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Cash transfers before pregnancy and infant health ☆

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ABSTRACT

We estimate the impact of a cash transfer targeting new mothers on their subsequent children's health outcomes at birth. We exploit the unexpected introduction of a generous, 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 women who received the benefit were much less likely to have low-birth-weight children in the future (while their subsequent fertility was unaffected). The overall effect is driven by poor women, unmarried women, and women with low education, and by births taking place relatively soon after the benefit receipt. The €2500 transfer led to a 0.7 percentage-point decline in the fraction of children born under 1500 g in poorer households in the following five years, an 83% reduction. We explore the underlying channels, and find evidence supporting faster intrauterine growth, possibly driven by better maternal health, nutrition, and behaviors. Gestation length, family structure, and parental employment do not seem to play a role. Recent research suggests that targeting pregnant women may be more effective than later interventions (such as cash transfers to families with children), 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.

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 prenatal and early postnatal shocks and interventions can have substantial effects on long-term human capital formation, including outcomes such as adult health and earnings (Currie, 2009; Almond and Currie, 2011; Currie and Almond, 2011; Almond et al., 2018). Combined with accumulating evidence suggesting that the early years are crucial for human capital production (Cunha and Heckman, 2008; Cunha et al., 2010), a strong case is building for policies that target (vulnerable) children early.

Several recent papers have examined the effects of 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, 2016; Amarante et al., 2016). We contribute to this literature by examining the effects of a universal cash transfer received *before conception* on children's health outcomes at birth, using data for Spain.

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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 the health outcomes of children born up to five years after the one-time payment was received. 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 €2500 to all women giving birth. The transfer was very generous, as it amounted to 1.5 times the average and 2.5 times the 25th percentile of gross monthly earnings. 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 the universal child benefit. We do not study the immediate effects on the child for whom the payment was made. Instead, we look up to five years later to see if subsequently born children (the younger siblings of those born around the benefit introduction) show any health impact of the cash payment received previously by the mother.

Previous literature has studied the effects of cash transfers received by poor households in utero or after birth on children's health outcomes at birth and later in life (see Section 2 for a more detailed literature review). Several papers find significant impacts of benefits paid during pregnancy. 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 profited from a benefit expansion while pregnant. Almond et al. (2011) and Hoynes et al. (2016) find (respectively) that the rollout of a near-cash benefit in the U.S. (Food Stamps) increased birth weight, especially among African Americans, and that in the long term the rollout reduced the incidence of metabolic syndrome (i.e. obesity, high blood pressure, diabetes, etc.). Amarante et al. (2016) find that cash social assistance aimed at the poor 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 to new mothers in Spain in the late 2000s, and evaluate its effects on the health outcomes at birth of children conceived after the benefit receipt (i.e. the *younger siblings* of the children born close to the benefit introduction). Recent work by Borra et al. (2021) documents that this benefit had no detectable effects on health nor on cognitive development (in the short or medium term) for children who were born *before* the benefit receipt (i.e. children born right around the benefit introduction).

We use publicly available birth-certificate microdata encompassing the universe of all births in Spain. We examine the extent to which children born in socio-economically disadvantaged households benefitted from the cash transfer differentially. We also assess the timing of the effects and the underlying channels.

We find that women who received the benefit were much less likely to have low-birth-weight children in the future, while their propensity to have another child (or the timing of the following birth) did not change. The overall effect is driven by disadvantaged households (poor women, unmarried women, and women with low education). The €2500 transfer led to a 0.9 and 0.7 percentage-point (pp) decline in the fraction of children born under 2000 and 1500 g in poorer households in the following five years, which represents a 49% and 83% reduction, respectively.

The effect on birth weight is driven by premature babies, but we find no effect on the fraction of children born prematurely. Additionally, we find an important interaction between socioeconomic disadvantage and prematurity: The biggest winners are preterm babies in poorer families, for whom birth weight increased by 228 g on average (a 10% increase) and the fraction of children born with less than 1500 g decreased by 12.9 pp (101% effect size). We find no effects among babies born on term in non-poor households.

With respect to timing, the observed effects are driven by births taking place relatively soon after the benefit receipt (births within the following 2.5 years after the policy introduction). In terms of the distribution of birth weight, the decrease in the fraction of children born under 1500 g was accompanied by an increase in the fraction of babies born between 2000 and 2500 g (or in higher bins). We find no effects on other perinatal health outcomes such as normality of birth, C-section birth, or early neonatal death.

We explore the channels underlying our results. The immediate causes of the observed effects on birth weight could be longer gestation and/or faster intrauterine growth. We find evidence only for the latter. The underlying channels that could contribute to faster intrauterine growth include better maternal health, which in turn is likely impacted by maternal nutrition, maternal habits and behaviors, and possibly changes in family structure (such as divorce) and/or in parental employment.

Using National Health Survey data, we find evidence consistent with the benefit improving mothers' overall health, together with their nutrition and patterns of healthy and unhealthy habits. An aggregate index of health-related factors improved by approximately 0.7 of a standard deviation for women who received the cash transfer. With Labor Force Survey data, we find no effects on cohabitation, nor on employment in the affected groups (unmarried, low-educated women).

We contribute to the existing literature on the effect of cash transfers to families on child outcomes. We show that a cash subsidy paid to (poor) women *before pregnancy* can have a significant positive impact on fetal health. We identify the effect through a natural experiment where a policy was announced unexpectedly and implemented immediately.

Our setting has several advantages. First, we provide evidence from a universal cash transfer, i.e. we are able to estimate the effect of a prenatal cash transfer in the whole population as well as in different subgroups. Previous studies typically evaluated (prenatal) policies specifically targeted at poor households. Second, in our setting, the benefit reached women *before* their (next) pregnancy, while previous studies have analyzed cash transfers delivered to women during pregnancy or after birth. Third, 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. Finally, we explore the underlying channels and find new results complementing those found in the previous literature.

The remainder of this paper is organized as follows: In Sections 2 and 3, we provide an overview of related literature and background information on the universal child benefit in Spain. Sections 4–6 describe the estimation methods, data, and results. The last two sections discuss the results and conclude.

2. Related literature

This paper is most closely related to the literature on the *effects of cash and near-cash transfers paid during pregnancy on birth outcomes*, much of which has focused on welfare programs in the U.S. While early work did not consistently find significant impacts of these programs (e.g. Currie and Cole, 1993), recent research generally finds positive effects on health at birth, as measured through average birth weight or incidence of low birth weight. The effects are concentrated among the most disadvantaged women. Hoynes et al. (2015) find reductions in the incidence of low birth weight among mothers who benefitted from the EITC expansion while pregnant, particularly among low-educated and ethnic minority mothers in the U.S. This result confirms and extends previous work of Strully et al. (2010), who focus on the same program and also find a positive effect on average birth weight.

Additional evidence comes from studies evaluating the Food Stamps Program (FSP) in the U.S., a near-cash benefit. Currie and Moretti (2008) find mixed impacts on birth outcomes in California, identifying a small positive effect on the probability of fetal survival for some groups, though also a slight increase in the probability of low birth weight. As discussed in Currie and Almond (2011), the negative effect on birth weight identified in California is likely due to increased fertility among women at higher risk. Later work by Almond et al. (2011) uses national data and finds that mothers exposed to FSP three months prior to birth had babies with higher birth weight, with larger effects among African American mothers.

In some cases, isolating the effect of cash transfers on health can be complicated because these types of welfare programs are often combined with additional benefits or conditions. For instance, Hoynes et al. (2011) and Rossin-Slater et al. (2013) find positive effects of participation in a supplemental nutrition program WIC for low-income pregnant women on average birth weight, particularly for disadvantaged women, such as teen mothers. However, this program included additional support such as nutritional advice, complicating the interpretation of the estimated results.

Research focusing on the effect of cash transfers on health at birth in non-U.S. settings remains relatively scarce and is concentrated in Latin America. Barber and Gertler (2008) study the effect of the Mexican government's conditional cash transfer program Progresa/Oportunidades on child outcomes. The authors find a large reduction in the incidence of low birth weight, though interpretation of the results is again complicated since the program also increased the local supply of health care and included health and economic conditions for participating women. More recently, Amarante et al. (2016) use rich microdata and longitudinal vital statistics to study the effect of cash transfers aimed at poor pregnant women in Uruguay, and find a sizable reduction in the incidence of low birth weight due to faster intrauterine growth.

Some studies have analyzed the *effects of cash (or in-kind) benefits paid during pregnancy on medium and long-term outcomes*. East (2020) finds that loss of eligibility for Food Stamps in utero or during early childhood worsened children's health outcomes in the medium run (at ages 6–16) in the U.S. As for the long run, Hoynes et al. (2016) and Bailey et al. (2020) find that in-utero or early childhood exposure to Food Stamps had positive effects on long-term health, longevity, and economic outcomes. Furthermore, East et al. (2019) found evidence of intergenerational health effects in the U.S., where girls' eligibility for Medicaid while in-utero (and to a lesser extent in early childhood) led to higher average birth weight of their own children, many years down the road.

Several authors have also studied the medium and long-term *health effects of cash transfers which take place after the birth of the child*, though the results are more mixed.[1] In North America, Aizer et al. (2016) and Milligan and Stabile (2011) found positive effects of childhood exposure to welfare programs on longevity and child health in the U.S. and Canada, respectively. Similarly, Goodman-Bacon (2021) and Brown et al. (2019) identified positive effects of childhood access to Medicaid on long-term health in the U.S. In contrast, evidence from European countries shows null effects of cash transfers after birth on child health (Cesarini et al., 2016 focus on Sweden, while Borra et al. (2021) examine the same child benefit in Spain as we do in this paper). As suggested by Cesarini et al. (2016), the observed limited impacts on health from these transfers may be due to the extensive social safety nets already present in these European countries. As for developing countries, Fernald et al. (2008) and Manley et al. (2015) find positive effects of Mexico's Progresa/Oportunidades program on medium-term health outcomes among children.

Finally, the literature on the *effects of income and (cash) transfers on fertility* is also closely related to our paper. In Becker's basic model of fertility (Becker 1960), families choose how much to consume and how many children to have, while also facing a budget constraint. Fertility can react (positively) to increases in household income (the "income effect") or to decreases in the price (or cost) of children (the "price effect"). As for the *income effect*, several papers using a quasi-experimental approach with data from the U.S. provide some evidence of significant effects of income or wealth shocks on fertility. Lindo (2010) examines husband's job loss, Black et al. (2013) focus on men's income, while Lovenheim and Mumford (2013) examine the role of housing wealth. As for the *price effect*, a number of papers evaluated the impact on fertility of direct birth-related cash transfers in different countries (Milligan, 2005; Cohen et al., 2013; González, 2013; Riphahn and Wiynck, 2017; González and Trommlerová, forthcoming) and of parental leave policies (Lalive and Zweimüller, 2009; Malkova, 2018; Raute, 2019), finding positive effects. All these price-effect papers examine income shocks which are conditional on having a child.

Particularly important in our context are the studies by González (2013) and González and Trommlerová (forthcoming), who evaluate the fertility effect of the same universal child benefit in Spain, as is the focus of this paper, and they both find a positive effect. Note that what they study is the *price effect* of the subsidy, where the benefit decreased the cost of having a child and therefore, *overall* fertility increased. In contrast, in this paper, we investigate the *income effect* of the benefit on fertility. We estimate the impact of the benefit receipt on the *subsequent* fertility, i.e. on the probability to have another child (and we find no effect).

