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The causal effect of an income shock on children's human capital

Cristina Borra, Ana Costa-Ramón,
Libertad González, and Almudena Sevilla

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Cristina Borra*

(Universidad de Sevilla)

Ana Costa-Ramón

(University of Zurich)

Libertad González*

(Universitat Pompeu Fabra and Barcelona GSE)

Almudena Sevilla*

(University College London)

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Abstract: We investigate the causal impact of a generous unconditional cash transfer at birth on children's later health outcomes and academic performance. Using rich administrative data, we take advantage of the unexpected introduction of a "baby bonus" in Spain in 2007, and implement a difference-in-discontinuity approach comparing children born in the surrounding months in different years. We find that the subsidy did not have a significant effect on health outcomes during childhood, nor on test scores in primary school. In line with this result, we show that the benefit did not affect the main potential mechanisms that could in turn have affected children's health and academic performance. Our results contribute to understanding which interventions are effective at improving children's health and human capital formation.

JEL Codes: I12, J13, H31, H24.
Keywords: Children, health, education, income shock, child benefit, Spain.

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1. Introduction

Cash transfers to families with children can help mitigate growing socio-economic inequalities stemming from gaps in early childhood investments. Recent proposals by the Biden administration for direct payments to families with children have brought back to the debate the difficulty in balancing the trade-offs between the two most popular policy options currently employed to reduce child poverty.

Highly targeted schemes, while cheaper, can be complicated to administer and risk not reaching the targeted population because of the complexity associated with claiming benefits. In the US, about 20 percent of eligible taxpayers do not claim $7.3 billion of the Earned Income Tax Credit (EITC) each tax year (TIGTA 2018). Unconditional universal cash-transfer schemes are easier to administer. Their widespread nature, however, tends to diminish the cash payout per household to restrain program costs, which may compromise their effectiveness.

In this paper, we exploit the unexpected introduction of a one-off universal child benefit paid at birth to credibly estimate the causal effect of an income shock on a rich set of child outcomes available in administrative registers.

Parental wealth is highly associated with better child outcomes at every stage of the lifecycle, starting early-on in a child's life. Moreover, traits determined during early childhood explain a large proportion of the variability in lifetime earnings (Cunha and Heckman 2007; Currie 2009). As a result, diverging destinies in childhood are likely to lead to intergenerational persistence of economic inequality (Black and Devereux 2011; Corak 2013).

Theories of child development predict large benefits of parental wealth that start early-on in the child's life and accumulate thereafter. Investment models emphasize the access to better material resources, such as better food and medical care, better housing, and more human-capital enhancing parental time (Becker and Tomes 1976; Caucutt and Lochner 2020). Parental stress models highlight parental emotional wellbeing and stress reduction as parental wealth increases (Yeung, Linver, and Brooks-Gunn 2002; Bradley and Corwyn 2002; Milligan and Stabile 2011; Akee et al. 2018; Conger, Rueter, and Elder 1999). Human-capital enhancing parenting behavior as a result of reduced stress is a potential mediator between wealth and children's outcomes, promoting warm and non-coercive parenting, which is in turn associated with better child outcomes (Fiorini and Keane 2014; Fryer Jr., Levitt, and List 2015; Doepke and Zilibotti 2017).

We exploit the introduction of a new, universal, one-time payment child benefit introduced in Spain in 2007, to estimate the causal effect of parental income on children's health and development. The benefit, which was announced retrospectively on July 3rd, was entirely unexpected, and all mothers giving birth from July 1st onwards were eligible to receive it immediately after birth (González 2013). The magnitude of the benefit was €2,500 (about $3,800), almost 4.5 times the monthly (gross) minimum wage for a full-time worker and 9 percent of the average annual disposable income (about €28,787, Spanish National Statistics Institute). The unexpected nature of the announcement implied no anticipation effects on the part of households, as it was not possible to shift the timing of births or change in-utero investments.

We use a rich set of administrative registers on children's test scores, primary health care, and hospital discharge records collected at several points during the child's early years. The large register data allow us to follow children's health trajectories, and to test for the theoretical predictions in the literature regarding the age, sex, and socio-economic status of the child.

Our identification strategy relies on the sharp eligibility cutoff of the policy by date of birth. We use a difference-in-discontinuity design, similar to Bertrand, Mogstad, and

Mountjoy's (2020), which compares the gaps in health and development outcomes between children born in July 2007 (right after the baby bonus) versus the outcomes of children born in June of 2007 (right before the baby bonus), relative to children born in June and July in the previous year.

We show that a generous one-time cash transfer to families shortly after having a child does not have any visible impact on the targeted child's later health and educational outcomes. The distribution of the number of births and other pre-determined variables around the cut-off shows no distinct jump, which rules out that our results may be driven by other discontinuities around the threshold. Additional checks from a difference-in-discontinuity model of a placebo introduction of the baby-check in 2006, using children born in 2005 as a control, suggest that the discontinuity at July 1st is constant over time in the absence of the policy change.

