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The Effect of a Universal Child Benefit on Conceptions, Abortions, and Early Maternal Labor Supply[†]

By LIBERTAD GONZÁLEZ*

I study the impact of a universal child benefit on fertility and maternal labor supply. I exploit the unanticipated introduction of a sizable child benefit in Spain in 2007. Following a regression discontinuity-type design, I find that the benefit significantly increased fertility, in part through a reduction in abortions. Families who received the benefit did not increase consumption. Instead, eligible mothers stayed out of the labor force longer after childbirth, which led to their children spending less time in formal child care. (JEL I38, J13, J16, J22)

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 of families and the 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 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

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¹ Families with children were entitled to some kind of cash benefit in all 33 OECD countries in 2007 (OECD 2011).

² See, for example, Milligan and Stabile (2009, 2011) for child outcomes, and Milligan (2005) or Cohen, Dehejia, and Romanov (2007) for fertility effects.

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 women who give birth are “as if” randomly assigned to a treatment group (who receives a large cash benefit) or a control group (that doesn’t). The source of this randomization is the sharp cutoff 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 3, and all mothers giving birth from July 1 on were eligible to receive it. The benefit was a one-time payment of €2,500 (about US\$3,900), or almost 4.5 times the monthly gross minimum wage for a full-time worker.

The larger goal of the benefit was to increase fertility, with a more specific aim at “compensating the increase in expenditures” associated with the birth of a new child. Thus, I first analyze the potential fertility effects, and then study the impact on household expenditure patterns. I also consider a potential unintended side effect on maternal labor supply. In order to do so, I exploit a range of independent datasets with information on the different variables of interest.

The identification strategy is quite straightforward for the expenditure and labor supply outcomes. I essentially compare the behaviour of households who had a child right before and right after the cutoff date. These families are statistically identical in all observable (and, plausibly, unobservable) 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 dataset (most often, not even to the mother). I use vital statistics data, which include date of birth but also weeks of gestation for all live births, 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 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 about 6 percent.

The estimation strategy for the fertility effects, based on combining estimated conception dates and abortions data, is a key novelty of the paper. The literature on fertility effects typically analyzes the time series of births, using information on date of birth only. The approach in this paper allows for a more precise focus on the timing of the individual (or family) decisions that eventually lead to a birth.

Regarding consumption and maternal labor supply, 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.

The main contribution of the paper is to credibly identify the (short-term) effects of a universal family benefit on a range of outcomes, including those explicitly targeted by the policy (fertility, expenditures) as well as others (maternal labor supply) 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 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 increases in other forms of unearned income (Bagues and Esteve-Volart 2011).

The remainder of the paper is organized as follows. Section I introduces some additional background on the policy change that gives rise to the natural experiment.

³See, for example, Milligan (2005) and Cohen, Dehejia, and Romanov (2007) for fertility; Dahl and Lochner (2012) and Milligan and Stabile (2011) for child outcomes; Lundberg, Pollak, and Wales (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.

⁴Imbens, Rubin, and Sacerdote (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 et al. (2011) studies consumption. Hankins, Hoekstra, and Skiba (2010) study individual bankruptcy.

It also details the identification strategy and describes the main data sources. Section II discusses the results for the different sets of outcomes, and Section III concludes.

I. Methodology and Data

A. Institutional Setting

On July 3, 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 (slightly over US\$3,900)⁵ to all new mothers, starting with those giving birth on or after the announcement date. The eligibility cutoff would subsequently be moved (for practical reasons) to July 1. 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 €570.60 in 2007, and about 20 percent of working women earned the minimum wage or below (2007 Wage 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 according to the 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 child-birth, 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 percent of all births taking place in Spain during the year (including ineligibles).

B. Empirical Strategy and Data

The identification strategy relies on the fact that the policy established a sharp cutoff in birth date for benefit eligibility, and the fact that this cutoff was announced

⁵In 2012 US dollars. Calculated using the US dollars to euro exchange rate from the second semester of 2007, and the US rate of inflation from 2007 to 2012.

⁶Law 35/2007 (November 15, 2007).

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

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 family expenditures and labor supply. If the policy successfully encouraged fertility, we expect to observe a sudden 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 of 2007, perhaps controlling for seasonality.

The second part of the analysis addresses the effect of benefit receipt on household expenditures and maternal labor supply. 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.

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 mid-1990s. 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 3, 2007. While intention to conceive is (to my knowledge) 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:

$$(1) \quad C_m = \alpha + \gamma_1 m + \gamma_2(m \cdot \text{post}) + \beta \cdot \text{post} + \lambda X_m + \varepsilon_m,$$

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.

⁸For each individual birth, I estimate its likely month of conception by subtracting 9 months to the month of birth if weeks of gestation were between 39 and 43; 8 months if weeks of gestation were fewer than 39; and 10 months if weeks of gestation were over 43.

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.⁹ The linear term in m accounts for any smooth fertility trends, and it is allowed to change after the policy reform.¹⁰ 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.

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

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 three to six 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.

The last 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. Twelve of them, representing about 88 percent of total Spanish population in 2007 (also about 88 percent 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.

⁹The whole fertility analysis is also performed at the quarterly, rather than monthly, level.

¹⁰I also estimate regressions where the trend captured by the polynomial is not allowed to change at the cutoff.

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 percent) of abortions take place at less than 13 weeks of gestation, a reduction in the incidence of abortions right after July 3, 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 overall effect on births to appear starting in January 2008 but perhaps increasing until September of 2008, if we assume it may take up to six months to conceive.

I illustrate the overall fertility effect by estimating the following equation:

$$(3) \quad B_m = \alpha + \gamma_1 m + \gamma_2 m^2 + \gamma_3 m^3 + \beta post + \sum_{c=2}^{12} \mu_c month_m + \varepsilon_m,$$

where B is the (natural log of) the number of births in month m , and “ $post$ ” takes value 1 starting in January 2008. A cubic trend is included as a control, as well as calendar month dummies. Coefficient β captures the overall increase in the number of births after benefit introduction. In order to capture any potential dynamics, I alternatively include a set of “ $post$ ” dummies, grouping the postbenefit period in 6-month bins. We expect the increase in births to appear between months 6 (January 2008) and 15 (September 2008).

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 38,000, while there are about 6,700 abortions per month in the data.¹¹

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

Household Expenditure and Labor Supply Effects.—The analysis of the expenditure and labor supply 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 cutoff.¹² Close enough to the threshold, treatment is “as if” randomly assigned (Lee and Lemieux 2010).

I estimate regressions of the following form:

$$(4) \quad Y_{im} = \alpha + \gamma_1 m + \gamma_2(m \cdot post) + \beta \cdot post + \Pi X'_{im} + \varepsilon_{im}.$$

¹¹ Since the data include about 88 percent of all abortions at the national level, this figure needs to be scaled up in order to obtain a national estimate.

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).

