Prenatal Transfers and Infant Health:

Evidence from Spain

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Abstract

We estimate the impact of a cash transfer targeting women who give birth on their subsequent fertility and their (future) children's health outcomes at birth, exploiting the introduction of a universal child benefit in Spain in 2007. Using population-wide, individual-level, high-quality administrative data from birth records and a regression discontinuity approach, we find that low-income women who received the benefit were much less likely to have low-birth-weight children, several years down the road. The €2,500 transfer led to a 1.2 and 1.0 pp decline in probability to be born with <2,000 and <1,500 grams among children born in poor households, which represents a 73% and 151% reduction, respectively. We also find a decrease in extreme prematurity in poor households, suggesting that the low-birth-weight effect is due to both lower prematurity prevalence and faster intrauterine growth, which might be related to improved maternal nutrition or maternal behaviors. Additionally, we find an important interaction between the socio-economic disadvantage and prematurity at birth. Previous evidence on the causal effects of cash transfers to poor families on child health and development has been mixed. Some recent research suggests that targeting pregnant women may be more effective than later interventions, given the strong persistence of fetal health effects. Our results suggest that the impact may be stronger if women are targeted even earlier, before conception.

JEL codes: H51, I18, J13

1. Introduction

What can we do as a society to make sure that all children reach their potential in life? A recent literature on the "fetal origins hypothesis" has documented that a range of early (prenatal and postnatal) shocks and interventions can have substantial effects on long-term human capital formation, including outcomes such as adult health and wages (Currie 2009, Almond and Currie 2011a, 2011b, Almond et al. 2018). Combined with accumulating evidence suggesting that the early years are crucial for human capital production (Cunha and Heckman 2010, Cunha et al. 2010), a strong case is building for policies that target (vulnerable) children early. A few recent papers have examined the effects of (conditional) cash transfers targeting poor households in utero and in early life on health outcomes of vulnerable children (Almond et al. 2011, Hoynes et al. 2015, Hoynes et al. 2016, Amarante et al. 2016). We contribute to this literature by examining the effects of a universal cash transfer received in utero or even prior to that on children's health outcomes at birth in Spain.

We argue that the specific way in which a universal child benefit was introduced in Spain in 2007 creates a natural experiment that enables us to study the impact of a generous lump-sum "maternity bonus" on subsequent fertility of women who received it and on health outcomes of their future children. In a national speech on July 3, 2007, the Spanish prime minister unexpectedly announced the introduction of a new unconditional family benefit, which would pay €2,500 to all women who give birth. The subsidy would be paid for all children born from July 1, 2007, onwards. This setting enables us to use a regression discontinuity design (RDD) to credibly estimate the effects of a universal child benefit on subsequent fertility, and any (unintended) consequences of this benefit on health outcomes of *future* children.

Previous literature focused on the effects of cash transfers received by poor households in utero on children's health outcomes at birth and later in life. Hoynes et al. (2015) examine the effects of the U.S. Earned Income Tax Credit Program (EITC), finding reductions in the incidence of low birth weight among mothers who benefited from a benefit expansion while pregnant. Almond et al. (2011) and Hoynes et al. (2016) find (respectively) that the roll out of a near-cash benefit in the US (Food Stamps) increased birth weights, especially among African Americans, and that in the long-term the roll out reduced the incidence of metabolic syndrome (i.e., obesity, high blood pressure, diabetes, etc.). Finally, Amarante et al. (2016) find that cash social assistance in Uruguay led to a reduction in the incidence of low birth weight due to faster intrauterine growth. The available evidence thus shows significant effects of cash transfers paid to poor pregnant women on their children's outcomes at birth.

In this paper, we exploit the unexpected introduction of a *universal* subsidy paid after a child's birth to evaluate its effect on subsequent fertility of women and on health outcomes of their future children in Spain. In line with the previous literature, we examine whether children born in socio-economically disadvantaged households were more likely to benefit from the policy changes. Additionally, we investigate whether prematurely born children, who are particularly affected by detrimental birth outcomes, benefitted more than children born on-term. Finally, we look at the interaction of prematurity at birth and economic status of the family. Apart from estimating the overall effects, we also examine their timing. We use publicly available birth-certificate microdata encompassing the universe of all births in Spain. We find that the policy did not change subsequent fertility behavior of women in the following 5 years. While some perinatal health outcomes of children born in the next 5 years were not affected (still birth, early neonatal death, normality of birth, prematurity of birth), the probability of a low birth weight decreased substantially. For instance, the probability of a very low birth weight (VLBW; <1,500 grams) decreased by 69% of the mean value. In the heterogeneity analysis, we find that this effect is driven by disadvantaged families and by vulnerable children (a decrease by 151% and 77% in poor households and among prematurely born children, respectively). Additionally, we find an important interaction between the socio-economic situation and prematurity at birth: children who are neither poor nor prematurely born do not benefit, children who are affected by only one adverse condition (either poverty or prematurity) benefit through a lower probability of low birth weight, and children affected by both conditions benefit extensively through a lower probability of low birth weight and through higher average birth weight by 261 grams or 16%. Furthermore, we find that the observed effects are driven mainly by births that took place in the second year after the policy introduction (with benefits being paid between 5 and 14 months after the introduction). Finally, we find suggestive evidence that at least part of the observed results in poor households is driven by a decrease in extremely premature births. We plan to explore the role of intra-uterine growth and of general underlying causes, such as maternal nutrition, stress, and behaviors, in the near future.

Our contribution to the literature is fourfold. First, compared to previous studies, we provide evidence from a *universal* cash transfer, i.e. we are able to estimate the effect of prenatal cash transfers in the whole population and in different subgroups of population. Previous studies evaluated policies targeted at poor households. Second, we use population-wide, individual-level, high-quality administrative data with detailed information on perinatal health outcomes of children (exact pregnancy weeks, exact birth weight, etc.) and on socio-demographic characteristics of their parents. Third, the policy that we study affected many women prior to their next pregnancy; most previous studies look at cash transfers delivered to pregnant women. Finally, we explore the interaction effect between socio-economic and biological vulnerability of children to poor health outcomes at birth, and find important results.

The remainder of this paper is organized as follows: In section 2, we provide background information on the universal child benefit in Spain. Sections 3-5 describe the data, estimation methods, and results. Last two sections discuss the results and conclude.

2. Institutional background

On July 3, 2007, Spanish prime minister Zapatero announced in a national speech that a universal child benefit would be introduced in Spain. For every child that was born or adopted starting from that day, families would receive a lump-sum payment of €2,500. This universal child benefit was to be paid in addition to any other child support or family benefits that the family was already entitled to. The proposal of the new law was approved by the Spanish government on July 13, 2007, and it was announced that the parliament would pass the law in an accelerated procedure in November 2007, with the actual payments of the benefit starting from December 2007. The government launched helplines informing about the law, provided request forms, and started accepting these requests in social security offices in mid-July 2007. The Spanish parliament passed the new law on November 15, 2007, which in its final form stipulated that all children born or adopted as of July 1, 2007, would be eligible for the universal child benefit of €2,500.[1]

Eligible parents encompassed both Spanish and foreign nationals who had resided legally in Spain for at least two consecutive years prior to the birth or adoption. The benefit would be delivered in form of a fiscal deduction or in cash. The timing of the payment to women who gave birth in the first months of the benefit depended on which form of payment the family chose: cash payments were delivered at the earliest in late November 2007 (after the law was passed by the parliament), while tax deductions would be typically paid between April and August 2008, in extreme cases up to December 2008. Approximately half of the families requested cash payments, while the other half opted for a fiscal deduction. At its introduction, the universal

[1] On May 12, 2010, the same prime minister announced that the so-called "baby check" would be available only until the end of the calendar year 2010, meaning that families with births or adoptions starting from January 1, 2011, would not receive the universal child benefit anymore. The cancellation of the policy was not expected because in 2009, Zapatero categorically denied any plans of cancelling the universal child benefit. Additionally, in 2010, the government's intention to substantially cut public expenditure due to the ongoing economic crisis was announced only one week prior to announcing the cancellation of the child benefit.

child benefit constituted 150% of the average gross monthly earnings in Spain.[2] In terms of child raising costs, this amount is estimated to have covered the first 5-6 months after childbirth.[3]

3. Data

Our data stem from the registry of births, which are collected and made publicly available by the Spanish statistical office (INE). These microdata encompass the universe of all births that take place in Spain every year. The publicly available data set includes information on month and year of birth of the current child and of its previous sibling (if any), basic demographic characteristics of the child, and socio-demographic characteristics of the parents. We requested additional information on the exact day of birth of each child born in years 2006 to 2013 and of its previously born sibling. This way we are able to implement the RDD estimation with maximum accuracy, and we can also measure birth spacing between two consecutive births precisely. We use these microdata to create two types of individual-level data sets.

3.1 Data on subsequent fertility

This data set includes all births that took place in Spain in 2007,[4] and its goal is to record whether each child born in 2007 had a "future sibling" born within the next 5 years. However, the birth certificate data does not allow us to follow families or mothers over time; each child is recorded separately and cannot be linked to its siblings born in different points in time. Therefore, we construct an individual-level data set from aggregate data: We count number of children born in 2007 ('all births' **AB**), and number of children born in 2007-2013 with a previous sibling born in 2007 within a 5-year window ('with a sibling' **WS**).[5] From these counts, we create an individual-level data set with **WS** number of observations with a sibling, and (**AB** – **WS**) number of observations without a sibling. Due to this data construction procedure, we have no information about the individual characteristics of children or of their parents. The Annex provides details about the exact way in which the data set was constructed, and discusses any imperfections in this procedure.

[2] In terms of income distribution, the real value of the benefit was 250%, 190%, and 130% of the 25th percentile, median, and 75th percentile of gross monthly earnings, respectively. For single mothers, the benefit had an even higher real value: 310%, 220%, and 150% of female gross monthly earnings, respectively. The data on earnings stem from a wage survey conducted in 2006 by the Spanish statistical office (INE).

[3] Our calculation is based on Save the Children (2018) report which estimates that costs related to raising children aged 0-3 years amounted to €479 and €551 per month in poorer and richer regions of Spain in 2018, respectively. Recalculating these costs into 2007 and 2010 prices, they correspond to €418-€481 in 2007 and €442-€508 in 2010.

[4] We include mothers with residence in any of the 52 Spanish provinces.

[5] We restrict the time window of 5 years with daily precision (i.e. birth spacing of up to 1,825 days).