Overall, our analysis based on large high-quality register data allows us to precisely bound our estimates to a tight interval around zero, so that we can comfortably reject positive effects of the magnitudes reported in some previous studies (see Almond, Currie, and Duque 2018 and Cooper and Stewart 2020 for reviews). Age-by-age estimates from birth to middle childhood as well as heterogeneous analyses by gender and socio-economic status also allow us to reject any significant effects of income on the child's health and educational outcomes in any of the subsamples.

We also find that the policy shock did not lead to either short- or long-run impact on parental behaviors. Using a variety of representative large-scale Spanish surveys, we fail to find significant effects on subsequent fertility, living arrangements, or parental time investments. Consistent with the findings in Akee et al. (2010) and Black et al. (2014), we also rule out any impact on maternal labor force participation and childcare use. Our estimates cannot be robustly bounded away from zero, although the nature of the surveys prevents us from attaining the precision afforded by the administrative records.

The last section offers a comparative benchmark for our results by placing them adjacent to the effect sizes in other relevant papers investigating the causal relationship between income shocks and children's outcomes in developed countries. We study child outcomes such as standardized test scores, anthropometric measures, and hospitalization rates, which have been extensively used in previous work. Of the seven studies that satisfied our inclusion criteria examining health and student outcomes during childhood, all but one report positive effects. Our comparative exercise reveals that the heterogeneity stemming from the type of outcome measure analyzed or the socio-economic characteristics of the targeted population cannot consistently explain the lack of positive marginal effects implied by our causal estimates.

We also fail to find any effect of the universal baby bonus on a sample of low-income households, which have traditionally been the focus of welfare-to-work and conditional cash programs analyzed in the literature (Akee et al. 2010; Black et al. 2014; Dahl and Lochner 2017; Duncan, Morris, and Rodrigues 2011; Milligan and Stabile 2011).

We are able to rule out that the one-time €2,500 baby bonus may have been too modest to be able to generate any changes in future income expectations or lifetime income (Blau 1999; Dahl and Lochner 2012). Studies analyzing income changes in the same order of magnitude find large impacts on children's health and cognitive outcomes.

Our lack of effects is more in line with the results in recent lottery studies (Cesarini et al., 2016), who find close to zero effects on child outcomes using income shocks that are an order of magnitude larger than the increase in annual income generated by the

Spanish baby bonus. One commonality between lotteries and universal income transfers like the Spanish baby bonus is that income supplements are not conditioned on household time-use investments or expenditures. It is plausible that universal income supplement policies like the Spanish baby bonus are more effective if their receipt is conditioned on expenditure or investment behaviors that directly affect children's outcomes, as is the case in most of the welfare-to-work experiments and childcare subsidy programs finding positive effects from income shocks.

Our paper contributes to the extensive literature aiming to identify the causal effect of income shocks on later-life child development (see Almond, Currie, and Duque (2018) and Cooper and Stewart (2020) for recent literature reviews). The rich nature of our administrative data allows us to causally assess the effect of an income shock on a range of middle-childhood health and educational outcomes, an important period in a child's life that is often overlooked (Almond, Currie, and Duque 2018; Garces, Thomas, and Currie 2002). Our age-by-age estimates contribute to the understanding of the medium-term effects of early-childhood cash-transfer policies that have been documented to successfully impact later-life socio-economic outcomes. With a few exceptions, most studies do not study the effect of income shocks on outcomes of children in middle-childhood. The evidence documented here is consistent with the results from the broader literature evaluating early intervention programs on the "missing-middle", which exhibit early and long-term effects but no effects at ages eight or nine (Almond and Currie 2011).

Compared to most previous studies evaluating income shocks during early life, the nature of our policy shock allows us to unambiguously separate a pure income effect from difficult-to-model substitution effects induced by the kind of policy changes analyzed in most of the previous work (Almond, Currie, and Duque 2018; Heckman and

Mosso 2014). Policy experiments entailing no-strings-attached income transfers are difficult to come by, so a common tool in the literature to identify pure income effects is to analyze income shocks stemming from lotteries (Cesarini et al. 2016; Bleakley and Ferrie 2016). The universal nature of the policy ensures that we can successfully overcome the two most important limitations in lottery studies (Bagues and Esteve-Volart 2016). First, we do not have external validity limitations from lottery studies arising from the fact that the sample of players may not necessarily be representative of the wider population, given that all Spanish mothers giving birth after July 1st, 2007 were eligible for the bonus. Second, we avoid the so-called *fungibility* problems that restrict the external validity of lottery prize responses due to its inherent difference from other forms of more common cash government programs such as ours (Thaler 1990).[1]