TABLE 1—DESCRIPTIVE STATISTICS

	Mean	SD	Median			
<i>Panel A. Vital statistics (2000–09)^a</i>						
Monthly number of conceptions	38,021	3,168	38,505			
Post-June 2007 dummy	0.25	0.435	0			
Month of conception	−30.5	34.8	−30.5			
<i>Panel B. Abortions statistics (2000–09)^b</i>						
Monthly number of abortions	6,725	1,348	6,535			
Post-June 2007 dummy	0.25	0.435	0			
Month of abortion	−30.5	34.8	−30.5			
<i>Panel C. Household budget survey (2008)^c</i>						
Total expenditure	30,507	17,721	26,145			
Child-related expenditure	4,778	4,275	3,561			
Durable expenditure	5,654	8,740	2,276			
Day care expenditure	306	848	0			
Post-June 2007 dummy	0.487	0.500	0			
Month of birth	−0.780	5.056	−1			
Age of mother	32.62	5.14	33			
Mother some secondary	0.234	0.424	0			
Mother high school graduate	0.326	0.469	0			
Mother college graduate	0.321	0.467	0			
Mother immigrant	0.165	0.371	0			
Not first born	0.539	0.499	1			
	2008			2009		
	Mean	SD	Median	Mean	SD	Median
<i>Panel D. Labor force survey^d</i>						
Worked last week	0.422	0.494	0	0.505	0.500	1
Currently employed	0.541	0.498	1	0.576	0.494	1
Post-June 2007 dummy	0.477	0.500	0	0.453	0.498	0
Month of birth	−0.815	5	−1	−0.415	5	0
Age of mother	32.38	5.25	33	33.40	5.26	34
Mother some secondary	0.236	0.425	0	0.239	0.427	0
Mother high school graduate	0.343	0.475	0	0.353	0.478	0
Mother college graduate	0.297	0.457	0	0.298	0.458	0
Mother immigrant	0.170	0.376	0	0.154	0.361	0
Not first born	0.523	0.500	1	0.532	0.499	1

^aThe sample includes all (estimated) conceptions in Spain between 2000 and 2009 (both included), by month. The number of observations is 120.

^bThe sample includes all abortions in 12 of the 17 Spanish regions between 2000 and 2009 (both included), by month. The number of observations is 120.

^cThe 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.

^dThe sample includes all households interviewed in 2008 (2009) who had a baby between October 2006 and March 2008 (both included). The number of observations is 8,691 (9,379).

This is very similar to equation (1), except for the individual-level subscript. Y is an outcome variable (say, total household expenditure or a measure of maternal labor supply) for household i who had a child on month m .¹³ Month of birth m (the “running” variable) is again normalized to zero at the threshold (July 2007).¹⁴

¹³The whole analysis is also performed at the quarterly, instead of monthly, level.

¹⁴Again, the equation includes a linear term in m that is allowed to change at the cutoff, but I also explore higher-order polynomials.

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 datasets.¹⁵ First, I explore whether benefit receipt translated into changes in household expenditure patterns (Section IIB). Then I analyze the potential labor supply effects. Finally, I also explore whether the benefit affected child care use.¹⁶

In the main specification, Y is observed in 2008, or about 12 months after birth (on average) for children born at the cutoff date. The main sample includes households with children born between nine months before and nine months after the policy change (i.e., between October 2006 and March 2008)¹⁷, so that the children are between zero and two years of age when observed.¹⁸ In additional specifications, I vary the number of months around the threshold included in the sample, up to only two 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.¹⁹ 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 cutoff 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 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 IIB 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 IIB.

¹⁵ 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 individual-level datasets that I use, I observe about 450 births per month.

¹⁶ I also analyze some additional outcomes: family stability (in particular, parental separation), and maternal and child health.

¹⁷ The main specification includes only children born up to nine months after the benefit announcement in order to minimize the likelihood of including births induced by the policy change (“selection effect”).

¹⁸ Technically I would need to exclude mothers with less than two 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.

¹⁹ No other policy changes in 2007 or thereafter applied differentially to children born before and after July 1, 2007. Note that the cutoff birth date that determines the year when a child starts school (or preschool) in Spain is December 31.

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 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, 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 zero and three. The second DID specification 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, 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 datasets 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 in 2008, including almost €5,000 in directly child-related goods or services. The main labor supply analysis uses data from the larger Labor Force Survey (LFS). When interviewed in 2008, Table 1 shows that 42 percent of mothers in the sample reported working the previous week (54 percent were employed, indicating that 12 percent were on leave from their job).²⁰

II. Results

A. Fertility

The fertility results are summarized in Table 2 and Figure 1. As described in Section IB, 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 bimonthly number of conceptions (resulting 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

²⁰ Additional labor supply regressions are estimated using administrative data from the Social Security administration (2009 Sample of Working Histories).

TABLE 2— FERTILITY RESULTS (CONCEPTIONS AND ABORTIONS)

	RDD 10 years (1)	RDD 5 years (2)	RDD 12–12m (3)	RDD 9–9m (4)	RDD 3–3m (5)	DID 10 years (6)	DID 7 years (7)	DID 5 years (8)
Conceptions	0.0514** (0.0221)	0.0695*** (0.0250)	0.0852** (0.0359)	0.0750** (0.0285)	0.0503 (0.0314)	0.0452*** (0.0105)	0.0551*** (0.0087)	0.0527*** (0.0136)
Abortions	–0.1248*** (0.0427)	–0.1730*** (0.0504)	–0.2187** (0.0781)	–0.2165*** (0.0613)	–0.1304** (0.0310)	–0.0751*** (0.0268)	–0.0610** (0.0254)	–0.0649* (0.0366)
Years included	2000–09	2005–09	2006–08	2006–08	2007	2000–09	2003–09	2005–09
<i>N</i> (number of months)	120	60	24	18	6	120	96	60
Linear trend in <i>m</i>	Y	Y	Y	Y	N	Y	Y	Y
Quadratic trend in <i>m</i>	Y	Y	Y	N	N	Y	Y	Y
Cubic term in <i>m</i>	Y	N	N	N	N	Y	N	N
Number days of the month	Y	Y	Y	Y	Y	Y	Y	Y
Calendar month dummies	N	N	N	N	N	Y	Y	Y

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 12 out of the 17 Spanish regions. The “m” in column headers stands for months.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

increased significantly, by about 5 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 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 bimonthly number of abortions between 2005 and 2009, with separate linear fits for the periods before and after the benefit. An overall positive trend is apparent, as well as strong seasonality, with fewer abortions in the second half of the year than in the first. There may have been a larger drop in the incidence of abortions in July–August of 2007 compared to the same months in the surrounding years.

