.029)	(0.023)	(0.028)	(0.016)	(0.019)
Father high school gr.	-0.026	-0.032	-0.005	-0.007	0.005	-0.044	-0.006	-0.025	-0.005	-0.006
	(0.090)	(0.071)	(0.088)	(0.051)	(0.060)	(0.031)	(0.024)	(0.030)	(0.017)	(0.020)
Father college grad.	-0.107	-0.064	-0.096	-0.024	-0.036	0.044*	0.021	0.015	0.008	0.003
	(0.089)	(0.070)	(0.087)	(0.049)	(0.059)	(0.027)	(0.021)	(0.026)	(0.015)	(0.018)
Mother immigrant	-0.038	-0.016	0.013	0.011	0.058	0.022	0.009	0.015	0.009	0.008
	(0.073)	(0.057)	(0.071)	(0.044)	(0.047)	(0.024)	(0.019)	(0.024)	(0.014)	(0.016)
Not first-born	-0.017	0.003	-0.036	-0.017	-0.045	0.046	0.007	0.045	-0.017	0.018
	(0.099)	(0.078)	(0.096)	(0.056)	(0.066)	(0.033)	(0.026)	(0.032)	(0.018)	(0.022)
N. obs.	958	651	446	319	234	8691	5813	4083	3026	2062
Linear trend in m	Y	Y	Y	N	N	Y	Y	Y	N	N
Quadratic trend in m	Y	N	N	N	N	Y	N	N	N	N
N. of months	18	12	8	6	4	18	12	8	6	4

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is indicated in each row header. (* p<0.1 ** p<0.05 *** p<0.01)

Table 4. Expenditure results (Household Budget Survey)

Dependent variables	1 **RDD 9m**	2 **RDD 6m**	3 **RDD 4m**	4 **RDD 3m**	5 **RDD 2m**	6 **RDD 2m**	7 **DiD 1**	8 **DiD 2**
Total exp.	-3175	-2247	405	-580	-1774	-1084	-1307	-621
	(2838)	(2244)	(2885)	(1553)	(2032)	(1832)	(2194)	(1258)
Total exp. (logs)	-0.142	-0.105	-0.034	-0.041	-0.072	-0.060	-0.049	-0.037
	(0.092)	(0.074)	(0.092)	(0.052)	(0.067)	(0.063)	(0.071)	(0,036)
Child-related exp.	-407	-599	357	-21	-150	10	94	-350
	(766)	(618)	(795)	(444)	(536)	(534)	(592)	(320)
Child-related exp. (logs)	-0.011	-0.034	0.087	0.018	0.002	-0.001	0.017	-0.097
	(0.195)	(0.151)	(0.194)	(0.103)	(0.127)	(0.128)	(0.132)	(0,062)
Durable goods exp.	-1513	-1849	-1011	-760	-1046	-999	-1721	-380
	(1688)	(1345)	(1704)	(927)	(1105)	(1099)	(1137)	(645)
Durable goods exp. (logs)	0.157	0.025	0.230	0.089	0.080	0.071	0.022	-0.063
	(0.297)	(0.239)	(0.302)	(0.170)	(0.200)	(0.213)	(0.208)	(0,097)
N. obs.	941	640	441	315	230	230	2249	2902
Linear trend in m	Y	Y	Y	N	N	N	Y	Y
Quadratic trend in m	Y	N	N	N	N	N	Y	N
Calendar month of birth dummies	N	N	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	N	Y	Y	Y
N. of months	18	12	8	6	4	4	48	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is indicated in each row header. Columns 1 to 7 use HBS data from 2008, col. 8 used merged HBS data for 2006-2009. The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, child parity, and month of interview dummies. See appendix for the exact definition of child-related expenditure and durable goods expenditure. (* p<0.1 ** p<0.05 *** p<0.01)

Table 5. Maternal labor supply results (at 12 months, Labor Force Survey)

Dependent variable	1 **RDD 9m**	2 **RDD 6m**	3 **RDD 4m**	4 **RDD 3m**	5 **RDD 2m**	6 **RDD 2m**	7 **DiD 1**	8 **DiD 2**
Working last week	-0.0640**	-0.0430*	-0.0577*	-0.0532***	-0.0547**	-0.0576***	-0.0435***	-0.0206*
	(0.0316)	(0.0249)	(0.0311)	(0.0179)	(0.0219)	(0.0213)	(0.0155)	(0.0116)
Employed	-0.0632**	-0.0393	-0.0799***	-0.0535***	-0.0612***	-0.0610***	-0.0200	-0.0186
	(0.0309)	(0.0243)	(0.0304)	(0.0174)	(0.0219)	(0.0208)	(0.0166)	(0.0114)
N	8691	5813	4083	3026	2062	2062	21185	25544
Linear trend in m	Y	Y	Y	N	N	N	Y	Y
Quadratic trend in m	Y	N	N	N	N	N	Y	N
Calendar month of birth dummies	N	N	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	N	Y	Y	Y
N. of months of birth	18	12	8	6	4	4	48	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is indicated in each row header (both are binary). Columns 1 to 7 use LFS data from 2008, col. 8 used merged LFS data for 2006-2009. The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, child parity, and month of interview dummies. (* $p<0.1$ ** $p<0.05$ *** $p<0.01$)

Table 6. Child care expenditure results (Household Budget Survey)