Lastly, our high-quality large-scale survey data allow us to examine the potential mechanisms that may impact child development. Unlike targeted schemes such as the Earned Income Tax Credit (EITC) in the US and the Welfare to Work experiments in the US and Canada, which are usually contingent on work, unconditional universal programs may discourage recipients from seeking paid work (Hoynes and Rothstein 2019; Jones and Marinescu 2018). We find no evidence of substitution effects leading to negative work incentives for mothers that were exposed to the bonus. This is consistent with recent evidence from evaluations of larger universal basic income experiments, such as the annual income transfer of $2,000 in the Alaska Permanent Fund or the €560 monthly income transfer in the Finnish Basic Income Experiment,

[1] For instance, unlike money received from government programs, lottery winners may use increased cash to ‘play with the house money’, taking on additional risks (Thaler and Johnson 1990; Hankins, Hoekstra, and Skiba 2011). Recent evidence has also shown that the specific source of income matters and labels can impact how funds are spent (Beatty et al. 2014; Hastings and Shapiro 2018).

found to have no impact on recipients' labor supply resulting from the additional cash (Jones and Marinescu 2018; Kangas et al. 2020).

The remainder of the paper is organized as follows. Section 2 describes the institutional framework. Section 3 lays out the identification strategy. Section 4 introduces the data and provides key descriptive statistics. Section 4 reports our main results. Section 5 presents evidence on the mechanisms, and Section 6 concludes.

2. Institutional background

Aimed at improving families' well-being, most OECD countries offer some type of financial aid to households with young children, spending on average 1.1 percent of GDP on these programs (OECD 2020). Financial support may take place either through the tax system (credits and allowances) or through cash transfers to households with young children. Targeted (means-tested) cash transfers at birth that continue throughout a child's life have the potential to reduce inequality, but are expensive to run and may be subject to qualifying errors. For instance, in 1995, the United States' annual administrative costs were \$3.7 billion for food stamps and \$3.5 billion for Aid to Families with Dependent Children (AFDC), while \$4.4 billion were claimed erroneously through the Earned Income Tax Credit (EITC) (Hotz, Hotz, and Scholz 2003). One-off universal cash transfer programs, which target the entire population, are particularly attractive to governments because of their simplicity and low administrative costs.

The Spanish universal child benefit was first announced by the Spanish president on July 3, 2007. The subsidy would grant all mothers with a child born after July 1st, 2007, a one-time bonus of €2,500. In addition to the date of birth, the only other eligibility condition would be that the beneficiary (usually the mother) was a legal resident in Spain for at least two years before giving birth. Additionally, the law introduced an extra €1,000 subsidy for lower-income families with at least 3 children including the newborn, single-parent families, mothers with a degree of disability higher than 65 percent, and all families having a child with at least a 33 percent degree of disability.

The explicit goal of the new policy was twofold. As stated in the law, the benefit was meant to help parents cope with the extra expenditures associated with childbirth, while also encouraging fertility in the face of Spain's prevailing low birth rates and ageing population. The law also mentioned an aim to facilitate the balance of work and family as well as help maintain the living standards of low-income families.

Before this reform, Spain already had several conditional child-related tax benefits, and most Spanish regions had some form of universal child benefit for their citizens. However, the newly announced cash transfer was sizably larger than most of them. To contextualize the size of the subsidy, we can compare it with monthly earnings. In 2007, the monthly gross minimum wage for a full-time job in Spain was €570.6, and about 20 percent of working women earned the minimum wage or below (2007 Wage Structure Survey). Thus, the benefit was equivalent to 4.4 months of pay for a low-wage, full-time worker. Similarly, assuming an equalizing factor of about 2 individuals per household, the child benefit represented about 11 percent of the median and 17 percent of the bottom quartiles of annual household income.[2]

While the first subsidies weren't paid until late November, the take-up was still large during the first year: tax authorities reported paying the benefit to 161,983 families in 2007, accounting for 65 percent of all births taking place after July 1st that year. This 65 percent take-up could be seen as a lower bound, as some of the births

[2] The median and bottom quartiles of annual equivalized per capita income in Spain in 2007 were €11,645 and €7,740, respectively (Eurostat 2020).

taking place in December would have requested the benefit in early 2008. As González (2013) points out, in the next year, take-up increased noticeably, reaching 95 percent of all births taking place in Spain during 2008.

3. Data

Our aim is to measure the impact of the policy on children's health and educational outcomes from early to middle childhood. To that end, we use a rich set of registers from which we derive the main health and educational outcome variables. Table 1 gives a summary overview of the administrative data sources used, containing the register name, the unit of observation in the original data set, a brief data description, the main outcomes measured, and the available controls.

3.1 Data Sources

Panel A in Table 1 describes the main features of the registers containing health outcomes, which include anthropometric measures, primary health problems, and hospitalization outcomes at different points in a child's life.