[1] This topic has been reviewed by Hoynes and Schanzenbach (2018), Almond et al. (2018), and Cooper and Stewart (2021).

We contribute to the existing literature on the effects of cash transfers to families on child outcomes by showing that a cash subsidy paid to (poor) women even *before pregnancy* can have a significant impact on fetal health.

3. Institutional background

On July 3, 2007, Spanish prime minister Zapatero announced in a national speech that a universal child benefit would be introduced. For every child born or adopted starting from that day, families would receive a lump-sum payment of €2500. 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 in 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 €2500.[2]

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 the form of a fiscal deduction or in cash, at the mother's choice. The timing of the payment to women who gave birth in the first months after the benefit introduction 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. Approximately half of the families requested cash payments, while the other half opted for a fiscal deduction. At the time of its introduction, the universal child benefit amounted to one and a half months of average gross earnings in Spain.[3] In terms of child-raising costs, this amount is estimated to have covered the first 5–6 months after childbirth.[4]

We are not aware of any other policies that would have affected differentially women who had a child just before versus just after July 1, 2007. The closest policy change affecting families was the introduction of 13 days of paternity leave for children born as of March 24, 2007. Our main specification only includes women with children born 8 weeks before or after July 1, 2007, so that all families in our sample (controls and treated) were eligible for paternity leave after their 2007 birth (as well as after subsequent births).

4. Methods

In order to analyze the effects of the universal child benefit on the health outcomes of later-born children, we use individual-level data and an RDD based on the date of birth of the older sibling, born close to the benefit introduction. More specifically, we compare the outcomes of interest (measures of fetal health for children born within the following five years) for mothers who gave birth to their previous child immediately before and after the policy introduction, i.e. in the weeks around July 1, 2007. The sample is restricted to the immediate neighborhood of the exact date of the baby bonus introduction ("cut-off"), ranging from 1 to 16 weeks.[5] Our preferred specification is an 8-week bandwidth (56 days).

The equation we estimate is:

$$Y_i = \alpha + \beta\, T + \gamma_1 t + \gamma_2 t * T + \theta_d + \theta_k + \delta_r + \varphi X_i + \varepsilon_i \quad \forall\, t\ \epsilon\ (c-56, c+55),\ T \equiv 1(t \geq c), \tag{1}$$

where the dependent variable **Y** is a measure of health at birth for child **i** (mainly birth weight). The forcing variable is day of birth **t** of the previous child born to the same mother in 2007. The cut-off **c** is July 1, 2007. Treatment **T** is a binary variable which takes a value of 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 shift in health outcomes of a woman's subsequent child, as a result of the mother receiving the baby bonus in 2007. We control for linear trends in the running variable that are allowed to differ before and after the cut-off, as well as for day-of-the-week fixed effects ($\boldsymbol{\theta_d}$) to account for seasonality effects in births throughout the week. We also include additional sets of fixed effects (FE) and covariates **X**. $\boldsymbol{\alpha}$ is a constant and $\boldsymbol{\varepsilon_i}$ is the error term. Standard errors are clustered at the level of the running variable, i.e. day of previous birth **t**.

The covariates **X** include parental characteristics: mother's and father's age (third-order polynomial), mother's parity before giving birth in 2007 (previously had 1 child, 2 children, 3 or more children), and indicators for married mother, cohabiting father, mother's and father's foreign-born status, and mother's and father's education (below secondary, secondary, more than secondary). Additionally, we include day-of-the-week fixed effects of the *current* birth ($\boldsymbol{\theta_k}$) and region fixed effects $\boldsymbol{\delta_r}$ (there are 19 regions).

[2] 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, so that families with births or adoptions starting from January 1, 2011, did not receive the universal child benefit anymore.

[3] The real value of the benefit was 250%, 190%, and 130% of the 25th percentile, median, and 75th percentile of gross monthly earnings, respectively. The benefit had an even higher real value relative to women's earnings: 310%, 220%, and 150% of female gross monthly earnings, respectively. The data on earnings stem from the Wage Structure Survey conducted in 2006 by the Spanish Statistical Office INE.

[4] Our calculation is based on a report by Save the Children Spain (2018), which estimated 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.

[5] The week variable is scaled such that births on July 1-7, 2007 (or 2006, or 2008) fall into the same week.

The identifying assumption is that in a close neighborhood of the threshold, there is no other factor affecting birth weight (fetal health) that jumps discontinuously for children whose older sibling was born on or after July 1, 2007, other than families' eligibility for the child benefit.

One concern is that eligibility for the child benefit could have affected families' subsequent fertility via an income effect. This could lead to composition effects, driven by additional births in the treated group. Thus, as a first validity check for the identification strategy, we show that this was not the case, since we find no discontinuity in subsequent fertility (or birth spacing) at the threshold. We also provide evidence of balance in covariates between the treated and control groups of mothers.

We estimate Eq. (1) in the full sample of births, but we expect that the effects of the child benefit on fetal health will be driven by mothers in more disadvantaged households. Thus, we also stratify the sample by multiple proxies of household income (predicted poverty status, marital status, and educational attainment of the mother).

5. Materials

Our main source of data is the administrative registry of births, which is maintained 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 as well as her/his 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 their older sibling, as well as a mother identifier. This allows us to implement our RDD estimation with more accuracy, and we can also measure birth spacing between two consecutive births precisely.

5.1. Data on health outcomes of subsequently born children

Our main data set comprises the younger siblings of all children born in 2007. More precisely, our sample includes all births that occurred in Spain in 2008–2012, to mothers with residence in any of the 52 Spanish provinces, with their previous child having been born alive (in Spain or in an unknown location) in 2007. This sample definition is based on the eligibility criteria for the baby bonus (see Annex A3). We include children born up to five years after their previous sibling who was born in 2007.

In terms of health outcomes, we have information on the following perinatal outcomes: birth weight, low birth weight, still birth, early neonatal death in the first 24 hours, normality of birth, C-section, weeks of gestation, and prematurity. Our main measures of fetal health are low birth weight and very low birth weight (below 2500 and 1500 g, respectively). Given that perinatal health outcomes tend to be worse for multiple births (for instance more complications at birth or lower birth weight), we include only singleton births. Since the data are extracted directly from the birth-certificate microdata, we have information about children's demographic and their parents' socio-demographic characteristics, and are able to perform heterogeneity analyses based on parental characteristics at the time of birth of their subsequent child (2008–2012).

5.2. Data on subsequent fertility

We also construct a second, auxiliary data set to test for potential effects of the child benefit on subsequent fertility.[6] We include all births that took place in Spain in 2007,[7] and our goal is to record whether each child born in 2007 had a younger sibling born within the next five years. The public birth-certificate data do not allow us to follow individual families or mothers over time; the birth of each child is recorded separately and cannot be linked to sibling(s) born at a different point in time. Therefore, we create an individual-level data set from data aggregated at the date of birth.

First, we observe how many children were born on each day of 2007. Second, for children born in 2008–2012, we observe the exact date of the previous birth to the same mother. Thus, we can count how many children born in 2008–2012 had a previous sibling who was born on a specific day in 2007, and we can relate this number to the total number of children born on that day in 2007. In other words, we focus on women who had a child on each day of 2007 and calculate the fraction of these women who went on to have another child within the following five years (1825 days). Then we create an individual-level data set based on these aggregate counts. Due to this data construction procedure, we lose the information about the individual characteristics of children and their parents. Therefore, we cannot estimate fertility effects for different groups based on family characteristics.[8]

In order to perform heterogeneity analysis of the fertility effects, we requested a mother identifier from INE, which allows us to link children born at different points in time and to incorporate their mothers' characteristics into the analysis.[9]

5.3. Income data

We expect the child benefit to affect fetal health mainly for women in lower-income households. Despite the availability of a relatively rich set of socio-demographic characteristics of the parents, INE does not collect any measure of household income in its

[6] Such fertility effects would lead to selection issues in our main analysis focusing on the health effects.
[7] We include mothers with residence in any of the 52 Spanish provinces.
[8] The Annex 3 provides additional details about how this data set was constructed, and discusses the challenges of our procedure.
[9] Our detailed inspection of the mother identifier raised doubts about the quality of the matching procedure performed by INE. Due to concerns about the quality of the identifier, we use it exclusively to perform heterogeneity analysis.

birth registry. In order to create a measure of income for our analysis, we combine the birth-registry data with information from the 2007 Survey on Income and Living Conditions (SILC) data. SILC 2007 collects data on households' disposable income in 2006, i.e. prior to the introduction of the baby bonus. We select households that are similar to those in our sample from the birth registry, i.e. households with children under the age of 6 (1580 households).

We then predict income in SILC households (per person household income using the OECD equivalence scale) based on various observable household and parental characteristics.[10] We save the coefficients of all explanatory variables, and use them to predict income in the birth-registry data, using a harmonized set of household and parental variables. Finally, we rank mothers in the birth-registry data based on their predicted household income, and create a poverty indicator (taking a value of 1 for families under the 40th or the 25th percentile of predicted income).

The R-squared in our main SILC regression is 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 are predicted correctly.

5.4. Data on channels

Our main data source – the registry of births – allows us to explore the immediate causes of higher birth weight (longer gestation and faster intrauterine growth). The underlying channels that could contribute to longer gestation or faster intrauterine growth include better maternal health, which in turn is likely impacted by factors like maternal nutrition, maternal habits and behaviors, family structure, and maternal or paternal employment. Since the registry of births does not contain this information, we use two additional data sources to explore the underlying channels.

First, we use the Spanish National Health Survey (NHS) conducted in 2011. Again, we requested additional information on the exact date of birth of each child, which we merge with the publicly available data. Potential channels included in this data set are: maternal health (measured as self-reported overall health, self-reported mental health, and mother's BMI), nutrition (consumption of different food items, as a proxy for nutritional intake), unhealthy habits (smoking, alcohol consumption), and healthy habits (sleep, physical exercise). Due to the small sample size, we include all women who gave birth in calendar year 2007 and were subsequently interviewed in the NHS in 2011.

Second, we also use the Spanish Labor Force Survey (LFS) from years 2007–2012. This survey is conducted on a quarterly basis and it collects demographic and labor market information on all household members. We use this data set to explore potential effects of the child benefit on parental employment and cohabitation.

6. Results

6.1. Validity checks

We conduct two main checks for the validity of our identification strategy. First, we analyze whether benefit eligibility in 2007 affected subsequent fertility, which would lead to composition effects in the sample of women who had another child within the following five years. Second, we check for balance in covariates in our main sample of mothers.

Since the universal child benefit was an unexpected income shock for those families who received it shortly after 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, including children), so that the baby bonus could have led to a higher subsequent fertility.

In order to test for potential fertility effects of benefit eligibility (via an income effect), we estimate Eq. (1) where the dependent variable is a binary indicator taking a value of 1 if a woman had another child (after the reference child in 2007) within the following one to five years. Table 1 shows the main results. As we see in Panel A, full sample, there was no significant change in the fraction of women who had another child in the following 1, 2, 3, 4, or 5 years. Additionally, the coefficients are small in magnitude (very close to zero).[11] Table A1 shows that this null result is robust to bandwidth choice. Note that these results measure subsequent fertility in a cumulative way, which may mask opposing effects in different periods. Panel B in Table 1 shows the fertility response to the baby bonus in each subsequent year separately. Again, we see no significant effect on fertility decisions in the full sample.[12]

Since we suspect that any potential effects of this cash transfer on fetal health would be driven by families with fewer economic resources, it is crucial to test for potential fertility effects specifically in the lower-income group. Table 1 also presents heterogeneous fertility responses by (predicted) poverty status. Our preferred definition of poverty includes women in the two lowest quintiles of per person income (1st-40th percentile). We find very small and insignificant effects in the lower-income group (and only one *negatively*

[10] OECD scales are defined as follows: the first adult receives a weight of 1, each additional adult receives a weight of 0.5, and each child weighs 0.3. An adult is a person aged 14+. We use the following variables as predictors of income: region dummies (19 regions), area of residence (high, medium, or low population density), marital status (married or not), cohabitation status (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 (less than primary, primary, secondary, or tertiary education), occupation (10 categories each), and age of the parents at birth of the youngest child (cubic specification). All SILC variables are harmonized to the maximum extent possible with the birth-certificate data.