3.2 Data on health outcomes of children born in the future

This data set comprises future siblings of children born in 2007. Since it is extracted directly from the registry of births microdata, we have information about children's demographic and their parents' socio-demographic characteristics. The sample includes all births that occurred in Spain in 2007-2013, to mothers with residence in any of the 52 Spanish provinces, with the previous sibling being born alive in Spain or in an unknown location in 2007. This sample definition is based on the eligibility criteria for baby bonus (see Annex). Again, we look at children who were born not more than 5 years after their previous sibling. In terms of health outcomes of these children, we have information on the following perinatal outcomes: still birth, early neonatal death in the first 24 hours, normality of birth, C-section, weeks of gestation, prematurity of birth, birth weight, and low birth weight. Given that perinatal health outcomes tend to be worse for multiple births (for instance more complications at birth or lower birth weight), we evaluate them only for singleton births.

3.3 Income data

Despite the availability of a relatively rich set of socio-demographic characteristics of the parents, INE does not collect any measure of income in its birth registry. In order to create a measure of income for our analyses, we combined the birth registry data with 2007 Spanish Statistics on Income and Living Conditions (SILC) data. SILC 2007 collected data on households' disposable income in 2006, i.e. prior to the introduction of baby bonus. We selected households that are similar to those in our sample from the birth registry, i.e. households with under-6 children (1,580 households). We predicted income per capita in these households (using OECD scale) based on various observable household and parental characteristics.[6] We saved the coefficients of all explanatory variables and used them to predict income in the birth registry data, using a harmonized set of household and parental variables. Finally, we ranked mothers in birth registry data based on their predicted household income and created a poverty indicator (poor if belongs to the 1st-25th percentile, alternatively to the 1st-40th percentile). R-squared in the SILC regression was 0.45, indicating a good model fit. Furthermore, when comparing the predicted with the actual poverty status in an in-sample prediction using the SILC 2007 data, 76-78% of cases were predicted correctly.

[6] OECD scales are defined as follows: first adult = weight 1, each additional adult = weight 0.5, each child = weight 0.3; an adult is a person aged 14+. Predictors of income are the following variables: regional dummies (19 regions), area of residence (high, medium, low population density), marital status (married or not), cohabitation (father is cohabiting or not), number of mother's children living in the household (set of dummy variables: 1, 2, 3, 4+). In addition, we include mother's and father's nationality (Spanish or not), education level (no, primary, secondary, tertiary education), occupation (10 categories each), age at birth of the youngest child (cubic specification each). All variables are harmonized to the biggest extent possible with the birth certificate data.

4. Methodology

In order to analyze the effects of the universal child benefit on subsequent fertility and health outcomes of future children, we use individual-level data and employ an RDD based on day of birth of the previous sibling. More specifically, we compare outcomes of interest (fertility within the next 5 years, health outcomes of children born within the next 5 years) for mothers who gave birth prior to the policy introduction, i.e. in weeks up to June 30, 2007, with outcomes of mothers who gave birth in weeks starting from July 1, 2007, and were therefore eligible for the baby bonus. The sample is restricted to the immediate neighborhood of the day of baby bonus introduction ("cut-off"), ranging from 1 to 16 weeks.[7] Our preferred specification is an 8-week bandwidth (56 days). The equation we estimate is:

$$Y_i = \alpha + \beta\, T + \gamma_1 t + \gamma_2 t * T + \theta_d + \theta_k + \delta_r + \varphi X_i + \varepsilon_i \qquad (1)$$

$$\forall\, t\, \epsilon\, (c - 56, c + 55), T \equiv 1(t \geq c),$$

where the dependent variable **Y** is one of the outcomes of individual **i** (mother in case of subsequent fertility, and child in case of health outcomes). The forcing variable is day of birth **t** of the previous child born in 2007. The cut-off **c** is July 1, 2007. Treatment **T** is a binary variable which takes value 1 if the previous child was born starting from July 1, 2007, and 0 otherwise. The key parameter of interest is $\boldsymbol{\beta}$, which identifies the change in subsequent fertility of the mother or in health outcomes of her future children, depending on whether the mother received baby bonus in 2007 or not. We include also linear time trends that differ before and after the cut-off, day-of-the-week fixed effects ($\boldsymbol{\theta_d}$) to account for seasonality effects in births throughout the week, and additional covariates **X**. $\boldsymbol{\alpha}$ is a constant and $\boldsymbol{\varepsilon_i}$ is the error term. Standard errors are clustered at values of the running variable, i.e. day of previous birth **t**.

In case of subsequent fertility, the only available covariate **X** is the overall number of births in Spain in week **w**. In case of health outcomes of future children, the covariates **X** are parental characteristics: mother's and father's age (cubic function), mother's parity before giving birth in 2007 (previously had 1 child, 2 children, 3 or more children), mother is married, father is cohabiting, mother and father are foreign-born (dummies), mother's and father's education (below secondary, secondary, more than secondary). Additionally, we include day-of-the-week fixed effects (FE) of the *current* birth ($\boldsymbol{\theta_k}$) and region fixed effects $\boldsymbol{\delta_r}$ (there are 19 regions).[8]

[7] Week variable is scaled in such a way that births on July 1-7, 2007 (or 2006 or 2008), fall into the same week.
[8] $\boldsymbol{\theta_k}$ and $\boldsymbol{\delta_r}$ are omitted in regressions of subsequent fertility due to their irrelevance and unavailability, respectively.

5. Results

5.1 Subsequent fertility effects

Since the universal child benefit was an unexpected income shock for those families who received it around its introduction, they could have reacted with a change in their subsequent fertility behavior. In particular, if we assume that children are normal consumption goods, we could expect an income effect (more income leads to a higher demand for consumption goods), i.e. baby bonus could have led to higher subsequent fertility. Table 1 shows the treatment effects of universal child benefit on the probability to have another child in the next 1-6 years. As we see in panel A, there was no significant change in the fraction of women who had another child in the following 1, 2, 3, 4, 5, or 6 years. Table A1 shows that this null result is robust to the bandwidth choice. Nevertheless, these first results measure subsequent fertility in a cumulative way. This approach might be problematic if effects going in opposite directions occurred over time. For instance, a significant effect occurring in later years could be hidden and go unreported if there was a (cumulative) effect going in the opposite direction in (one of the) earlier years. In order to eliminate this possibility, panel B in Table 1 shows subsequent fertility response to baby bonus in the 1st, 2nd, 3rd, 4th, 5th, and 6th year separately. Again, we see no significant changes in subsequent fertility.

5.2 Main health effects

Given that the universal child benefit led to no changes in subsequent fertility, the next question is whether this positive income shock could have affected the heath of "future" children. We have access to information about future children's perinatal health. As some birth outcomes might be worse for multiple births, all estimations are performed only for singleton births. In order to check whether this does not pose a problem, column 1 in Table 2 shows the effects of the universal child benefit on the probability to be born as a singleton birth as opposed to a multiple birth. Unsurprisingly and reassuringly, we find no change in this probability at the cut-off. Columns 2-7 show the first set of perinatal health outcomes. In terms of the most severe outcomes, we see that the probability of a still birth, death within the first 24 hours, and normality of birth was not affected. Neither did change the probability of a C-section. However, since a C-section can be requested by the woman even without a medical reason, it is not an indisputable health indicator. Finally, the length of pregnancy did not change and neither did the probability of a premature birth.[9]

[9] A histogram showing the distribution of pregnancy weeks at birth in the estimation sample is shown in Figure A1.

Table 3 shows the treatment effects of universal child benefit on birth weight of future children and on various indicators of low birth weight.[10] Overall, we see no effect on birth weight, neither in absolute nor in relative terms (columns 1-2). When looking at indicators of low birth weight, we find no effects for the indicator typically used in the medical literature (LBW; birth weight<2,500 grams).[11] However, for the typical medical measure of *very* low birth weight (VLBW; <1,500 grams), we find a statistically and economically significant effect: women who were entitled to receive the baby bonus in 2007 were by 0.36 percentage points (pp) less likely to give birth to a baby with VLBW in the next 5 years, which constitutes a 69% decrease relative to the mean of the variable. Due to this large effect on VLBW but no effect on LBW, we examine also other cut-offs between the traditionally used 1,500 and 2,500 grams, in 250-gram steps. We find effects for all low-birth-weight cut-offs lower than 2,500 grams; the relative magnitude of the effect is increasing as the cut-off is decreasing (i.e. becoming more extreme): it ranges from 25% at 2,250 grams to 56% at 1,750 grams. This is demonstrated also in Figure 1 where we plot the effects of universal child benefit on different measures of low birth weight, with cut-offs ranging from 1,500 to 3,000 grams, in 100-gram steps. We show relative effect sizes to account for the fact that the lower is the threshold, the lower is the fraction of children falling into that category, and consequently the smaller is also the potential magnitude of any observed effect. We see that the relative effect size is large for VLBW (<1,500 grams), it gradually decreases as the cut-off increases (i.e. as low birth weight becomes less extreme), and it becomes a zero effect starting from LBW (<2,500 grams) onwards. This means that the universal child benefit positively affected children at the lower end of birth weight distribution.

Having confirmed that the treatment effect is present for a multitude of low-birth-weight cut-offs, i.e. it is *not* an artifact of a specific cut-off, some of the results will be shown only for one outcome: the medically relevant VLBW (<1,500 grams). Figure 2 shows the treatment effect on VLBW at the cut-off graphically. In terms of robustness, Table 4 and Figure 3 show that the estimated treatment effect is robust to the choice of the bandwidth: it remains stable both in magnitude and significance for bandwidths 1-16 weeks around the cut-off. Table A2 shows robustness to bandwidth also for the remaining dependent variables. Additionally, we examined also robustness to the inclusion of covariates. We first estimated regressions as in Table 3 with only linear trends, and then added cumulatively: (1) day-of-the-week FE, (2) region FE, (3) day-of-the-week FE of the current birth, and (4) parental characteristics. The results (not shown) are independent of the

[10] A histogram showing the distribution of birth weight in the estimation sample is shown in Figure A2.

[11] Note that throughout the paper, we use the expression "low birth weight" in general terms, i.e. not referring specifically to the 2,500 grams cut-off. When we refer to that specific cut-off, we either explicitly mention it or we use the abbreviation LBW.

covariates included, both in terms of magnitude and significance. As an example, the relative effect on VLBW ranges from –69% to –75% and is significant at 1% or 5% in all five variations of the estimation.

Finally, it is important to show that the estimated treatment effects are not seasonality effects, i.e. they do not occur every year around July 1. In order to do that, we estimate two sets of placebo regressions where we include either women with children born around July 1, 2006 (i.e. neither women giving birth before nor after the cut-off received the benefit) or around July 1, 2008 (i.e. both women giving birth before and after the cut-off received the benefit). As shown in Table 5, we find no effects at the cut-off that would improve (low) birth weight of the next child.