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

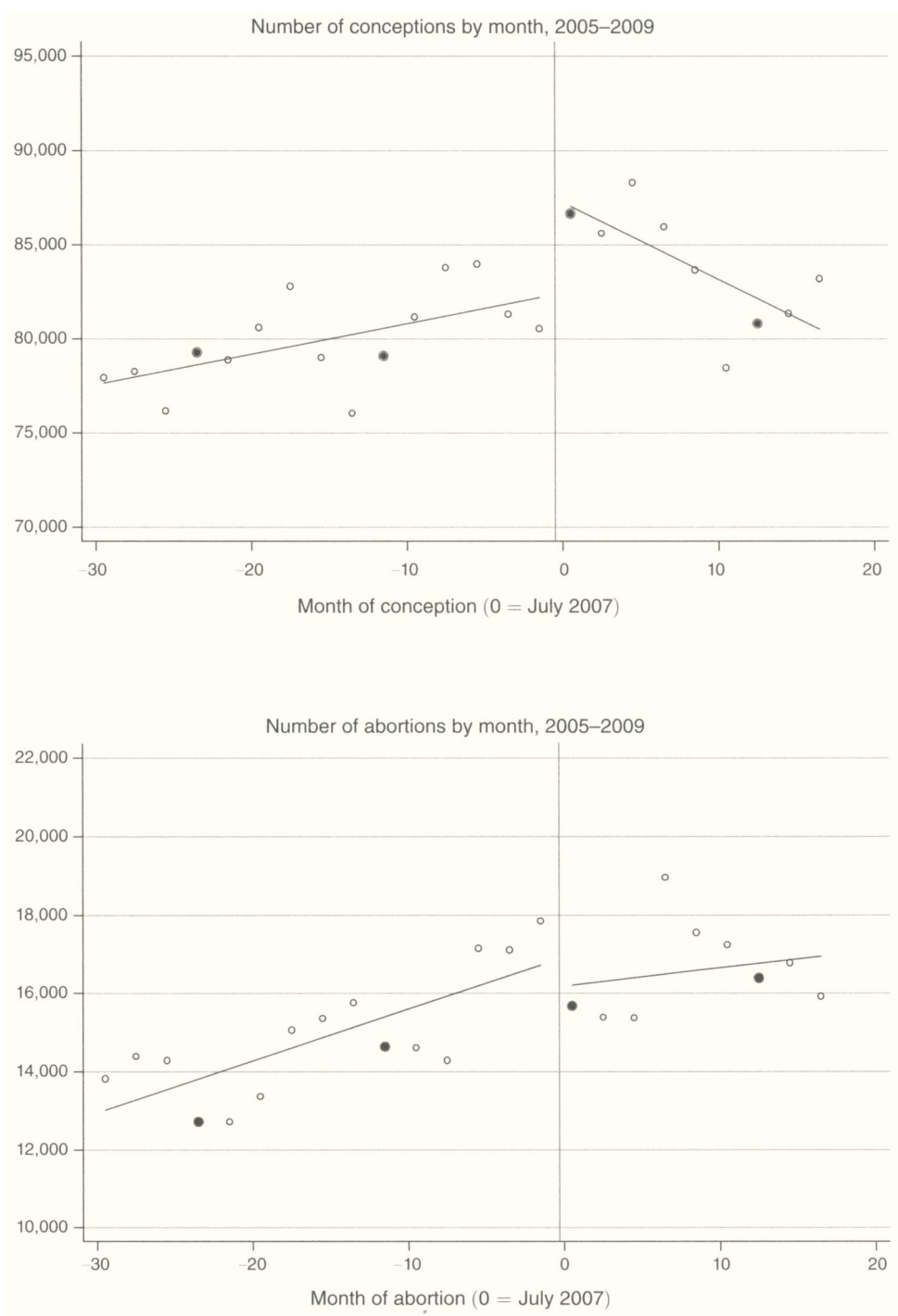


FIGURE 1. FERTILITY EFFECT: CONCEPTIONS AND ABORTIONS BY MONTH

Notes: Bimonthly averages shown, as well as separate linear fits on both sides of July 2007. Months of July–August indicated with a solid circle.

magnitudes range between 12 and 22 percent. However, Figure 1 suggested that seasonality was important. Once the month fixed effects are included (in columns 6 to 8), the magnitude of the effect is reduced to about 6–7 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.²¹ My estimates based on equation (3) indicate that the benefit may have led to an overall (sustained) increase in the number of births of about 6 percent (see Appendix Table A1).²²

B. Household Expenditure and Labor Supply

Next, I report the results from the analysis of the effects of the child benefit on a range of outcomes related to household expenditure patterns and maternal labor supply. 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 one 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 are presented in Table A2.²³ 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 birth dummies, suggests a small (0.8 log-points) effect, but the coefficient is far from statistically significant.²⁴

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:

$$(5) \quad X_{im} = \alpha + \gamma_1 m + \gamma_2(m \cdot post) + \beta \cdot post + \varepsilon_{im}.$$

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 datasets (mainly vital statistics, Household Budget Survey, and Labor Force Survey). Results are reported in Figure 2 and Table 3.

²¹ This is confirmed by the parallel analysis performed at the quarterly rather than monthly level (see online Appendix for results).

²² By combining the results from the separate specifications on conceptions and abortions with the average number of conceptions and abortions in the pre-reform months, we can attribute slightly over 80 percent of the overall fertility effect to the increase in conceptions, and almost 20 percent to the fall in abortions.

²³ Note that the specifications in Table A2 are parallel to those in Table 2.

²⁴ The regressions in Table A2 can also be thought of as a placebo test for the fertility results in Section IIA.

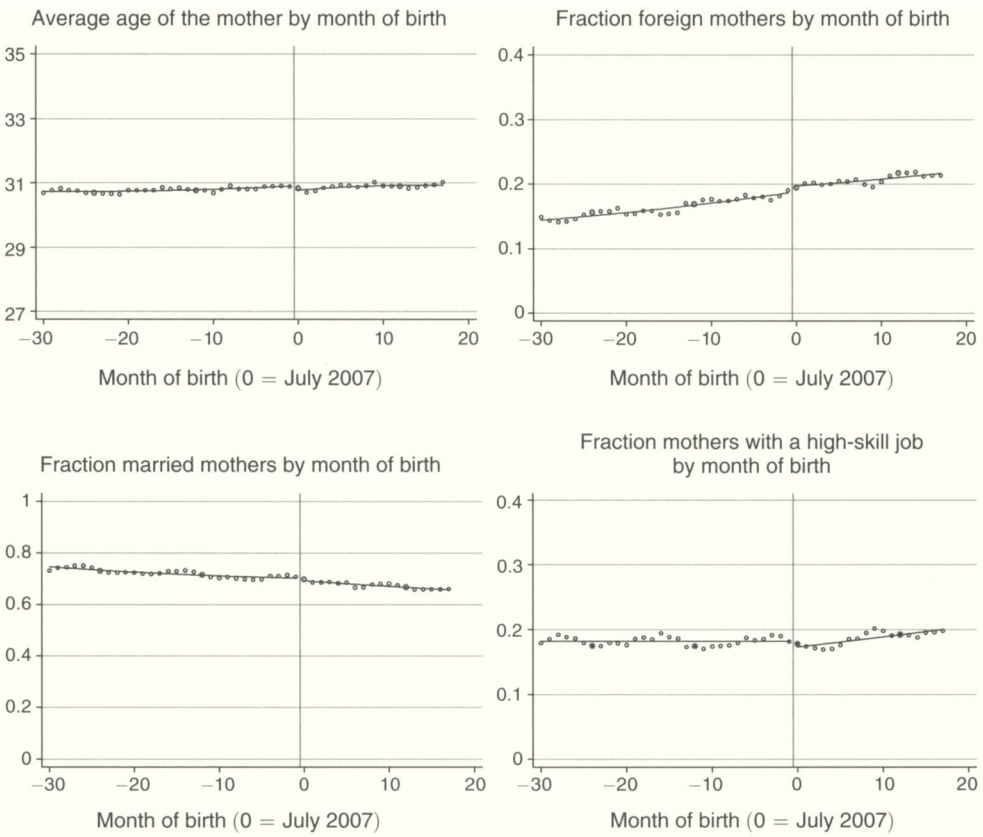


FIGURE 2. BALANCE IN COVARIATES (VITAL STATISTICS)

Notes: Monthly averages shown, together with separate linear fits on both sides of July 2007. Month of July indicated with a solid circle.

