The Effects of a Universal Child Benefit: A Regression Discontinuity Design

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Abstract

I study the impact of an income shock at the time of childbirth on household expenditure patterns, maternal labor supply, and child care arrangements. I exploit the unanticipated introduction of a new, sizeable universal child benefit in Spain in 2007, granted to all mothers giving birth on or after July 1, 2007. The regression discontinuity design allows for a more credible identification of the causal effects compared with the difference-in-difference estimates available in the literature. Using three independent data sets (the Household Budget Survey, the Labor Force Survey and the Survey of Income and Living Conditions), I find that families who received the benefit did not increase their overall expenditure or their expenditure on 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 the first year of life. Twelve months after birth, mothers who had a child right after the cutoff date were 3 to 6% less likely to be working than mothers who gave birth right before. Consistently, treated mothers were 3 to 10% less likely to have used any formal day care the year after birth. These results suggest that child benefits may affect child well-being through their impact on maternal time at home after birth.

1. Introduction

Does higher family income improve the well-being of children? This question has received a great deal of attention from several disciplines, including economics, sociology and child development. Simple correlations show that children who grow up in higher-income households tend to fare better than those with lower family income. However, it is hard to draw causal inferences, since income is associated with multiple other factors that may also affect child outcomes. An ideal "experiment" to address this question would randomly select some families to receive a large sum of money, say at the time of the birth of a child, and compare the children of treated and untreated families along several dimensions related to child well-being.

Governments in many countries offer cash benefits to families with young children out of a concern for horizontal equity. Studying the effects of child benefits may get us close to our ideal experiment setting, provided we can find a proper control group. Previous research has tended to follow a difference-in-differences approach, where one compares families who have children before and after the introduction of a child benefit, and use other regions or non-eligible families as controls (Milligan and Stabile, 2008, 2009). However, both kinds of control groups suffer from comparability issues, and it is hard to rule out other sources that may be responsible for their different trajectories.

In this paper, I exploit a natural experiment that allows us to replicate a randomized experiment of the sort described above, where mothers 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 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 is 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, or more than 4 times the monthly (gross) minimum wage for a full-time worker.

I analyze the effect of receiving the new child benefit on three main outcomes. First, I evaluate its impact on household expenditure. By increasing household income, the benefit may lead to higher consumption of goods and services.[1] In particular, we may expect to see a positive effect on child-related expenditures. An explicit goal of the subsidy was to help families cope with the extra expenditures associated with the birth of a new child. In addition to increasing household income, the benefit also increases the share of income "controlled" (or, at least, received) by the mother, which may also lead to higher child-related expenditures, as suggested by previous literature (Lundberg et al. 1997; Ward-Batts, 2002). The benefit may improve child well-being if the extra income is used to purchase goods and services that are valuable for basic child welfare (food, clothing, housing) or for enhancing child development (books, etc).

Second, I study the potential effect of the subsidy on maternal labor supply. The household can use the extra income to buy more goods and services, but it can also use it to "buy" maternal time at home after childbirth.[2] Paid maternity leave in Spain is

[1] The basic static labor supply model predicts that an increase in uearned income will increase consumption of all normal goods and services.

[2] In the basic static labor supply model, an increase in unearned income will reduce labor supply as long as leisure is a normal good.

short compared with other European countries (16 weeks). Any labor supply effects would dampen the effect of the subsidy on household income, but could also affect children if they lead to changes in childcare use, and in particular increase maternal care time.[3]

Finally, I analyze the effect of the subsidy on child care arrangements. I study both expenditures on different forms of paid childcare (day-care, nannies, etc) and the weekly hours spent by the child in different forms of child care, including informal unpaid arrangements (grandparent care, etc).

There are two main contributions of the paper. First, the RDD strategy allows for a more credible identification of the underlying causal effects, compared with the previous literature. Second, I combine a more conventional analysis of the labor supply effects of the benefit with a richer study of additional channels through which the subsidy may affect child well-being, including household consumption patterns and the use of different child care arrangements. This is valuable given the virtual absence of studies addressing the effects of changes in income on parental investments in children (Ginja, 2010).

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 nine months after birth, with the labor supply effect dissipating by the child's first birthday. I also find that receiving the benefit led to significantly lower expenditure on day-care and fewer weekly hours of day-care. I conclude that any effects

[3] Both the expenditure and labor supply effects could further affect child well-being if they contribute to a lower level of stress and conflict in the household (Milligan and Stabile 2008).

of the benefit-driven increase in income on the well-being of children did not take place through increases in consumption, but through changes in maternal time at home and day-care use.

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. In section 3 I detail the identification strategy and discuss the potential threats to validity. The next section introduces the data sources and describes the main variables. Section 5 discusses the results on the three sets of outcomes, and section 6 concludes.

2. Institutional background

On July 3rd, 2007, the Spanish President announced during the "State of the Union" address that a new, universal child benefit would be introduced. The new, one-time subsidy would pay 2,500 Euros to all new mothers, starting with those giving birth after the announcement date. The eligibility cut-off would subsequently be moved (for practical reasons) to July 1st. The proposal became law in November,[4] and the first "baby-checks", as they were referred to in the media, were paid in December 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 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 worker. The child benefit also more than doubled median female gross monthly earnings (about 1,190 Euros).

The explicit goal of the new policy was twofold. The benefit was meant to help parents cope with the extra expenditures associated with childbirth, while it also

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

intended to encourage fertility given the low prevailing birth rates in Spain. It 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. In 2008, more than 95% of all new mothers received the subsidy.

Since I examine the effects on maternal labor supply, maternity leave regulations are also relevant. In Spain, paid maternity leave is 16 weeks. There are 2 additional weeks that are reserved for the father, while 6 out of the 16 are reserved for the mother. The remaining 10 weeks can be shared between the parents, although in the large majority of cases, mothers take the full 16 weeks. Additionally, mothers can take unpaid leave for up to 3 years after birth, with the right to go back to their previous job (or an "equivalent" one). Finally, for up to 8 years after birth, the parents have the right to work part-time (meaning a reduction in work hours between 12 and 50%), with a proportional reduction in their salary. These latter two clauses are however not used frequently.

3. Identification strategy

The introduction of the Spanish child benefit lends itself naturally to a (sharp) regression discontinuity analysis, so that we can compare the outcomes of households who had a child right before and right after the eligibility cut-off.[5] Close enough to the threshold, treatment is "as if" randomly assigned (Lee and Lemieux, 2010).

I estimate regressions of the following form:

$$Y_{id} = \alpha + f(d) + g(d) \cdot 1[d \geq 0] + \gamma 1[d \geq 0] + \Pi X_{im} + \varepsilon_{im}, \qquad (1)$$

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

Where Y is an outcome variable (say, household expenditure or a measure of maternal labor supply) for household i who had a child on date d. Date of birth d (our "running" variable) is normalized to zero at the threshold (July 1, 2010), so that $d \geq 0$ indicates benefit eligibility. The regression includes a polynomial in d that is allowed to change at the cut-off, i.e. functions $f(.)$ and $g(.)$ are polynomials and $1[.]$ is a binary indicator function. Parameter γ captures any potential discontinuity or "jump" in Y at the threshold, and is our main parameter of interest. The vector X includes household-level controls, and ε is the residual.

In practice, we do not observe exact dates of birth, but only month of birth.[6] Thus, d in equation (1) is replaced by m, and m equals -1 for June 2007 births and 0 for July 2007 births (1 for August 2007 births, etc). 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 sample includes households with children born between 2005 and 2008, so that the children are between 0 and 3 years of age when observed (in additional specifications I restrict the sample to fewer months around the threshold). By including multiple birth years in the analysis, I am able to control for seasonality effects (by means of 12 month dummies). 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.

[6] 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.

The identifying assumption is that no other factor affected children born after June 30, 2007 discontinuously.[7] We do allow for a smooth trend 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 when observed in 2008.

There are two checks that should be performed in order to confirm the validity of the RDD analysis (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 unlikely, given that the benefit was announced three days after the threshold date. In any case, we run regressions such as (1) where the dependent variable is the total number of births by month (without individual-level controls but with month dummies), and show that there was no discontinuity in number of births around July 1, 2007 (see section 5.1 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 (1) where the dependent variables are different household characteristics (age and education level of the parents, immigrant status, etc). The results are reported in section 5.1.

4. Data and descriptive statistics

I use three independent household surveys as my main data sources: the Household Budget Survey (HBS), the Labor Force Survey (LFS), and the Survey on Income and

[7] No other policy changes in 2007 or thereafter applied differentially to children born before and after July 1, 2007.

Living Conditions (SILC). The main analysis is performed on the 2008 samples of each survey. Additional results are shown that also include earlier or later years.

The Family Budget Survey is a yearly survey of about 24,000 households, run since 2006. Its main purpose is to register detailed information on family expenditures, doing so by means of diaries kept during two weeks, and supplemented by interviews meant to capture purchases that happen with a higher periodicity. It is designed to measure total household expenditure during the (previous) year at a fairly disaggregated level. The interviews are distributed evenly during the whole year.