We use the 2011-2015 Spanish ***Primary Care Clinical Dataset*** (*Base de Datos Clínicos de Atención Primaria*, BDCAP), which collects annual standardized clinical data from a 10 percent random sample of primary-care electronic health records of the Spanish population (about 5 million observations). BDCAP is curated and maintained by the Spanish Ministry of Health and includes information on health problems, referrals, prescriptions, and diagnostic procedures. Health problems are coded using the International Classification of Primary Care (ICPC-2). Because this dataset starts in 2011, it only covers children in our sample from 5 years of age.

We complement this register with the 2006-2011 ***Primary Care Drugs Prescription Dataset*** (*Base de Datos para la Investigación Farmacoepidemiológica en Atención Primaria*, BIFAP) for children aged 0 to 4. BIFAP is an administrative dataset that contains clinical data from the electronic health records of all patients who attended primary care for 5 out of the 16 autonomous regions in Spain and about 20 percent of patients in two other autonomous regions, covering 17.3 percent of all Spanish patients attending primary care (Maciá-Martínez et al. 2020). BIFAP is curated and maintained by the Spanish Agency of Medicines and includes information on anthropometric measures, health problems (coded using the ICPC-2 classification), referrals, prescriptions, and diagnostic procedures.[3]

We also use the 2006-2015 ***Hospital Morbidity Survey***, an annual census of all overnight hospitalizations in Spain that includes 96 percent of hospitals, both public and private, and 99 percent of all overnight hospital stays (see Borra, González, and Sevilla 2019). This registry contains information at the level of the individual hospital stay, such as the date of release, age (in years, months and days), main diagnosis, and length of stay. Because not every child is hospitalized in a given year, our population data of overnight stays includes a selected sample of children. We therefore conduct the analysis at the date of birth level by computing daily hospitalization rates. For each date in our sample, we compute daily hospitalization rates (by age and diagnosis) for children born on a given date as the number of hospital stays (from the 2006-2015 Hospital Morbidity Survey) divided by the total number of children born on that date (from 2006-2007 Vital Statistics data).

To that end, we link the 2006-2015 Hospital Morbidity Survey to the micro data from ***birth certificates*** in the vital statistics register, a population-level dataset providing detailed information on the universe of births taking place annually in Spain as recorded

[3] BIFAP is collected by collaborating physicians and offers basically population-level data on 5 of the autonomous communities (Aragon, Asturias, Castille-Leon, Murcia and Navarra). As shown in Maciá-Martínez et al. (2020) the age and sex distribution of the dataset is representative of the Spanish population.

in the official national registry, supplemented with the files containing the exact date of birth for each newborn for the years 2006 and 2007, purchased from the Spanish National Statistical Institute.[4] The Hospital Morbidity Survey does not provide direct information on procedures, drugs administered, or costs. Diagnoses are provided at the 3-digit level and grouped in 17 "chapters", following the International Classification of Diseases (ICD-9-CM).

Panel B in Table 1 describes the datasets from our education registers. We use several years of administrative data on children's ***primary school performance*** in second grade from two large Spanish regions making up approximately 36 percent percent of the total Spanish student population. Andalusian Diagnostic Tests Data, provided by the Andalusian Agency of Educational Evaluation (*Agencia Andaluza de Evaluación Educativa*-AGAEVE), consists of annual external diagnostic-assessment tests for the whole population of Andalusian 2nd graders, with the objective of evaluating students' basic competences.

Catalonian Grades Data, provided by the Catalan Statistical Institute (*Institut d'Estadística de Catalunya* -IDESCAT), consists of teachers' internal end-of-year subject grades for 2nd graders attending public schools. In the case of Catalonia, the dataset provides information for 144,213 students, representing 70 percent of Catalonia's public-school student body during the 2013/14 and 2014/15 academic years. In the case of Andalusia, we have information for 279,917 students in the 2013/14, 2015/15, and 2015/16 academic years, the universe of Andalusian students across that period.

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4 Parents are required to register the birth in a Civil Registry office between 24 hours and 8 days after the delivery takes place, by presenting the original birth certificate provided by the health center (see Casado, 2008, p. 56). The birth certificate is filled out by the hospital (not the parents) at the time of birth, and contains the date and time of birth, as well as the doctor's signature.

Both datasets include performance in Mathematics and Spanish. Additionally, the Catalonian dataset includes performance in Catalan and English as well as the overall GPA grade for the same cohorts obtained in 3rd grade the following year for 2014/15 and 2015/16. For Andalusia, we also have information on diagnostic tests of students who were retained. Administrative data from Catalonia and Andalusia are also available for the year 2015/16, that is, for the cohort born in 2008. We use this information together with data on the children born in June and July 2007 to perform additional robustness checks.

Column 5 in Table 1 shows the demographic characteristics available in each dataset. The primary health data contain basic demographic information on the child and limited family income information. Hospitalization registers contain some additional variables such as province and sex, while the education registers are the most complete, containing several socio-demographic characteristics of parents and their children.