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 percent 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.

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 up to nine months before and after the cutoff. Later columns restrict the data to births closer and closer to the threshold, so that in column 5, I only include children born between two months before and two months after July 1, 2007.

TABLE 3—BALANCE IN COVARIATES

	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Household Budget Survey (2008)</i>					
Age of the mother	0.359 −1.029 (0.814)	0.095 (0.997)	−0.024 (0.597)	−0.044 (0.708)	−0.407 (0.708)
Age of the father	0.117 −1.165 (0.912)	0.120 (0.679)	−0.415 (0.769)	0.465 (0.052)	−0.324 (0.052)
Mother secondary	0.044 (0.079)	0.045 (0.063)	0.071 (0.077)	−0.010 (0.047)	−0.005 (0.052)
Mother high school graduate	−0.003 (0.095)	−0.055 (0.074)	−0.043 (0.092)	0.010 (0.053)	0.001 (0.063)
Mother college graduate	−0.026 (0.093)	0.012 (0.073)	0.018 (0.090)	0.011 (0.052)	0.012 (0.062)
Father secondary	0.127 (0.086)	0.084 (0.069)	0.118 (0.085)	0.044 (0.050)	0.041 (0.058)
Father high school graduate	−0.026 (0.090)	−0.032 (0.071)	−0.005 (0.088)	−0.007 (0.051)	0.005 (0.060)
Father college graduate	−0.107 (0.089)	−0.064 (0.070)	−0.096 (0.087)	−0.024 (0.049)	−0.036 (0.059)
Mother immigrant	−0.038 (0.073)	−0.016 (0.057)	0.013 (0.071)	0.011 (0.044)	0.058 (0.047)
Not first born	−0.017 (0.099)	0.003 (0.078)	−0.036 (0.096)	−0.017 (0.056)	−0.045 (0.066)
Observations	958	651	446	319	234
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No
Quadratic trend in <i>m</i>	Yes	No	No	No	No
Number of months	18	12	8	6	4
<i>Panel B. Labor Force Survey (2008)</i>					
Age of the mother	−0.307 (0.342)	−0.283 (0.269)	−0.292 (0.332)	−0.692*** (0.189)	−0.164 (0.227)
Age of the father	−0.430 (0.353)	−0.405 (0.274)	−0.728** (0.344)	−0.912*** (0.191)	−0.605*** (0.233)
Mother secondary	0.009 (0.028)	0.013 (0.022)	0.050* (0.028)	0.024 (0.015)	0.040** (0.019)
Mother high school graduate	−0.092*** (0.031)	−0.077*** (0.025)	−0.097*** (0.031)	−0.061*** (0.017)	−0.066*** (0.021)
Mother college graduate	0.063** (0.030)	0.047* (0.024)	0.062** (0.030)	0.041** (0.017)	0.029 (0.020)
Father secondary	−0.015 (0.029)	−0.023 (0.023)	0.002 (0.028)	−0.001 (0.016)	−0.012 (0.019)
Father high school graduate	−0.044 (0.031)	−0.006 (0.024)	−0.025 (0.030)	−0.005 (0.017)	−0.006 (0.020)
Father college graduate	0.044* (0.027)	0.021 (0.021)	0.015 (0.026)	0.008 (0.015)	0.003 (0.018)
Mother immigrant	0.022 (0.024)	0.009 (0.019)	0.015 (0.024)	0.009 (0.014)	0.008 (0.016)
Not first born	0.046 (0.033)	0.007 (0.026)	0.045 (0.032)	−0.017 (0.018)	0.018 (0.022)
Observations	8,691	5,813	4,083	3,026	2,062
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No
Quadratic trend in <i>m</i>	Yes	No	No	No	No
Number of months	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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Mother characteristics appear to be fairly balanced around the threshold. In the HBS regressions (Table 3, panel A), none of the coefficients are statistically significant at the 90 percent 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 (panel B), 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 datasets, 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 expenditures, labor supply and child care use. The main results are presented in tables 4 through 6. 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.

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 faced 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. 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, 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 bottom 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).²⁵

²⁵I 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 significant effect on overall expenditure for families with low-educated fathers. Results are available upon request.

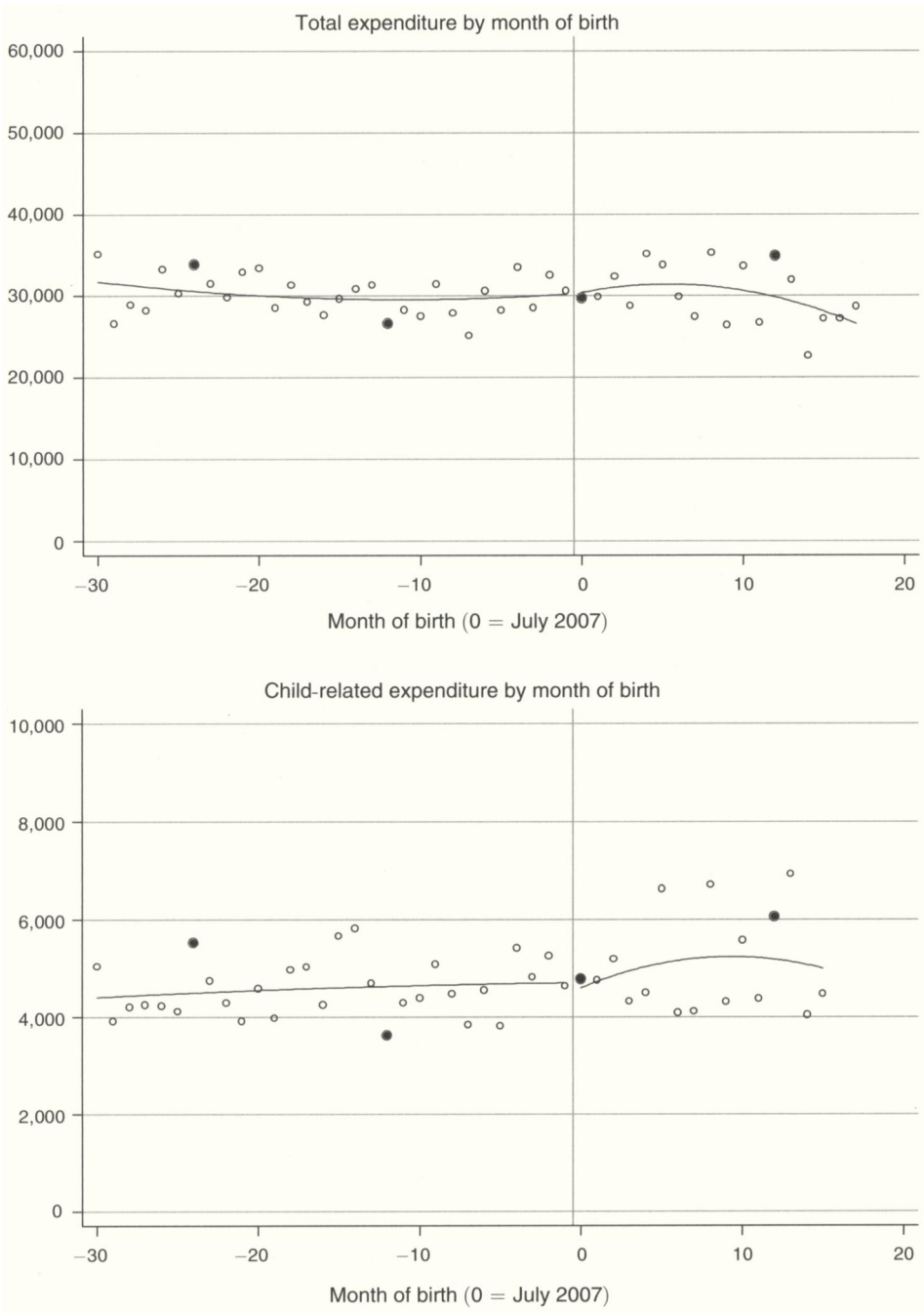


FIGURE 3. HOUSEHOLD EXPENDITURE (ANNUAL) BY MONTH OF BIRTH (HBS 2008)

Notes: Second-order polynomial fits on both sides of July 2007 are shown. Month of July indicated with a solid circle.