I use the 2008 wave of this survey to analyze the effect of the child benefit on household consumption patterns. Expenditures are grouped into 12 large categories (food and drink, clothing, etc, see appendix for the full list), which are further disaggregated into 3- and 4-digit items. As an example, expenditure category 1194 is "baby food". I first study the effect of the child benefit on total expenditure, and then focus on child-related expenditures, a category that I construct by adding up expenditures in the fourteen 4-digit items that can be considered directly child-related (see appendix). Next I break both child-related and non-child-related expenditures into the 12 main expenditure categories, and then I further study each 3- and 4-digit item.

Once we restrict the 2008 sample to households with a child born between 2005 and 2008, the sample size is 2,289 households, or about 50 births per month. There are 30 pre-treatment months and 18 post-treatment. Table 1 (first panel) reports some descriptive statistics for the main HBS variables. Average (yearly) household expenditure was about 30,000 Euros, including about 4,700 Euros on child-related goods and services.

The Labor Force Survey interviews about 60,000 households each quarter, and is the official data source used to construct quarterly unemployment rates, thus focusing

on labor market variables. I use the LFS to analyze the effect of the child benefit on maternal labor supply. As dependent variables, I use both binary employment indicators and a continuous measure of weekly hours of work. The main sample merges all 4 quarters of 2008 and includes households with a child born between 2005 and 2008. the resulting sample size is 21,185 observations, or about 450 births per month. Table 1 (second panel) shows that about 57% of mothers had a job when interviewed in 2008, although only 45% had actually worked the previous week. They worked an average of 14.5 hours (including the zeros).

Finally, the Survey of Income and Living Conditions interviews about 16,000 households a year and is part of a EU-wide survey designed to provide comparable data on poverty and inequality across countries. The smaller sample size makes it less suitable for our analysis (with only about 20 observations per month of birth). However, it includes a set of variables measuring the weekly hours spent by each child in different forms of child care, including official infant education centers, unofficial day-care centers, nannies, and informal unpaid care. We use the 2008 sample and include all births between 2004 and 2008, resulting in a sample size of 1,409. About two-thirds of children in the sample attend formal day care, for an average of 24 hours a week (table 1, third panel). Only 4% spend any time with a nanny or babysitter, and about 16% spend some time in informal, unpaid care, such as grandparent care, for 5 hours a week on average.[8]

Maternal characteristics are very similar across the three data sets. Mothers are on average 33 years old, with a fraction of immigrants in the 15-19% range. The proportion

[8] Note that child care hours are aggregated at the household level, so that they include more than one child in households with multiple children.

of high school graduates is around 63-65%, and the fraction of college graduates is about 40%.

5. Results

5.1 Validity checks

First I confirm that there was no discontinuity in the number of births around the cutoff date. Figure 1 shows the monthly number of births in Spain between 1975 and 2009. In 2007, there were about 41,000 births a month. There were more births in July (42,810) than in June (40,210), but there is clearly some seasonality that we need to account for. Thus, I estimate the following regression:

$$N_m = f(m) + g(m) \cdot 1[m \geq 0] + \gamma 1[m \geq 0] + \sum_{j=1}^{12} \delta_j month_j + \varepsilon_m, \qquad (2)$$

where N is the natural log of the number of births per month, *f(.)* and *g(.)* are polynomials in month of birth m, and the δ's are the coefficients on the 12 month dummies. Our parameter of interest is again γ. The results of six different specifications are presented in table 2. There is no evidence of a jump in the number of births around July 1, 2007. The last column, which includes 30 months before and 30 after the threshold and a third-order polynomial in m, suggests a small (2 log-points) *negative* effect, but the coefficient is far from statistically significant.

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 (1) with different household characteristics as dependent variables:

$$Y_{im} = f(m) + g(m) \cdot 1[m \geq 0] + \gamma 1[m \geq 0] + \sum_{j=1}^{12} \delta_j month_j + \varepsilon_{im} \qquad (3)$$

In particular, we check for balance in age, educational attainment and immigrant status of the mother.[9] We do so in our three data sets. Table 3 reports the results of four different specifications. The first three columns include all 48 months of birth (2005-2008). The initial specification includes only a linear trend in month of birth (allowed to vary after the cutoff date). Column 2 adds 12 month dummies, and column 3 also includes a quadratic term. Finally, the last column only uses 3 months before and after the threshold.

In both the HBS and the SILC samples (table 3, first and third panel), mother characteristics are fairly balanced around the threshold. There is no significant discontinuity in age, education or immigrant status of the mother. Control and treated mothers are similar in their observable covariates, as expected. In the LFS sample (second panel), control and treated mothers are similar in their high school graduation rates and the fraction of immigrants (see also figure 2). However, treated mothers appear slightly younger (between 0.4 and 0.7 years) and have higher college graduation rates (between 4 and 7 percentage points, for an average of about 40%). The age jump is small, but the discontinuity in the fraction of college graduates, most likely a chance occurrence, suggests that we should control for education in all our LFS specifications.

5.2 Expenditure

I now analyze the effect of receiving the child benefit on total household expenditure. Note that households were interviewed (at any point during) 2008, and overall expenditure is supposed to capture all expenditures between the interview date and one year before. Families who had a child between July and December of 2007 would have

[9] We also check for balance in father characteristics, and find no evidence of jumps at the cutoff (results available upon request).

received their check in December of 2007, and mothers who gave birth in December 2007 or later received it a few weeks after the birth, so we expect the impact on expenditure to show up fully in our data.

I estimate equation 3 using total annual household expenditure in Euros as the dependent variable. The results of four different specifications are reported in the first row of table 4. The initial two columns include a linear trend (allowed to change at the cutoff), and column 2 adds mother characteristics and month dummies. The coefficients suggest that households who received the benefit spent about 2,000 Euros more than the controls in the subsequent year. However, the estimates are very imprecise. Column 3 adds a quadratic trend, and the coefficient turns negative and remains insignificant. Finally, the last column includes only 3 months before and after the cutoff with no controls, and the coefficient becomes closer to zero and is still far from significant. I conclude that the families that received the benefit did not consume significantly more goods and services the following year. [10]

The result can be seen graphically in figure 3 (first panel). It shows average expenditure in 1-month bins for 2005-2008 births, with second-order polynomials fit on each side of the cutoff. First, average expenditure is very close on both sides of the threshold (say, comparing June versus July 2007 births). Second, there is no visible jump between the polynomials on the left and right sides of the cutoff date. The evidence strongly suggests that total expenditure did not react to receiving the extra 2,500 Euros.

[10] Regarding the control variables, expenditure was significantly higher for younger, more educated mothers, and lower for immigrant mothers. Several of the month dummies are also significant.

The same holds if we focus on child-related expenditure (second row of table 4). There is no evidence that families that received the benefit subsequently spent more on child-related goods and services. The coefficients are fairly close to zero and insignificant in all specifications. The graph (second panel of figure 3) also confirms this conclusion.

Two concerns may limit the credibility of these results. First, there is quite a lot of noise in the data, so that average expenditure by month of birth jumps considerably from one bin to the next. This is likely related to the small sample size (less than 50 observations per month). Second, some of the families who had a child in late 2007 may have spent the check too early to be captured in the 2008 HBS interview.[11] In order to address these two issues, we re-estimate the regressions including also the 2006 and 2007 HBS samples.[12] The merged sample has more observations per month of birth (especially before the cutoff), thus improving precision, and it includes interviews with families who had a child in 2007 during the year of benefit receipt.

The results when using the merged 2006-2008 sample are reported in the second panel of table 4. All specifications now incorporate year dummies. The main findings remain. There is no robust evidence of an increase in overall or child-related expenditure as a result of benefit eligibility.

I next analyze systematically the effects on expenditure by 2-, 3- and 4-digit categories, separately for child-related and non-child-related items. The large standard errors in table 4 suggest that there may be room for effects on some of the smaller

[11] Even though the first checks were paid in December 2007, since families anticipated its receipt since July 3rd, they may have increased expenditure in advance.

[12] The 2007 sample is restricted to households with 2004-2007 births, and the 2006 sample to 2003-2006 births.

expenditure groups. However, I find no effect on any of the 12 large, 2-digit categories.[13] Once I disaggregate into 3-digit items, there are 12 different child-related categories and 46 non-child related ones. Table 5 shows the results for all the items with at least one specification suggesting significant results at the 95% confidence level.

By far, the strongest and most consistent result across specifications refers to day care services. Families who received the child benefit spent about 200 Euros *less* in day care the following year. This is more than a 50% reduction compared with an average expenditure of 372 Euros (see table 1).[14] This may seems surprising, since we expected expenditure to increase following receipt of the subsidy. We postpone the interpretation to the following two subsections.

The second strongest result is that eligible households spent 150 to 200 Euros more on "other personal products", which include jewellery, watches, travel products, bags, purses, and tobacco-related products. Families who received the benefit also spent more on personal hygiene products and household maintenance and repairs, and somewhat *less* on transportation services, but these results are not robust across specifications.[15]

Finally, I disaggregate the 3-digit expenditure categories in table 5 into their 4-digit components, in order to identify the specific items driving the results. The two child-related categories have no further disaggregation. The increase in household

[13] None of the 12 non-child related categories or the 6 child-related ones led to results significant at the 95% confidence level in any of the 4 main specifications.