3.2 Outcome variables

We study outcomes that have previously been shown to differ between children with different access to material resources, including respiratory problems, injuries, mental disorders, BMI, obesity, and cognitive test scores (Currie 2009; Heckman and Mosso 2014; Almond, Currie, and Duque 2018). In all our analyses, we select children born in June and July of 2006 and 2007. Table 2 reports summary statistics for this sample.

There are three sets of primary health outcomes observed for children between the ages of 0 and 4 (see Panel A of Table 2). The first set of primary health outcomes refers to ***anthropometric measures*** at age 4. Children's height and weight are directly recorded in the registers. We derive height-for-age, weight-for-age, and BMI z-scores using the World Health Organization's (WHO 2006) universally applicable growth standards. Our sample of Spanish children (about 3,921 four-year-olds) is, on average, 0.28 standard deviation units heavier than the international WHO averages. They also present higher BMI by 0.35 standard deviation units. We also construct indicators for overweight (BMI at or above the 85th percentile and below the 95th percentile) and obesity (BMI at or above the 95th percentile) using Cole et al. (2000) standards. Our sample of children is 15 percent overweight and 3 percent obese.

The second set of primary health outcomes recorded in the registers is the ***number of health problems and referrals*** for children up to 8 years of age as well as the ***number of primary healthcare visits and prescriptions*** for children 0-4. Our sample includes 12,062 children at ages 0 to 4 and 16,435 at ages 5 to 8. Health problems are episodes of care with at least one diagnosis. On average, Spanish children have 23 health problems in their initial four years of life, and just 5 at ages 5-8. Most of the health problems do not need the intervention of specialist physicians.

Children are referred to specialists 1.5 times during their first 4 years of life and 0.2 times from ages 5 to 8. The number of visits to the doctor during the first 4 years of life is about 43, larger than the corresponding number of health problems because of well-child visits. We also study cause-specific health problems: respiratory problems (ICPC-2 Chapter R), infections (ICPC-2 Process codes for infections), injuries (ICPC-2 Process codes for injuries), and psychological problems (ICPC-2 Chapter P).

The third set of primary health outcomes is daily ***inpatient hospitalization rates***. We consider inpatient hospitalization rates for the most common health problems in children: respiratory disease (ICD-9 Chapter 8), infections (ICD-9 Chapter 1), external causes (accidents, injuries, and poisoning) (ICD-9 Chapter 17), mental disorders (ICD-9 Chapter 5), and an omnibus (‘‘all-cause’’) category covering all hospitalizations with the exception of perinatal health problems. Our sample consists of 122 days, one for each potential date of birth from June and July of 2006 and 2007. Panel A in Table 2 also shows that there were 694 hospitalizations between the ages of 0 and 8 for every 1,000 children born in a given day. On average, 128 hospitalizations were due to respiratory disease, 101 to infections, 35 to injuries, and 2 to mental disorders.

Our main educational outcomes are ***Spanish and Mathematics student performance in second grade***, i.e., students of ages 7 or 8 for children born in June and July 2006 and 2007, respectively (see Panel B in Table 2). For Catalonia, we exclude children born outside Spain, since they were not eligible for the benefit. That information is not available in the Andalusian sample.[5] Our sample includes 32,002 children in Andalusia and 15,696 in Catalonia. In Andalusia, student performance is measured by a continuous variable with maximum scores of 40 and minimum of 10. In Catalonia, student performance is given in annual average grades, a categorical variable that takes values 2.5 (fail), 5 (pass), 6 (C), 7.5 (B), and 9.5 (A).

We standardize all scores at the subject-cohort-region level. The different grading, together with the fact that Andalusian measures are external evaluations while Catalonian grades are internal evaluations, means that we need to study each region separately. In Andalusia, we also know whether students were retained in a grade. Panel C in Table 3 shows that about 4.7 percent of the students are repeaters. For Catalonia, we also have information on two additional 2nd-grade subjects, Catalan and English, and the overall 3rd grade mean Grade Point Average (GPA). While the 3rd grade GPA is a continuous variable, 2nd grade subject grades are categorical variables, similar to Spanish and Mathematics grades. We also standardize these additional scores to have a mean of zero and a standard deviation of 1 at the subject-cohort level.

[5] According to Population Figures of the Spanish National Institute, approximately less than 3 percent of the Andalusian population aged 5 to 9 years was foreign born on January 1st, 2015.