[11] The fertility results are very similar whether we use the regular sample or the one with mother identifiers (see section 5.2).

[12] Note that the lack of an *income* effect does not contradict the positive *price* effect of this child benefit found by González (2013) and González and Trommlerová (forthcoming).

Table 1
Treatment effects of the universal child benefit on subsequent fertility.

	Panel A: Woman had another child in the following X years:					**Panel B:** Woman had another child in the following year:				
	1	2	3	4	5	1st	2nd	3rd	4th	5th
Full sample	0.0006	0.0030	−0.0007	0.0011	0.0032	0.0006	0.0023	−0.0037	0.0018	0.0021
	(0.0005)	(0.0028)	(0.0043)	(0.0053)	(0.0059)	(0.0005)	(0.0028)	(0.0028)	(0.0029)	(0.0028)
Average Y	*0.0029*	*0.0667*	*0.1560*	*0.2360*	*0.2930*	*0.0029*	*0.0638*	*0.0892*	*0.0796*	*0.0571*
% Effect	*21.7%*	*4.5%*	*−0.5%*	*0.5%*	*1.1%*	*21.7%*	*3.7%*	*−4.1%*	*2.3%*	*3.7%*
Poor (40%)	0.0004	0.0022	0.0019	0.0004	−0.0007	0.0004	0.0018	−0.0003	−0.0015	−0.0011
	(0.0009)	(0.0034)	(0.0048)	(0.0058)	(0.0062)	(0.0009)	(0.0033)	(0.0036)	(0.0034)	(0.0033)
Average Y	*0.0030*	*0.0517*	*0.1080*	*0.1600*	*0.2050*	*0.0030*	*0.0487*	*0.0564*	*0.0516*	*0.0449*
% Effect	*12.2%*	*4.2%*	*1.8%*	*0.2%*	*−0.3%*	*12.2%*	*3.8%*	*−0.5%*	*−3.0%*	*−2.4%*
Non-poor (60%)	0.0002	0.0046	−0.0031	−0.0030	−0.0011	0.0002	0.0044	−0.0078**	0.0001	0.0019
	(0.0007)	(0.0031)	(0.0045)	(0.0057)	(0.0062)	(0.0007)	(0.0031)	(0.0037)	(0.0036)	(0.0036)
Average Y	*0.0016*	*0.0637*	*0.1630*	*0.2530*	*0.3110*	*0.0016*	*0.0621*	*0.0995*	*0.0893*	*0.0584*
% Effect	*9.6%*	*7.2%*	*−1.9%*	*−1.2%*	*−0.3%*	*9.6%*	*7.2%*	*−7.8%*	*0.1%*	*3.3%*
Very poor (25%)	0.0000	0.0076*	0.0110**	0.0100	0.0144*	0.0000	0.0076*	0.0034	−0.0010	0.0044
	(0.0012)	(0.0041)	(0.0055)	(0.0065)	(0.0080)	(0.0012)	(0.0040)	(0.0041)	(0.0039)	(0.0042)
Average Y	*0.0036*	*0.0496*	*0.1020*	*0.1500*	*0.1920*	*0.0036*	*0.0460*	*0.0522*	*0.0482*	*0.0416*
% Effect	*0.2%*	*15.4%*	*10.8%*	*6.7%*	*7.5%*	*0.2%*	*16.5%*	*6.5%*	*−2.1%*	*10.7%*
Non-poor (75%)	0.0003	0.0024	−0.0047	−0.0048	−0.0051	0.0003	0.0021	−0.0072**	−0.0001	−0.0003
	(0.0006)	(0.0029)	(0.0040)	(0.0054)	(0.0055)	(0.0006)	(0.0030)	(0.0031)	(0.0032)	(0.0032)
Average Y	*0.0017*	*0.0621*	*0.1550*	*0.2380*	*0.2950*	*0.0017*	*0.0603*	*0.0925*	*0.0831*	*0.0568*
% Effect	*17.5%*	*3.9%*	*−3.1%*	*−2.0%*	*−1.7%*	*17.5%*	*3.6%*	*−7.8%*	*−0.1%*	*−0.5%*

Notes: OLS regressions. The running variable is day of birth around July 1, 2007 (cut-off). Bandwidth is +/− 8 weeks around the cut-off. Sample sizes are: 147,913 observations in the full sample; 57,998 in the poor (40%) and 89,900 in the non-poor (60%) sample; 36,267 in the very poor (25%) and 111,631 in the non-poor (75%) sample. Poor households (40%) and very poor households (25%) belong to the 1st-40th and 1st-25th percentile of per person household income, respectively; the remaining households are non-poor (60% and 75%, respectively). Standard errors are clustered at the values of the running variable, i.e. day of birth in 2007. Covariates: linear trend (varies before/after the cut-off), day-of-the-week FE, total number of births in Spain on each day.

significant coefficient in the non-poor group, see Panel B). Thus, it seems that the benefit receipt did not affect subsequent fertility in a significant way, even in lower-income families.

We also create an alternative definition of poverty including women in the lowest income quartile (1st-25th percentile). The final rows in Table 1 show the results. We find weak evidence of a small fertility effect among the poorest women in the second year after the birth leading to benefit eligibility in 2007 (Panel B).[13] Their propensity to have another child in the second year was higher by 0.8 pp, which corresponds to a 17% increase. This effect is somewhat persistent, as is reflected in cumulative fertility effects in the following years (Panel A). This finding raises concerns about possible composition effects in the very poor sample.[14]

We next check for balance in covariates at the threshold, where we would capture any composition effects in observable family characteristics. We focus on women who had another child within the following five years, i.e. the women whose children will be the subject of our main analysis. We estimate Eq. (1) using observable characteristics of the family as the dependent variables. Table 2 shows the results for 12 socio-demographic characteristics, including maternal and paternal age, education, and immigrant status, (predicted) household income, and family structure (marital status and cohabitation of parents). We also consider the parity of the child, and the birth interval between the 2007 birth and the subsequent birth. We perform the analysis in the full, in the poor, and in the very poor sample. None of the 36 coefficients are statistically significant at the 95% confidence level, and the magnitudes are mostly very small. Figure A1 shows graphically that there is no visible jump at the threshold for four selected covariates. We thus find no evidence of discontinuity in any observable family characteristic at the threshold determining benefit eligibility. Note that this is also true in the very poor sample, which assuages previous concerns about possible selection driven by the fertility effects found among very poor women.

6.2. Main effects on fetal health

6.2.1. Birth weight effects

We have shown that the universal child benefit did not lead to changes in subsequent fertility. Next, we examine whether this positive income shock had an effect on fetal health in those families who went on to have another child within the following five years. We focus on birth weight as one of the main markers of fetal health. Since birth weight might be systematically lower for multiple births, all estimations are performed only for singleton births. In order to verify that this does not pose a problem, column 1 in Table 3 shows the effects of the universal child benefit on the probability of a singleton birth as opposed to a multiple birth. We find no change in this probability at the cut-off, neither in the full nor in the poor sample.

[13] Note that families did not receive the benefit immediately after birth of their child in July 2007. Depending on whether they opted for cash or a tax deduction, they would receive the benefit starting in November 2007 (cash), or between April and August 2008 (tax deduction).

[14] In our main analysis, we focus on the broader definition of poverty (1st-40th percentile).

Table 2
Balance in covariates (treatment effects of the universal child benefit on parental characteristics).

	Mother's age	Father's age	Mother educated	Father educated	Mother born abroad	Father born abroad	HH income percentile (scale 0–1)	Mother not married	Father not cohabiting	Parity	First-born child in 2007	Birth interval in days
Full sample	−0.0153	0.0307	0.0085	0.0091	−0.0099	−0.0098	0.0080	0.0122	−0.0036	0.0126	−0.0005	3.3324
	(0.0972)	(0.1277)	(0.0093)	(0.0097)	(0.0071)	(0.0070)	(0.0054)	(0.0085)	(0.0034)	(0.0125)	(0.0084)	(7.7583)
Average Y	*32.32*	*34.97*	*0.6337*	*0.5544*	*0.2053*	*0.2003*	*0.5076*	*0.2381*	*0.0448*	*1.26*	*0.8198*	*1073*
% Effect	*0.0%*	*0.1%*	*1.3%*	*1.6%*	*−4.8%*	*−4.9%*	*1.6%*	*5.1%*	*−8.1%*	*1.0%*	*−0.1%*	*0.3%*
Poor (40%)	−0.1000	0.2946	0.0120	−0.0017	−0.0150	−0.0171	−0.0003	0.0215	−0.0065	0.0380	0.0019	−4.3593
	(0.1683)	(0.2462)	(0.0134)	(0.0133)	(0.0103)	(0.0114)	(0.0034)	(0.0170)	(0.0072)	(0.0240)	(0.0142)	(14.3581)
Average Y	*29.74*	*33.61*	*0.2566*	*0.2326*	*0.3836*	*0.3823*	*0.2070*	*0.3617*	*0.0830*	*1.43*	*0.7198*	*1048*
% Effect	*−0.3%*	*0.9%*	*4.7%*	*−0.7%*	*−3.9%*	*−4.5%*	*−0.2%*	*5.9%*	*−7.8%*	*2.7%*	*0.3%*	*−0.4%*
Very poor (25%)	0.0644	0.4309	0.0053	0.0124	−0.0288*	−0.0179	−0.0026	0.0108	−0.0010	0.0454	−0.0008	−8.1504
	(0.2224)	(0.3240)	(0.0131)	(0.0127)	(0.0154)	(0.0141)	(0.0025)	(0.0220)	(0.0095)	(0.0351)	(0.0205)	(15.5443)
Average Y	*28.92*	*33.36*	*0.1489*	*0.1469*	*0.4485*	*0.4506*	*0.1328*	*0.4118*	*0.1010*	*1.57*	*0.6444*	*1016*
% Effect	*0.2%*	*1.3%*	*3.6%*	*8.4%*	*−6.4%*	*−4.0%*	*−1.9%*	*2.6%*	*−1.0%*	*2.9%*	*−0.1%*	*−0.8%*

Notes: OLS regressions. The running variable is day of previous birth around July 1, 2007 (cut-off). Bandwidth is +/− 8 weeks around the cut-off. Parental characteristics of singleton children born up to five years after the original birth around July 2007. Sample sizes are 42,867 in the full sample, 17,198 in the poor sample, and 10,761 in the very poor sample. Poor households belong to the 1st-40th percentile of per person household income; very poor households belong to the 1st-25th percentile. Standard errors are clustered at the 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, day-of-the-week FE of current birth, region FE (19 regions); parental characteristics are omitted.

Table 3
Treatment effects of the universal child benefit on perinatal health outcomes of subsequent children.