Dependent variables	1 RDD 9m	2 RDD 6m	3 RDD 4m	4 RDD 3m	5 RDD 2m	6 RDD 2m	7 DiD 1	8 DiD 2
Private daycare	-138	-123	-195	-157*	-147	-158	-177**	-94
	(170)	(121)	(161)	(80)	(101)	(103)	(84)	(65)
Private daycare (binary)	-0.0795	-0.0985	-0.1041	-0.0943*	-0.1096*	-0.1248*	-0.0364	-0.0627*
	(0.0940)	(0.0743)	(0.0938)	(0.0538)	(0.0624)	(0.0644)	(0.0500)	(0.0324)
Official infant care center	38	-16	89	-33	-21	32	-14	-6
	(147)	(134)	(139)	(99)	(125)	(115)	(76)	(61)
Official infant care center (binary)	-0.1135	-0.0471	-0.0538	0.0070	-0.0470	-0.0158	0.0068	-0.0129
	(0.0911)	(0.0713)	(0.0897)	(0.0510)	(0.0609)	(0.0622)	(0.0475)	(0.0298)
Nanny/ babysitter	120	187	424	182	277	411	196	-10
	(342)	(278)	(321)	(205)	(240)	(255)	(179)	(127)
Nanny/ babysitter (binary)	-0.0384	-0.0038	0.0418	0.0129	0.0002	0.0191	-0.0161	-0.0281
	(0.0931)	(0.0728)	(0.0904)	(0.0532)	(0.0635)	(0.0630)	(0.0502)	(0.0319)
N	958	651	446	319	234	234	2289	2904
Linear trend in m	Y	Y	Y	N	N	N	Y	Y
Quadratic trend in m	Y	N	N	N	N	N	Y	N
Calendar month of birth dummies	N	N	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	N	Y	Y	Y
N. of months	18	12	8	6	4	4	48	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is indicated in each row header. Columns 1 to 7 use HBS data from 2008, col. 8 used merged HBS data for 2006-2009. The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, child parity, and month of interview dummies. (* p<0.1 ** p<0.05 *** p<0.01)

Table 7. Parental separation results (at 12 months, Labor Force Survey)

Dependent variable	1 **RDD 9m**	2 **RDD 6m**	3 **RDD 4m**	4 **RDD 3m**	5 **RDD 2m**	6 **RDD 2m**	7 **DiD 1**	8 **DiD 2**
Mother separated or divorced	-0,036***	-0,0285***	-0,0518***	-0,0204***	-0,0288***	-0,0255***	-0,0158*	-0,0115***
	(0,0132)	(0,0099)	(0,0126)	(0,0062)	(0,0083)	(0,0081)	(0,0084)	(0,0042)
N	8691	5813	4083	3026	2062	2062	21185	25544
Linear trend in m	Y	Y	Y	N	N	N	Y	Y
Quadratic trend in m	Y	N	N	N	N	N	Y	N
Calendar month of birth dummies	N	N	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	N	Y	Y	Y
N. of months of birth	18	12	8	6	4	4	48	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is a binary indicator that takes value 1 if the mother is separated or divorced at the time of the interview (about 12 months after childbirth). Columns 1 to 7 use LFS data from 2008, col. 8 used merged LFS data for 2006-2009. The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, child parity, and month of interview dummies. (* p<0.1 ** p<0.05 *** p<0.01)

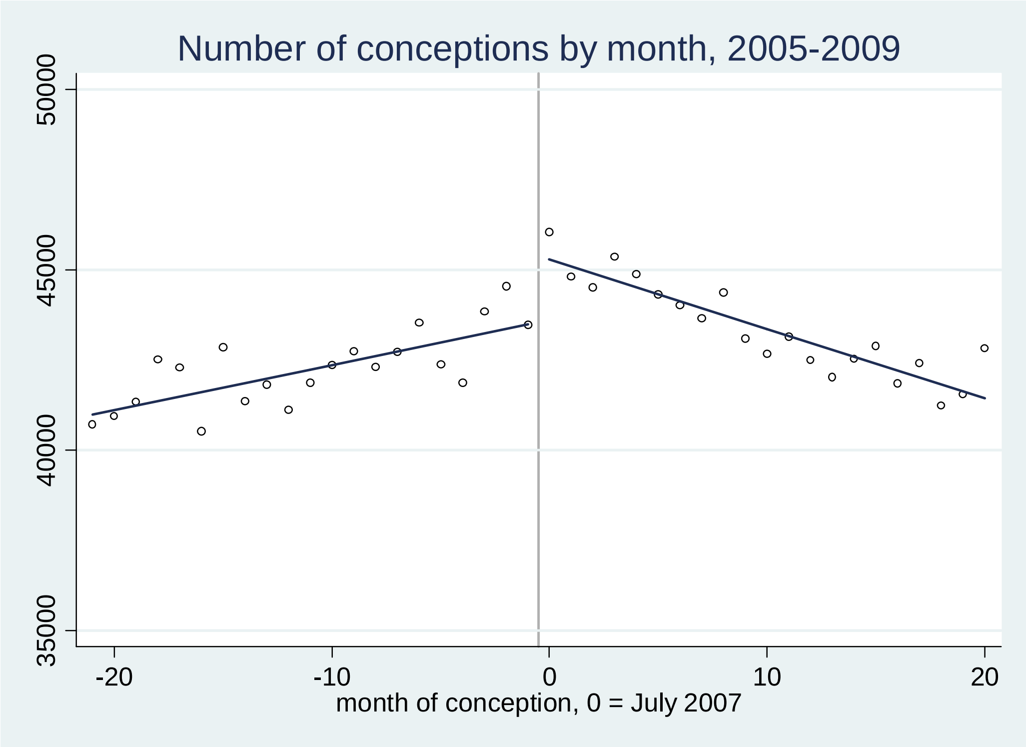
Table 8. Child mortality results (Vital Statistics)