4. Identification Strategy

Our research design builds on the policy's sharp eligibility cutoff by date of birth. Children born after July 1st, 2007 were eligible to receive the bonus and constitute the treatment group; children born before that date are the control group. A regression discontinuity (RD) design that compares outcomes for children born right after the cutoff to those born right before is the common research method to identify causal impacts of such a policy with a clean cutoff rule. However, a plausible concern in our specific setting is that, even in the small window where treatment assignment can be assumed to be quasi-random, the running variable might still be related to potential outcomes. For instance, the medical literature has identified significant associations between month of birth and the risk of different diseases such as asthma, attention deficit hyperactivity disorder, and myopia. Boland et al. (2015) found that individuals born in July, together with those born in May, had lower risks at different diseases. Similarly, Berniell and Estrada (2020) show that Spanish children born before July tend to have better-educated mothers than those born after.

To the extent that seasonality affects health and schooling outcomes in a non-monotonic way, RD estimates could potentially be biased. We address potential seasonality concerns by estimating the causal impacts of the policy change using a difference-in-discontinuity design similar to Grembi, Nannicini, and Troiano (2016) and Bertrand, Mogstad, and Mountjoy (2020). In essence, this model implements an RD strategy using as additional controls children born on the same dates in an earlier year, in order to take into account unobserved characteristics of children born in different months of the year. That is, our model estimates the discontinuity in outcomes between children born before and after the cutoff of July 1st, 2007, subtracting any discontinuity between children born before and after the July 1st cutoff in 2006. For estimation models where the dependent variable is measured at the child level, we use the following equation:

$$Y_i = \alpha + \gamma_1 Reform_i + \gamma_2 Post_i + \beta Reform_i * Post_i + f(Date_i) * [\gamma_3 + \gamma_4 Reform_i + \gamma_5 Post_i + \gamma_6 Reform_i * Post_i] + \varepsilon_i \quad (1)$$

where Y_i denotes the studied outcome of child i. $Date_i$ is the running variable defined as the difference between the date of birth of the child and the July 1st cutoff within each window, $Reform_i$ is an indicator variable equal to 1 if child i was born in the reform window of ± 30 days surrounding the cutoff date July 1, 2007, and $Post_i$ is an indicator variable that is equal to 1 if the child was born after the July 1st cutoff in either year (2006 and 2007). The interactions with $Date_i$ allow slopes to vary arbitrarily on each side of the cutoff as well as across the reform vs. the control windows.[6] We cluster standard errors by date of birth.

In the case of models where the dependent variable is measured as a daily-aggregate, Equation 1 takes the form:

$$Y_t = \alpha + \gamma_1 Reform_t + \gamma_2 Post_t + \beta Reform_t * Post_t + f(Date_t) * [\gamma_3 + \gamma_4 Reform_t + \gamma_5 Post_t + \gamma_6 Reform_t * Post_t] + \varepsilon_t \quad (2)$$

where Y_t denotes the average outcome for children born in date t. $Date_t$ is the running variable defined as the difference between the date of birth t and the July 1st cutoff within each window. $Reform_t$ is an indicator variable equal to 1 if the date belongs to

[6] The exact date of birth is not available in the BIFAP data set. We use information on month and year of birth instead and estimate equation 1 as: $Y_i = \alpha + \gamma_1 Reform_i + \gamma_2 Post_i + \beta Reform_i * Post_i + \varepsilon_i$, where we assume that the treatment is as-if randomly assigned within the smallest window possible, that is, one month around the cutoff (D. S. Lee and Lemieux 2010; Cattaneo, Titiunik, and Vazquez-Bare 2017).

the reform window of ± 30 days surrounding July 1, 2007, and $Post_t$ is an indicator variable that is equal to 1 if the date is after the July 1st cutoff of either year. The interactions with $Date_i$ allow slopes to vary arbitrarily on each side of the cutoff as well as across the reform vs. the control windows. We cluster standard errors by date of birth.

In both equations, β is our main parameter of interest and captures the discontinuity in health and schooling outcomes caused by the introduction of the universal child benefit, controlling for the differences that may exist between children born right before and after July 1st in regular years, regardless of the reform. We also explore the possibility of heterogeneous effects of the reform, analyzing results by sex and age of the child and by socioeconomic status of the family.

Our research design assumes, first, that potential outcomes are continuous at the July 1st threshold. To back this assumption, we show in Panel A of Figure 1 that there was no differential change in the daily number of births around the cutoff date in 2007 compared to 2006. Consistent with this, Panel B shows that there is no bunching in the number of births around the July 1st cutoff in 2007, and we also show that the pattern in the number of births around July 1st in 2006 is very similar (Panel C). The absence of strategic sorting around the July 1st cutoff in 2007 is consistent with the policy being introduced unexpectedly, using a date in the past as an eligibility cutoff and consequently preventing birth delays with the aim of qualifying for the policy (González 2013).

To further demonstrate that outcomes are continuous at the July 1st threshold, we also show that there is no difference in discontinuities for available pre-determined variables (Figure 2). We estimate the model in Equation (1) with pre-determined child and family characteristics as outcome variables. None of the point estimates, plotted in

Figure 2, are significant at the 5 percent level, suggesting again that there is no differential selection of babies around the July 1st cutoff in 2007, compared to 2006. In Section 5, we additionally show that controlling for pre-determined covariates in the baseline estimations leave point estimates almost unchanged while increasing accuracy, as expected.