	Singleton birth	Still birth	Death within 24 hours	Birth classified as not normal	C-section	Weeks of gestation	Premature birth
Full sample	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	*0.9860*	*0.0025*	*0.0004*	*0.1020*	*0.1800*	*39.04*	*0.0493*
% Effect	*0.1%*	*49.9%*	*17.0%*	*3.0%*	*2.0%*	*0.0%*	*4.1%*
Poor (40%)	−0.0014	0.0037**	0.0001	0.0052	−0.0059	−0.0428	0.0103
	(0.0036)	(0.0019)	(0.0008)	(0.0090)	(0.0124)	(0.0653)	(0.0073)
Observations	*17,437*	*17,198*	*17,136*	*17,198*	*17,198*	*12,844*	*17,198*
Average Y	*0.9873*	*0.0032*	*0.0007*	*0.1096*	*0.1685*	*39.00*	*0.0573*
% Effect	*−0.1%*	*115.7%*	*10.1%*	*4.8%*	*−3.5%*	*−0.1%*	*17.9%*
Very poor (25%)	0.0023	0.0068***	−0.0009	0.0109	0.0030	−0.0030	0.0130
	(0.0044)	(0.0025)	(0.0011)	(0.0112)	(0.0143)	(0.0840)	(0.0097)
Observations	*10,915*	*10,761*	*10,720*	*10,761*	*10,761*	*7753*	*10,761*
Average Y	*0.9873*	*0.0031*	*0.0008*	*0.1099*	*0.1638*	*39.00*	*0.0578*
% Effect	*0.2%*	*220.2%*	*−122.6%*	*9.9%*	*1.8%*	*0.0%*	*22.5%*

Notes: OLS regressions. The running variable is day of previous birth around July 1, 2007 (cut-off). Bandwidth is +/− 8 weeks around the cut-off. Poor households belong to the 1st-40th percentile of per person household income; very poor households belong to the 1st-25th percentile. Standard errors are clustered at the 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, day-of-the-week FE of current birth, region FE (19 regions), and parental characteristics: mother's and father's age (a cubic function for each), 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 (a dummy for each), mother's and father's education (below secondary, secondary, more than secondary for each).

Table 4
Treatment effects of the universal child benefit on birth weight and low birth weight indicators of subsequent children.

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2500 g	<2250 g	<2000 g	<1750 g	<1500 g
Full sample	4.5195	0.0021	0.0006	−0.0059**	−0.0055***	−0.0045**	−0.0036**
	(10.7070)	(0.0035)	(0.0040)	(0.0029)	(0.0020)	(0.0018)	(0.0014)
Average Y	*3304*		*0.0450*	*0.0248*	*0.0130*	*0.0089*	*0.0058*
% Effect	*0.1%*		*1.4%*	*−23.9%*	*−42.3%*	*−51.0%*	*−61.6%*
Poor (40%)	2.1239	0.0036	−0.0057	−0.0094*	−0.0086***	−0.0088***	−0.0070***
	(16.4978)	(0.0055)	(0.0066)	(0.0053)	(0.0032)	(0.0031)	(0.0024)
Average Y	*3300*		*0.0574*	*0.0327*	*0.0174*	*0.0119*	*0.0084*
% Effect	*0.1%*		*−9.9%*	*−28.7%*	*−49.2%*	*−74.1%*	*−82.5%*
Non-poor (60%)	7.3334	0.0017	0.0043	−0.0041	−0.0038	−0.0020	−0.0017
	(12.6923)	(0.0041)	(0.0046)	(0.0033)	(0.0024)	(0.0019)	(0.0016)
Average Y	*3307*		*0.0373*	*0.0198*	*0.0102*	*0.0070*	*0.0042*
% Effect	*0.2%*		*11.5%*	*−20.5%*	*−36.7%*	*−28.2%*	*−40.1%*
Very poor (25%)	3.7842	0.0049	−0.0086	−0.0142*	−0.0111**	−0.0091**	−0.0088***
	(22.7288)	(0.0076)	(0.0096)	(0.0076)	(0.0047)	(0.0039)	(0.0030)
Average Y	*3303*		*0.0599*	*0.0352*	*0.0172*	*0.0116*	*0.0083*
% Effect	*0.1%*		*−14.3%*	*−40.3%*	*−64.6%*	*−78.4%*	*−105.8%*
Non-poor (75%)	4.8869	0.0014	0.0032	−0.0036	−0.0038*	−0.0031*	−0.0020
	(10.7359)	(0.0035)	(0.0042)	(0.0030)	(0.0022)	(0.0019)	(0.0016)
Average Y	*3305*		*0.0404*	*0.0215*	*0.0117*	*0.0080*	*0.0050*
% Effect	*0.1%*		*7.9%*	*−16.5%*	*−32.9%*	*−39.2%*	*−40.2%*

Notes: OLS regressions. The running variable is day of previous birth around July 1, 2007 (cut-off). Bandwidth is +/− 8 weeks around the cut-off. Sample sizes are: 41,281 observations in the full sample; 16,155 in the poor (40%) and 25,126 in the non-poor (60%) sample; 10,001 in the very poor (25%) and 31,280 in the non-poor (75%) sample. Poor households (40%) and very poor households (25%) belong to the 1st-40th and 1st-25th percentile of per person household income, respectively; the remaining households are non-poor (60% and 75%, respectively). Standard errors are clustered at the values of the running variable, i.e. day of previous birth. Covariates as in Table 3.

Table 4 shows our main results – the treatment effect of child benefit eligibility on the birth weight of the next child. We have three main outcome variables: the natural log of birth weight, and two medical indicators of low weight at birth: low birth weight (<2500 g; LBW) and very low birth weight (<1500 g; VLBW).[15] We also show results for birth weight in grams, as well as three intermediate birth weight cutoffs.[16]

[15] Note that throughout the paper, we use the expression "low birth weight" in general terms, i.e. not referring specifically to the 2500-gram cut-off. When we refer to that specific cut-off, we either explicitly mention it or we use the abbreviation LBW.
[16] A histogram showing the distribution of birth weight in the estimation sample is shown in Figure A2.

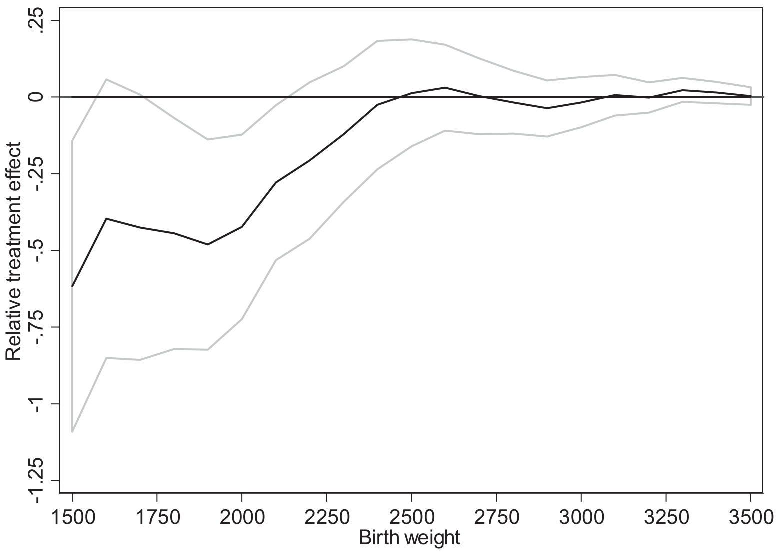

Fig. 1. Treatment effects of the universal child benefit on birth weight of subsequent children.
Notes: Treatment effects from 21 regressions with dependent variables defined as birth weight of subsequent children below a certain threshold; the thresholds vary between 1500 and 3500 g in 100-gram steps. Displayed are relative treatment effects, i.e. estimated treatment effects divided by the mean of the dependent variable in the pre-treatment sample. The gray lines mark 95% confidence intervals. Bandwidth is +/– 8 weeks around the cut-off. Sample size is 41,281 observations. Standard errors are clustered at the values of the running variable, i.e. day of previous birth. Covariates as in Table 4.

We find no effect of the benefit on birth weight, neither in levels nor in logs (columns 1–2). The coefficients are very close to zero and statistically insignificant. Even though there is no effect on LBW either, we do find a statistically and economically significant effect on the more extreme measure of VLBW: women who were entitled to receive the baby bonus in 2007 were 0.36 pp less likely to give birth to a baby with VLBW in the next five years – a 62% decrease relative to the mean of the variable in the control group. Due to this large effect on VLBW but no effect on LBW, we examine also other cut-offs between the traditionally used 1500 and 2500 g, in 250-gram steps.

We find effects for all low-birth-weight cut-offs smaller than 2500 g; the relative magnitude of the effect is increasing as the cut-off is decreasing (i.e. becoming more extreme): it ranges from 24% at 2250 g to 51% at 1750 g. This is illustrated in Fig. 1, where we plot the effects of the universal child benefit on different measures of low birth weight, with cut-offs ranging from 1500 to 3500 g, in 100-gram steps. We show relative effect sizes in order to account for the fact that the lower (i.e. more extreme) is the threshold, the lower is also the incidence of low birth weight, and consequently the smaller is also the potential magnitude of any observed effect. We see that the relative effect size is large for VLBW (<1500 g), it gradually decreases as the cut-off increases (i.e. as it becomes less extreme), and it becomes a zero effect starting from LBW (<2500 g) onwards. Thus, it seems that the universal child benefit positively affected fetal health for children at the lower end of the birth weight distribution.

6.2.2. Robustness checks

In order to understand better the effects across the birth weight distribution, we also estimate regressions where the dependent variable takes a value of 1 for a birth weight located in a specific bin. First, this enables us to determine whether the effects observed in Table 4 for cut-offs between 1500 and 2500 g are driven exclusively by decreases in VLBW or whether there are improvements in other low-birth-weight categories as well. Second, we can determine how the reduction in the fraction of VLBW babies translates into an increased density in other bins along the birth weight distribution. Table A2 reveals that, at the same time as there were fewer VLBW children born (0.36 pp effect), the fraction of children with birth weight between 2000 and 2500 g increased significantly by 0.61 pp. Thus, it seems that as a result of the universal child benefit, children who would have otherwise been born with a birth weight below 1500 g (and with a birth weight between 1500 and 2000 g; this effect is statistically insignificant but economically meaningful) were born with a higher birth weight (2000–2500 g) instead.

Having shown that in the full sample, the treatment effects on low birth weight are driven mainly by the medically relevant VLBW indicator (<1500 g), some of the following results will be shown predominantly for this outcome. Panel A in Fig. 2 shows the treatment effect on VLBW at the cut-off graphically, while Panel C displays the same graph for a higher cut-off (<2000 g). In terms of robustness, Fig. 3 shows that the estimated treatment effect on VLBW 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. Additionally, Panel A in Table A3 shows that our results for VLBW also remain robust when using different types of optimal bandwidths, while Panel B shows robustness to bandwidths 1–16 weeks for all of the (low) birth weight measures.