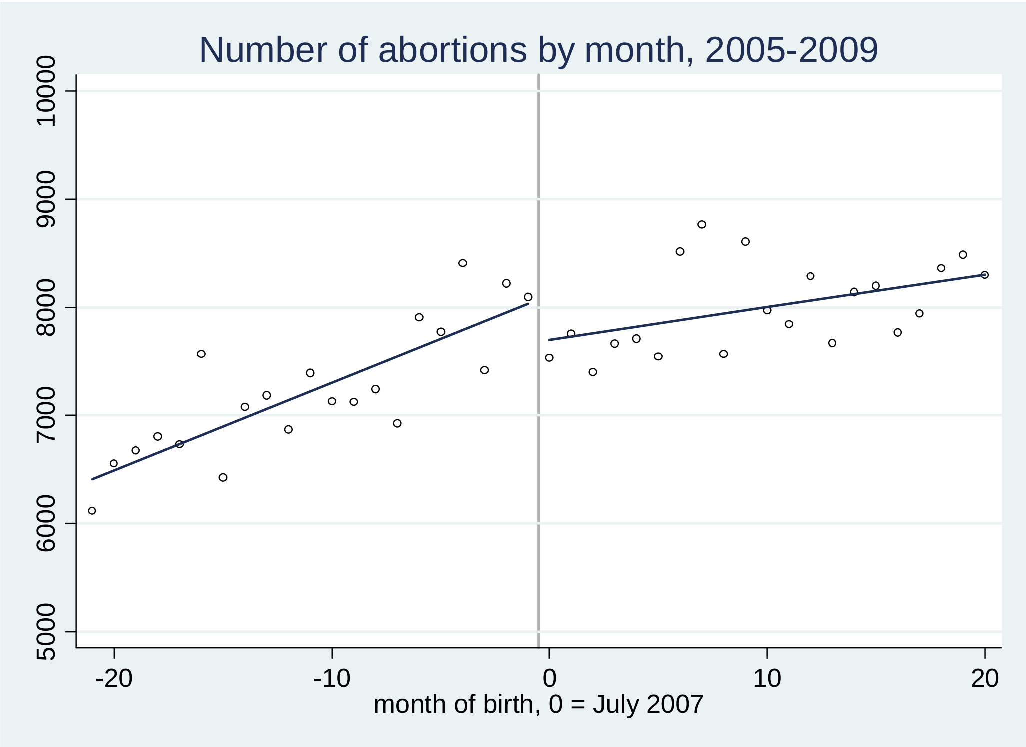
	RDD 4y		RDD 12m		RDD 9m		RDD 6m	RDD 4m	RDD 3m	DiD1		DiD2	
	1		2		3		4	5	6	7		8	
Mortality by age 2	0,623	***	0,532	*	0,565	*	0,424	0,036	-0,602	0,51	*	0,569	***
	(0,2160)		(0,2640)		(0,3120)		(0,3780)	(0,5350)	(0,3720)	(0,2760)		(0,1960)	
Mortality 1m-2y	0,291	**	0,151		0,173		0,247	0,121	0,146	0,3	*	0,189	
	(0,1200)		(0,1360)		(0,1620)		(0,1680)	(0,2030)	(0,2780)	(0,1600)		(0,1300)	
Mortality 3m-2y	0,2340	***	0,1340		0,0730		0,1860	0,0420	0,1740	0,1840		0,1280	
	(0,0830)		(0,1400)		(0,1530)		(0,1500)	(0,1210)	(0,1160)	(0,1270)		(0,1000)	
Years of birth included	2004-2007		2005-2007		2006-2007		2006-2007	2007	2007	2004-2007		2004-2007	
N (number of months)	48		18		15		12	8	6	48		48	
Linear trend in m	Y		Y		Y		Y	N	Y	Y		Y	
Quadratic trend in m	Y		Y		Y		N	N	Y	Y		Y	
Cubic term in m	Y		N		N		N	N	Y	N		N	
N. days of the month	Y		Y		Y		Y	Y	Y	Y		Y	
Calendar month dummies	N		N		N		N	N	Y	Y		Y	

(* p<0.1 ** p<0.05 *** p<0.01)

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable in the first row is the number of children who died by age 2 by month of birth (times 1,000), divided by the monthly number of births. The second row excludes the deaths that occurred during the first month of life of the baby, and the third row excludes deaths during the first 2 months.

Figure 1. Fertility effect: Conceptions and abortions by month





Note: Monthly averages shown, together with separate linear fits on both sides of July 2007

Figure 2. Balance in covariates (Vital Statistics)