A second identification assumption of our estimation strategy is that, in the absence of the policy change, the effect of being born after July 1st is constant over time (see Grembi, Nannicini, and Troiano 2016). This is analogous to the parallel trends assumption for difference-in-differences, which under our specification must hold only for the observations in a tight interval around the policy change. To test for this assumption, we estimate the difference-in-discontinuity impact of a placebo introduction of the baby-check in 2006, using children born in 2005 as a control. We find that the differences in the outcomes between children born before and after July 1st are constant over time before the introduction of the baby-check, with no significant effect for the placebo policy. Table A.1 in the Appendix shows no significant impacts of being born after July 1st on primary healthcare outcomes of children 5 to 8 years-old born in 2006 compared to similar children born in 2005. This confirms that there are no pre-existing systematic differences in the outcomes before the policy change. [7]

[7] We have no data for the 2005 cohort on the other datasets, but with BDCAP data we show that this is not a concern. Table A.2 presents the results of another placebo exercise that finds that there are no significant impacts of being born after July 1st on education outcomes for Andalusian children in 2009 when compared to children born in 2008.

5. Results

5.1. Main Results

Health Outcomes

Results on anthropometric measures from estimating Equation (1) are presented in Table 3. The first three outcomes in columns (1) to (3) are normalized so that the effect size estimates can be interpreted in standard deviation units. The estimated effects range between 0.02 and 0.10 standard deviation units, with a 95 percent CI ranging from -0.12 to 0.23. We can therefore reject impacts larger than 15 percent of a standard deviation for height, 19 percent of a standard deviation for weight, and 23 percent of a standard deviation for BMI.[8]

We do not find evidence that income affects the likelihood of being overweight or obese at 4 years of age. These are both negative outcomes that increased income may reduce. We can reject impact reductions in overweight probabilities below 18 percent.[9]

In sum, we find no evidence that income impacts any of the six anthropometric measures considered, and cannot rule out the null hypothesis that the effect of the cash transfer is zero.

Table 4 summarizes the impact of the baby bonus on healthcare measures obtained by estimating Equation (1). Panel A reports results for primary care outcomes for 0 to 4 year-olds from BIFAP data, and Panel B reports results for primary care outcomes for 5 to 8 year-olds from BDCAP data.

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[8] For height, the estimated coefficient of 0.02 (standard error 0.066) leads to an upper limit of 0.151 at the 95 percent level. Dividing that number by the sample standard deviation of 1.02 leads to an upper confidence impact of 0.15 standard deviations. For weight, the estimated coefficient of 0.064 leads to an upper confidence impact of 0.19 standards, using a similar method. Similarly, for BMI, the estimated coefficient of 0.097 leads to an upper confidence impact of 0.23 standard deviations.

[9] For overweight, the lower confidence impact is -0.026, that is, 18.3 percent for an average rate of 0.142 overweight children in the sample.

The estimated impact of the baby-check on health problems, referrals, and drug prescriptions in primary care are shown in columns (1), (2), and (3), while column (4) displays results for overall visits in primary care. None of the estimated effects for the introduction of the policy are statistically significant. For the primary care health outcomes in Panels A and B, the magnitude of the €2,500 income shock's effect is between 0.01 and 0.02 standard deviation units in those cases where the coefficient is negative, indicating a health improvement. In all cases, estimates are not statistically distinguishable from zero.[10] Taken together, the precision of the estimated effects is high, and we can bound the effect within ± 0.09 standard deviation units.

Columns 5 to 8 in Table 4 display the results for different health problems. As before, a €2,500 increase in income does not have a statistically significant effect on primary care health outcomes due to respiratory issues (Column 5), infection (Column 6), injuries (Column 7), or mental health conditions (Column 8). The estimates for primary care outcomes are precisely estimated such that we can reject health improvements that reduce the number of health problems by more than 0.09 standard deviation units.[11]

[10] The estimated impact is -0.009 standard deviation units (-0.139/15.269) for health problems at 0 to 4 years of age and 0.078 (0.499/6.349) for health problems at 5 to 8 years; the impact is 0.03 (0.074/2.699) standard deviations for health referrals at 0 to 4 years of age and -0.025 (-0.019/0.754) at 5 to 8 years. For 0 to 4 year-old visits to primary care the impact is 0.009 standard deviations (0.264/26.869) and for 0 to 4 year-old prescriptions, 0.007 (0.258/37.664).