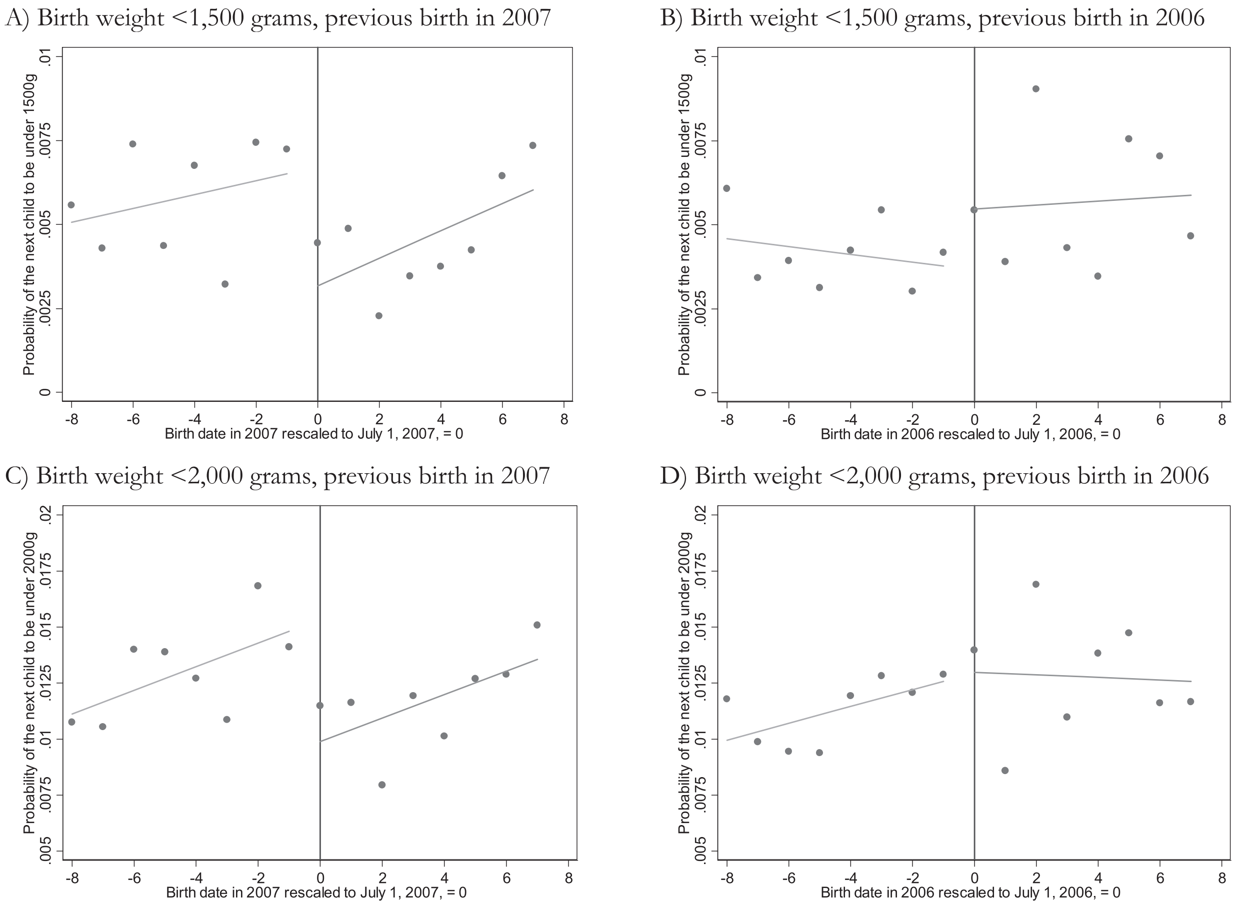

Fig. 2. Probability of subsequent children to be born below 1500 or 2000 g around the cut-off in the policy introduction year and in a placebo year. *Notes:* X-axes show the date of birth of the child born in 2007 or 2006, respectively. Y-axes depict fraction of subsequent siblings who were born with birth weight below 1500 g (Panels A and B) or below 2000 g (Panels C and D) in the next five years. Averages are calculated by week of birth of the previous child born around July 1, 2007 or 2006, respectively. The vertical lines depict linear fits.

In the next step, we examine robustness to the inclusion of covariates. We first estimate regressions as in Table 4 but include (1) only linear trends, and then we add cumulatively: (2) day-of-the-week FE, (3) region FE, (4) day-of-the-week FE of the current birth, and (5) parental characteristics. Table A4 shows that our results are robust to the covariates included, both in terms of magnitude and significance. In particular, including day-of-the-week FE decreases the coefficient size by approximately 8% of its original value (from 0.39 pp to 0.36 pp in case of VLBW), but adding further covariates has virtually no impact on our estimates.

Additionally, it is important to show that the estimated treatment effects are not driven by seasonality, i.e. by other factors that affect children with an older sibling born after July 1 in any year. In order to do that, we estimate two sets of placebo regressions where we include either women with children born around July 1, 2006 (one year before the unexpected benefit introduction) or around July 1, 2008 (both "control" and "treated" mothers are eligible for 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.

In fact, we find that the probability that the birth weight of the next child is below 1750 or 1500 g is *higher* for mothers who had their previous child after July 1 (significant only in 2008). This suggests that second semester mothers tend to be different (less healthy) from first semester ones. If anything, this finding strengthens our results for the effect of the child benefit. Panels B and D in Fig. 2 illustrate our "placebo" results graphically for VLBW and <2000 g in 2006, respectively.

Finally, we examine other indicators of perinatal health, in addition to birth weight. We find no effect on the most severe outcomes, such as still birth, early neonatal death (i.e. death within the first 24 hours), and normality of birth (Table 3).[17] We also find no change in the fraction of C-sections. Lastly, the length of pregnancy was not affected, and neither was the probability of a premature birth.[18]

[17] The Spanish birth-certificate data do not contain information on APGAR scores or mortality beyond the first 24 hours.

[18] A histogram showing the distribution of pregnancy weeks at birth in the estimation sample is shown in Figure A3.

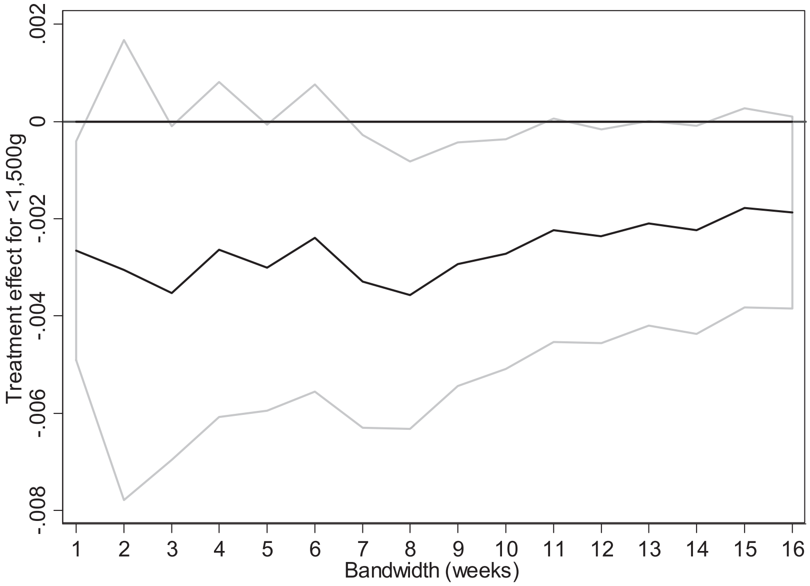

Fig. 3. Treatment effects of the universal child benefit on VLBW of subsequent children for different bandwidths.
Notes: Treatment effects on probability of a very low birth weight (<1500 g) of subsequent children. The gray lines mark 95% confidence intervals. Based on regressions in the last column of Panel B in Table A3.

Table 5
Placebo regressions.

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2500 g	<2250 g	<2000 g	<1750 g	<1500 g
Year 2006	2.5454	0.0003	−0.0010	0.0006	0.0004	0.0022	0.0019
	(8.6113)	(0.0029)	(0.0037)	(0.0028)	(0.0020)	(0.0017)	(0.0013)
Average Y	*3311*		*0.0445*	*0.0232*	*0.0113*	*0.0071*	*0.0042*
% Effect	*0.1%*		*−2.2%*	*2.5%*	*3.6%*	*30.6%*	*45.4%*
Year 2008	−7.4481	−0.0025	−0.0012	0.0022	0.0023	0.0025*	0.0021*
	(7.6330)	(0.0025)	(0.0031)	(0.0024)	(0.0017)	(0.0014)	(0.0011)
Average Y	*3318*		*0.0429*	*0.0213*	*0.0103*	*0.0063*	*0.0044*
% Effect	*−0.2%*		*−2.9%*	*10.2%*	*22.7%*	*40.6%*	*49.1%*

Notes: OLS regressions. The running variable is day of previous birth around July 1, 2006 or 2008 (cut-off). Bandwidth is +/− 8 weeks around the cut-off. Sample sizes are 41,303 observations in 2006 and 41,289 observations in 2008. Standard errors are clustered at the values of the running variable, i.e. day of previous birth. Covariates as in Table 3.

Since we are considering a broad set of outcomes related to fetal health, we also verify and confirm the robustness of our birth-weight results to multiple hypothesis testing in Table A5.

6.3. Heterogeneity analysis of effects on birth weight

So far we have shown that the receipt of a universal child benefit of €2500 led to a lower probability that the next child would be born with very 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).

6.3.1. Socioeconomic disadvantage

In terms of disadvantaged families, heterogeneity by household income is shown in lower panels of Table 4. As expected, the main effect is driven by children born in the lower two quintiles of the income distribution (poor 40%): the treatment effect on VLBW is twice as large there than in the full sample (–0.70 pp versus –0.36 pp), and the relative effect is larger too (–83% versus –62%). In the remaining three quintiles of the income distribution (non-poor 60%), we find a negative effect on different measures of low birth weight, but they are smaller in magnitude (e.g. less than half the size for VLBW) and statistically indistinguishable from zero. The effects are even larger in the very poor sample (25%; the lowest quartile of the income distribution): the coefficient size for VLBW in the very poor sample increases to –0.88 pp (as opposed to –0.70 pp in the poor sample), while the relative effect size is –106% (compared to –83%). Fig. 4 shows the corresponding effects for VLBW and <2000 g graphically.

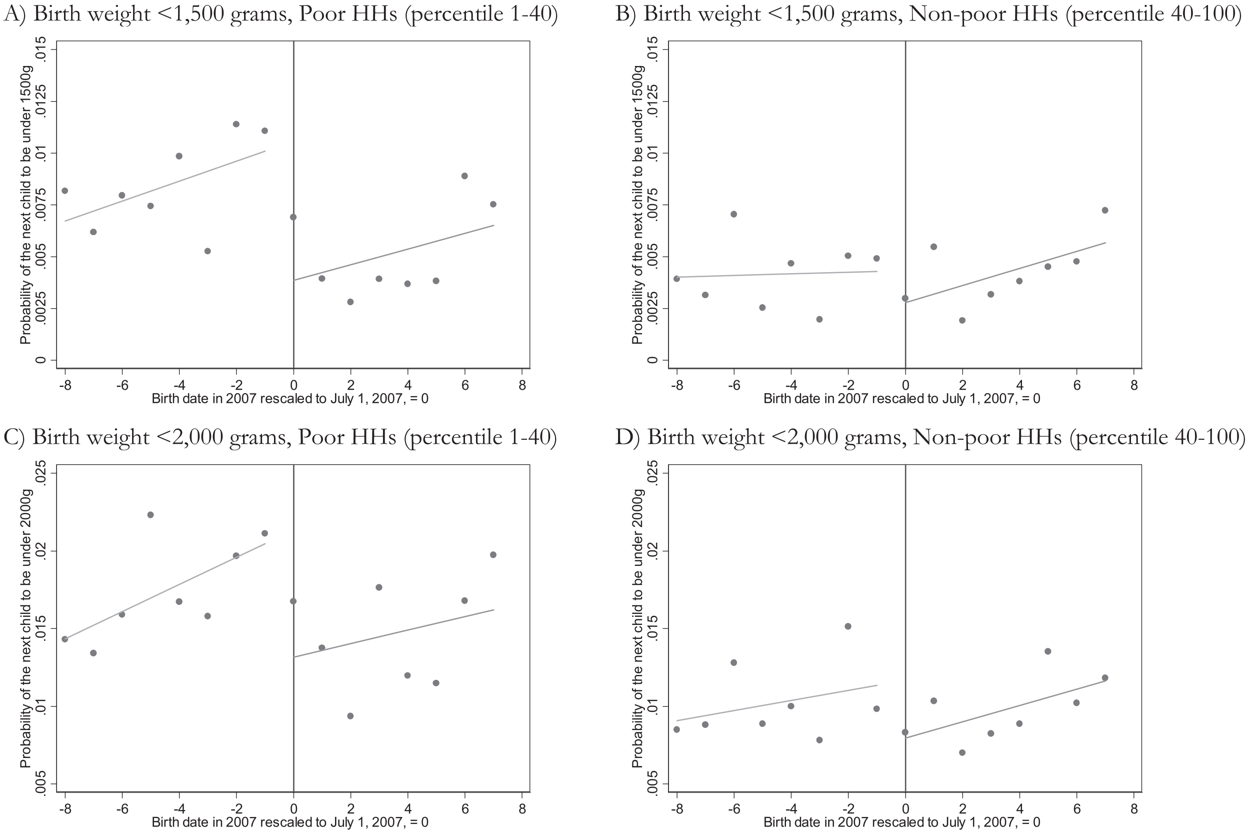

Fig. 4. Probability of subsequent children to be born below 1500 or 2000 g around the cut-off in the policy introduction year and in a placebo year by income level.