5.3 Heterogeneity analysis of effects on birth weight

So far we have shown that the receipt of a universal child benefit of €2,500 led to a lower probability that the next child would be born with a low birth weight. We would expect that this effect is driven by children born in disadvantaged families (who are most likely to benefit from financial support) and by children born prematurely (who are more likely to be born with low birth weight).

5.3.1 Socio-economic disadvantage

In terms of disadvantaged families, Table 6 shows heterogeneity by household income. As expected, the main effect is driven by children born in the lowest quartile of income distribution: the treatment effect on VLBW is three times larger here than in the full sample (–1.03 pp versus –0.36 pp) and the relative effect size is two times larger (–151% versus –69%). In the remaining three quartiles of income distribution, we see a negative effect on different measures of low birth weight, but they are smaller in magnitude and imprecisely estimated. Figure 4 shows the corresponding effects graphically. In terms of robustness, we find qualitatively similar results when we define poor households as those in the first two quintiles of income distribution (results not shown): the treatment effect on VLBW in poor sample is –0.70 pp (–106%, significant at 1% level) whereas the effect in the non-poor sample is –0.18 pp (–41%, insignificant). Apart from household income, which was estimated based on parental and household characteristics, other dimensions of socio-economic disadvantage are directly available in the data.[12] Table A3 shows heterogeneity analysis by maternal education and marital status, and it confirms that the main effects are driven by the disadvantaged, i.e. less educated and unmarried, women. More specifically, the estimated effects among low-educated mothers are

[12] One caveat regarding our household income measure is that it is estimated based on parental and household characteristics at the time of birth of the *next* child, which was born in 2007-2013, *not* based on characteristics in 2007. The further away the next birth took place from 2007, the less might certain characteristics resemble those at birth of the previous child in 2007. However, this problem arises due to data limitations and it applies also to any other dimension of socio-economic disadvantage directly available in the data (such as maternal education and marital status, based on which the household income is predicted).

similar to those among poor mothers albeit they are somewhat smaller in relative magnitude (panel A). Among unmarried mothers, the relative effect sizes are larger and they correspond to those among poor mothers (panel B). What is more, also LBW (<2,500 grams) and actual birth weight improved due to baby bonus among unmarried women: their next child was born with a birth weight larger by 1.4% and with a lower probability of LBW by 2.1 pp (35%).

5.3.2 Prematurity at birth

Apart from unfavorable socio-economic conditions, a premature birth is another important factor that makes a child vulnerable to low birth weight. In our estimation sample, 87.9% of VLBW children were born prematurely whereas only 4.6% of children with birth weight ≥1,500 grams were born prematurely. To show this relationship from another angle, Figure A3 shows the fraction of children who are born with VLBW, conditional on the pregnancy week. Expectedly, VLBW children are overrepresented among prematurely born children (9.0% on average) and hardly present among children born on term (0.1% on average).[13] Therefore, Table 7 presents the effects of baby bonus separately for children born on term and prematurely. Perhaps unsurprisingly, the effects on different measures of low birth weight are found only among prematurely born children and the relative magnitudes are somewhat larger than in the full sample. What is new here is that baby bonus led not only to a lower probability of a low birth weight but it also increased the average birth weight of prematurely born children by 5.2% or 101 grams. This is a substantial magnitude.

5.3.3 Interaction between socio-economic disadvantage and prematurity at birth

In the last step, we look at the interaction between adverse socio-economic conditions and prematurity of birth. Table 8 shows the effects among poor premature, poor on-term, non-poor premature, and non-poor on-term children. Expectedly, non-poor on-term children were not affected. Interestingly, non-poor premature children did benefit from baby bonus through a lower probability of having low birth weight (e.g. relative effect size –54% for <2,000 grams). Also poor on-term babies benefitted through a lower probability of low birth weight but the effects are several times larger than in the previous group (e.g. relative effect size –142% for <2,000 grams). Finally and most importantly, poor pre-term babies benefitted hugely from baby bonus. The probability that they would be born with low birth weight decreased: the decrease is by 208% for VLBW and by 41% for LBW, an indicator that was unaffected in almost all other samples (with the exception

[13] Admittedly, VLBW is an extreme and relatively rare birth outcome: only 0.5% of children are born with VLBW. On the other hand, LBW (<2,500 g) is more common with 4.6% of children being affected. The corresponding statistics for LBW and prematurity are the following: In our estimation sample, 59.8% of LBW children were born prematurely whereas only 2.4% of children with birth weight ≥2,500 grams were born prematurely; see Figure A3 for a graphical representation. In addition, LBW children are the majority of prematurely born children (54.3% on average) but only a small minority of children born on term (2.0% on average).

of unmarried women whose children benefitted from a similarly large relative decrease of LBW: –35%). Importantly, the effects are large not only in relative terms but also in absolute terms: the estimated drop is 20 pp and 25 pp for VLBW and LBW, respectively. What is more, we see that the average birth weight reacted positively as well: it increased by 16.0% or 261 grams, which is a substantial improvement.

5.4 Timing of the effects and potential mechanisms

The effects of a universal child benefit on birth-weight-related outcomes of the next child that were documented so far are important. Thus, there are two key policy-relevant questions that should be answered: (1) when did these effects take place, and (2) what were the mechanisms behind them.

5.4.1 Timing of the effects

In our main analyses, we included all children that were born within the next 5 years after birth of the previous child around July 1, 2007. Since the universal cash transfer amounted to approximately 150% of the average and 250% of the 25th percentile gross monthly earnings, it is reasonable to assume that this amount might have improved any aspects of family life only for a limited period of time. Therefore, Tables A4 and 9 show results for different birth-weight-related measures separately for the following 1-5 years after birth of the previous child. In the full sample, the effects are stemming from births taking place in the 1st and 2nd year after birth of the previous child (Table A4). Somewhat surprisingly, the effects appear also in the 4th year. When looking at the more affected poor subsample, there are three noteworthy results (Table 9). First, in the 2nd year, all indicators of low birth weight and also birth weight itself are affected substantially. This is plausible given that the benefit was paid approximately 5-14 months after July 1, 2007, which would be some months prior to or during pregnancy with the next child born in the 2nd year. Second, we see that multiple measures of low birth weight (except for LBW, <2,500 g) were affected in the 1st, 2nd, 4th, and 5th year. Significant are only those measures at the lower end of the distribution (<1,500 g and <1,750 g); the remaining ones (<2,000 g and <2,250 g) are substantial in magnitude but insignificant. Again, it is somewhat surprising that births taking place even 4 or 5 years after the benefit receipt would be affected. On the other hand, what is affected are the worst, most extreme birth-weight outcomes in poor household, which might be plausible. Finally, somewhat surprisingly, the results in the 3rd year do not coincide with the other years, not even qualitatively. What is needed in the next step is an exploration of potential mechanisms, which might help to explain both the plausible (1st and 2nd year) and somewhat surprising (4th and 5th year) results.

5.4.2 Potential mechanisms

In general, a lower fraction of children born with low birth weight, or an increase in the average birth weight, can be caused either by faster intra-uterine growth or by fewer premature births. Both of these biological channels have, in turn, their own determinants and underlying causes. Therefore, the exploration of causal mechanisms includes both changes in behaviors or in household conditions, and the resulting biological channels (less prematurity, faster intra-uterine growth).

In order to distinguish between faster intra-uterine growth and prematurity, it is useful to look at the probability of a child to be born prematurely. In Table 2, we found no impact of the benefit receipt on prematurity as defined in the medical literature (<37 weeks of gestation). However, as already shown in Figure A3, VLBW happens at very low gestational ages. This is highlighted also in Figure A4 which shows that the majority (56%) of VLBW children in our estimation sample are born at <30 weeks.[14] Therefore, Table A5 explores prematurity using also lower cut-offs than 37 weeks.[15] In the full sample, we find no significant effects (panel A). However, we see a decrease in prematurity defined as <30, <29, and <28 pregnancy weeks (panel B). The relative effect sizes are large at 87%-155% of the sample mean. Note that we see substantial effects also for surrounding measures of <31, <27, and <26 pregnancy weeks but they are imprecisely estimated. When separating these effects by year of birth of the next child (results not shown), they are driven by births in the 2nd year (significance at 10% level); large but insignificant effects are observed also in the 4th and 5th year. Importantly, the timing of these effects on extreme prematurity corresponds to the time periods when VLWB decreased among children born in poor households (Table 9).

6. Discussion

One of the contributions of this paper is that we are able to estimate population-wide effects of a *universal* cash transfer paid in prenatal period on children's health outcomes at birth. Even though we found an overall decrease in probability of low birth weight, we also saw that this effect is not necessarily universal, as it is driven mainly by disadvantaged or prematurely born children. In comparison to other papers, which focus on cash transfers to disadvantaged women (Hoynes et al. 2016) or poor families (Amarante et al. 2016), our results are qualitatively similar. Most previous studies find either no effect (e.g. Currie and Cole 1993) or a decrease in LBW (<2,500 grams). In general, papers focusing on social or tax credit programs in the US find an effect on LBW of 7-12% (Almond et al. 2011, Hoynes et al. 2016), while a paper focusing on social assistance in Uruguay finds a larger, 20% decrease in LBW (Amarante et al. 2016). In our case, we find an

[14] Fraction of VLBW births taking place at <29/<28/<27/<26 weeks is 46%/30%/20%/11%, respectively.

[15] Note that information on pregnancy weeks is missing for some observations while information on prematurity of birth is always reported. Therefore, estimations at other cut-offs than 37 weeks are done in a smaller sample than in Table 2. In order to see whether the sample reduction might be leading to any irregularities, we estimate the probability of a premature birth both with all observations (column 'Premature birth' in Table 2) and with observations where information on exact pregnancy weeks is available (column 'less than 37 weeks' in Table A5). We find no irregularities.

effect on LBW only in the subgroup of unmarried women, which is a group similar to that in the literature from the US, and our effect is substantially larger at –35%. The main result of our paper is a decrease in VLBW, which is a more extreme measure than LBW, and perhaps that is why we find substantially larger effects than previous literature found for LBW (–69% in the full sample and –151% in the poor sample). Even within the same measure, our effect size is larger than that of Amarante et al. (2016) who find an effect of approximately –40% for VLBW. As a caveat for interpreting and comparing these relative effect sizes, note that the incidence of LBW in Spain (approximately 4.6% in full sample and 6.1% in poor sample) is half of that in the US and Uruguay (approximately 10%). Also, note that women in our sample oftentimes received the benefit already prior to pregnancy which might partly explain the larger magnitude of our effects.