[14] This result is still present, strong and of a similar magnitude when using the merged 2006-2008 sample (results available upon request).

[15] They also spent slightly more on postal services and phone and fax equipment, but the magnitudes of the effects are so small (always under 15 Euros) that we omit these categories from the discussion.

maintenance expenditure is driven entirely by maintenance and repair services (versus materials for DIY maintenance and repairs).[16] The decline in transportation services comes from road transportation (bus and taxi). The increased expenditure in personal products is driven by jewelry and watches (130-170 Euros), with a small effect also on travel products, bags and purses (20-30 euros).

In sum, households eligible for the new child benefit did not increase overall expenditure (or child-related expenditure) significantly the year following child birth. This suggests that, either the benefit increased savings, or it led to a decrease in other sources of income. The next section explores one potential effect consistent with the second channel: that the benefit induced mothers to reduce their labor supply after child birth. This would also be consistent with the observed reduction in day care expenditure. In section 5.4 we explore the benefit's impact on child care arrangements in more detail.

5.3 Maternal labor supply

Next I analyze the effect of the child benefit on maternal labor supply. In a standard, static labor supply model, an increase in unearned income can reduce labor supply (provided that leisure is a normal good). I use the 2008 Labor Force Survey to study participation and hours of work for recent mothers. Children born at the threshold (July 1, 2007) are on average 12 months old when observed in 2008.[17] I estimate equation 3 with three alternative dependent variables. The first is an employment indicator that

[16] Regression results for the 4-digit categories are available upon request.

[17] Note that we don't observe the exact date or even the month when the LFS interview takes place, only the quarter, thus we can only approximate the exact age of the child when maternal labor supply is observed.

takes value 1 if the woman held a job at the time of the interview. The second is also a binary variable, 1 if the woman reported working positive hours the week before the interview. Finally, I also use a continuous dependent variable that measures the number of hours worked the week before, including the zeros. The results for four different specifications are reported in table 6 (first panel).

The first specification includes a linear trend in month of birth with no additional controls. The results suggest that mothers who received the benefit were 5 percentage points less likely to hold a job 12 months after childbirth (3.4 points less likely to have worked the previous week). Adding the controls and the month dummies increases the magnitude of the estimated effects to 5 and 6 points, respectively. Magnitude and significance remain virtually unchanged in a specification without controls that includes only 3 months before and after the threshold (column 4). Both magnitude and significance are lower in specification 3 (which includes a quadratic trend). Specifications 2 and 4 also suggest a significant reduction in hours worked, of about 2 hours a week.

The main result can be seen graphically in figure 5. The first panel shows the proportion of mothers working in 2008 by month of birth (excluding mothers within the maternity-leave protected 4 months after birth), with second-order polynomial fits on each side of the cutoff. Participation was about 48% among mothers who had a child shortly before July 2007, down to about 43% for the months right after the threshold.

The fact that we find an effect both on employment and actual work the week before the interview suggests that the negative effect on labor supply is not driven by treated mothers taking extended, unpaid leave, but that they actually left their jobs or took longer to find one after childbirth.

We can say more about the timing of the labor supply effect by breaking the 2008 LFS sample into the first and second semester samples. In the first semester of 2008, children born at the threshold were 9 months old on average (15 in the second semester sample). The results broken down by semester are shown in the second and third panels of table 6.

The negative effect on participation is strong in the 1st semester sample, with eligible mothers about 10 percentage points less likely to be working than control mothers. Note than the effect on work is larger in magnitude than the employment effect in all specifications, suggesting that part of the effect is due to treated mothers taking extended leave without leaving their job. The effect is less clear in the 2nd semester, with significant negative coefficients on employment but small, insignificant coefficients on work and hours.

In order to further investigate the dynamics of the labor supply effect, I compare maternal labor supply for treated and control mothers two years after birth (in 2009). The results are shown in table 7. There are no significant jumps in participation (or hours, not shown) at the cutoff date in the first or the second semester of 2009. This can also be seen graphically in the second panel of figure 5. The results suggest, therefore, that the negative effect on labor supply was transitory, so that the benefit delayed mothers' return to work after childbirth, but the effect had fully dissipated after the first year of life of the child, without long-term effects on maternal participation.

5.4 Childcare use

The results so far indicate that the child benefit led to a reduction in maternal labor supply during the first year of the child's life. This, combined with the finding that eligible families spent less on day care, suggests that the subsidy may have affected children through changes in child care arrangements. We further investigate this

hypothesis by combining the HBS data on expenditure and the SILC data on weekly hours in different forms of child care. In particular, I once again estimate equation 3, using as dependent variable, either household expenditure in different forms of (formal) day care, or weekly hours in different forms of care. The results are presented in tables 8 and 9.

The HBS allows us to identify separately three forms of day care: official infant education centers, nannies or babysitters, and other (external, formal) child care centers, which I refer to as day care centers. Infant education centers are part of the public education system. They take in children aged between 0 and 6 (before compulsory education starts) and are heavily subsidized. Demand exceeds supply, so there is a lengthy application process and little flexibility. Low-income families are given preference. There are two stages, that accommodate children 0 to 3 and 3 to 6, respectively. Since our sample is composed of children 0 to 3, I only look at expenditure on the first stage of infant education.

Table 8 reports the results for expenditure in the three forms of formal child care, both in Euros and as binary indicators that take value 1 for positive expenditure in each category. I find that there is no jump in expenditure on infant care centers or nannies at the cutoff date of birth. As already shown in table 5 (and figure 4, first panel), I do find a (negative) discontinuity in day care expenditure, of about 200 Euros, or about a 50% decrease. This effect is also present at the extensive margin, i.e. when the dependent variable is a binary indicator for positive day care expenditure. I find that the child benefit led to fewer families using any day care at all, a decrease of about 9 percentage points, for an average of 38%.

Finally, the SILC includes variables measuring the weekly hours spent by each child in the household on different forms of child care, including informal care by friends of

relatives other than the parents. Again, I estimate equation 3 using as dependent variables both the continuous number of hours and binary indicators for positive hours. Note that infant care centers now include both the 0-3 and 3-6 stages, since they are not disaggregated in the data set. Table 9 displays the results of the different specifications.

The results very much parallel those in table 8. I find no significant effect on hours in official infant care centers. Note that the coefficients are negative in most specifications in both data sets, but always far from statistically significant. There is no discontinuity in hours spent with a nanny or baby-sitter, either (although coefficients are mostly positive in both data sets), or on hours in informal care. Finally, I do find a significant, although small jump in hours in day care, always under an hour a week. When I analyze the extensive margin, I find that there is a 3 to 7 percentage-point decrease in the proportion of families using day care. This is roughly in the range of the 3 to 10 percentage-point decline in the fraction of households reporting positive day care expenditure (table 8).

The results from the two independent data sets both suggest that families who had a child shortly after June 30, 2007, used significantly less day care the following year, compared with families who gave birth right before the cutoff. This is consistent with the decline in maternal employment that we find in a third, also independent data set.

5.5 Heterogeneous effects

We may expect the child benefit to have different effects depending on family characteristics. For instance, maternal employment rates are very different for high- and low-educated mothers, suggesting that the two groups may have different elasticities. In our 2008 LFS sample, 75% of college-graduate mothers had a job, compared with 45% of mothers without a college degree.

Table 10 presents the results of estimating equation 3 separately for mothers with and without a college degree, for the different expenditure and labor supply outcomes.[18] I do not find an effect of the benefit on total or child-related expenditure for either of the two groups. However, the negative effect on daycare expenditure seems driven by college graduate mothers, as well as the increase in "other personal products".

Regarding the labor supply effects, both low- and high- educated treated mothers were less likely to be working 9 months after birth, with part of the effect driven by extended leave and part by actual employment effects. The employment effects appear larger for less-educated mothers, especially relative to their much lower baseline rates (a 14 to 20% decline, compared with up to 12% for college-grads).

At 15 months (2nd semester results), only low-educated treated mothers are still less likely to be working than the controls, with coefficients that are in fact positive for college graduates. The results are less robust for employment, although they also suggest a more negative effect for low-educated mothers (up to 21%, compared with 11% for college graduates).

In sum, we find a discontinuity in maternal participation the year after birth for both high- and low-educated mothers, although both the magnitude and duration of the effect seem larger for the lower-educated. At the same time, however, we find that the reduction in day care expenditure is driven by college graduate mothers, who were much more likely to use it in the first place (average expenditure of 560 Euros, versus 225 for the less educated).

[18] I also explore heterogeneous effects by age of the mother, immigrant status, and gender of the child. Results are available upon request.

6. Conclusions

The results suggest that the 2,500-Euro child benefit introduced in Spain in 2007 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 9 months of age, eligible mothers were 7 to 11 percentage points less likely to working compared with control mothers. Consistent with this labor supply response, children born after the threshold were 6 to 10 percentage points less likely to be in day care. Also consistent is a negative effect on expenditure on transportation services.