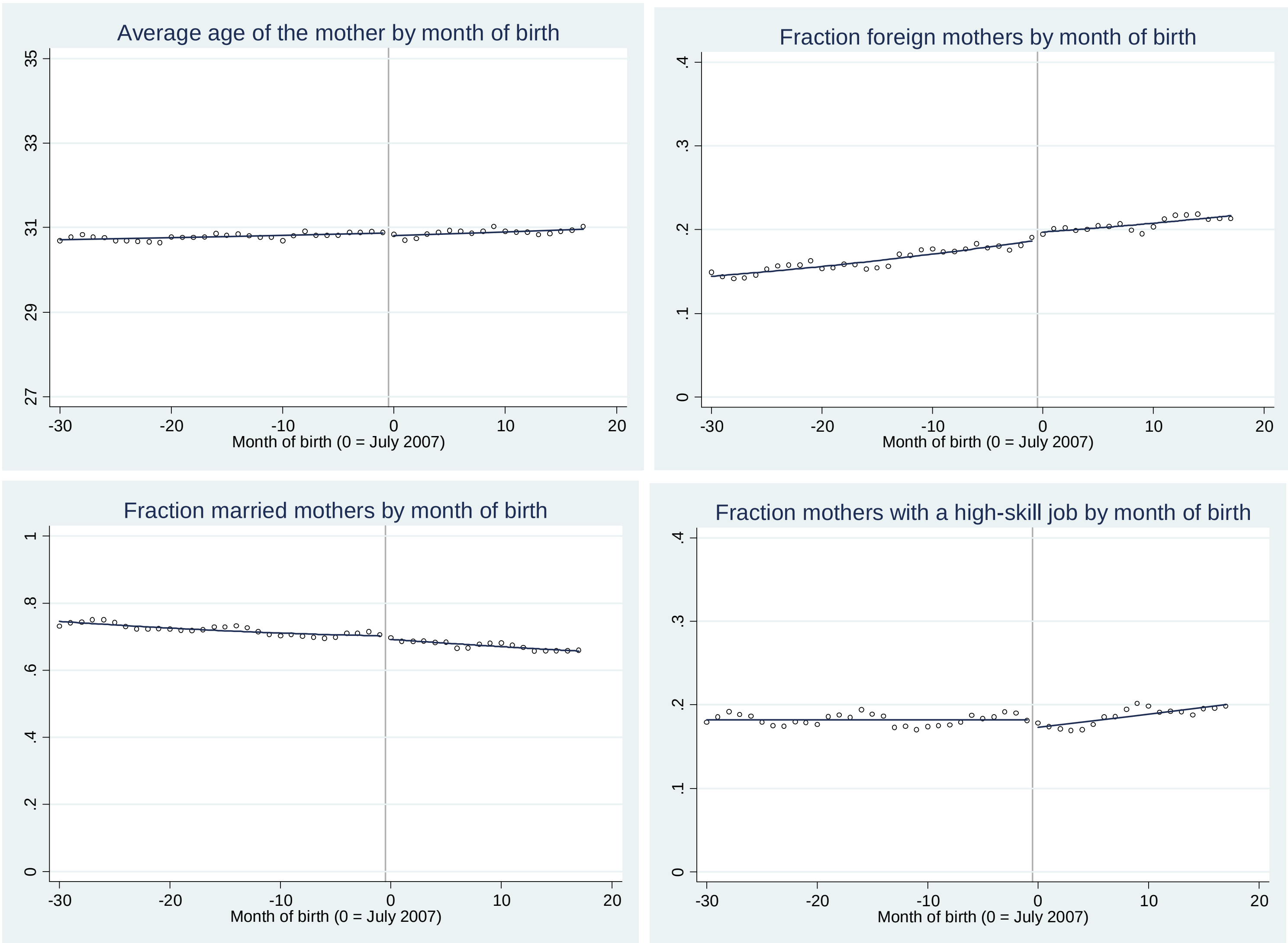
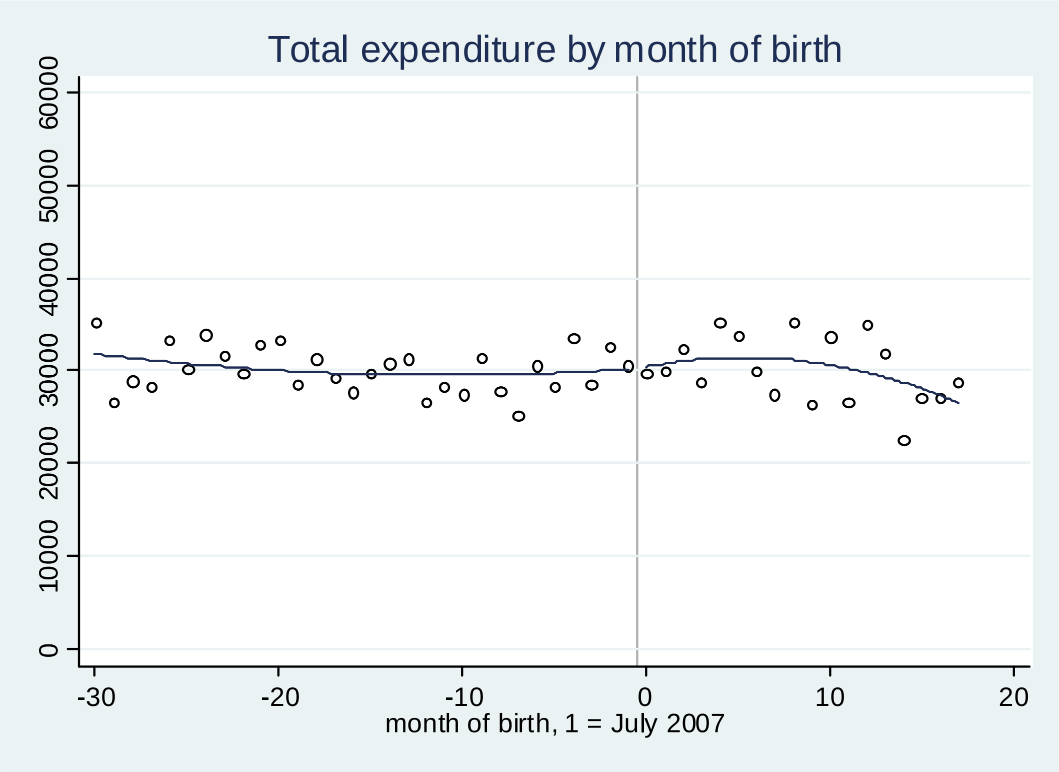
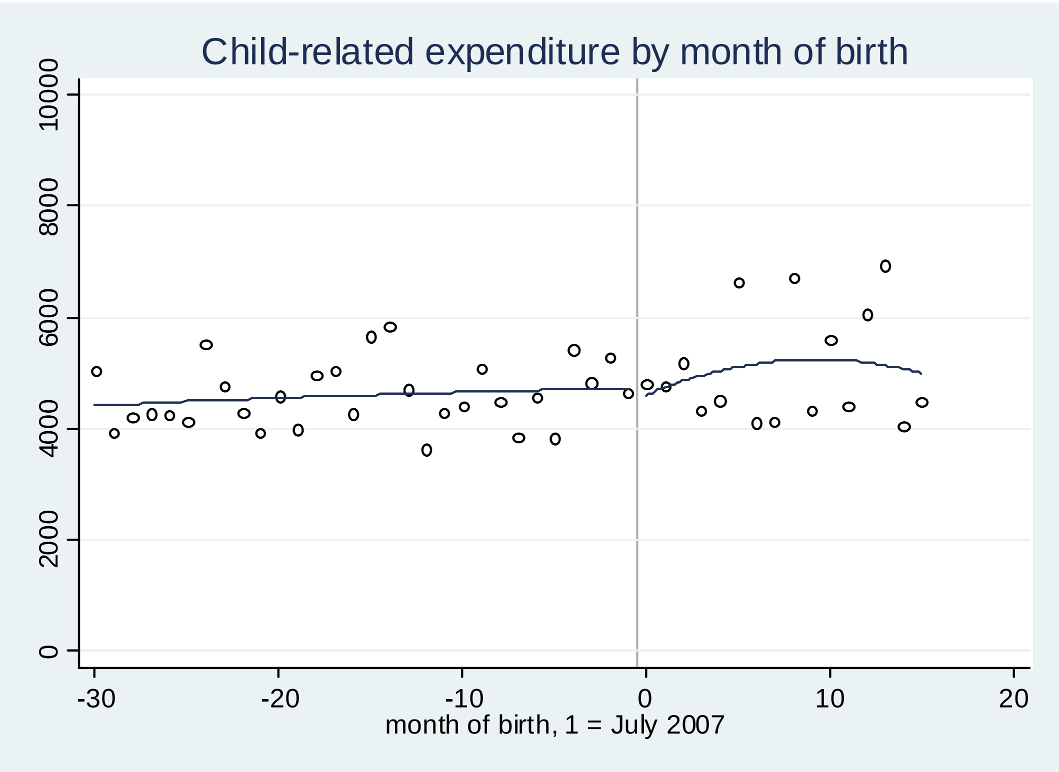