Particularly relevant is the analysis of interaction between poverty and child's biological prematurity at birth. The effects are particularly large in this specific group and they go beyond a lower probability of low birth weight, as we find a positive and large effect on the average birth weight as well (261 grams or 16%). Despite these large effects, the group concerned is relatively small (1.44% in our estimation sample) and one could thus dismiss the practical relevance of these results. Nevertheless, these effects are policy-relevant since "extremely low birth weight infants are over-represented in terms of treatment costs", as Guldi et al. (2018, p.29) point out in their study of the effects of income transfers to low educated women with extremely low birth weight children (<1,200 grams) on health of these children.

Similarly to Amarante et al. (2016), we find no effect of the benefit on gestational length, i.e. pregnancy weeks at birth, and on medical prematurity (<37 weeks). However, when expanding the definition of prematurity, we find effects at the lower end of gestational length distribution in poor households. Importantly, the estimated decrease in probability of extreme prematurity coincides with a decrease in incidence of VLWB both in terms of the affected population and in terms of timing. Thus, this evidence suggests that at least part of the observed effect of baby bonus on VLBW of the next child was likely driven by a decrease in extreme prematurity of births. Note that this finding does not eliminate faster intra-uterine growth, i.e. fetuses being larger and heavier conditional on the pregnancy week, as another contributing factor. In the near future, we plan to explore both the role of intra-uterine growth and of the underlying causes of prematurity and intra-uterine growth. Specifically, we plan to look at the role of improved maternal nutrition (measured as BMI and nutritional intake), less maternal stress (measured by self-reported mental health and prevalence of depression and anxiety), less smoking or alcohol consumption, physical exercise, self-reported health etc.

7. Conclusion

In this paper, we provided evidence on (unintended) consequences of a universal child benefit on health outcomes of the subsequent children. We showed that mothers who just received the €2,500 lump-sum benefit had subsequently children who were less likely to suffer from low birth weight, as compared to mothers who just did not receive the transfer. This effect is driven by children born in disadvantaged families, such as poor households, unmarried women, and women with little education. It is also driven by children born prematurely, who are at higher risk of low birth weight than children born on-term. When looking at the interaction between prematurity and socio-economic disadvantage, we see that poor prematurely born babies benefit extensively; poor on-term and non-poor prematurely born babies benefit to some extent; while non-poor on-term babies' low birth weight is not affected by the policy. We do not find any effects on other perinatal health outcomes such as still birth, early neonatal death, and normality of birth.

In terms of channels, we find suggestive evidence that additional income provided to disadvantaged families in the prenatal period likely led to fewer low-birth-weight births at least partly through a lower probability of extremely premature births. In the near future, we plan to explore the role of better intra-uterine growth (i.e. faster fetal growth conditional on pregnancy week) and the underlying causes of any potential changes in prematurity or intra-uterine growth, such as maternal nutrition, stress, and behaviors. An important result from the policy perspective is that we find an effect on low birth weight among children born in the next 4-5 years after the benefit receipt, i.e. cash transfers seem to matter not only during in utero period but also prior to that.

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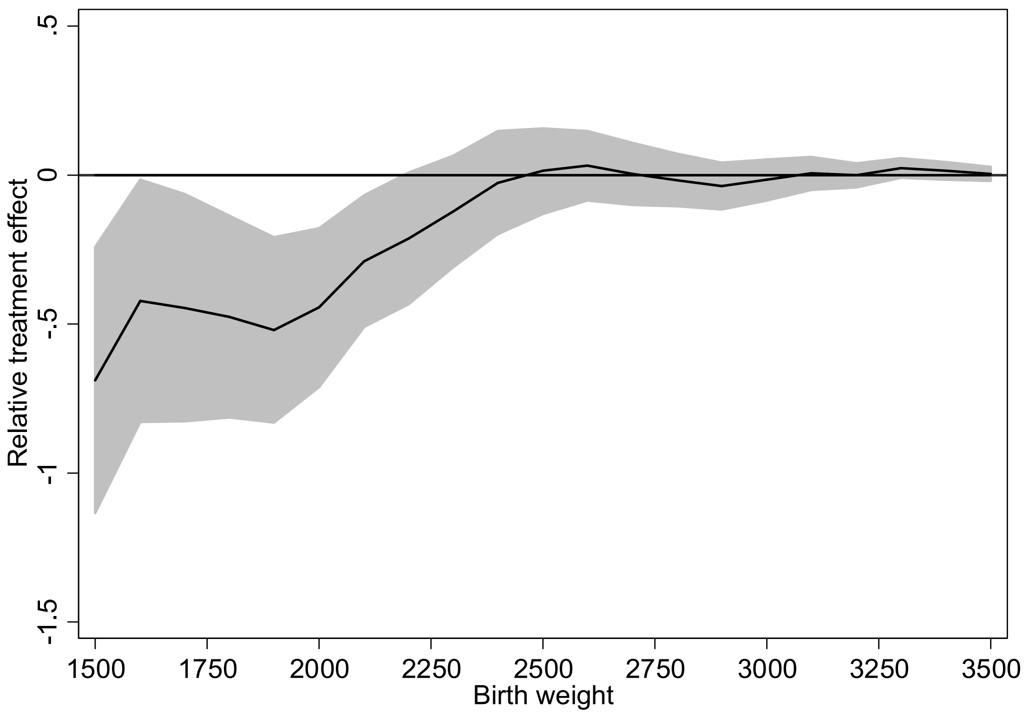
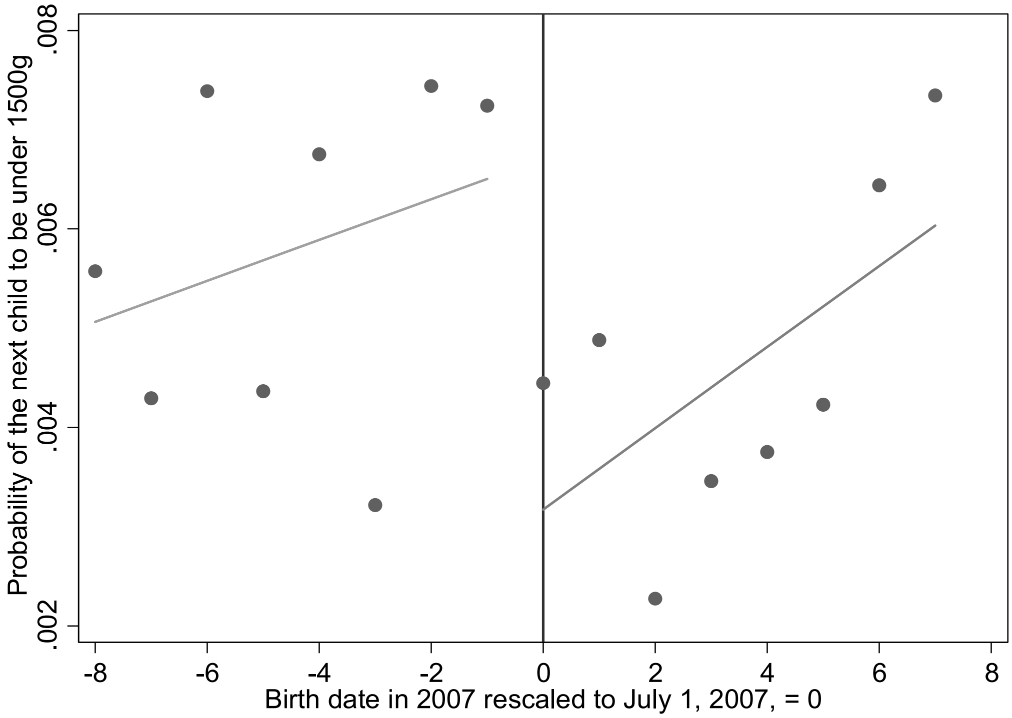
Figures

Figure 1: Treatment effects of universal child benefit on birth weight of future children



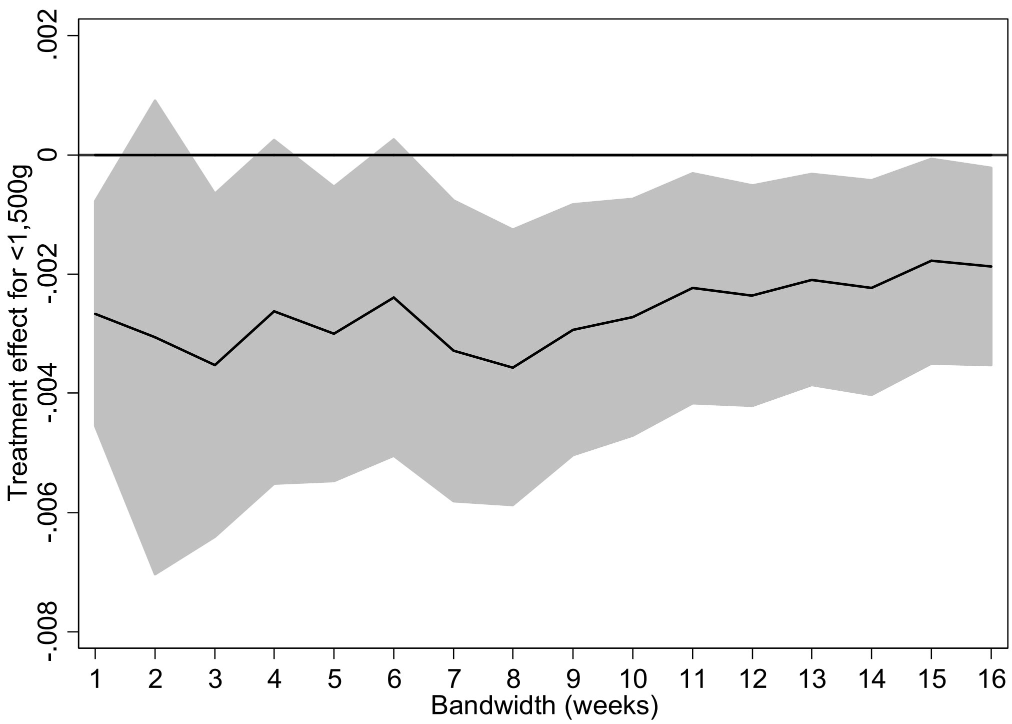
Notes: Treatment effects from 21 regressions with dependent variables defined as birth weight of future children below a certain threshold; thresholds vary between 1,500 and 3,500 grams in 100-gram steps. Displayed are relative magnitudes, i.e. effect sizes divided by the mean of the dependent variable in the sample. Grey areas mark 90% confidence intervals. Bandwidth +/- 8 weeks around the cut-off. Sample size 41,281 observations. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2.

Figure 2: VLBW of future child around the cut-off



Notes: X-axis shows the date of birth of the child born in 2007. Y-axis depicts fraction of future siblings who were born with very low birth weight in the next 5 years. Averages are calculated by week of birth of the previous child born around July 1, 2007. Vertical lines depict linear fits.