Notes: X-axes show the date of birth of the child born in 2007 or 2006, respectively. Y-axes depict fraction of subsequent siblings who were born with birth weight below 1500 g (Panels A and B) or below 2000 g (Panels C and D) in the next five years. Poor households belong to the 1st-40th percentile of per person household income; the remaining households are non-poor. Averages are calculated by week of birth of the previous child born around July 1, 2007 or 2006, respectively. The vertical lines depict linear fits.

In terms of how the reduction in the fraction of VLBW babies translated into more children in other bins along the birth weight distribution, Table A2 shows an increase in the 2000–3000 g interval, even though the coefficients are imprecisely estimated. Most other perinatal outcomes remain unaffected also in the poor sample (Table 3).[19]

In addition to household income, which was estimated based on parental and household characteristics, we can exploit other dimensions of socioeconomic disadvantage that are directly available in the data.[20] Table A6 shows heterogeneity analysis by maternal education and marital status, and it confirms that the main effects are driven by the disadvantaged, i.e. low-educated and unmarried, women. More specifically, the estimated effects among low-educated mothers are similar to those among poor mothers (1st-40th percentile), both in absolute and relative magnitudes (Panel A). Among unmarried mothers (Panel B), the relative effect sizes are larger and they roughly correspond to the effects among the poorest mothers (1st-25th percentile). Note that the baby bonus also affected LBW (<2500 g) and the continuous measure of birth weight 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 (33% effect size).

6.3.2. Prematurity

In addition to unfavorable socioeconomic conditions, a premature birth is an important factor that makes a child vulnerable to low birth weight. In our estimation sample, 88% of VLBW children were born prematurely, compared to only 5% premature births

[19] The fraction of still births appears to have increased, and the estimated effect is quite large. We have no explanation for this effect and it is unclear whether and how it could be related to improvements in birth weight.

[20] 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 later-born child in 2008-2012, *not* based on characteristics in 2007. This problem arises due to data limitations and it applies also to the other dimensions of socio-economic disadvantage directly available in the data (such as maternal education and marital status, based on which the household income is predicted). In Table 2 we show that control and treated families are balanced across these characteristics.

among children with birth weight ≥1500 g. To show this association from another angle, Panel A in Figure A4 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).[21]

Table A7 presents the effects of the baby bonus separately for children born on term and prematurely (in Section 6.2.2, we showed that the benefit did not affect prematurity rates, see Table 3). Perhaps unsurprisingly, the effects on the different measures of low birth weight are found only among prematurely born children, and the relative magnitudes are similar or only somewhat larger than in the full sample. What is new here is that the 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 100 g. This is a substantial magnitude.

6.3.3. Interaction between socioeconomic disadvantage and prematurity

In a last step, we study the interaction between adverse socioeconomic conditions and prematurity. Table A8 shows the main 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. Non-poor premature children did benefit from the baby bonus through a lower probability of low birth weight (e.g. relative effect size –50% for <2000 g). Also poor on-term babies had a lower likelihood of low birth weight thanks to the child benefit (e.g. relative effect size –72% for <2250 g). Finally and most importantly, poor pre-term babies were the biggest winners. The fraction of low birth weight decreased: the decline is by 101% for VLBW and by 27% for LBW, an indicator that was unaffected in almost all other samples (with the exception of unmarried women, whose children benefitted from a similarly large relative decrease in LBW: –33%). The effects are large not only in relative terms but also in absolute terms: the estimated drop is 13 pp and 18 pp for VLBW and LBW, respectively. What is more, we see that average birth weight also reacted positively: it increased by 12.5% or 228 g, a substantial improvement.

6.4. Timing of the effects

In all of our analyses, we work with a sample of children who were born up to five years after their mother gave birth to her previous child (close to July 1, 2007). We find important effects of the child benefit on birth-weight-related outcomes of the next child. The cash transfer amounted to about 1.5 times the average and 2.5 times the 25th percentile of gross monthly earnings. Given the magnitude of the income shock, we expect that the effects on fetal health are likely to be stronger for births that were closer in time to the receipt of the benefit.

We test this hypothesis by splitting our sample by birth spacing between the two children (a shorter birth interval of under 2.5 years, versus a longer one of 2.5 to 5 years). The results are shown in Table 6. In the full sample, the birth weight effects are concentrated in births taking place less than 2.5 years after the previous child.[22] In the poor, more affected subsample, we also find effects only for children born relatively early after the benefit receipt. Additionally, we find not only a reduction in the fraction of poor children with VLBW, but also with LBW (under 2500 g) by 2.1 pp or 29%.[23]

To put these results into perspective, note that the benefit payments started in late November 2007 in cash and in April 2008 as a tax deduction. Lower-income women are more likely to have opted for the (faster) cash transfer. Average birth spacing in the subsample of poor women with a shorter birth interval (<2.5 years) is 21 months. Thus, the average poor woman in the treated group would have received the benefit in November-December 2007, and would have conceived her next child in July 2008, about 8 months after the benefit receipt. She would have given birth in April 2009 (21 months after July 2007).

6.5. Potential mechanisms

In the final step of our analysis, we explore the mechanisms that could explain the positive effects of the universal child benefit on the birth weight of subsequently born children. In general, the immediate (biological) causes of higher birth weight could be longer gestation and/or faster intrauterine growth. Both of these biological channels have, in turn, their own determinants and underlying causes, which we also explore.

First, we examine the effect of the cash transfer on gestational length. In Table 3, we find no impact of the benefit receipt on weeks of gestation or on prematurity as defined in the medical literature (<37 weeks of gestation). However, as already shown in Figure A4, VLBW happens at very low gestational ages. This is highlighted also in Figure A5, which shows that the majority (56%) of VLBW children in our estimation sample are born at <30 weeks.[24] Therefore, we also explore more extreme measures of prematurity, using cut-offs lower than 37 weeks. We find no negative significant effects in the full sample nor in the poor

[21] Admittedly, VLBW is an extreme and relatively rare birth outcome: only 0.5% of children are born with VLBW. On the other hand, LBW (<2500 g) is more common, affecting 4.6% of all children. The corresponding statistics for LBW and prematurity are the following: In our estimation sample, 59.8% of LBW children were born prematurely, compared with 2.4% premature births among children with birth weight ≥2500 g; see Panel B in Figure A4. 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).

[22] Note that we find effects of a similar relative magnitude also in the group of births taking place 2.5-5 years after the previous birth, but they are imprecisely estimated.

[23] Note that no effect on LBW was detected in the poor sample covering the entire period 0-5 years (Table 4).

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

Table 6
Heterogeneity analysis by timing of next birth.

	Birth weight	Log (birth weight)	Dummy variable = 1 if birth weight is:				
			<2500 g	<2250 g	<2000 g	<1750 g	<1500 g
Full sample							
Year 0–2.5	10.7527	0.0052	−0.0051	−0.0059	−0.0071**	−0.0066**	−0.0045**
	(19.1845)	(0.0065)	(0.0068)	(0.0049)	(0.0034)	(0.0027)	(0.0021)
Average Y	*3289*		*0.0504*	*0.0298*	*0.0167*	*0.0119*	*0.0075*
% Effect	*0.3%*		*−10.2%*	*−19.9%*	*−42.3%*	*−55.9%*	*−60.2%*
Year 2.5–5	0.5123	0.0001	0.0044	−0.0057	−0.0043*	−0.0030	−0.0029
	(13.1469)	(0.0044)	(0.0057)	(0.0038)	(0.0025)	(0.0022)	(0.0018)
Average Y	*3313*		*0.0419*	*0.0218*	*0.0107*	*0.0071*	*0.0048*
% Effect	*0.0%*		*10.5%*	*−26.0%*	*−39.8%*	*−42.8%*	*−60.7%*
Poor (40%)							
Year 0–2.5	31.7606	0.0150	−0.0207*	−0.0190**	−0.0173***	−0.0131**	−0.0108**
	(27.5552)	(0.0100)	(0.0117)	(0.0091)	(0.0064)	(0.0052)	(0.0043)
Average Y	*3275*		*0.0712*	*0.0429*	*0.0238*	*0.0160*	*0.0113*
% Effect	*1.0%*		*−29.1%*	*−44.2%*	*−72.6%*	*−81.9%*	*−95.5%*
Year 2.5–5	−20.1690	−0.0049	0.0053	−0.0023	−0.0020	−0.0058	−0.0042
	(20.8115)	(0.0071)	(0.0079)	(0.0061)	(0.0039)	(0.0040)	(0.0031)
Average Y	*3318*		*0.0479*	*0.0257*	*0.0130*	*0.0091*	*0.0065*
% Effect	*−0.6%*		*11.0%*	*−9.1%*	*−15.8%*	*−63.9%*	*−64.3%*
Very poor (25%)							
Year 0–2.5	42.1784	0.0198*	−0.0291**	−0.0280**	−0.0242***	−0.0159**	−0.0139***
	(31.9001)	(0.0111)	(0.0141)	(0.0117)	(0.0083)	(0.0064)	(0.0050)
Average Y	*3268*		*0.0768*	*0.0487*	*0.0257*	*0.0173*	*0.0112*
% Effect	*1.3%*		*−37.9%*	*−57.5%*	*−94.1%*	*−91.7%*	*−124.1%*
Year 2.5–5	−27.2519	−0.0070	0.0077	−0.0035	−0.0005	−0.0041	−0.0048
	(32.6815)	(0.0109)	(0.0130)	(0.0097)	(0.0056)	(0.0051)	(0.0036)
Average Y	*3331*		*0.0465*	*0.0245*	*0.0104*	*0.0071*	*0.0060*
% Effect	*−0.8%*		*16.5%*	*−14.1%*	*−5.2%*	*−57.6%*	*−80.6%*

Notes: OLS regressions. The running variable is day of previous birth around July 1, 2007 (cut-off). Bandwidth is +/− 8 weeks around the cut-off. Sample sizes are: 15,297 observations in the 1st-2.5th year and 25,984 observations in the 2.5th-5th year after the birth in 2007 in the full sample; 6509 in the 1st-2.5th year and 9646 in the 2.5th-5th year after the birth in 2007 in the poor (40%) sample; 4330 in the 1st-2.5th year and 5671 in the 2.5th-5th year after the birth in 2007 in the very poor (25%) sample. Standard errors are clustered at the values of the running variable, i.e. day of previous birth. Covariates as in Table 3.

sample.[25] These results suggest that the observed effects on birth weight seem to be driven in large part by faster intrauterine growth.