Essentially the only items where we find a robust increase in expenditure are "jewelry and watches", and "travel products and purses". If we think that jewelry and purses are "feminine" products, this result is consistent with previous findings that show that, when the female spouse controls a higher fraction of household resources, household expenditure in "male" goods and services falls, while expenditure in "female" products increases (Lundberg et al. 1997, Ward-Batts 2010).

We 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. Milligan and Stabile (2008) found that increases in child benefits in Canada were associated with higher test scores and improved child and maternal health. Our results suggest that increased maternal time at home after birth may be one factor contributing to those effects. However, recent research (Dustmann and Schonberg 2009) suggests that maternity leave expansions in Germany did not lead to significant improvements in long-term educational attainment or wages for the children.

The long-term effects of the Spanish child benefit on children remain to be seen.[19] In fact, 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.

[19] Note that children born at the threshold date just turned 3 years old at the time of writing.

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Appendix: Main Expenditure Categories

The Household Budget Survey breaks household expenditure into 12 main (2-digit) categories:

1	Food and non-alcoholic drinks
2	Alcohol, tobacco and drugs
3	Clothing and shoes
4	Housing and utilities
5	Furniture, household equipment and household maintenance
6	Health
7	Transportation
8	Communications
9	Leisure and culture
10	Education
11	Hotels, cafes and restaurants
12	Other goods and services

I construct child-related expenditure by including any 4-digit item that is child-specific. There are 14 such items:

1	Baby food (and drinks) (1194)
2	Children or baby clothes (3123)
3	Children or baby shoes (3213)
4	Large furniture (5111), including cribs, play-pens, high-chairs and other baby furniture
5	Kitchen utensils (non-electric) and other household articles (5413), including baby bottles
6	Domestic service excluding Social Security payments (5621), including nannies and baby-sitters
7	Social security payments to domestic service employees (5622)
8	Toys, games, hobbies and small musical instruments (9311)
9	Books (excluding textbooks) (9511)
10	Paper and painting products (9541), including pens, crayons, paint, chalk, etc.
11	Official infant (0-3) education centers (10111)
12	Personal hygiene non-electric products (12122), including soap, lotion, diapers, etc.
13	Other baby products (12222), including strollers, baby carriers, car seats, pacifiers, etc.
14	Center-based child services excluding schools (day care centers) (12312)

Table 1. Descriptive statistics

Variable	N. Obs.	Mean	Std. Dev.	Min	Max
HBS 2008					
Total expenditure	2289	30281	16713	2700	182173
Child-related exp.	2289	4681	4299	0	41779
Daycare exp.	2289	372	903	0	6600
Positive daycare exp.	2289	0,379	0,485	0	1
Age mother	2278	33,3	5,1	16	50
Mother hs graduate	2278	0,652	0,476	0	1
Mother college grad	2278	0,443	0,497	0	1
Mother immigrant	2289	0,151	0,358	0	1
Month of birth	2289	-9,7	12,2	-30	17
Month	2289	6,2	3,4	1	12
LFS 2008					
Mother employed	21185	0,571	0,495	0	1
Mother worked last week	21185	0,450	0,497	0	1
Hours worked last week	21004	14,5	17,8	0	97
Age mother	21185	33,0	5,3	15	50
Mother hs graduate	21185	0,633	0,482	0	1
Mother college grad	21185	0,409	0,492	0	1
Mother immigrant	21185	0,161	0,368	0	1
Month of birth	21185	-9,7	12,3	-30	17
Quarter	21185	2,6	1,1	1	4
Month	21185	6,2	3,4	1	12
SILC 2008					
Any formal day care	1409	0,681	0,466	0	1
Any hours with a nanny	1409	0,043	0,202	0	1
Any informal care	1409	0,165	0,371	0	1
Hours of formal day care	1409	24,4	22,9	0	160
Hours with a nanny	1409	1,3	7,8	0	100
Hours of informal care	1409	4,5	12,7	0	98
Age mother	1391	33,5	5,4	16	48
Mother hs graduate	1360	0,645	0,479	0	1
Mother college grad	1360	0,394	0,489	0	1
Mother immigrant	1409	0,194	0,396	0	1
Month of birth	1409	-17,2	15,0	-42	11
Month	1409	6,3	3,5	1	12

Note: HBS refers to the Household Budget Survey, LFS is the Labor Force Survey, and SILC is the Survey of Income and Living Conditions. The sample includes all households with a child born between 2005 and 2008 (between 2004 and 2008 for the SILC sample). Each observation is one child. "Month" indicates the natural month (1 to 12), while "Month of birth" takes value 0 for July 2007 births (1 for August 2007, -1 for June 2007, etc).

Table 2. Discontinuity in number of births around the threshold

	1	2	3	4	5	6
Post June 2007 (*m≥0*)	0,0253	0,0218	-0,0541	-0,072 ***	-0,0199	-0,0231
	(0,0322)	(0,0238)	(0,0414)	(0,0274)	(0,0177)	(0,0219)
Years included	1975-2009	1975-2009	1975-2009	1975-2009	1999-2009	2005-2009
N. of months	420	420	420	420	132	60
Linear term in m?	Y	Y	Y	Y	Y	Y
Quadratic term?	Y	Y	Y	Y	Y	Y
Cubic term?	N	N	Y	Y	Y	Y
Month dummies?	N	Y	N	Y	Y	Y

Note: The dependent variable is the natural log of the number of births per month in Spain. Each column reports the results of a separate regression. Month of birth (*m*) is normalized so 0 is July 2007, 1 is August 2007, and so in. *Post* is a binary indicator for months after June 2007 (*m≥0*). The linear, quadratic and cubic terms are always interacted with *post*. The month dummies are 12 binary variables for each month of the year and control for seasonality. The data on births comes from the Spanish National Statistical Institute (www.ine.es). One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 3. Discontinuity in characteristics around the threshold

	1	2	3	4
HBS 2008				
Age mother	0,345	0,388	-0,384	-0,044
	(0,437)	(0,461)	(0,756)	(0,597)
Mother hs grad	0,011	-0,003	-0,033	0,025
	(0,040)	(0,043)	(0,070)	(0,053)
Mother college	0,009	0,001	-0,030	0,027
	(0,042)	(0,045)	(0,074)	(0,056)
Mother immigrant	0,026	0,023	0,001	0,011
	(0,032)	(0,034)	(0,055)	(0,044)
N. obs.	2278	2278	2278	318
Linear trend	Y	Y	Y	N
Quadratic trend	N	N	Y	N
Month dummies	N	Y	Y	N
Number of months	48	48	48	6
LFS 2008				
Age mother	-0,147	-0,094	-0,437 **	-0,692 ***
	(0,148)	(0,148)	(0,208)	(0,189)
Mother hsgrad	0,009	0,009	-0,031	-0,019
	(0,014)	(0,014)	(0,019)	(0,017)
Mother college	0,042 ***	0,046 ***	0,069 ***	0,059 ***
	(0,014)	(0,013)	(0,020)	(0,018)
Mother immigrant	0,009	0,008	-0,009	0,009
	(0,011)	(0,011)	(0,015)	(0,014)
N. obs.	21185	21185	21185	3026
Linear trend	Y	Y	Y	N
Quadratic trend	N	N	Y	N
Month dummies	N	Y	Y	N
Quarter dummies	N	Y	Y	N
Number of months	48	48	48	6
SILC 2008				
Age mother	-0,484	0,104	-0,034	-1,380
	(0,768)	(0,844)	(1,160)	(0,895)
Mother hs grad	-0,125 *	-0,096	-0,078	-0,124
	(0,067)	(0,073)	(0,105)	(0,082)
Mother college	-0,073	-0,021	0,036	-0,086
	(0,065)	(0,072)	(0,102)	(0,083)
Mother immigrant	0,095 *	0,095	0,051	0,093
	(0,057)	(0,062)	(0,091)	(0,070)
N. obs.	1409	1409	1409	144
Linear trend	Y	Y	Y	N
Quadratic trend	N	N	Y	N
Month dummies	N	Y	Y	N
Number of months	60	60	60	6

Note: HBS refers to the Household Budget Survey, LFS is the Labor Force Survey, and SILC is the Survey of Income and Living Conditions. The sample includes all households with a child born between 2005 and 2008 (between 2004 and 2008 for the SILC sample). Each observation is one child. Each column reports the results from a different specification, and each row is for a different dependent variable. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 4. Total household expenditure and child-related expenditure

	1	2	3	4
HBS 2008				
Total expenditure	1941	1958	-1484	-274
	(1384)	(1437)	(2324)	(1735)
Child-related expenditure	-34	2	118	-12
	(355)	(362)	(640)	(471)
N	2289	2289	2289	319
Number of months	48	48	48	6
HBS 2006-08				
Total expenditure	2739 **	3303 ***	1311	-542
	(1176)	(1189)	(1609)	(1729)
Child-related expenditure	677 **	933 ***	468	185
	(339)	(333)	(472)	(455)
N	6563	6563	6563	466
Number of months	72	72	72	6
Linear trend	Y	Y	Y	N
Month dummies	N	Y	Y	N
Controls	N	Y	N	N
Quadratic trend	N	N	Y	N