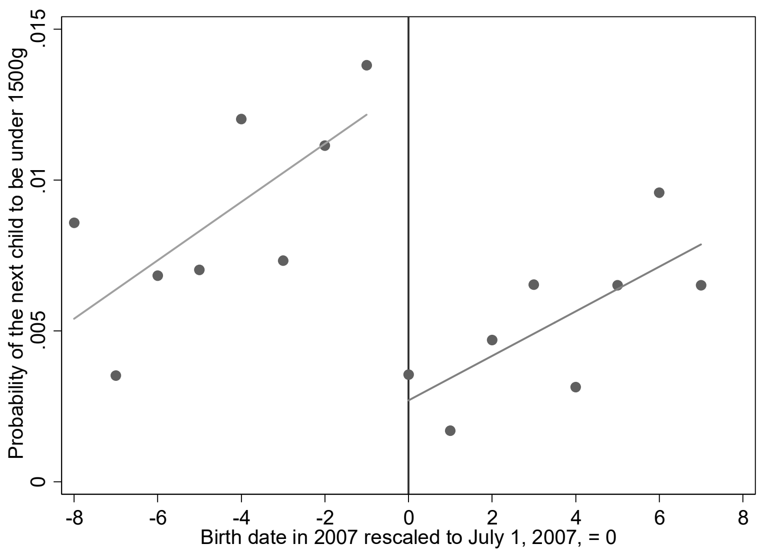
Figure 3: Treatment effects of universal child benefit on VLBW for different bandwidths



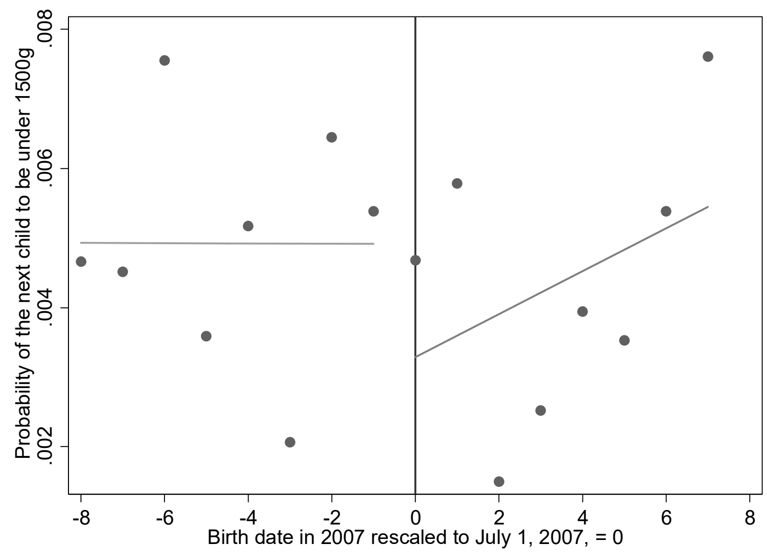
Notes: Treatment effects on probability of a very low birth weight (<1,500 grams) of future children. Grey areas mark 90% confidence intervals. Based on regressions in Table 4.

Figure 4: VLBW of future child by income level

a) Poor households (percentile 1-25)



b) Non-poor households (percentile 26-100)



Notes: X-axis shows date of birth of the child born in 2007. Y-axis depicts fraction of future siblings who were born with birth weight that was very low (<1,500 g) in the next 5 years. Averages are calculated by week of birth of the previous child born around July 1, 2007. Vertical lines depict linear fits.

Tables

Table 1: Treatment effects of universal child benefit on subsequent fertility

Panel A

	Woman had another child in the following X years:					
	1	2	3	4	5	6
Treated	0.0006	0.0030	-0.0007	0.0011	0.0032	0.0056
	(0.0005)	(0.0028)	(0.0043)	(0.0053)	(0.0059)	(0.0056)
Average Y	0.0031	0.0664	0.1560	0.2370	0.2940	0.3310
% Effect	20%	4%	0%	0%	1%	2%

Panel B

	Woman had another child in the following year:					
	1st	2nd	3rd	4th	5th	6th
Treated	0.0006	0.0023	-0.0037	0.0018	0.0021	0.0025
	(0.0005)	(0.0028)	(0.0028)	(0.0029)	(0.0028)	(0.0017)
Average Y	0.0031	0.0633	0.0898	0.0806	0.0574	0.0369
% Effect	20%	4%	-4%	2%	4%	7%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample size 147,913 observations. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates: linear trend (varies before/after the cut-off), day-of-the-week FE, number of births on the day around July 1, 2007.

Table 2: Perinatal health outcomes

	Singleton birth	Still birth	Death within 24 hours	Birth classified as not normal	C-section	Weeks of gestation	Premature birth
Treated	0.0012	0.0012	0.0001	0.0030	0.0036	-0.0189	0.0020
	(0.0023)	(0.0009)	(0.0003)	(0.0058)	(0.0074)	(0.0417)	(0.0041)
Observations	43,512	42,867	42,770	42,867	42,867	34,909	42,867
Average Y	0.9850	0.0023	0.0004	0.1010	0.1820	39.0300	0.0504
% Effect	0%	54%	19%	3%	2%	0%	4%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates: linear trend (varies before/after the cut-off), day-of-the-week FE, region FE (19 regions), day-of-the-week FE of current birth, and parental characteristics: mother's and father's age (cubic function), mother's parity (previously had 1 child, 2 children, 3 or more children), mother is married, father is cohabiting, mother and father are foreign-born (dummies), mother's and father's education (below secondary, secondary, more than secondary).

Table 3: Treatment effects of universal child benefit on birth weight and low birth weight indicators of future children

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Treated	4.5051	0.0021	0.0006	-0.0059**	-0.0055***	-0.0045**	-0.0036**
	(10.7298)	(0.0035)	(0.0040)	(0.0029)	(0.0020)	(0.0018)	(0.0014)
Average Y	3304	3304	0.0460	0.0241	0.0123	0.0081	0.0052
% Effect	0%		1%	-25%	-45%	-56%	-69%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample size 41,281 observations. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2. In case of log(birth weight), the average of actual weight in grams is reported.

Table 4: Robustness of effects on VLBW of future children

	Dependent variable: Birth weight <1,500 g							
Bandwidth (in weeks)	1	2	3	4	5	6	7	8
Treated	-0.0027**	-0.0031	-0.0035*	-0.0026	-0.0030**	-0.0024	-0.0033**	-0.0036**
	(0.0011)	(0.0024)	(0.0017)	(0.0018)	(0.0015)	(0.0016)	(0.0015)	(0.0014)
Observations	5,320	10,538	15,667	20,784	25,969	31,138	36,183	41,281
Average Y	0.0058	0.0060	0.0049	0.0050	0.0048	0.0050	0.0050	0.0052
% Effect	-46%	-51%	-72%	-53%	-63%	-48%	-66%	-69%
Bandwidth (in weeks)	9	10	11	12	13	14	15	16
Treated	-0.0029**	-0.0027**	-0.0022*	-0.0024**	-0.0021*	-0.0022**	-0.0018*	-0.0019*
	(0.0013)	(0.0012)	(0.0012)	(0.0011)	(0.0011)	(0.0011)	(0.0010)	(0.0010)
Observations	46,379	51,493	56,525	61,837	67,063	72,293	77,560	82,701
Average Y	0.0051	0.0050	0.0051	0.0050	0.0050	0.0051	0.0052	0.0052
% Effect	-58%	-54%	-44%	-47%	-42%	-43%	-35%	-36%

Notes: OLS regressions. Dependent variable takes value 1 if birth weight was very low (<1,500 g). The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates: linear trend (varies before/after the cut-off; not included in +/- 1 week sample), day-of-the-week FE, region FE (19 regions), day-of-the-week FE of current birth, and parental characteristics: mother's and father's age (cubic function), mother's parity (previously had 1 child, 2 children, 3 or more children), mother is married, father is cohabiting, mother and father are foreign-born (dummies), mother's and father's education (below secondary, secondary, more than secondary).

Table 5: Placebo regressions

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Year 2006							
"Treated"	2.5374	0.0003	-0.0010	0.0006	0.0004	0.0022	0.0019
	(8.6136)	(0.0029)	(0.0037)	(0.0028)	(0.0020)	(0.0017)	(0.0013)
Average Y	3307	3307	0.0452	0.0237	0.0120	0.0080	0.0049
% Effect	0%		-2%	2%	3%	27%	39%
Year 2008							
"Treated"	-7.4300	-0.0025	-0.0012	0.0022	0.0023	0.0025*	0.0021*
	(7.6394)	(0.0025)	(0.0031)	(0.0024)	(0.0017)	(0.0014)	(0.0011)
Average Y	3313	3313	0.0446	0.0229	0.0114	0.0071	0.0050
% Effect	0%		-3%	10%	21%	36%	43%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2006 or 2008 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 41,303 in 2006 and 41,289 in 2008 sample. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2. In case of log(birth weight), the average of actual weight in grams is reported.

Table 6: Heterogeneity analysis by income level

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Poor HHs (Percentile 1-25)							
Treated	16.3399	0.0095	-0.0124	-0.0163*	-0.0120**	-0.0102**	-0.0103***
	(24.9214)	(0.0084)	(0.0103)	(0.0083)	(0.0049)	(0.0041)	(0.0031)
Average Y	3304	3304	0.0608	0.0338	0.0165	0.0108	0.0069
% Effect	0%		-21%	-48%	-73%	-94%	-151%
Non-poor HHs (Percentile 26-100)							
Treated	1.3891	0.0002	0.0041	-0.0032	-0.0038*	-0.0030	-0.0017
	(10.7237)	(0.0035)	(0.0042)	(0.0032)	(0.0022)	(0.0019)	(0.0016)
Average Y	3304	3304	0.0417	0.0213	0.0111	0.0073	0.0047
% Effect	0%		10%	-15%	-34%	-41%	-37%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 9,326 in poor and 31,955 in non-poor sample. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2. In case of log(birth weight), the average of actual weight in grams is reported.

Table 7: Heterogeneity analysis by prematurity of birth

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Premature							
Treated	100.6083*	0.0521*	-0.0581	-0.1022**	-0.1240***	-0.0906**	-0.0692***
	(56.9640)	(0.0280)	(0.0400)	(0.0418)	(0.0338)	(0.0348)	(0.0264)
Average Y	2434	2434	0.5430	0.3430	0.2000	0.1370	0.0899
% Effect	4%		-11%	-30%	-62%	-66%	-77%
On term							
Treated	1.0850	0.0001	0.0026	-0.0018	0.0000	-0.0001	-0.0002
	(10.5368)	(0.0032)	(0.0023)	(0.0015)	(0.0008)	(0.0006)	(0.0004)
Average Y	3351	3351	0.0195	0.0071	0.0023	0.0012	0.0007
% Effect	0%		13%	-25%	2%	-5%	-25%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 2,091 in premature and 39,190 in on-term sample. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2. In case of log(birth weight), the average of actual weight in grams is reported.