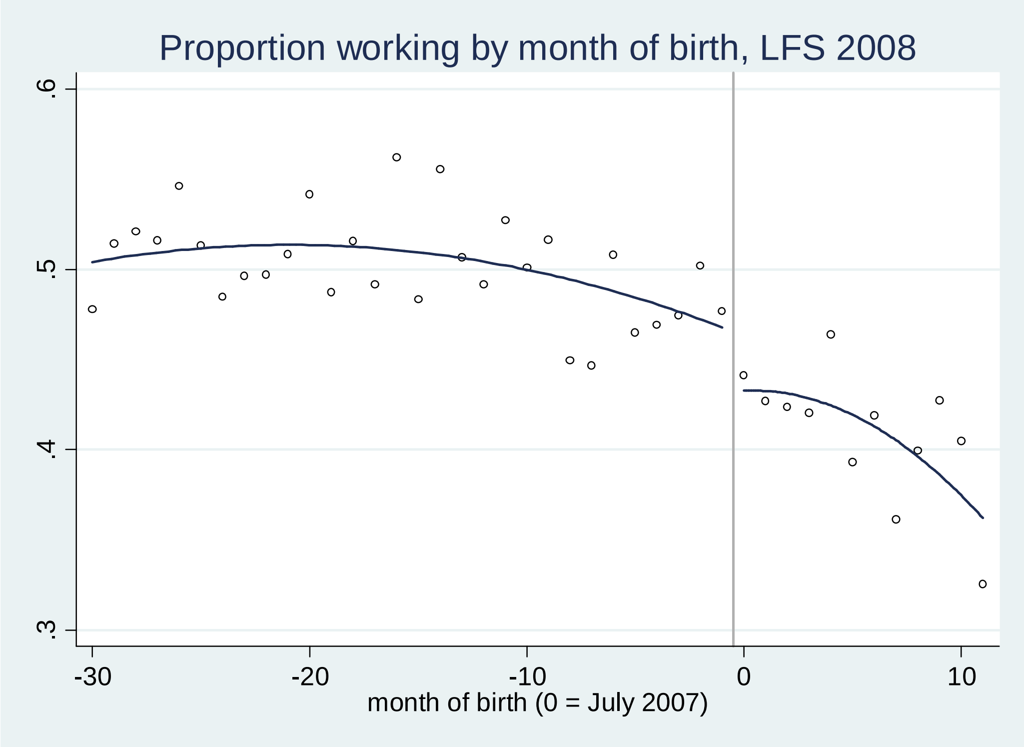
Figure 3. Household expenditure (annual) by month of birth (HBS 2008)