Note: HBS refers to the Household Budget Survey, the data source. The sample includes all households with a child born between 2005 and 2008 (between 2003 and 2008 in the merged 2006-2008 sample). Each observation is one child-household. Each column reports the results from a different specification. The dependent variable is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. The controls include maternal age (plus squared and cubed terms), an immigrant dummy, and two maternal education dummies (indicating high school graduation and college graduation). One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 5. Expenditure results, 3-digit (HBS 2008)

	1	2	3	4
Child-related				
Personal hygiene (121)	75	34	185 **	145
	(71)	(77)	(94)	(88)
Daycare (123)	-242 ***	-245 ***	-165 *	-170 **
	(63)	(68)	(89)	(80)
Non-child-related				
Household maintenance and repairs (43)	121	55	231	205 **
	(107)	(107)	(159)	(104)
Transportation services (73)	-22	-28	-143 **	-101
	(52)	(56)	(69)	(72)
Postal services (81)	6	6	11 **	5 *
	(4)	(4)	(5)	(3)
Telephone and fax equipment (82)	12 **	9 *	13	8
	(5)	(5)	(8)	(9)
Other personal products (122)	178 **	186 **	136	151 **
	(75)	(80)	(94)	(66)
N	2289	2289	2289	319
Linear trend	Y	Y	Y	N
Month dummies	N	Y	N	N
Controls	N	Y	N	N
Quadratic trend	N	N	Y	N
Number of months	48	48	48	6

Note: Household Budget Survey 2008 data. The sample includes all households with a child born between 2005 and 2008. Each observation is one child-household. Each column reports the results from a different specification. The dependent variable (different expenditure categories) is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. The controls include maternal age (plus squared and cubed terms), an immigrant dummy, and two maternal education dummies (indicating high school graduation and college graduation). One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 6. Maternal labor supply effects, LFS 2008

	1	2	3	4
FULL YEAR (at 12 months)				
Employed	-0,049 ***	-0,052 ***	-0,023	-0,067 ***
	(0,014)	(0,013)	(0,019)	(0,018)
Working last week	-0,034 **	-0,063 ***	-0,032 *	-0,06 ***
	(0,014)	(0,013)	(0,019)	(0,019)
Hours last week	-0,689	-1,636 ***	-0,434	-2,18 ***
	(0,487)	(0,474)	(0,688)	(0,681)
N	21185	21185	21185	3112
1st semester of 2008 (at 9 months)				
Employed	-0,067 ***	-0,06 ***	-0,019	-0,063 **
	(0,022)	(0,022)	(0,030)	(0,027)
Working last week	-0,071 ***	-0,099 ***	-0,066 **	-0,107 ***
	(0,021)	(0,021)	(0,030)	(0,027)
Hours last week	-2,25 ***	-3,137 ***	-2,062 **	-3,998 ***
	(0,752)	(0,734)	(1,053)	(0,983)
2nd semester of 2008 (at 15 months)				
Employed	-0,055 ***	-0,054 ***	-0,032	-0,073 ***
	(0,019)	(0,019)	(0,026)	(0,025)
Working last week	0,057 ***	-0,022	-0,001	-0,019
	(0,019)	(0,020)	(0,026)	(0,026)
Hours last week	2,63 ***	0,169	0,969	-0,578
	(0,682)	0,72	(0,967)	(0,943)
Linear trend	Y	Y	Y	N
Month dummies	N	Y	Y	N
Quarter dummies	N	Y	Y	Y
Controls	N	Y	Y	Y
Quadratic trend	N	N	Y	N
N. of months	48	48	48	6

Note: Labor Force Survey 2008 data. The first panel uses the 4 quarters, while the second uses only quarters 1 and 2, and the third panel uses quarters 3 and 4. The sample includes all households with a child born between 2005 and 2008. Each observation is one child-household. Each column reports the results from a different specification. The dependent variable (three labor supply measures) is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. The controls include maternal age (plus squared and cubed terms), an immigrant dummy, two maternal education dummies (indicating high school graduation and college graduation), and an indicator for whether the woman is observed during the 4-month post-birth maternity leave period. One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 7. Maternal labor supply effects, LFS 2009

	1	2	3	4
1st semester of 2009 (at 21 months)				
Employed	-0,018	-0,016	-0,053	-0,04 *
	(0,019)	(0,019)	(0,033)	(0,023)
Working last week	0,081 ***	0,039 **	-0,035	-0,025
	(0,019)	(0,019)	(0,034)	(0,023)
2nd semester of 2009 (at 27 months)				
Employed	0,053 ***	0,039 **	-0,005	0,026
	(0,019)	(0,018)	(0,033)	(0,023)
Working last week	0,065 ***	0,051 ***	0,002	0,035
	(0,019)	(0,019)	(0,034)	(0,024)
N	25592	25592	25592	3119
Linear trend	Y	Y	Y	N
Month dummies	N	Y	Y	N
Quarter dummies	N	Y	Y	Y
Controls	N	Y	Y	Y
Quadratic trend	N	N	Y	N
N. of months	48	48	48	6

Note: Labor Force Survey 2009 data. The first panel uses merged quarters 1 and 2, while the second panel uses quarters 3 and 4. The sample includes all households with a child born between 2005 and 2008. Each observation is one child-household. Each column reports the results from a different specification. The dependent variable (three labor supply measures) is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. The controls include maternal age (plus squared and cubed terms), an immigrant dummy, two maternal education dummies (indicating high school graduation and college graduation), and an indicator for whether the woman is observed during the 4-month post-birth maternity leave period. One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 8. Child care expenditure (HBS 2008)

	1	2	3	4
Infant care center exp.	-85	-87	38	-51
	(65)	(67)	(101)	(107)
Nanny/ babysitter exp.	27	38	16	131
	(153)	(160)	(220)	(206)
Daycare exp.	-242 ***	-245 ***	-165 *	-170 **
	(63)	(68)	(89)	(80)
Any formal care exp. (a+b+c)	-300	-294	-111	-90
	(193)	(196)	(283)	(270)
Infant care center (binary)	-0,006	-0,027	0,101 *	-0,001
	(0,038)	(0,041)	(0,055)	(0,052)
Nanny/ babysitter (binary)	-0,004	-0,024	0,012	0,009
	(0,040)	(0,043)	(0,057)	(0,054)
Daycare (binary)	-0,094 **	-0,083 **	-0,032	-0,096 *
	(0,040)	(0,042)	(0,057)	(0,053)
Any formal care (a+b+c) (binary)	-0,108 ***	-0,114 ***	-0,036	-0,093 *
	(0,042)	(0,044)	(0,059)	(0,056)
N	2289	2289	2289	319
Linear trend	Y	Y	Y	N
Month dummies	N	Y	N	N
Controls	N	Y	N	N
Quadratic trend	N	N	Y	N
Number of months	48	48	48	6

Note: Household Budget Survey 2008 data. The sample includes all households with a child born between 2005 and 2008. Each observation is one child-household. Each column reports the results from a different specification. The dependent variable (different expenditure categories) is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. The controls include maternal age (plus squared and cubed terms), an immigrant dummy, and two maternal education dummies (indicating high school graduation and college graduation). One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Table 9. Weekly child care hours (SILC 2008)

	1	2	3	4
Hours in infant care center	-2,43	-2,92	-1,03	-2,49
	(2,58)	(2,89)	(3,67)	(2,80)
Hours with nanny/ babysitter	1,76	0,91	3,25	0,6
	(1,89)	(1,82)	(3,26)	(1,77)
Hours in other (external, formal) care	-0,36 **	-0,35 *	0,11	-0,16 *
	(0,14)	(0,20)	(0,19)	(0,09)
Hours in any formal care	-1,03	-2,36	2,32	-2,05
	(3,23)	(3,46)	(4,97)	(3,38)
Hours in informal, unpaid care	-1,55	-1,08	0,12	0,94
	(1,87)	(2,00)	(2,47)	(1,89)
Infant care center	0,002	0	-0,03	-0,038
	(0,064)	(0,068)	(0,089)	(0,072)
Nanny/ babysitter	0,015	0,003	0,02	-0,003
	(0,028)	(0,030)	(0,043)	(0,032)
Other (external, formal) care	-0,069 ***	-0,064 **	-0,029	-0,038
	(0,022)	(0,029)	(0,026)	(0,025)
Any formal care	-0,015	-0,009	-0,001	-0,053
	(0,065)	(0,069)	(0,090)	(0,073)
Informal, unpaid care	0,008	0,008	0,038	0,071
	(0,055)	(0,058)	(0,077)	(0,058)
N	1409	1409	1409	186
Linear trend	Y	Y	Y	N
Month dummies	N	Y	Y	N
Quadratic trend	N	N	Y	N
Number of months	60	60	60	8

Note: Survey of Income and Living Conditions 2008 data. The sample includes all households with a child born between 2004 and 2008. Each observation is one child-household. Each column reports the results from a different specification. The dependent variable (hours in different child care arrangements) is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