Table 8: Heterogeneity analysis by household income and prematurity of birth

Panel A

Poor HHs (Percentile 1-25)	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is: <2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Premature							
Treated	260.5762*	0.1596***	-0.2456***	-0.1962**	-0.2309***	-0.2018***	-0.1985***
	(132.0401)	(0.0601)	(0.0889)	(0.0929)	(0.0733)	(0.0652)	(0.0482)
Average Y	2380	2380	0.6050	0.4000	0.2260	0.1450	0.0952
% Effect	11%		-41%	-49%	-102%	-140%	-208%
On term							
Treated	15.6260	0.0062	-0.0063	-0.0112***	-0.0034*	-0.0018	-0.0010
	(21.3730)	(0.0064)	(0.0061)	(0.0039)	(0.0017)	(0.0015)	(0.0011)
Average Y	3367	3367	0.0241	0.0092	0.0024	0.0018	0.0009
% Effect	0%		-26%	-122%	-142%	-96%	-113%

Panel B

Non-poor HHs (Percentile 26-100)	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is: <2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Premature							
Treated	55.7611	0.0226	-0.0046	-0.0826	-0.1025***	-0.0651*	-0.0346
	(62.6460)	(0.0319)	(0.0456)	(0.0516)	(0.0377)	(0.0379)	(0.0296)
Average Y	2454	2454	0.5190	0.3210	0.1900	0.1340	0.0878
% Effect	2%		-1%	-26%	-54%	-49%	-39%
On term							
Treated	-2.6752	-0.0015	0.0048*	0.0007	0.0009	0.0004	0.0000
	(11.0155)	(0.0034)	(0.0028)	(0.0016)	(0.0009)	(0.0006)	(0.0005)
Average Y	3346	3346	0.0181	0.0065	0.0023	0.0010	0.0006
% Effect	0%		27%	11%	41%	36%	7%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 588 in premature and 8,738 in on-term sample among the poor households (panel A), and 1,503 in premature and 30,452 in on-term sample among the non-poor households (panel B). Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2; the heterogeneity variable is always omitted. In case of log(birth weight), the average of actual weight in grams is reported.

Table 9: Treatment effects of universal child benefit on birth weight and low birth weight indicators of future children in poor households, by timing of next birth

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is: <2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
1st year							
Treated	76.2735	0.0342	0.0461	-0.0304	-0.1160	-0.1073*	-0.0531
	(192.2121)	(0.0832)	(0.1090)	(0.0949)	(0.0770)	(0.0593)	(0.0469)
Average Y	2,993	2,993	0.2040	0.1380	0.0918	0.0561	0.0408
% Effect	3%		23%	-22%	-126%	-191%	-130%
2nd year							
Treated	60.6551	0.0280*	-0.0571***	-0.0387**	-0.0306***	-0.0208**	-0.0157**
	-41.9171	-0.0146	-0.0174	-0.0151	-0.0105	-0.0096	-0.007
Average Y	3,280	3,280	0.0695	0.0379	0.0182	0.0130	0.0075
% Effect	2%		-82%	-102%	-169%	-160%	-209%
3rd year							
Treated	-17.8347	-0.0092	0.0085	-0.0012	0.0071	0.0116	0.0040
	-39.8963	-0.0137	-0.0175	-0.0134	-0.0087	-0.0071	-0.0049
Average Y	3,332	3,332	0.0509	0.0294	0.0139	0.0072	0.0038
% Effect	-1%		17%	-4%	51%	162%	107%
4th year							
Treated	-37.2762	-0.0054	-0.0007	-0.0052	-0.0092	-0.0144**	-0.0144**
	-55.039	-0.0185	-0.0204	-0.0142	-0.0084	-0.007	-0.0058
Average Y	3,318	3,318	0.0515	0.0280	0.0110	0.0078	0.0055
% Effect	-1%		-1%	-18%	-84%	-184%	-262%
5th year							
Treated	77.594	0.0316*	-0.0103	-0.0213	-0.0117	-0.0189*	-0.0169**
	-48.3039	-0.016	-0.0189	-0.0145	-0.0114	-0.0102	-0.0082
Average Y	3,317	3,317	0.0589	0.0300	0.0164	0.0119	0.0085
% Effect	2%		-18%	-71%	-72%	-159%	-199%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 196; 2,533; 2,654; 2,176; 1,767 observations for births that took place in the 1st–5th year after the birth in 2007, respectively. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2. In case of log(birth weight), the average of actual weight in grams is reported.

Annex

A1. Subsequent fertility data set

In the following, we provide the details on how the subsequent fertility data set is created. First, we give an intuitive summary, which is then followed by more details and a critical reflection about the data construction procedure.

Intuitive summary

We construct the individual-level data set from aggregate data in three steps:

1. Number of children ("births in 2007"):

   We calculate daily number of births that took place on each calendar day in 2007. For instance, there were 1,413 children born in Spain on July 5, 2007.
2. Number of future siblings ("future births in 2007-2013"):

   We focus on children who were born in 2007-2013, whose previous sibling was born in 2007, and where the time between the two births was less than 5 years. We count number of such children by day of birth of their previous sibling in 2007. For instance, there were 415 children born in 2007-2013 whose previous sibling was born on July 5, 2007, and where the birth spacing was less than 5 years.
3. Individual-level data set:

   We match daily number of births on each calendar day in 2007 ("births in 2007") with number of future siblings born in 2007-2013 ("future births in 2007-2013") and we expand the data set. For instance, we create 1,413 observations with birth date on July 5, 2007; 415 of them are coded as having a future sibling whereas the remaining 998 are coded as not having a future sibling within the next 5 years.

In addition, we need to restrict which births from the birth certificate registry are included in our sample, based on the eligibility criteria for baby bonus, which were:

1. Birth took place in Spain
2. Mother was Spanish or had resided in Spain in the preceding 2 years
3. The newborn lived at least 24 hours

Given the information that is available in the birth registry data, we exclude those births that were clearly not eligible for the baby bonus. "Births in 2007" include live births that occurred in Spain in

2007, to mothers with residence in any of the 52 Spanish provinces. On the other hand, "births in 2007-2013" include all births that occurred in Spain in 2007-2013, to mothers with residence in any of the 52 Spanish provinces, where the previous sibling was born alive in Spain or in an unknown location in 2007.

Details and a critical reflection

In the following, we provide further details on the exact way in which the sample is defined, and we discuss any imperfections in the sample definition and their consequences.

<u>Births in 2007:</u>

We want to include all children eligible for the baby bonus in 2007.

1. A priori, we keep all births because the birth certificate registry encompasses all births that took place in Spain.
2. We include only mothers who resided in Spain (in any of the 52 provinces) in 2007. We have no information about the length of foreign mothers' residency in Spain. 19% of mothers in 2007 were foreign nationals. Note that by including all mothers residing in Spain, we are implicitly assuming that all foreign mothers who were residents in Spain in 2007 were eligible for the benefit, i.e. that they had resided in Spain for at least 2 years.
3. We consider only live births. We decided not to exclude live births which resulted in a death within the first 24 hours (early neonatal death) from the sample in order to be consistent with the count of "future births in 2007-2013", see below. 0.05% of live births resulted in an early neonatal death in 2007.

➔ We include live births that occurred in Spain in 2007, to mothers with residence in any of the 52 Spanish provinces.

<u>Future births in 2007-2013:</u>

We want to include all children <u>whose previous sibling</u> was eligible for the baby bonus in 2007. We consider only those births where the entire date of previous birth is reported; it is missing for 2% of observations in 2007-2013.

1. Data from the birth certificate registry include only births that took place in Spain; the previous birth could have taken place in Spain or elsewhere. We aim at including all children whose previous sibling was born in Spain. Thus, we exclude children whose previous sibling was born

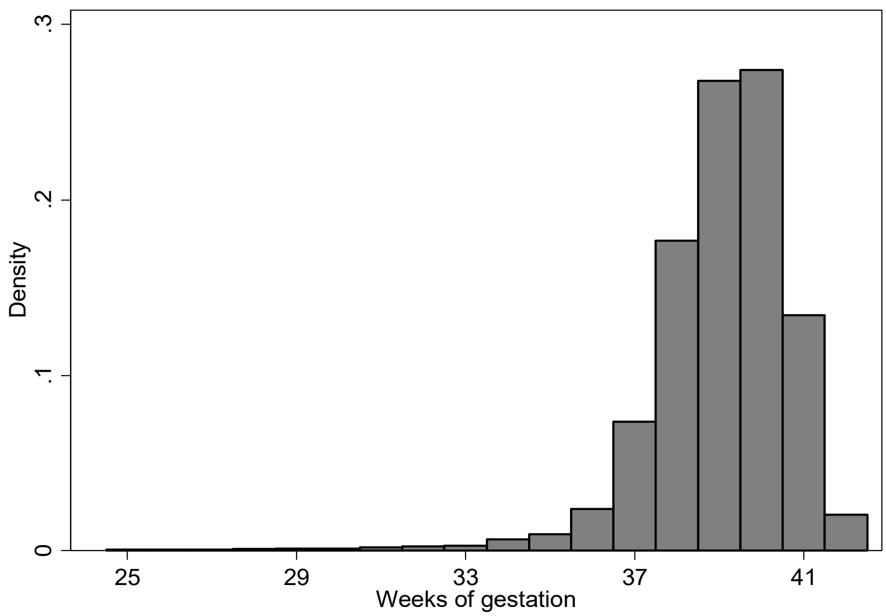
abroad (6%), and we keep children where the previous birth took place in Spain (75%) or where the location of previous birth is missing (19%). Note that there might be a (negligible) number of women who gave birth in Spain in 2007 and then gave birth abroad in 2007-2013; these "future siblings" are not included in the birth registry and are therefore missing in our sample.

2. We include only mothers who reside in Spain (in any of the 52 provinces). Note that by including all mothers residing in Spain, we are implicitly assuming that all foreign mothers residing in Spain in 2007-2013 were eligible for the benefit when they gave birth to their previous child in 2007 (i.e. that they had been residing in Spain in the 2 years preceding their previous childbirth in 2007). 20% of mothers in 2007-2013 were foreign nationals. We are also implicitly assuming that there was no migration of mothers to and from Spain, i.e. each mother that was a resident in Spain when giving birth in 2007-2013 had been a resident in Spain when she gave birth to her previous child in 2007, and vice versa.
3. The question about the date of previous birth is asked only for the most recent <u>live</u> birth. Thus, we automatically include only children whose previous sibling was born <u>alive</u> in 2007. Ideally, we would like to exclude both previous still births and children whose previous sibling died in the first 24 hours. However, information on early neonatal survival of the previous sibling is not available. Since we cannot exclude children born in 2007-2013 whose previous sibling was affected by an early neonatal death, we decided not to exclude children who died in the first 24 hours also in "births in 2007", see above. That way we keep the "numerator" (births in 2007) and "denominator" (births in 2007-2013) as consistent as possible.