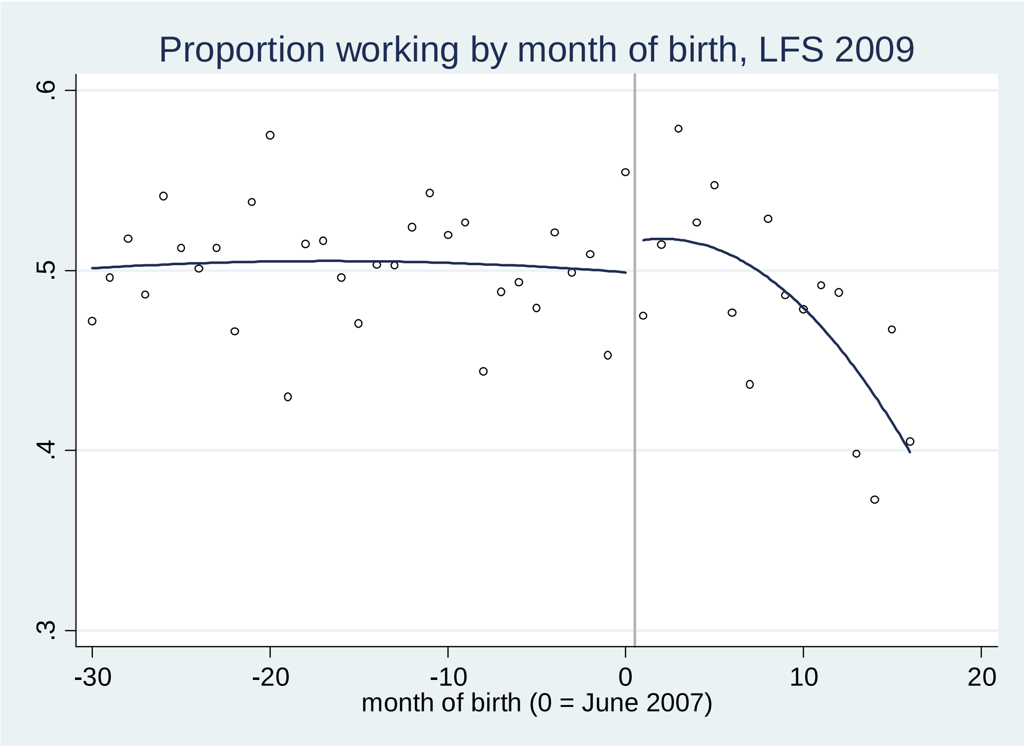




Note: Second-order polynomial fits on both sides of July 2007 are shown.

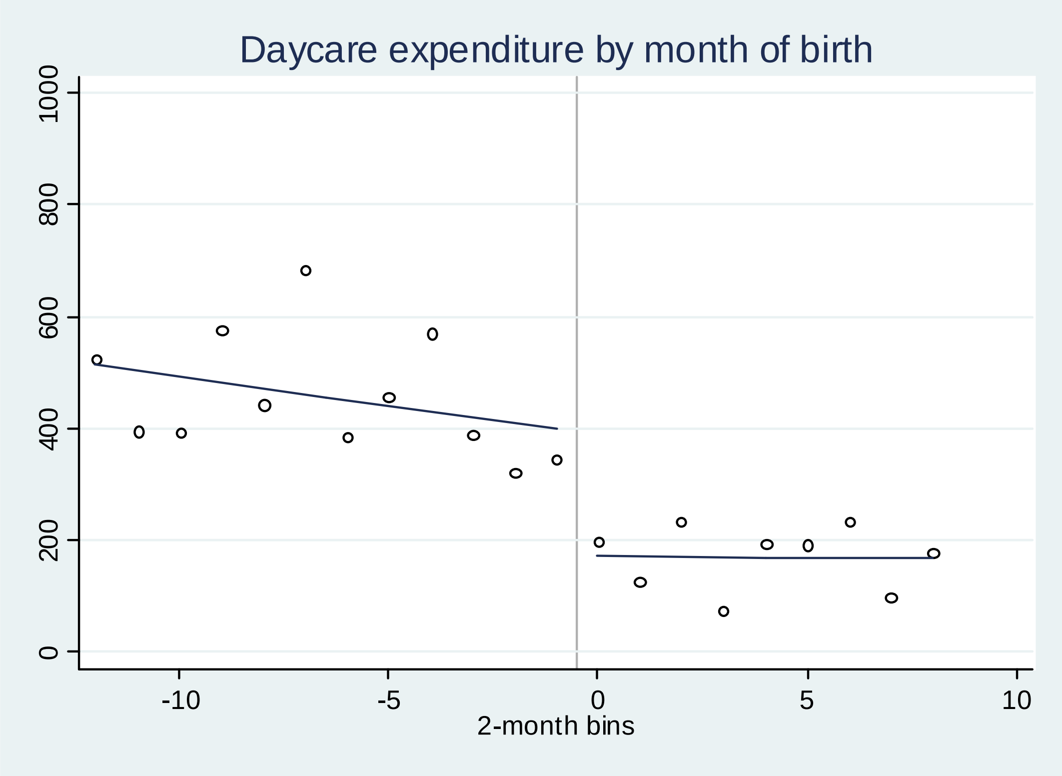
Figure 4. Maternal employment in 2008 and 2009 by month of birth