TABLE 4—EXPENDITURE RESULTS (HOUSEHOLD BUDGET SURVEY)

	RDD 9m	RDD 6m	RDD 4m	RDD 3m	RDD 2m	RDD 2m	DID 1	DID 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total expenditure	−3,175 (2,838)	−2,247 (2,244)	405 (2,885)	−580 (1,553)	−1,774 (2,032)	−1,084 (1,832)	−1,307 (2,194)	−621 (1,258)
Total expenditure (logs)	−0.142 (0.092)	−0.105 (0.074)	−0.034 (0.092)	−0.041 (0.052)	−0.072 (0.067)	−0.060 (0.063)	−0.049 (0.071)	−0.037 (0.036)
Child-related expenditure	−407 (766)	−599 (618)	357 (795)	−21 (444)	−150 (536)	10 (534)	94 (592)	−350 (320)
Child-related expenditure (logs)	−0.011 (0.195)	−0.034 (0.151)	0.087 (0.194)	0.018 (0.103)	0.002 (0.127)	−0.001 (0.128)	0.017 (0.132)	−0.097 (0.062)
Durable goods expenditure	−1,513 (1,688)	−1,849 (1,345)	−1,011 (1,704)	−760 (927)	−1,046 (1,105)	−999 (1,099)	−1,721 (1,137)	−380 (645)
Durable goods expenditure (logs)	0.157 (0.297)	0.025 (0.239)	0.230 (0.302)	0.089 (0.170)	0.080 (0.200)	0.071 (0.213)	0.022 (0.208)	−0.063 (0.097)
Number of observations	941	640	441	315	230	230	2,249	2,902
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No	No	Yes	Yes
Quadratic trend in <i>m</i>	Yes	No	No	No	No	No	Yes	No
Calendar month of birth dummies	No	No	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Number 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, column 8 used merged HBS data for 2006–2009. The sample in the RDD specifications includes families who had a child between two and nine 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. The “m” in column headers stands for months.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

The available evidence suggests that families did not, on average, increase expenditures as a result of receiving the benefit.²⁶ 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.

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.

²⁶I 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).

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) about 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.²⁷

The six different RDD specifications suggest that mothers who received the benefit were four 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.²⁸

The result is illustrated graphically in Figure 4 (top 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.²⁹

Parallel regressions are estimated using 2009 LFS data, when the children were on average 24 months old (see Table A3 in the Appendix).³⁰ As shown in the bottom 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.

The LFS results are supplemented with the analysis of Social Security data.³¹ Table A4 reports the results from labor supply specifications parallel to those in Table 5, where the dependent variable is now either a dummy for whether a mother was employed six (or 12) months after birth, or a continuous variable for the number of months during the first six (or 12) after birth that the woman was not employed. The results suggest that the benefit led to affected mothers working on average between 0.2 and 0.4 fewer months during the first year after childbirth. This translates into one to two weeks of work on average (or, alternatively, about one third of affected mothers delaying their return to paid work by one month).

The last row in Table A4 shows the results for a set of regressions where the dependent variable is total earnings of the mother during the 12 months following childbirth. I find that women who received the benefit earned about €700 less during the following 12 months (about 30 percent of the benefit amount). These results suggest that the additional €2,500 in household income led to a reduction in maternal labor supply that lowered annual earnings by about €700, and thus an

²⁷ Parallel specifications are estimated for fathers' labor supply, with no significant effects found.

²⁸ These results are confirmed when the analysis is performed at the quarterly, rather than monthly, level, with precision increasing slightly (see online Appendix).

²⁹ Note that employment rates in the later months of birth shown are very low. This is because all women are interviewed in 2008, but vary in the month of birth of their child. Thus, women who gave birth later have younger children at the time of the interview. The decreasing trend in employment rates just reflects that women tend to stay at home for some months after giving birth.

³⁰ Note that specification 8 is absent from Table A3. 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.

³¹ I use the 2009 sample of the Spanish Social Security data on working histories ("Muestra Continua de Vidas Laborales").