➔ We include all births that occurred in Spain in 2007-2013, to mothers with residence in any of the 52 Spanish provinces, where the previous sibling was born alive in Spain or in an unknown location in 2007.

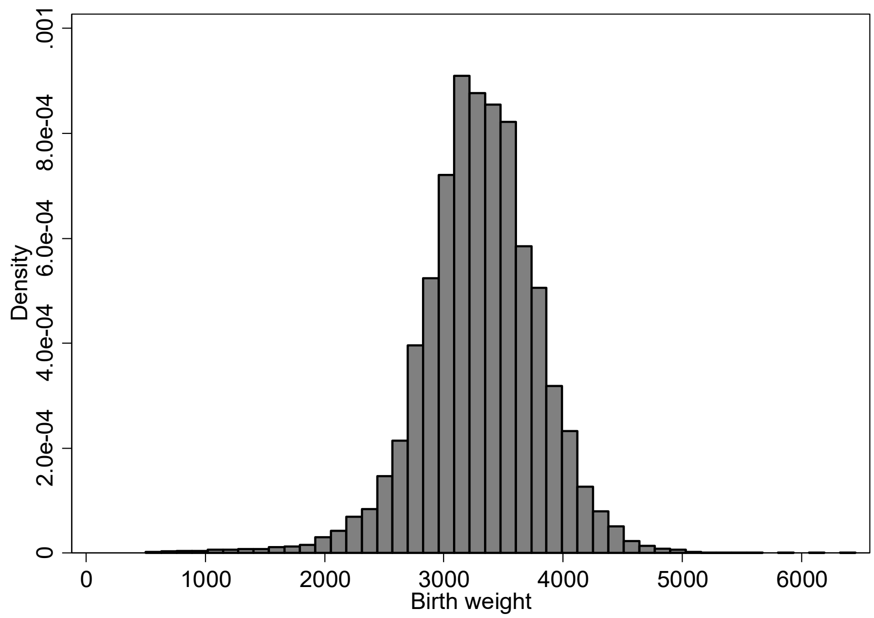
A2. Figures

Figure A1: Histogram of pregnancy weeks at birth



Notes: Bandwidth +/- 8 weeks around the cut-off. Sample size 34,909 observations (only singleton births). Less than 25 weeks (0.04%) are pooled with 25 weeks, and more than 42 weeks (0.05%) are pooled with 42 weeks.

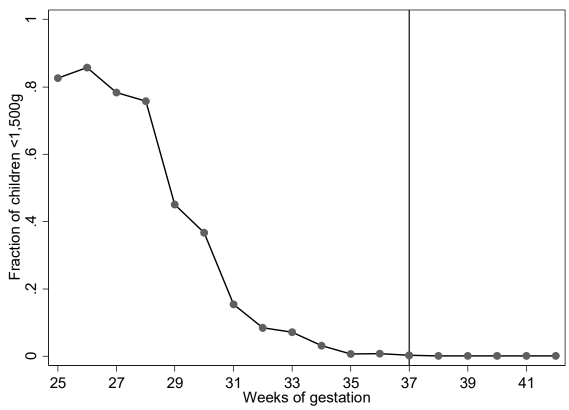
Figure A2: Histogram of birth weight



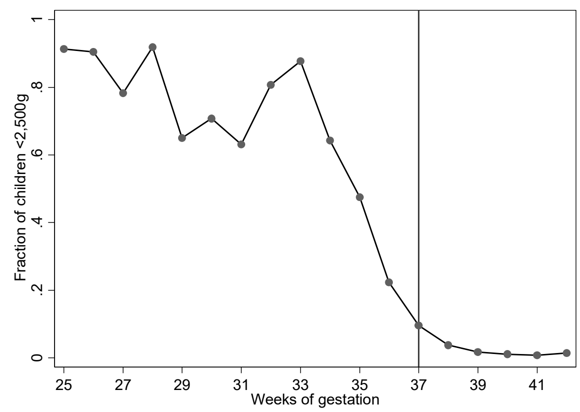
Notes: Bandwidth +/- 8 weeks around the cut-off. Sample size 41,281 observations (only singleton births).

Figure A3: Fraction of children with VLBW and LBW by weeks of pregnancy

a) VLBW (<1,500 g)



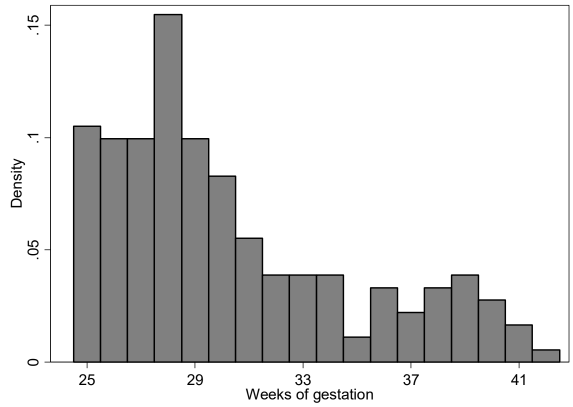
b) LBW (<2,500 g)



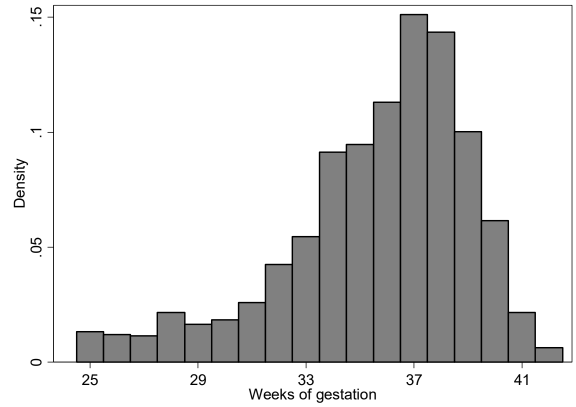
Notes: Bandwidth +/- 8 weeks around the cut-off. Sample size 34,909 observations (only singleton births). Less than 25 weeks (0.04%) are pooled with 25 weeks, and more than 42 weeks (0.05%) are pooled with 42 weeks.

Figure A4: Histogram of pregnancy weeks at birth for VLBW and LBW children

c) VLBW (<1,500 g)



d) LBW (<2,500 g)



Notes: Bandwidth +/- 8 weeks around the cut-off. Sample size 34,909 observations (only singleton births). Less than 25 weeks (0.04%) are pooled with 25 weeks, and more than 42 weeks (0.05%) are pooled with 42 weeks.

A3. Tables

Table A1: Robustness of effects of universal child benefit on subsequent fertility

Treatment effects in samples with following bandwidths (in weeks):	Woman had another child in the following X years:					
	1	2	3	4	5	6
1 week (N=18,753)	0.0010*	0.0034	-0.0015	0.0018	0.0069	0.0134
	(0.0004)	(0.0042)	(0.0057)	(0.0057)	(0.0092)	(0.0086)
2 weeks (N=37,532)	0.0006	-0.0007	-0.0034	-0.0061	-0.0025	0.0030
	(0.0008)	(0.0067)	(0.0088)	(0.0105)	(0.0124)	(0.0120)
3 weeks (N=55,947)	-0.0000	0.0015	-0.0053	-0.0081	-0.0036	0.0004
	(0.0008)	(0.0052)	(0.0068)	(0.0082)	(0.0098)	(0.0097)
4 weeks (N=74,492)	0.0002	0.0009	-0.0056	-0.0028	0.0019	0.0063
	(0.0006)	(0.0045)	(0.0059)	(0.0077)	(0.0086)	(0.0087)
5 weeks (N=93,000)	0.0008	0.0013	-0.0024	-0.0002	0.0046	0.0090
	(0.0006)	(0.0038)	(0.0053)	(0.0064)	(0.0073)	(0.0070)
6 weeks (N=111,241)	0.0011*	0.0020	-0.0019	0.0013	0.0034	0.0076
	(0.0006)	(0.0035)	(0.0050)	(0.0062)	(0.0070)	(0.0067)
7 weeks (N=129,598)	0.0010*	0.0027	-0.0016	0.0011	0.0031	0.0063
	(0.0005)	(0.0031)	(0.0047)	(0.0057)	(0.0065)	(0.0062)
8 weeks (N=147,913)	0.0006	0.0030	-0.0007	0.0011	0.0032	0.0056
	(0.0005)	(0.0028)	(0.0043)	(0.0053)	(0.0059)	(0.0056)
9 weeks (N=166,165)	0.0007	0.0035	0.0021	0.0039	0.0065	0.0091*
	(0.0005)	(0.0027)	(0.0042)	(0.0051)	(0.0057)	(0.0054)
10 weeks (N=184,854)	0.0003	0.0016	-0.0001	0.0029	0.0056	0.0083*
	(0.0005)	(0.0025)	(0.0038)	(0.0047)	(0.0051)	(0.0050)
11 weeks (N=203,745)	0.0003	0.0005	-0.0005	0.0025	0.0038	0.0067
	(0.0004)	(0.0024)	(0.0036)	(0.0044)	(0.0049)	(0.0048)
12 weeks (N=222,787)	0.0005	0.0001	-0.0013	0.0011	0.0025	0.0052
	(0.0004)	(0.0023)	(0.0034)	(0.0042)	(0.0046)	(0.0045)
13 weeks (N=241,576)	0.0004	-0.0005	-0.0004	0.0014	0.0033	0.0060
	(0.0004)	(0.0021)	(0.0032)	(0.0039)	(0.0043)	(0.0043)
14 weeks (N=260,739)	0.0004	-0.0012	-0.0007	0.0021	0.0044	0.0066
	(0.0004)	(0.0021)	(0.0031)	(0.0038)	(0.0041)	(0.0041)
15 weeks (N=279,379)	0.0004	-0.0017	-0.0015	0.0017	0.0041	0.0055
	(0.0004)	(0.0020)	(0.0030)	(0.0036)	(0.0040)	(0.0039)
16 weeks (N=297,837)	0.0004	-0.0013	-0.0011	0.0021	0.0046	0.0056
	(0.0004)	(0.0019)	(0.0029)	(0.0035)	(0.0039)	(0.0039)

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates: linear trend (varies before/after the cut-off; not included in +/- 1 week sample), day-of-the-week FE, number of births on the day around July 1, 2007.