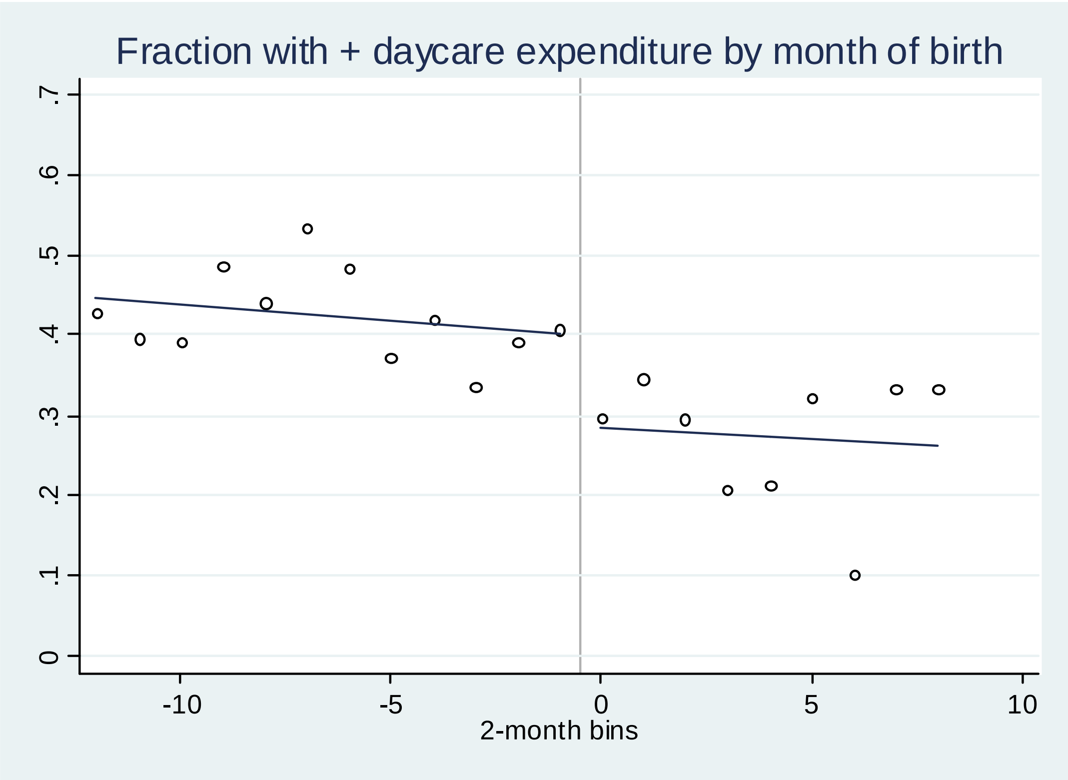




Note: Second-order polynomial fits on both sides of July 2007 are shown.

Figure 5. Daycare expenditure by month of birth (HBS 2008)





Note: Bi-monthly averages shown, as well as separate linear fits on both sides of July 2007.

Appendix tables

Table A1. Discontinuity in number of births at the threshold

	RDD 90-30m	RDD 30m	RDD 12m	RDD 9m	RDD 3m	DiD1	DiD2	DiD3
	1	2	3	4	5	6	7	8
Post	0.0069	0.0274	0.0469	0.0511	0.0416	-0.0349	-0.0163	0.0080
	(0.0250)	(0.0221)	(0.0446)	(0.0331)	(0.0332)	(0.0230)	(0.0172)	(0.0177)
Years included	2000-2009	2005-2009	2006-2008	2006-2008	2007	1990-2009	2000-2009	2005-2009
N	120	60	24	18	6	240	120	60
Linear trend in m	Y	Y	Y	Y	N	Y	Y	Y
Quadratic trend in m	Y	Y	Y	N	N	Y	Y	Y
Cubic term in m	Y	N	N	N	N	Y	Y	N
Control days of the month	Y	Y	Y	Y	Y	Y	Y	Y
Calendar month of birth dummies	N	N	N	N	N	Y	Y	Y

(* p<0.1 ** p<0.05 *** p<0.01)

Notes: Vital Statistics data. The coefficients reported are for the binary indicator taking value 1 for births taking place after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is the natural log of the monthly number of births in Spain.