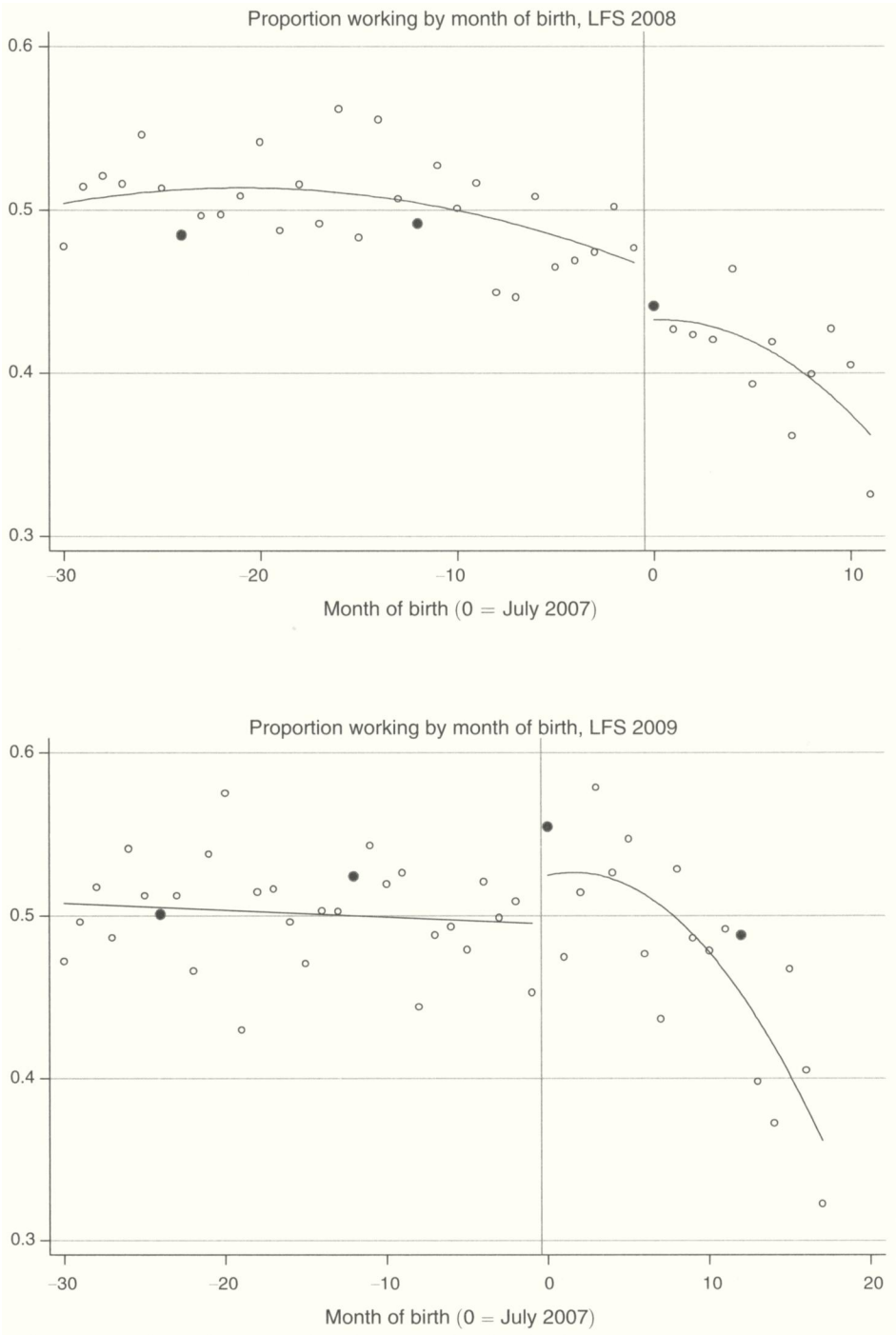


FIGURE 4. MATERNAL EMPLOYMENT IN 2008 AND 2009 BY MONTH OF BIRTH

Notes: Second-order polynomial fits on both sides of July 2007 are shown. Month of July indicated with a solid circle.

TABLE 5—MATERNAL LABOR SUPPLY RESULTS (AT 12 MONTHS, LABOR FORCE SURVEY)

	RDD 9m	RDD 6m	RDD 4m	RDD 3m	RDD 2m	RDD 2m	DID 1	DID 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Working last week	−0.0636** (0.0316)	−0.0430* (0.0249)	−0.0577* (0.0311)	−0.0532*** (0.0179)	−0.0547** (0.0219)	−0.0576*** (0.0213)	−0.0437*** (0.0155)	−0.0206* (0.0116)
Employed	−0.0626** (0.0309)	−0.0392 (0.0243)	−0.0799*** (0.0304)	−0.0535*** (0.0174)	−0.0612*** (0.0219)	−0.0610*** (0.0208)	−0.0203 (0.0166)	−0.0186 (0.0114)
Observations	8,691	5,813	4,083	3,026	2,062	2,062	21,185	25,544
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No	No	Yes	Yes
Quadratic trend in <i>m</i>	Yes	No	No	No	No	No	Yes	No
Calendar month of birth dummies	No	No	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Number 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, column 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. The “m” in column headers stands for months.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

increase in household savings for the remaining amount (since I find no increase in expenditures).

It appears that mothers may have used the benefit (in part) to “buy” time at home during the first few months of their child’s life.³² If this is true, then we should observe differences in child care use between eligible and ineligible families. We turn to this analysis in the next subsection.

Child Care Use.—Child care 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 children in the household in different forms of child care. Although this dataset 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 day care: private day care centers, public infant care centers, and nannies or babysitters. I estimate the same eight specifications for each category of expenditure in levels. In addition, I create three binary variables that indicate whether a family spent any positive amount in each type of child care during the year.

The regressions results in Table 6 suggest that families receiving the benefit may have spent significantly less in private day care during the first year of the child’s life. The regressions in levels (first row) suggest that the magnitude of this effect was between €100 and €200, for an average of 300 (although only two of the eight specifications in levels show statistical significance above 90 percent).

³²I also estimate separate regressions for different subgroups of the population. The labor supply effects are more pronounced for single mothers and low-educated mothers.

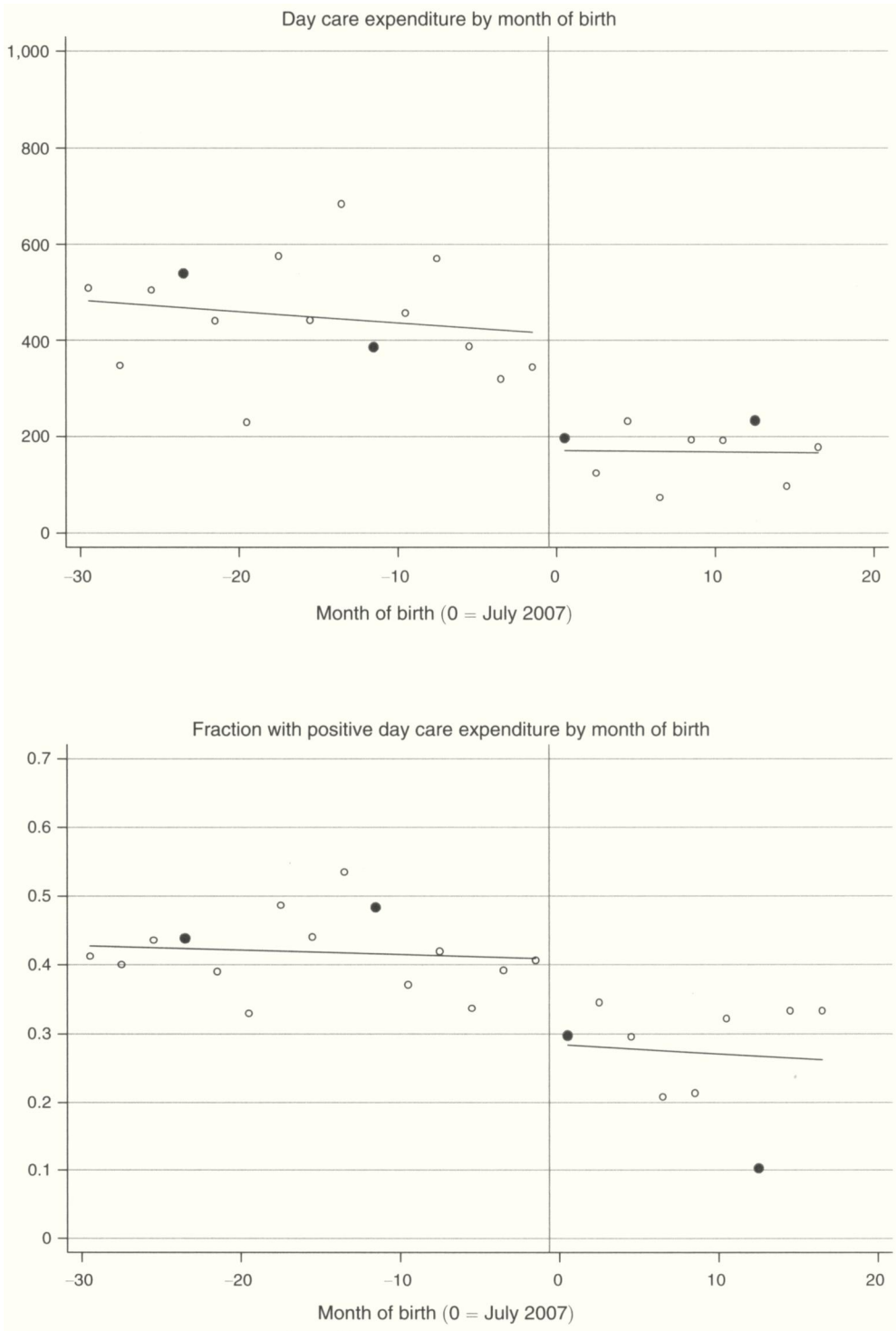


FIGURE 5. DAY CARE EXPENDITURE BY MONTH OF BIRTH (HBS 2008)

Notes: Bimonthly averages shown, as well as separate linear fits on both sides of July 2007. Months of July–August indicated with a solid circle.