Table A2: Robustness of effects of universal child benefit on birth weight and low birth weight indicators of future children

Treatment effects in samples with following bandwidths (in weeks):	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
1 week (N=5,320)	10.9871	0.0034	-0.0013	-0.0079*	-0.0029	-0.0026	-0.0027**
	(12.7684)	(0.0039)	(0.0054)	(0.0038)	(0.0020)	(0.0017)	(0.0011)
2 weeks (N=10,538)	1.0339	0.0002	0.0007	-0.0083	-0.0017	-0.0028	-0.0031
	(23.1171)	(0.0071)	(0.0088)	(0.0062)	(0.0035)	(0.0029)	(0.0024)
3 weeks (N=15,667)	24.5281	0.0072	-0.0059	-0.0105**	-0.0036	-0.0029	-0.0035*
	(19.6083)	(0.0060)	(0.0072)	(0.0050)	(0.0030)	(0.0023)	(0.0017)
4 weeks (N=20,784)	23.7777	0.0077	-0.0057	-0.0116***	-0.0050*	-0.0023	-0.0026
	(15.5736)	(0.0049)	(0.0057)	(0.0042)	(0.0027)	(0.0022)	(0.0018)
5 weeks (N=25,969)	19.1151	0.0059	-0.0023	-0.0082**	-0.0038	-0.0028	-0.0030**
	(13.9219)	(0.0044)	(0.0049)	(0.0037)	(0.0023)	(0.0020)	(0.0015)
6 weeks (N=31,138)	9.8933	0.0029	0.0017	-0.0057	-0.0040*	-0.0025	-0.0024
	(13.0008)	(0.0042)	(0.0049)	(0.0034)	(0.0022)	(0.0021)	(0.0016)
7 weeks (N=36,183)	7.7286	0.0029	0.0001	-0.0061*	-0.0048**	-0.0036*	-0.0033**
	(11.7623)	(0.0038)	(0.0043)	(0.0031)	(0.0021)	(0.0019)	(0.0015)
8 weeks (N=41,281)	4.5051	0.0021	0.0006	-0.0059**	-0.0055***	-0.0045**	-0.0036**
	(10.7298)	(0.0035)	(0.0040)	(0.0029)	(0.0020)	(0.0018)	(0.0014)
9 weeks (N=46,379)	5.3159	0.0024	0.0003	-0.0056**	-0.0054***	-0.0039**	-0.0029**
	(10.0879)	(0.0033)	(0.0038)	(0.0028)	(0.0018)	(0.0016)	(0.0013)
10 weeks (N=51,493)	-0.7923	0.0005	0.0017	-0.0037	-0.0048***	-0.0035**	-0.0027**
	(9.7845)	(0.0032)	(0.0037)	(0.0026)	(0.0017)	(0.0015)	(0.0012)
11 weeks (N=56,525)	-0.2563	0.0003	0.0025	-0.0040	-0.0044***	-0.0032**	-0.0022*
	(9.3677)	(0.0031)	(0.0036)	(0.0026)	(0.0016)	(0.0015)	(0.0012)
12 weeks (N=61,837)	2.2412	0.0010	0.0030	-0.0039	-0.0039**	-0.0034**	-0.0024**
	(8.7401)	(0.0029)	(0.0034)	(0.0025)	(0.0016)	(0.0014)	(0.0011)
13 weeks (N=67,063)	3.7010	0.0014	0.0037	-0.0023	-0.0030*	-0.0030**	-0.0021*
	(8.2535)	(0.0027)	(0.0033)	(0.0024)	(0.0015)	(0.0013)	(0.0011)
14 weeks (N=72,293)	5.7265	0.0020	0.0031	-0.0030	-0.0033**	-0.0028**	-0.0022**
	(8.1274)	(0.0027)	(0.0032)	(0.0023)	(0.0015)	(0.0013)	(0.0011)
15 weeks (N=77,560)	2.3082	0.0008	0.0037	-0.0023	-0.0023	-0.0024*	-0.0018*
	(8.0827)	(0.0027)	(0.0031)	(0.0022)	(0.0015)	(0.0013)	(0.0010)
16 weeks (N=82,701)	2.9073	0.0011	0.0034	-0.0024	-0.0025*	-0.0026**	-0.0019*
	(7.6542)	(0.0025)	(0.0030)	(0.0021)	(0.0014)	(0.0013)	(0.0010)

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 4.

Table A3: Heterogeneity analysis by marital status and maternal education

Panel A

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is: <2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Low education							
Treated	-2.5510	0.0023	-0.0102	-0.0080	-0.0085**	-0.0080***	-0.0076***
	(16.3437)	(0.0054)	(0.0065)	(0.0050)	(0.0033)	(0.0030)	(0.0023)
Average Y	3292	3292	0.0572	0.0322	0.0163	0.0107	0.0070
% Effect	0%		-18%	-25%	-52%	-74%	-109%
High education							
Treated	9.3254	0.0023	0.0063	-0.0051	-0.0040*	-0.0027	-0.0014
	(11.1631)	(0.0036)	(0.0049)	(0.0031)	(0.0023)	(0.0019)	(0.0017)
Average Y	3311	3311	0.0396	0.0195	0.0100	0.0066	0.0042
% Effect	0%		16%	-26%	-40%	-41%	-33%

Panel B

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is: <2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
Unmarried							
Treated	31.5753	0.0140**	-0.0210**	-0.0183**	-0.0131***	-0.0124***	-0.0084***
	(19.1817)	(0.0068)	(0.0095)	(0.0087)	(0.0046)	(0.0036)	(0.0028)
Average Y	3272	3272	0.0595	0.0328	0.0158	0.0100	0.0058
% Effect	1%		-35%	-56%	-82%	-124%	-145%
Married							
Treated	-4.7921	-0.0019	0.0077*	-0.0018	-0.0031	-0.0021	-0.0021
	(11.5941)	(0.0036)	(0.0040)	(0.0026)	(0.0022)	(0.0019)	(0.0017)
Average Y	3315	3315	0.0417	0.0213	0.0112	0.0075	0.0050
% Effect	0%		18%	-9%	-28%	-28%	-42%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 15,008 in low-educated and 26,273 in high-educated sample, and 10,034 in unmarried and 31,247 in married sample. Low-educated women are those with less than secondary education. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2; the heterogeneity variable is always omitted. In case of log(birth weight), the average of actual weight in grams is reported.

Table A4: Treatment effects of universal child benefit on birth weight and low birth weight indicators of future children, by timing of next birth

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2,500 g	<2,250 g	<2,000 g	<1,750 g	<1,500 g
1st year							
Treated	34.1424	0.0250	0.0396	0.0050	-0.0566	-0.0625**	-0.0443*
	(122.0534)	(0.0494)	(0.0640)	(0.0587)	(0.0434)	(0.0287)	(0.0253)
Average Y	3,075	3,075	0.1540	0.1060	0.0554	0.0361	0.0289
% Effect	1%		26%	5%	-102%	-173%	-153%
2nd year							
Treated	14.812	0.0076	-0.0135*	-0.0105	-0.0106**	-0.0086**	-0.0058*
	-23.953	-0.0084	-0.0078	-0.0066	-0.0047	-0.0043	-0.0032
Average Y	3,289	3,289	0.0495	0.0264	0.0144	0.0102	0.0060
% Effect	0%		-27%	-40%	-74%	-85%	-98%
3rd year							
Treated	0.5085	-0.0006	-0.0014	-0.0047	-0.0011	-0.0004	0.0006
	-15.2821	-0.0051	-0.0074	-0.0044	-0.0037	-0.0029	-0.0023
Average Y	3,313	3,313	0.0408	0.0215	0.0119	0.0075	0.0046
% Effect	0%		-3%	-22%	-10%	-5%	13%
4th year							
Treated	18.7376	0.0066	0.0043	-0.0097**	-0.0083***	-0.0065**	-0.0058***
	-17.7531	-0.0059	-0.0088	-0.0048	-0.0029	-0.0026	-0.0021
Average Y	3,311	3,311	0.0452	0.0222	0.0100	0.0062	0.0041
% Effect	1%		9%	-44%	-83%	-106%	-144%
5th year							
Treated	-19.8644	-0.0065	0.0129*	0.0016	-0.0002	-0.0012	-0.0024
	-22.922	-0.0074	-0.0078	-0.0062	-0.0046	-0.0038	-0.0032
Average Y	3,310	3,310	0.0459	0.0244	0.0118	0.0080	0.0056
% Effect	-1%		28%	6%	-2%	-15%	-44%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 415; 8,866; 12,632; 11,328; and 8,040 observations for births that took place in the 1st–5th year after the birth in 2007, respectively. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2. In case of log(birth weight), the average of actual weight in grams is reported.

Table A5: Treatment effects on different measures of prematurity of birth of the next child

Panel A

	Dummy variable = 1 if birth at less than X weeks:											
	37	36	35	34	33	32	31	30	29	28	27	26
Full sample												
Treated	0.0037	0.0025	-0.0003	-0.0011	-0.0010	-0.0013	-0.0017	-0.0013	-0.0009	-0.0007	0.0005	0.0004
	(0.0051)	(0.0036)	(0.0029)	(0.0022)	(0.0018)	(0.0016)	(0.0014)	(0.0013)	(0.0011)	(0.0009)	(0.0008)	(0.0006)
Average Y	0.0527	0.0288	0.0195	0.0129	0.0100	0.0075	0.0056	0.0044	0.0032	0.0021	0.0014	0.0007
% Effect	7%	9%	-1%	-9%	-10%	-17%	-30%	-31%	-27%	-32%	35%	55%

Panel B

	Dummy variable = 1 if birth at less than X weeks:											
	37	36	35	34	33	32	31	30	29	28	27	26
Poor sample												
Treated	0.0242*	0.0073	0.0006	-0.0047	-0.0030	-0.0039	-0.0045	-0.0053*	-0.0064**	-0.0047**	-0.0025	-0.0015
	(0.0132)	(0.0097)	(0.0085)	(0.0064)	(0.0054)	(0.0048)	(0.0036)	(0.0031)	(0.0027)	(0.0020)	(0.0016)	(0.0011)
Average Y	0.0692	0.0412	0.0298	0.0184	0.0144	0.0100	0.0075	0.0061	0.0047	0.0031	0.0018	0.0013
% Effect	35%	18%	2%	-25%	-21%	-39%	-61%	-87%	-136%	-155%	-141%	-117%

Notes: OLS regressions. The running variable is day of previous birth which took place around July 1, 2007 (cut-off). Bandwidth +/- 8 weeks around the cut-off. Sample sizes are 34,909 in the full sample and 7,213 in the poor sample. Standard errors are clustered at values of the running variable, i.e. day of previous birth. Covariates as in Table 2.