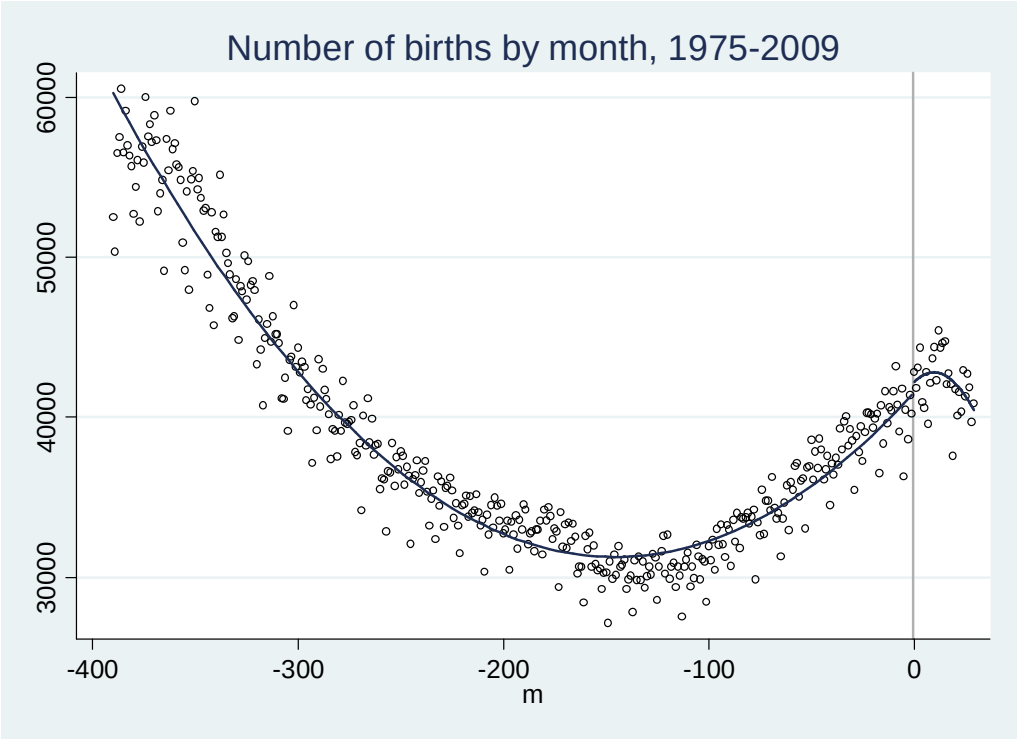
Table 10. Results by maternal education

	No college		College	
	1	2	1	2
Total expenditure	3132	982	550	-2269
	(1942)	(1954)	(2216)	(2889)
Child-related expenditure	498	290	-614	-521
	(407)	(466)	(648)	(828)
Daycare expenditure	-50	-21	-485 ***	-364 **
	(70)	(75)	(124)	(153)
Other personal products exp.	90	81	293 **	216
	(111)	(94)	(119)	(144)
Work, 1st sem.	-0,082 ***	-0,147 ***	-0,156 ***	-0,174 ***
	(0,030)	(0,034)	(0,036)	(0,041)
Work, 2nd sem.	-0,05 *	-0,067 **	0,061 *	0,02
	(0,028)	(0,033)	(0,033)	(0,040)
Emp., 1st sem.	-0,061 *	-0,088 **	-0,088 **	-0,058
	(0,033)	(0,035)	(0,035)	(0,039)
Emp., 2nd sem.	-0,042	-0,094 ***	-0,043	-0,082 **
	0,029	(0,034)	(0,030)	(0,036)
Linear trend	Y	N	Y	N
Month dummies	Y	N	Y	N
Controls	N	N	N	N
Quadratic trend	N	N	N	N
Number of months	48	6	48	6

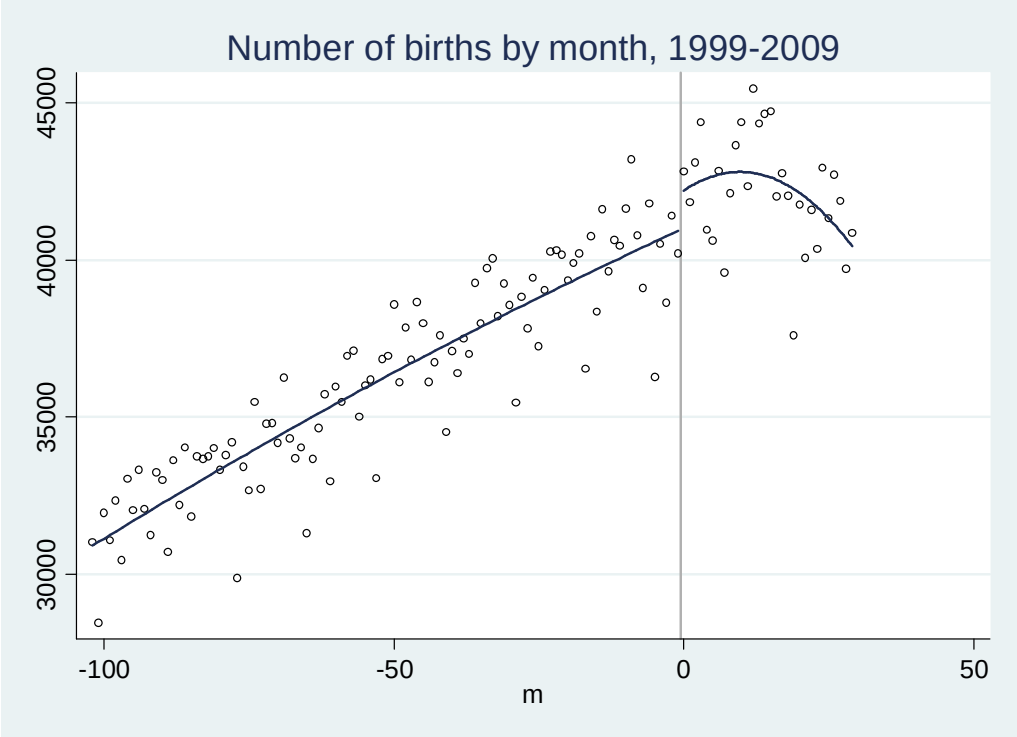
Note: Household Budget Survey and Labor Force Survey 2008 data. The sample includes all households with a child born between 2004 and 2008. Each observation is one child-household. Each column reports the results from a different specification. The dependent variable is indicated in the row header. The main explanatory variable of interest (coefficient reported, standard error in parenthesis) is a binary indicator for births on or after July 1, 2007. One asterisk indicates significance at the 90% confidence level, two at 95%, and three at 99%.