TABLE 6—CHILD CARE EXPENDITURE RESULTS (HOUSEHOLD BUDGET SURVEY)

	RDD 9m	RDD 6m	RDD 4m	RDD 3m	RDD 2m	RDD 2m	DID 1	DID 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private day care	−138 (170)	−123 (121)	−195 (161)	−157* (80)	−147 (101)	−158 (103)	−177** (84)	−94 (65)
Private day care (binary)	−0.0795 (0.0940)	−0.0985 (0.0743)	−0.1041 (0.0938)	−0.0943* (0.0538)	−0.1096* (0.0624)	−0.1248* (0.0644)	−0.0364 (0.0500)	−0.0627* (0.0324)
Official infant care center	38 (147)	−16 (134)	89 (139)	−33 (99)	−21 (125)	32 (115)	−14 (76)	−6 (61)
Official infant care center (binary)	−0.1135 (0.0911)	−0.0471 (0.0713)	−0.0538 (0.0897)	0.0070 (0.0510)	−0.0470 (0.0609)	−0.0158 (0.0622)	0.0068 (0.0475)	−0.0129 (0.0298)
Nanny/ babysitter	120 (342)	187 (278)	424 (321)	182 (205)	277 (240)	411 (255)	196 (179)	−10 (127)
Nanny/ babysitter (binary)	−0.0384 (0.0931)	−0.0038 (0.0728)	0.0418 (0.0904)	0.0129 (0.0532)	0.0002 (0.0635)	0.0191 (0.0630)	−0.0161 (0.0502)	−0.0281 (0.0319)
Observations	958	651	446	319	234	234	2,289	2,904
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No	No	Yes	Yes
Quadratic trend in <i>m</i>	Yes	No	No	No	No	No	Yes	No
Calendar month of birth dummies	No	No	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Number 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, column 8 used merged HBS data for 2006–2009. The sample in the RDD specifications includes families who had a child between two and nine 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. The “m” in column headers stands for months.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Perhaps more convincing are the results in the second row, showing that receiving the benefit decreased the fraction of families using private day care by 4 to 12 percentage points.

These results are illustrated in Figure 5. We observe a drop in average expenditure in private day care from about €400 to about €200 at the benefit cutoff (top panel). The bottom panel shows a drop in the fraction of families with positive expenditure in private day care 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.³³ 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 day care using EU-SILC data (see Appendix Table A5).

³³Families apply for public day care, which is heavily subsidized, months in advance. Demand is much higher than supply, and part-time hours are usually not offered.

These results suggest that families that received the benefit were four to 6 percentage points less likely to use private day care during the first year of the child's life.

Taken together, the results in this section 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 child care during that time.³⁴

III. Conclusions

This paper analyzes the effects of a €2,500 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 the annual number of births increased by about 6 percent as a result of the new policy. Part of this effect took place through a reduction in the number of abortions.

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 two 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 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 of life, with no significant change in the consumption of child-related goods or services. This may well have an impact on child well-being.³⁵ Recent research suggests that maternal employment during a child's first year(s) of life may have detrimental effects on cognitive development and health.³⁶ Also, a recent study by Carneiro, Loken, and Salvanes (2010) finds that an extension of maternity leave in Norway had positive long-term effects on children's educational attainment.³⁷

The results reported in this paper should be of broad interest outside of Spain. Spain is not atypical compared with most other OECD countries in either the relevant institutions or demographic and labor market trends. All OECD countries offer some form of cash benefit to families with young children, and most of them, including Spain, spend between 0.5 and 1.5 percent of GDP in such programs (OECD 2011). In many countries, such benefits include a "birth grant" similar to the Spanish one.

³⁴I have also explored additional outcomes that may provide a fuller picture of the overall well-being effects of the benefit (in the short term). I find evidence suggesting that the benefit may have (temporarily) lowered parental separation rates. Regarding health effects, I do not find any impact on infant mortality, but mothers who received the benefit were somewhat less likely to report poor health the year following childbirth (results can be found in González 2011).

³⁵Milligan 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.

³⁶See Blau and Grossberg (1992); Ruhm (2000, 2004); Baum (2003); Berger, Hill, and Waldfogel (2005); James-Burdumy (2005); Bernal (2008); Bernal and Keane (2010).

³⁷Although other studies have found no effect of maternity leave expansions on child outcomes in other countries (see, for instance, Baker and Milligan (2010) for Canada, and Dustmann and Schönberg (2012) for Germany).

In addition, low and/or declining fertility is a matter of concern for policy makers in most OECD countries (OECD 2011). The total fertility rate in OECD countries declined from 2.7 children per woman in 1970 to 1.7 in 2009. Many countries have adopted policies with the explicit goal of encouraging fertility.

In terms of maternal employment, Spain is very close to the OECD average, with about 50 percent of women with children under age three working for pay. The proportion is very similar in Japan and Germany, while it is lower in Italy or Greece, and higher in France or the United Kingdom (around 60 percent) or the United States (at 65 percent). On average, OECD countries granted about 18 weeks of paid maternity leave in 2008, with most countries offering between 14 and 20 weeks. Spain grants 16 weeks, the same as France, Austria, or the Netherlands.

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.

APPENDIX

TABLE A1—OVERALL FERTILITY RESULTS

	(1)	(2)	(3)	(4)	(5)	(6)
Post (January 2008 and on)	0.0627*** (0.0118)	0.0613*** (0.0118)	0.0517*** (0.0139)	0.0634*** (0.0115)		
Pre (January–June 2007)						−0.0022 (0.0124)
Post0 (July–December 2007)					0.0265** (0.0113)	0.0247* (0.0147)
Post1 (January–June 2008)					0.0701*** (0.0176)	0.0673*** (0.0229)
Post2 (July–December 2008)					0.0864*** (0.0204)	0.083*** (0.0276)
Post3 (January–June 2009)					0.053** (0.0265)	0.0482 (0.0360)
Post4 (July–December 2009)					0.0741** (0.0350)	0.0681 (0.0462)
Years included	2000–2009	2000–2009	2002–2009	2002–2009	2000–2009	2000–2009
Observations (number of months)	120	120	96	96	120	120
Linear trend	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic trend	Yes	Yes	Yes	Yes	Yes	Yes
Cubic trend	Yes	Yes	No	Yes	Yes	Yes
Quartic trend	No	Yes	No	No	No	No
Number days of the month	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The coefficients reported are for binary variables taking value 1 for the months indicated. Each column refers to a different regression. Standard errors are shown in parentheses. The dependent variable is the natural log of the monthly number of births in Spain (from vital statistics data, 2000–2009).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE A2—DISCONTINUITY IN NUMBER OF BIRTHS AT THE THRESHOLD

	RDD 10 years (1)	RDD 5 years (2)	RDD 12–12m (3)	RDD 9–9m (4)	RDD 3–3m (5)	DID 20 years (6)	DID 10 years (7)	DID 5 years (8)
Post	0.0069 (0.0250)	0.0274 (0.0221)	0.0469 (0.0446)	0.0511 (0.0331)	0.0416 (0.0332)	−0.0349 (0.0230)	−0.0163 (0.0172)	0.0080 (0.0177)
Years included	2000–2009	2005–2009	2006–2008	2006–2008	2007	1990–2009	2000–2009	2005–2009
Observations	120	60	24	18	6	240	120	60
Linear trend in <i>m</i>	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Quadratic trend in <i>m</i>	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Cubic term in <i>m</i>	Yes	No	No	No	No	Yes	Yes	No
Control days of the month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month of birth dummies	No	No	No	No	No	Yes	Yes	Yes

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. The “m” in column headers stands for months.