The underlying channels that could contribute to faster intrauterine growth include better maternal health, which in turn may be impacted by factors including maternal nutrition, maternal habits and behaviors, and changes in family structure and parental employment.

We first focus on maternal health, nutrition, and behaviors. Information on these dimensions is available in the Spanish NHS 2011. Since the sample of women who were interviewed in 2011 and had a child close to July 2007 is very small, we include all women who gave birth in calendar year 2007. In Table A9, we show that there are no significant discontinuities in covariates at the threshold in our NHS sample.

Table A10 shows that women who were eligible for the benefit in 2007 seem to be in better health when interviewed in 2011: they are more likely to have a healthy BMI, to report good overall health, and to report good mental health. In terms of factors that could be driving better maternal health, we observe that treated women seem to be eating healthier (more fruits and vegetables, more proteins, less junk food), they seem more likely to avoid unhealthy habits (they are less likely to drink alcohol and smoke on a daily basis), and they seem to have healthier habits (they sleep and exercise more). None of these effects are individually statistically significant, which could be driven by the small sample size.

In order to address multiple hypothesis testing concerns, we create an index of maternal health and behaviors, which combines the 10 indicators reported in Table A10. We use three different aggregation methods to judge the robustness of our results. Table 7 shows that the aggregation method is irrelevant for the overall result: we find that treated women report significantly better health in 2011. These results are robust to including a broader sample of women who had children in 2006–08 and 2005–09. Table 7 also shows the results from two "placebo" specifications where we use July 2006 or 2008 as the threshold. Those specifications lead to small, insignificant coefficients.

[25] These results are available upon request. In a second step, we estimate the same regressions as in Table 4, but we include gestational week fixed effects as additional covariates. If the effect on low birth weight disappeared, it would mean that it is driven by changes in gestational length. If, on the other hand, the effect remained relatively stable, it would mean that it is driven by faster intrauterine growth. Controlling for gestational weeks decreases the coefficient size to 70%–90% of the original estimate, both in the full and poor sample, for cut-offs 1500-2000 g.

Table 7
Treatment effects on indices of maternal health and healthy behaviors.

	Average	Standardized average	Summary index
Year 2007	0.1030**	0.2533**	0.2478**
	(0.0485)	(0.1218)	(0.1160)
St. dev. of Y	*0.1541*	*0.3774*	*0.3566*
Year 2007, Sample 2006–2008	0.1312**	0.3075**	0.2983**
	(0.0585)	(0.1461)	(0.1434)
St. dev. of Y	*0.1463*	*0.3536*	*0.3471*
Year 2007, Sample 2005–2009	0.1229**	0.2900**	0.2799**
	(0.0561)	(0.1395)	(0.1364)
St. dev. of Y	*0.1469*	*0.3605*	*0.3564*
Placebo Year 2006	0.0177	0.0556	0.0369
	(0.0416)	(0.0955)	(0.0936)
St. dev. of Y	*0.1470*	*0.3618*	*0.3589*
Placebo Year 2008	0.0692*	0.1435	0.0740
	(0.0393)	(0.0957)	(0.0931)
St. dev. of Y	*0.1459*	*0.3419*	*0.3250*

Notes: OLS regressions. Outcomes are aggregate indices based on 10 underlying indicators. The aggregation methods are: a simple average (column 1), a simple average of standardized outcomes (column 2), a summary index (weighted average of standardized outcomes, following Anderson, 2008, column 3). The 10 underlying indicators are: mother has normal weight (BMI), self-reported overall health is good, self-reported mental health is good, eats fruits/vegetables daily, eats proteins daily, does not eat junk food daily, does not drink alcohol daily, does not smoke daily, sleeps at least 8 hours, does sports weekly. The running variable is day of birth. Cut-off is July 1, 2007 (2006 and 2008 in case of placebo regressions). Sample in the five panels includes all births in calendar years 2007, 2006–2008, 2005–2009, 2006, and 2008, respectively, to mothers interviewed in the NHS 2011. Trends vary before and after the cut-off, and they are linear, cubic, quartic, linear, and linear, respectively. Sample sizes are 208, 608, 932, 223, and 235, respectively. Robust standard errors.

Our results in Table 7 show large effects: the improvement in the different maternal health indices is by almost 0.7 of their standard deviation. However, these estimates are quite imprecise. The 95% confidence intervals include effect sizes of 0.05 of a standard deviation. The point estimates on the individual components of the indices are also smaller: Table A10 shows that the overall health status among treated women improved by 0.28 standard deviations, while their mental health was better by 0.17 standard deviations.[26]

In order to explore the potential impact of changing family structure and parental employment on intrauterine growth, we use data from the LFS from years 2007–2012. This survey, which is conducted on a quarterly basis, collects information on parental cohabitation and employment. Our sample includes all women who were interviewed in the same quarter or up to 19 quarters after the birth of their child (i.e. 20 quarters or 5 years in total). Unlike the previous data sources, the LFS does not report the child's exact date of birth, only the month of birth. Thus, we stay as close to the cut-off as possible (we include women who gave birth one or two months around the cut-off) and do not control for trends in the running variable.

As shown in Table 8, we find no evidence of changing cohabitation patterns among affected parents, neither in the full sample nor in the subsamples of lower and higher-educated women. We also find no changes in employment patterns among unmarried and lower-educated women (Panel A in Table 9), i.e. the two demographic groups driving the birth weight effects.[27] We do find a negative effect on maternal employment in the unaffected groups (married and higher-educated women) when we use the broader bandwidth (2 months around the cut-off), consistent with the findings in González (2013).

Panel B in Table 9 shows the results for paternal employment. We find some evidence of higher employment rates in the initial 2.5 years after the 2007 birth. The effect is present in all subsamples when using the smaller 1-month bandwidth; with the broader 2-month bandwidth it remains significant only in families with lower maternal education.

To probe these results further, we conduct an additional analysis of labor market outcomes using administrative, Social Security data. Being longitudinal, this data set allows us to restrict observations to our sample of interest: parents who had a child close to July 2007 and another child within the following five years. The results, shown in Table A11, confirm the previously documented short-lived negative employment effect for mothers, driven especially by higher-educated women. However, paternal employment and earnings seem to be unaffected.

[26] There is considerable global evidence on the impact of cash transfer programs on (pregnant) women's nutrition and health status in low-income settings, including outcomes such as food security, dietary diversity, utilization of healthcare services, morbidity, and anemia (Raghunathan et al., 2017; Harris-Fry et al., 2018 and 2022, Gram et al., 2019). There is also evidence that the nutritional status of women before and during pregnancy is important for fetal growth and other pregnancy outcomes (Imdad and Bhutta, 2012).

[27] Note that there is a negative employment effect among lower-educated women in the period 2.5-5 years when we use a 2-month bandwidth. However, the effect on VLBW is driven by births in 0-2.5 years, while the employment effect is found for the period 2.5-5 years, and is present only with a 2-month bandwidth (no effect with 1-month bandwidth).

Table 8
Treatment effects on cohabitation of parents.

	1 month bandwidth			2 months bandwidth		
	Parental cohabitation in the next X years:					
	0–2.5	2.5–5	0–5	0–2.5	2.5–5	0–5
All women	−0.0004	−0.0164	−0.0087	−0.0038	−0.0011	−0.0017
	(0.0109)	(0.0104)	(0.0075)	(0.0076)	(0.0076)	(0.0054)
Observations	*2400*	*2852*	*5252*	*4812*	*5525*	*10,337*
Less educated	−0.0041	−0.0278	−0.0127	−0.0003	−0.0046	0.0106
	(0.0235)	(0.0202)	(0.0147)	(0.0167)	(0.0149)	(0.0108)
Observations	*846*	*1065*	*1911*	*1686*	*1991*	*3677*
More educated	0.0022	−0.0066	0.0038	−0.0060	0.0031	−0.0022
	(0.0114)	(0.0120)	(0.0082)	(0.0081)	(0.0086)	(0.0059)
Observations	*1554*	*1787*	*3341*	*3126*	*3534*	*6660*

Notes: OLS regressions. Cut-off is month of birth in July 2007. No linear trend is included. Covariates: province FE (52 provinces) and maternal characteristics: mother's age and parity in 2007, and her education (below secondary, secondary, more than secondary). Educational variables are excluded in heterogeneity analysis by education. Sample includes births in June-July 2007 (left panel) or May-August 2007 (right panel) to mothers interviewed in the LFS in years 2007–2012. Robust standard errors.

7. Discussion

One of the contributions of this paper is our ability to estimate population-wide effects of a *universal* cash transfer paid in the prenatal period, on children's health outcomes at birth. Even though we find an overall decrease in the fraction of low-birth-weight babies, we also see that this effect is not universal, as it is driven mainly by children from disadvantaged backgrounds.

In comparison to other papers, which focus on transfers to disadvantaged women in the U.S. (Almond et al., 2011; Hoynes et al., 2015) or on cash transfers to poor families in developing countries (Barber and Gertler, 2010; Amarante et al., 2016), our results are qualitatively similar. Most previous studies found a decrease in LBW (<2500 g), even though some papers did not find any effect (e.g. Currie and Cole, 1993 and Currie and Moretti, 2008 for the U.S.). In general, papers focusing on social or tax credit programs in the U.S. found smaller effects, while papers focusing on cash transfers in developing countries estimated larger effects.

In particular, Almond et al. (2011) find an effect of the FSP in the U.S. on LBW in the magnitude of –7% to –12%, while Hoynes et al. (2015) examine the EITC in the U.S. and find an effect of –2% to –3% for a $1000 income increase. Larger effects were found in Latin America, where a social assistance program in Uruguay led to a 19% to 25% decrease in LBW (Amarante et al., 2016), while the well-known conditional cash transfer program Oportunidades in Mexico led to an even larger decrease of 45% (Barber and Gertler, 2010). In our case, we find an effect on LBW only among unmarried women, which is a demographic group similar to that in the literature from the U.S., but our effect size of –33% is substantially larger than in the U.S. and more comparable to the Latin American countries.

The main result of our paper is a decrease in VLBW, which is a more extreme measure than LBW. In the U.S., the effects on VLBW are surprisingly mostly insignificant (Hoynes et al., 2015; Almond et al., 2011 find insignificant results for whites and a significant 14% decrease in VLBW only among blacks). Conversely to that and similarly to us, Amarante et al. (2016) find a large effect on VLBW in the ballpark of –40%, while we find –62% in the full sample and –83% in the (more comparable) poor sample.

One explanation for the sizeable differences in the relative effect sizes between our and previous studies are the differentials in the incidence of lower birth weights. LBW in Spain (approximately 5% in the full sample and 6% in the poor and very poor samples) is half of that in the U.S. (approximately 10% in the entire target population and between 7% and 15% in the different ethnic groups) and of that in Uruguay or Mexico (approximately 10% among poor households). Thus, a similar reduction in LBW or VLBW in absolute terms would result in a larger relative effect in our case, since the baseline LBW or VLBW prevalence is smaller.