Figure 1. Number of births per month in Spain

Panel A. 1975-2009

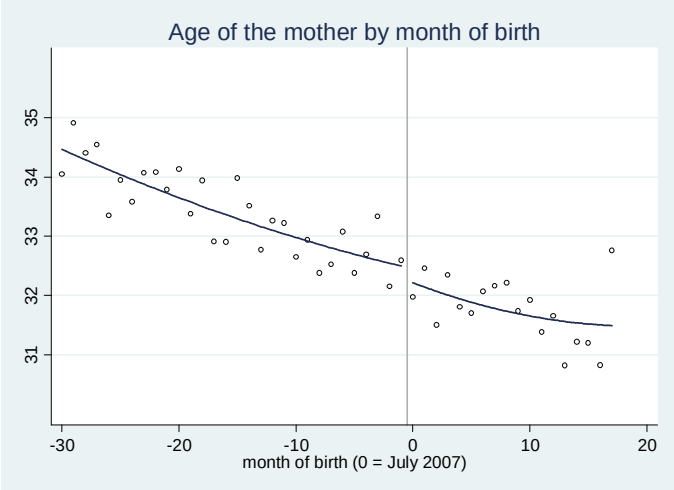


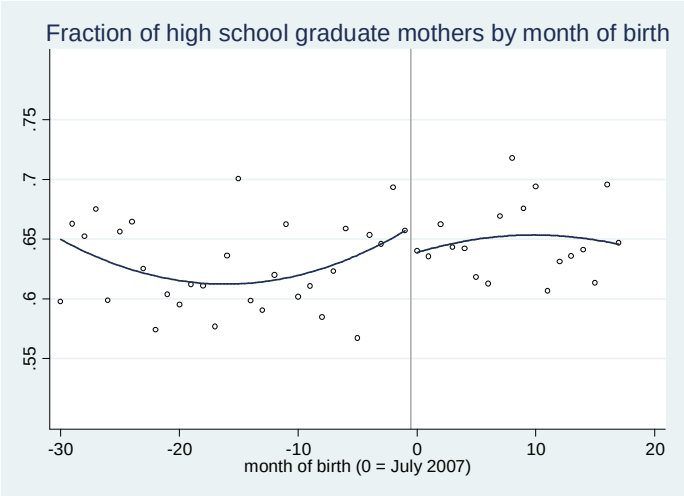
Panel B. 1999-2009

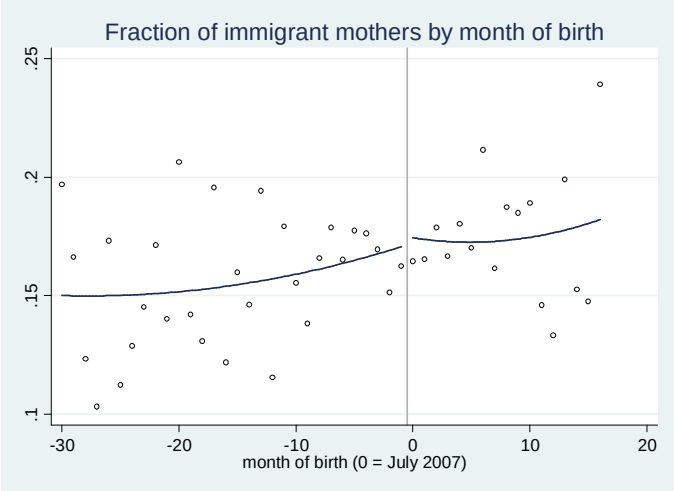


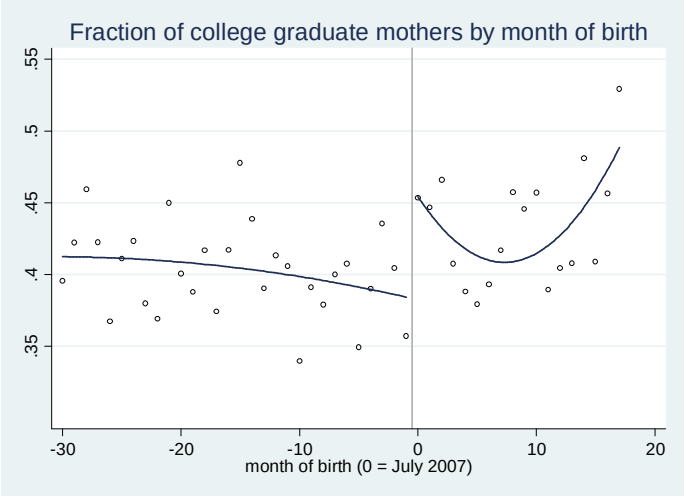
Source: Instituto Nacional de Estadística (INE), (http://www.ine.es/jaxi/menu.do?type=pcaxis&path=%2Ft20%2Fe301&file=inebase&L=)
Note: One-month bins. Month ("m") is normalized to 0 in July of 2007. The fits shown are second-order polynomials (different for months before and after the cutoff).

Figure 2. Mother characteristics by month of birth (LFS 2008)



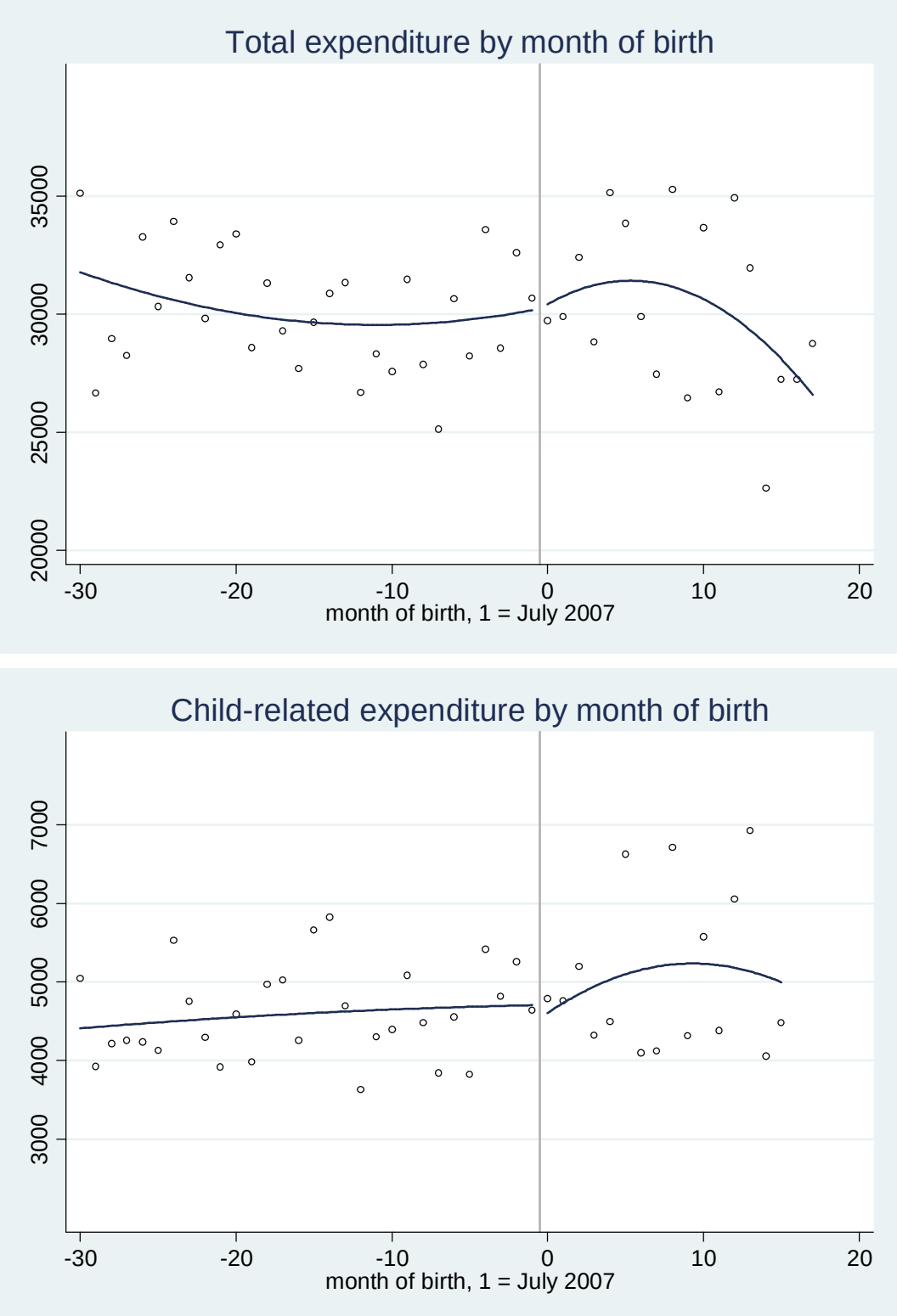






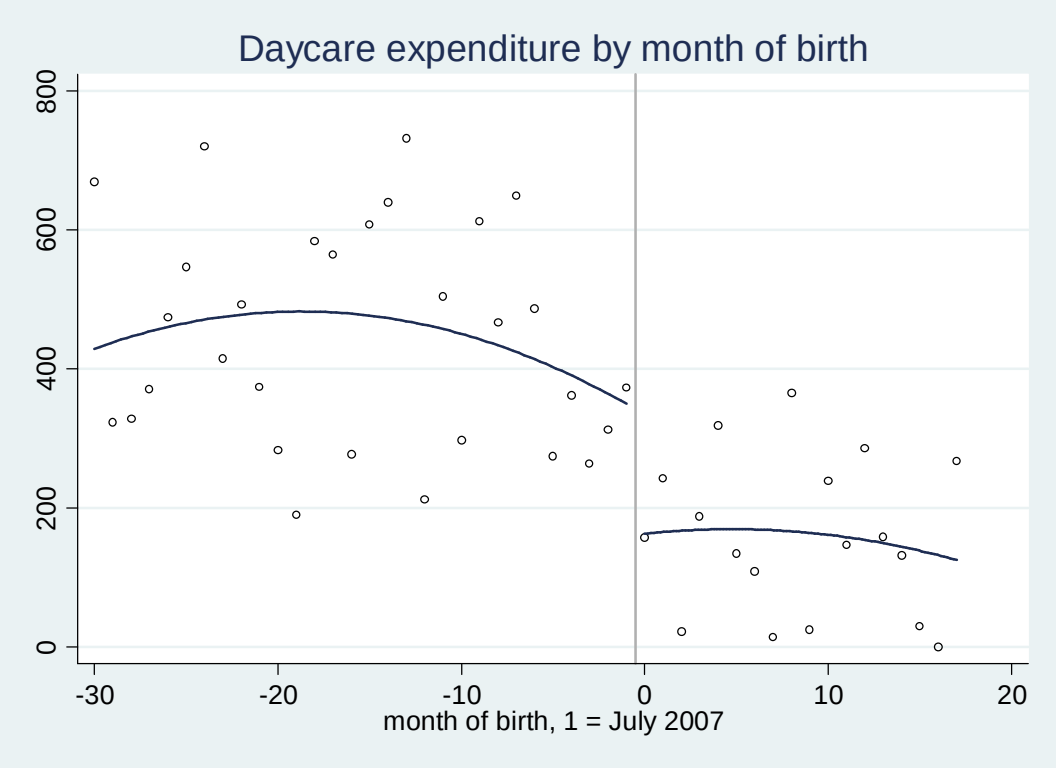
Note: LFS 2008 data, births between 2005 and 2008. Month ("m") is normalized to 0 in July of 2007. The fits shown are second-order polynomials (different for months before and after the cutoff).