TABLE A3—LONG-TERM EFFECTS ON EXPENDITURE AND LABOR SUPPLY

	RDD 9m (1)	RDD 6m (2)	RDD 4m (3)	RDD 3m (4)	RDD 2m (5)	RDD 2m (6)	DID 1 (7)
Total expenditure	2,234 (2,771)	1,129 (2,248)	3,430 (2,897)	1,958 (1,559)	2,309 (1,963)	2,736 (1,942)	−756 (2,015)
Child-related expenditure	−855 (640)	−806 (549)	−371 (637)	−222 (362)	−205 (483)	−63 (436)	−417 (525)
Durable goods expenditure	1,387 (1,277)	1,122 (1,009)	1,767 (1,229)	872 (746)	1,077 (926)	1,193 (895)	84 (918)
Total expenditure (logs)	0.085 (0.104)	0.044 (0.082)	0.154 (0.104)	0.063 (0.055)	0.086 (0.073)	0.112 (0.070)	0.003 (0.074)
Child-related expenditure (logs)	−0.074 (0.196)	−0.105 (0.156)	0.139 (0.201)	−0.021 (0.106)	0.077 (0.142)	0.122 (0.133)	−0.105 (0.141)
Durable goods expenditure (logs)	0.304 (0.268)	0.247 (0.211)	0.410 (0.260)	0.211 (0.146)	0.203 (0.176)	0.229 (0.171)	0.237 (0.205)
Private day care expenditure	29 (249)	−38 (201)	128 (247)	−25 (137)	45 (171)	113 (166)	−108 (169)
Private day care expenditure (binary)	0.029 (0.099)	−0.054 (0.081)	0.039 (0.010)	−0.021 (0.056)	−0.048 (0.066)	−0.016 (0.070)	−0.067 (0.073)
Working last week	−0.0561* (0.0327)	−0.0293 (0.0255)	−0.0955*** (0.0350)	−0.0109 (0.0179)	−0.0347 (0.0250)	−0.0360 (0.0239)	0.0785*** (0.0244)
Employed	−0.0781** (0.0318)	−0.0449* (0.0247)	−0.1087*** (0.0339)	−0.0273 (0.0173)	−0.0495** (0.0249)	−0.0488** (0.0231)	0.0137 (0.0168)
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No	No	Yes
Quadratic trend in <i>m</i>	Yes	No	No	No	No	No	Yes
Calendar month of birth dummies	No	No	No	No	No	No	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes
Number 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 two and nine 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. The “m” in column headers stands for months.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

TABLE A4—MATERNAL LABOR SUPPLY AND EARNINGS RESULTS (SOCIAL SECURITY DATA, 2009 SAMPLE)

	RDD 9m	RDD 6m	RDD 4m	RDD 3m	RDD 2m	RDD 2m	DID 1	DID 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employed 6 months postbirth (binary)	−0.0444** (0.0185)	−0.0321** (0.0144)	−0.0345* (0.0180)	−0.0065 (0.0100)	−0.0253* (0.0140)	−0.0187 (0.0122)	−0.0101 (0.0167)	0.0029 (0.0127)
Employed 12 months postbirth (binary)	−0.0398** (0.0189)	−0.0273* (0.0145)	−0.0311* (0.0181)	−0.001 (0.0100)	−0.0283** (0.0135)	−0.022* (0.0123)	−0.0042 (0.0169)	−0.0014 (0.0128)
Months not employed out of 6 (after birth)	0.1927** (0.0772)	0.1543*** (0.0602)	0.2091*** (0.0756)	0.0428 (0.0418)	0.1598** (0.0730)	0.1048** (0.0507)	0.1487** (0.0701)	0.043 (0.0531)
Months not emp. out of 12 (after birth)	0.4191*** (0.1600)	0.3142** (0.1248)	0.3522** (0.1567)	0.0804 (0.0868)	0.2946** (0.1383)	0.2019* (0.1056)	0.1764 (0.1463)	0.0243 (0.1107)
Earnings 12 months postbirth (in euros)	−853** (341)	−723*** (265)	−653* (336)	−117 (183)	−714*** (275)	−370* (221)	−320 (303)	−137 (229)
Observations	20,039	13,613	9,208	6,926	4,663	4,663	48,987	48,987
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No	No	Yes	Yes
Quadratic trend in <i>m</i>	Yes	No	No	No	No	No	Yes	No
Calendar month of birth dummies	No	No	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Number 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 mothers giving birth 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 sample in the RDD specifications includes women who had a child between two and nine months before or after July 1, 2007. The DID specifications include women 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 indicator of the woman being self-employed three months before birth, and an indicator for the mother being on a permanent contract three months before birth. The “m” in column headers stands for months.

***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

TABLE A5—CHILD CARE HOURS RESULTS (EU–SILC 2008)

	RDD 9m	RDD 6m	RDD 4m	RDD 3m	RDD 2m	RDD 2m	DID 1	DID 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Day care (binary)	−0.0787 (0.0657)	−0.0506 (0.0470)	−0.0594 (0.0696)	−0.0392* (0.0228)	−0.0577* (0.0327)	−0.0633* (0.0368)	−0.0153 (0.0355)	−0.0409 (0.0402)
Official (binary)	0.1972 (0.1486)	0.1257 (0.1175)	0.1323 (0.1468)	0.0463 (0.0841)	−0.0245 (0.0988)	−0.0146 (0.1026)	0.0312 (0.0719)	0.0147 (0.0843)
Nanny (binary)	0.0258 (0.0691)	0.0386 (0.0535)	0.0833 (0.0735)	0.0216 (0.0385)	0.0297 (0.0469)	0.0661 (0.0567)	−0.0199 (0.0330)	−0.0043 (0.0378)
Informal (binary)	0.1606 (0.1214)	0.2089** (0.0977)	0.1136 (0.1127)	0.0958 (0.0704)	0.0122 (0.0826)	0.0646 (0.0847)	−0.0268 (0.0633)	0.0962 (0.0723)
Observations	441	288	181	139	96	93	1,358	1,358
Linear trend in <i>m</i>	Yes	Yes	Yes	No	No	No	Yes	Yes
Quadratic trend in <i>m</i>	Yes	No	No	No	No	No	Yes	No
Calendar month of birth dummies	No	No	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Number 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 child care (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. The “m” in column headers stands for months.

**Significant at the 5 percent level. *Significant at the 10 percent level.

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