In addition, note that the women in our sample received the benefit mostly prior to their pregnancy, while the rest of the literature has focused on cash transfers in utero. This difference in the timing of the transfer might thus be another factor potentially explaining the differences in the magnitudes of the estimated effects.

Some studies have found that the largest effect of additional in-utero income occurs if it is received in the last trimester of pregnancy, and the effect is smaller or insignificant in earlier phases of pregnancy (e.g. Almond et al., 2011). Our effects, stemming from additional income mostly prior to pregnancy, are larger than those previously found for the last trimester of pregnancy, potentially hinting towards a non-linearity of the effect with respect to the timing of the income shock.

Regarding the precise timing of the benefit receipt with respect to pregnancy, we know that families who had a child shortly after the introduction of the benefit (July 2007) received cash payments in late November or in December 2007, while those who opted for tax deductions would be paid mainly between April and August 2008. Since Spanish families whose annual income falls under a certain threshold are exempt from filing a tax declaration, many poor families in our sample likely opted for a cash payment and would have received the payment within half a year (late 2007).

Average birth spacing in our poor sample is 34 months (see Table 3), which implies that the average lower-income woman would have received the benefit more than one year before the start of her next pregnancy. However, we found that the effect on birth

Table 9
Treatment effects on maternal and paternal employment.

	1 month bandwidth			2 months bandwidth		
	Panel A: Maternal employment in the next X years:					
	0–2.5	2.5–5	0–5	0–2.5	2.5–5	0–5
All women	−0.0225	−0.0245	−0.0153	−0.0332**	−0.0434***	−0.0339***
	(0.0190)	(0.0176)	(0.0127)	(0.0134)	(0.0125)	(0.0090)
Observations	*2400*	*2852*	*5252*	*4812*	*5525*	*10,337*
Unmarried women	0.0006	0.0093	0.0162	−0.0332	−0.0147	−0.0181
	(0.0456)	(0.0425)	(0.0312)	(0.0298)	(0.0291)	(0.0201)
Observations	*578*	*574*	*1152*	*1161*	*1116*	*2277*
Married women	−0.0312	−0.0267	−0.0190	−0.0382**	−0.0436***	−0.0364***
	(0.0216)	(0.0195)	(0.0142)	(0.0152)	(0.0140)	(0.0102)
Observations	*1822*	*2278*	*4100*	*3651*	*4409*	*8060*
Less educated women	0.0339	0.0183	0.0243	−0.0373	−0.0460**	−0.0251
	(0.0362)	(0.0313)	(0.0232)	(0.0246)	(0.0221)	(0.0160)
Observations	*846*	*1065*	*1911*	*1686*	*1991*	*3677*
More educated women	0.0237	−0.0369*	−0.0102	−0.0068	−0.0385**	−0.0249**
	(0.0237)	(0.0218)	(0.0157)	(0.0167)	(0.0156)	(0.0112)
Observations	*1554*	*1787*	*3341*	*3126*	*3534*	*6660*
	Panel B: Paternal employment in the next X years:					
	0–2.5	2.5–5	0–5	0–2.5	2.5–5	0–5
All men	0.0346***	−0.0268*	−0.0021	0.0174*	−0.0303***	−0.0070
	(0.0132)	(0.0146)	(0.0100)	(0.0091)	(0.0105)	(0.0071)
Observations	*2208*	*2591*	*4799*	*4431*	*5029*	*9460*
Mother less educated	0.0546*	−0.0456	0.0084	0.0624***	−0.0344	0.0203
	(0.0312)	(0.0307)	(0.0215)	(0.0199)	(0.0218)	(0.0151)
Observations	*742*	*926*	*1668*	*1478*	*1732*	*3210*
Mother more educated	0.0350**	−0.0208	−0.0037	0.0060	−0.0326***	−0.0156**
	(0.0139)	(0.0146)	(0.0103)	(0.0097)	(0.0111)	(0.0073)
Observations	*1466*	*1665*	*3131*	*2953*	*3297*	*6250*
Father less educated	0.0550**	−0.0610**	−0.0218	0.0173	−0.0507**	−0.0143
	(0.0266)	(0.0306)	(0.0201)	(0.0171)	(0.0202)	(0.0134)
Observations	*901*	*990*	*1891*	*1863*	*1987*	*3850*
Father more educated	0.0305**	−0.0113	0.0101	0.0133	−0.0278**	−0.0054
	(0.0142)	(0.0156)	(0.0107)	(0.0098)	(0.0116)	(0.0077)
Observations	*1307*	*1601*	*2908*	*2568*	*3042*	*5610*

Notes: OLS regressions. Cut-off is month of birth in July 2007. No linear trend is included. Covariates in Panel A: province FE (52 provinces) and maternal characteristics: mother's age and parity in 2007, and her education (below secondary, secondary, more than secondary). Covariates in Panel B: province FE (52 provinces) and paternal characteristics: father's age and parity in 2007, and his education (below secondary, secondary, more than secondary). Educational variables are excluded in heterogeneity analysis by mother's education in Panel A, and in heterogeneity analysis by father's education in Panel B. Sample includes births in June-July 2007 (left panel) or May-August 2007 (right panel) to mothers (Panel A) and fathers (Panel B) interviewed in the LFS in years 2007–2012. Robust standard errors.

weight is concentrated in families who had the next child relatively early, within 2.5 years (Table 6). Average birth spacing in this subsample is 21 months, implying that the average affected woman would have received the benefit in November-December 2007, and would have conceived her next child in July 2008, i.e. 8 months after the benefit receipt.

Particularly relevant in our study is also the interaction between poverty and the child's biological disadvantage at birth (prematurity). The positive health effects that we find are particularly large among premature babies in poor families, and they go beyond a lower probability of low birth weight. In particular, we find a positive and large effect on average birth weight as well (228 g or 13%, see Table A8). Despite these large effects, the group concerned is relatively small (982 out of 41,281 observations or 2.4% of our estimation sample) and one could thus dismiss the practical relevance of these results. However, these effects are policy-relevant because "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 (<1200 g). Additionally, according to Schwartz (1989), newborns below 2500 g account for 57% of neonatal hospital care costs. In other words, even if only a relatively smaller group of infants (such as poor premature babies) would have benefitted from the child benefit, the benefits (and avoided costs) for this particular group may still have been very large.

In terms of the underlying channels, we find no effect of the benefit on gestational length, i.e. pregnancy weeks at birth, or on prematurity, which is in line with Almond et al. (2011) and Amarante et al. (2016).[28] Both their and our evidence suggests that the main biological factor behind the observed effects of cash transfers on low birth weight of the next child is faster intrauterine growth, i.e. fetuses being larger and heavier conditional on the pregnancy week, and not changes in gestational length.

When we explore the underlying causes of improvements in intrauterine growth, some of our results are in line with the previous literature, while others are different. Hoynes et al. (2015) and Amarante et al. (2016) find that cash transfers decreased maternal employment in the U.S. and in Uruguay, respectively, whereas we find essentially no effect in the relevant groups of population (unmarried and lower-educated women). When we examine paternal employment – a channel not considered in the previous literature, we find no systematic effects. In terms of family structure, while Amarante et al. (2016) find a decrease in out-of-wedlock births, we find no effect on parental cohabitation.[29] One of the potential channels (and confounding factors) that both us and the previous literature are able to rule out is a potential effect on fertility.

In terms of additional factors that we are able to explore and confirm as channels are improved maternal health, improved maternal nutrition, and more of the healthy and less of the unhealthy habits. Previous literature was largely lacking data and could not explore these factors. For example, Almond et al. (2011) and Amarante et al. (2016) propose maternal nutrition as a candidate channel by excluding other candidate factors or by providing suggestive evidence, but they lack the data to confirm it as a channel. Similarly, maternal stress has been proposed as a relevant channel but could not be tested. In our data, we find improvements in mother's health (measured by her BMI), as well as in her self-reported overall and mental health, confirming the BMI-based results and hinting towards less stress. We also find evidence consistent with better maternal nutrition (more consumption of healthy and less consumption of unhealthy foods), more healthy habits (sleep and physical exercise) and less unhealthy habits (drinking and smoking).[30] These last results are backed by Hoynes et al. (2015), who also found a decrease in smoking and drinking.[31]

Finally, it is worth mentioning that Spain entered a deep economic crisis in 2008. The child benefit, whose payments started in late 2007, may have helped families to cushion the crisis. In particular, it may have helped low-income women to avoid poor nutrition, risky behaviors, or poor health. The cash transfer may have had smaller effects on mothers and children in the absence of the crisis, which is worth keeping in mind when considering the external validity of our results.

8. Conclusion

In this paper, we show that a universal child benefit introduced in Spain in the late 2000s had (unintended) positive effects on health outcomes of children, whose mothers received the cash transfer after a previous birth. Following a regression discontinuity design based on the exact date of birth, we show that mothers who received the €2500 lump-sum benefit later had children who were less likely to be born with low birth weight. This effect is driven by children born in disadvantaged families, such as poor households, unmarried women, and women with little education.

In terms of channels, we are able to shed light on both the immediate biological causes as well as the social and behavioral factors underlying them. Among the two potential immediate causes of the observed positive effects on birth weight, we find evidence for faster intrauterine growth, but not for longer gestation. The underlying channels that could contribute to faster intrauterine growth include better maternal health, which in turn is likely impacted by maternal nutrition, maternal habits and behaviors, as well as other factors such as changing family structure and parental employment. We find evidence that the benefit receipt improved mothers' overall health, together with their nutrition and patterns of healthy and unhealthy habits. We find no evidence of changing cohabitation patterns or employment in the affected groups.

Overall, the child benefit appears to have been an effective tool in reducing the incidence of very low birth weight in certain population groups, through improved maternal health in utero. The effect was unintended, and it was concentrated among children conceived in poor households relatively soon after the benefit receipt. The large amount of the lump-sum payment as well as the onset of a deep economic crisis in Spain likely contributed to the observed positive impact. Births that took place significantly after the payments were made, did not see the same improvement.

From a policy perspective, we find an effect on low birth weight among children conceived *after* the benefit receipt, which suggests that cash transfers matter not only during the in-utero period (as found in previous studies), but also prior to conception. Earlier evidence on the causal effects of cash transfers on child health and development in poor families has been mixed, as discussed in Borra et al. (2021). Some recent research suggests that targeting pregnant women may be more effective than later interventions, given the strong persistence of fetal health effects (Amarante et al., 2016; Hoynes et al., 2015). Our results suggest that the impact may be stronger if women are targeted even earlier, (shortly) before conception.

Declarations of Competing Interest

None.

[28] Note that even though Hoynes et al. (2015) do find a statistically significant decrease in prematurity, it is economically negligible (1% effect size).

[29] Hoynes et al. (2015) report that the previous literature has found no effect of the EITC on family formation but they do not estimate the effect themselves.

[30] Note that these effects are estimated in a very small sample and are individually insignificant. However, jointly they show a substantial improvement in our aggregate measures of maternal health.

[31] Unlike some previous studies, we have no information on prenatal care. Prenatal care utilization was found to be a relevant factor in the U.S. (Hoynes et al., 2015), while better quality prenatal care was identified as an important mechanism in Mexico (Barber and Gertler, 2010).

CRediT authorship contribution statement

Libertad González: Conceptualization, Funding acquisition, Methodology, Validation, Writing – original draft, Writing – review & editing. **Sofia Trommlerová:** Conceptualization, Data curation, Formal analysis, Methodology, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2022.102622.

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