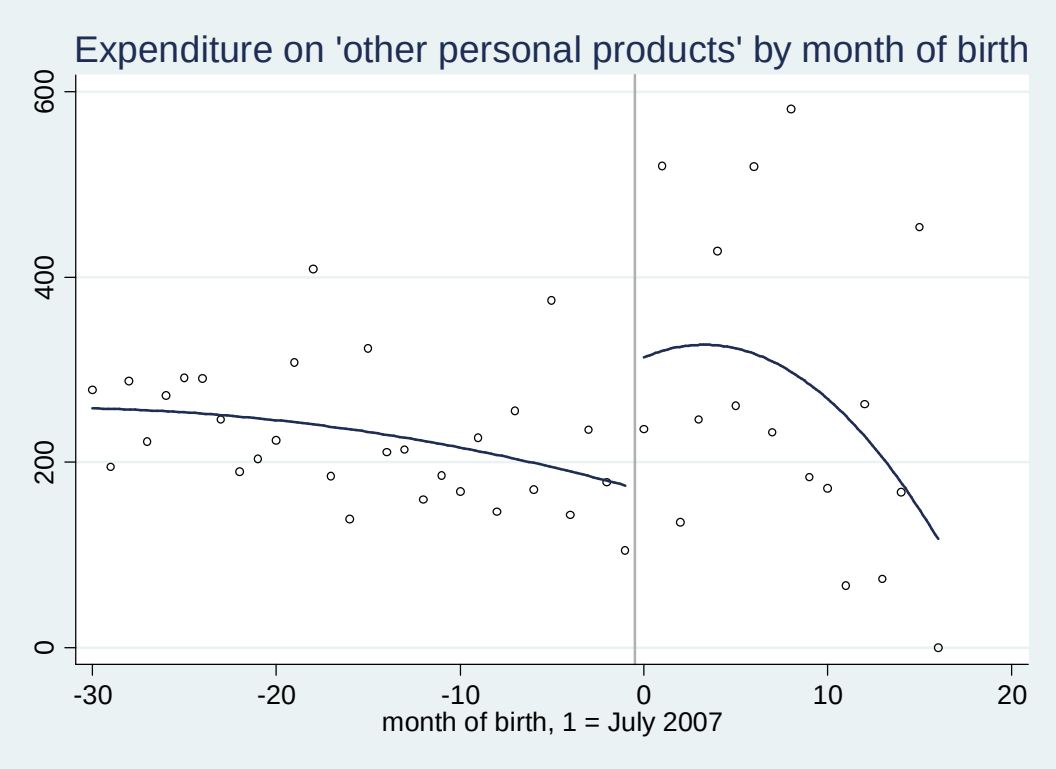
Figure 3. Total expenditure and child-related expenditure by month of birth



Note: Household Budget Survey 2008 data, births between 2005 and 2008. One-month bins. Month of birth is normalized to 0 in July of 2007. The fits shown are second-order polynomials (different for months before and after the cutoff).

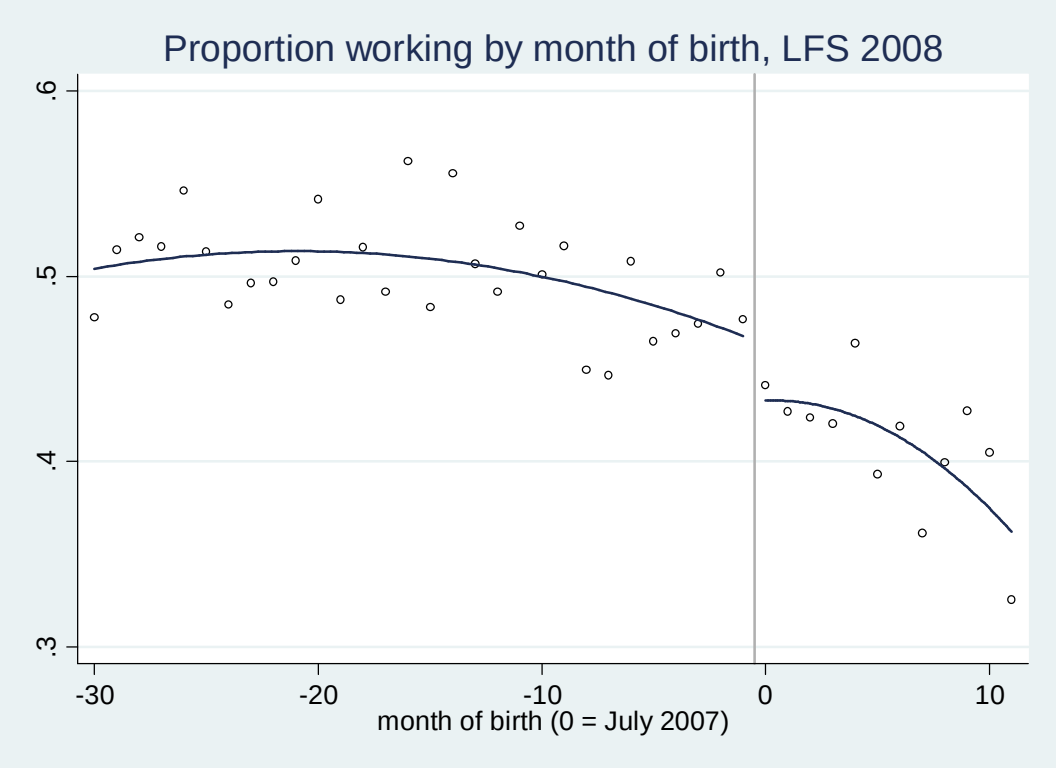
Figure 4. Day care expenditure and expenditure on "other personal products" (HBS 2008)

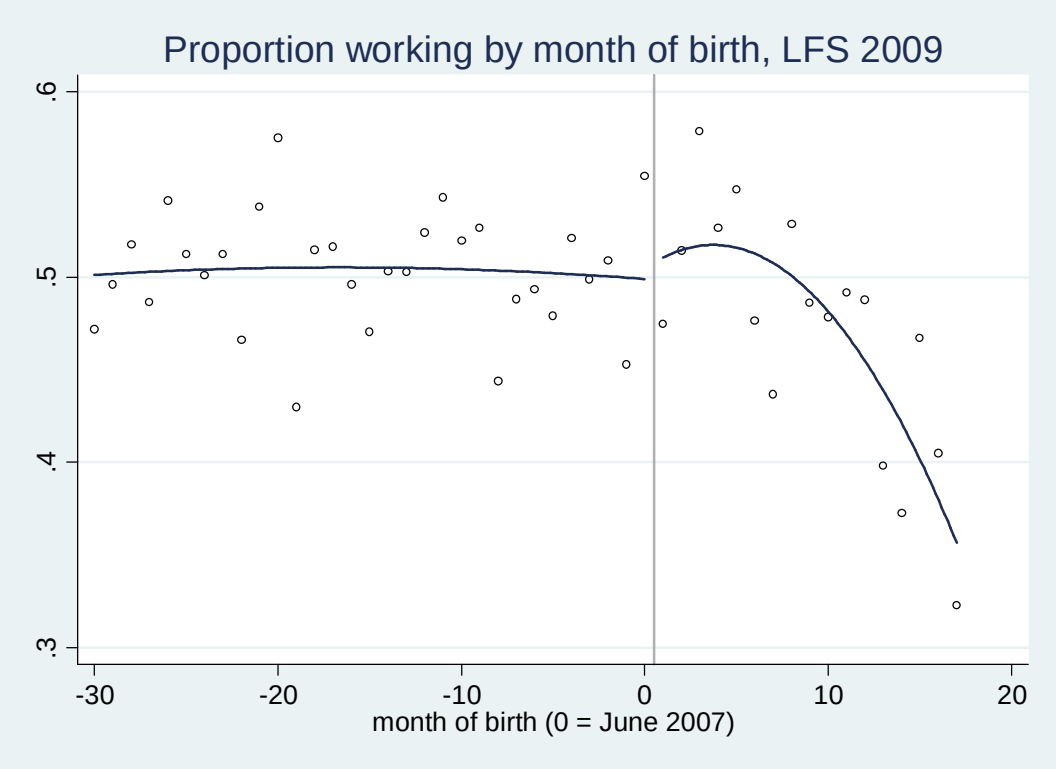




Note: Household Budget Survey 2008 data, births between 2005 and 2008. One-month bins. Month of birth is normalized to 0 in July of 2007. The fits shown are second-order polynomials (different for months before and after the cutoff).

Figure 5. Labor supply effect





Note: Labor Force Survey data (2008 and 2009 if 1st and 2nd panel, respectively), births between 2005 and 2008. One-month bins. Month of birth is normalized to 0 in July of 2007. The fits shown are second-order polynomials (different for months before and after the cutoff). I drop mothers within the 4-month post-birth period (paid maternity leave).