Table A2. Long-term effects on expenditure, labor supply and separation

Dependent variables	1 RDD 9m	2 RDD 6m	3 RDD 4m	4 RDD 3m	5 RDD 2m	6 RDD 2m	7 DiD 1
Total exp.	2234	1129	3430	1958	2309	2736	-756
	(2771)	(2248)	(2897)	(1559)	(1963)	(1942)	(2015)
Child-related exp.	-855	-806	-371	-222	-205	-63	-417
	(640)	(549)	(637)	(362)	(483)	(436)	(525)
Durable goods exp.	1387	1122	1767	872	1077	1193	84
	(1277)	(1009)	(1229)	(746)	(926)	(895)	(918)
Total exp. (logs)	0.085	0.044	0.154	0.063	0.086	0.112	0.003
	(0.104)	(0.082)	(0.104)	(0.055)	(0.073)	(0.070)	(0.074)
Child-related exp. (logs)	-0.074	-0.105	0.139	-0.021	0.077	0.122	-0.105
	(0.196)	(0.156)	(0.201)	(0.106)	(0.142)	(0.133)	(0.141)
Durable goods exp. (logs)	0.304	0.247	0.410	0.211	0.203	0.229	0.237
	(0.268)	(0.211)	(0.260)	(0.146)	(0.176)	(0.171)	(0.205)
Private daycare expenditure	29	-38	128	-25	45	113	-108
	(249)	(201)	(247)	(137)	(171)	(166)	(169)
Private daycare exp. (binary)	0,029	-0,054	0,039	-0,021	-0,048	-0,016	-0,067
	(0,099)	(0,081)	(0,010)	(0,056)	(0,066)	(0,070)	(0,073)
Working last week	-0.0561*	-0.0293	-0.0955***	-0.0109	-0.0347	-0.0360	0.0785***
	(0.0327)	(0.0255)	(0.0350)	(0.0179)	(0.0250)	(0.0239)	(0.0244)
Employed	-0.0781**	-0.0449*	-0.1087***	-0.0273	-0.0495**	-0.0488**	0.0137
	(0.0318)	(0.0247)	(0.0339)	(0.0173)	(0.0249)	(0.0231)	(0.0168)
Mother sep. or divorced	0.0034	0.0056	0.0054	-0.0059	0.0078	0.0062	-0.0111
	(0.0123)	(0.0095)	(0.0140)	(0.0066)	(0.0099)	(0.0096)	(0.0073)
Linear trend in m	Y	Y	Y	N	N	N	Y
Quadratic trend in	Y	N	N	N	N	N	Y
Cal. month of birth	N	N	N	N	N	N	Y
Controls	Y	Y	Y	Y	N	Y	Y
N. of months	18	12	8	6	4	4	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variable is indicated in each row header. The data sources are: HBS (2009) for the expenditure variables, and LFS (2009) for the rest. The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, child parity, and month of interview dummies. See appendix for the exact definition of child-related expenditure and durable goods expenditure.
(* p<0.1 ** p<0.05 *** p<0.01)

Table A3. Childcare hours results (EU-SILC 2008)

Dependent variables	1 **RDD 9m**	2 **RDD 6m**	3 **RDD 4m**	4 **RDD 3m**	5 **RDD 2m**	6 **RDD 2m**	7 **DiD 1**	8 **DiD 2**
Daycare (binary)	-0.0787	-0.0506	-0.0594	-0.0392*	-0.0577*	-0.0633*	-0.0153	-0.0409
	(0.0657)	(0.0470)	(0.0696)	(0.0228)	(0.0327)	(0.0368)	(0.0355)	(0.0402)
Official (binary)	0.1972	0.1257	0.1323	0.0463	-0.0245	-0.0146	0.0312	0.0147
	(0.1486)	(0.1175)	(0.1468)	(0.0841)	(0.0988)	(0.1026)	(0.0719)	(0.0843)
Nanny (binary)	0.0258	0.0386	0.0833	0.0216	0.0297	0.0661	-0.0199	-0.0043
	(0.0691)	(0.0535)	(0.0735)	(0.0385)	(0.0469)	(0.0567)	(0.0330)	(0.0378)
Informal (binary)	0.1606	0.2089**	0.1136	0.0958	0.0122	0.0646	-0.0268	0.0962
	(0.1214)	(0.0977)	(0.1127)	(0.0704)	(0.0826)	(0.0847)	(0.0633)	(0.0723)
N	441	288	181	139	96	93	1358	1358
Linear trend in m	Y	Y	Y	N	N	N	Y	Y
Quadratic trend in m	Y	N	N	N	N	N	Y	N
Calendar month of birth dummies	N	N	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	N	Y	Y	Y
N. of months	18	12	8	6	4	4	48	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variables are indicated in each row header, they are all binary indicators that take value 1 if the family reports that the child spends a positive number of hours in each for of childcare (excluding direct parental care). The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, and child parity.
(* p<0.1 ** p<0.05 *** p<0.01)

Table A4. Maternal health results

Dependent variable	1 RDD 9m	2 RDD 6m	3 RDD 4m	4 RDD 3m	5 RDD 2m	6 RDD 2m	7 DiD 1	8 DiD 2
Mother's health bad or very bad	-0,064**	-0,026*	-0,037	-0,011	-0,019	-0,018	-0,022	-0,026**
	(0,0294)	(0,0144)	(0,0230)	(0,0149)	(0,0212)	(0,0225)	(0,0185)	(0,0127)
Mother limited daily activities	-0,014	-0,013	-0,06	0,003	-0,007	-0,018	-0,102*	-0,045
	(0,0940)	(0,0690)	(0,0857)	(0,0417)	(0,0542)	(0,0552)	(0,0610)	(0,0442)
N	441	288	181	139	95	93	1358	1358
Linear trend in m	Y	Y	Y	N	N	N	Y	Y
Quadratic trend in m	Y	N	N	N	N	N	Y	N
Calendar month of birth dummies	N	N	N	N	N	N	Y	Y
Controls	Y	Y	Y	Y	N	Y	Y	Y
N. of months of birth	18	12	8	6	4	4	48	48

Notes: The coefficients reported are for the binary indicator taking value 1 for months after June 2007. Each coefficient is from a different regression. Standard errors are shown in parentheses. The dependent variables are indicated in each row header; both are binary. The sample in the RDD specifications includes families who had a child between 2 and 9 months before or after July 1, 2007. The DiD specifications include all families who had a child between 2005 and 2008, both included. Control variables are: age of the mother, age squared, age cubed, three educational attainment dummies, an immigrant status dummy, and child parity. (* p<0.1 ** p<0.05 *** p<0.01)