[11] Following similar calculations to those explained in note 8, the lower confidence interval estimate for respiratory health problems is -0.080 standard deviation units for children aged 0 to 4, and -0.032 for children aged 5 to 8; the lower estimated confidence interval for health problems due to infections is -0.066 standard deviation units for children aged 0 to 4 and -0.074 for children aged 5 to 8 years; the lower estimated confidence interval for health problems due to injuries is -0.092 for children aged 0 to 4 years and -0.021 for children aged 5 to 8 years; the lower estimated confidence interval for health problems due to mental health is -0.075 for children aged 0 to 4 and -0.025 for children aged 5 to 8 years.

The results of estimating Equation (2) for the hospitalization outcomes are reported in columns (1)-(5) of Table 5. According to our estimates, a €2,500 increase in income does not appear to have a statistically significant effect on the probability of an all-cause hospitalization, or on hospitalizations due to respiratory, infection, injuries, or mental disorders. These estimates are not statistically distinguishable from zero.

Due to the aggregate nature of the data, the precision of the hospitalization estimates is not as high as in the case of primary care outcomes. However, we can tightly bound the effect of the income shock on the probability of being hospitalized around +0.04 and -0.02 percentage points, and, with the exception of hospitalizations due to injuries, we can rule out reductions in the likelihood of being hospitalized larger than 10 percent.[12]

Tables A.3 and A.4 in the Appendix show that our estimates for the impact of the bonus on health outcomes are robust to different model specifications, choice of bandwidth, polynomial order, and the inclusion of pre-determined variables as controls.

Educational Outcomes

Table 6 shows the estimated effects of the bonus on primary school outcomes from estimating Equation (1) in the two regions for which we had access to administrative data. The results suggest that the benefit had no impact on children's school outcomes. Columns (1) and (2) present estimated effects on Math and Spanish, the two subjects for

[12] For hospitalization rates, we offer the size of confidence intervals as a percentage of the average rate in the population. The estimated impact for all hospitalizations in column 1 of Table 5 is 0.031, that is, about 4% for an average hospitalization likelihood of 0.694. The corresponding lower 95% confidence interval is -0.41, that is, -5.9%. For respiratory diseases the lower confidence interval is -0.007, that is, -5.9 percent of the average population rate of hospitalization; for infections the lower confidence interval is -0.007, that is, -6.6 percent of the average rate of hospitalization for infections; for injuries, the lower confidence interval is -0.008, and that involves a reduction of about 22 percent; for mental disorders, the lower confidence interval is -0.008, that is, about 10 percent of the average likelihood of hospitalization for mental health problems.

which we have information across both regions. The estimated effect on Math achievement is negative but non-significant, -0.048 standard deviation units in Andalusia and -0.042 standard deviation units in Catalonia.

The 95 percent confidence intervals (-0.138 to 0.042 for Andalusia and -0.179 to 0.095 for Catalonia) allow us to rule out fairly small effects: for math test scores, we can discard effects larger than 5 percent of a standard deviation for Andalusia and larger than 9 percent of a standard deviation for Catalonia.

The results for Spanish test scores paint a very similar picture. The estimated effect on Spanish achievement is also negative and non-significant for Andalusia (-0.064 standard deviation units, 95 percent CI -0.162 to 0.034), and borderline significant for Catalonia (-0.125, 95 percent CI -0.272 to 0.022). Similarly, we can reject causal effects of the bonus larger than 3 percent of a standard deviation for Andalusia and 2 percent of a standard deviation for Catalonia.

Columns (3) to (6) in Table 6 report estimated effects of the bonus on additional schooling outcomes: the likelihood of grade retention for Andalusia, standardized second-year Catalan and English grades, and third-year GPA for Catalonia. We find no evidence that the bonus significantly impacts any of these outcomes. We can tightly bound the effect of the income shock on the likelihood of being retained around +/- 0.3 percentage points, and can rule out reductions in the likelihood of being a repeater larger than 6 percent.[13] For Catalonia, the impacts reported in columns (4) to (6) are all insignificant, ranging from -0.082 standard deviation units for GPA to -0.031 standard

[13] The 95 percent confidence interval for the estimated impact on being a repeater is (-0.003, 0.029).

deviation units for English. The corresponding 95 percent confidence intervals allow us to reject causal effects larger than 0.124 standard deviation units.[14]

Table A.5 in the Appendix shows that our estimates for the impact of the bonus on school outcomes are robust to different model specifications, including the full set of family background covariates as controls (child's sex, single-parent family, education level of both parents, and indicators for at least one parent with a high-skill job and at least one parent unemployed).

5.2. Results by Age, Sex, and Socio-Economic Status

In this section, we explore heterogeneous effects of the baby bonus by age of the child, sex of the child, and socio-economic status.

We consider age first. Most of the literature focuses on effects either early on in a child's life, or much later when the child has reached adulthood, but, as recently emphasized by Almond et al. (2018), much less is known on the impact of increased income during middle childhood. The literature looking at early childhood intervention programs has documented immediate gains in test scores that fade out as children enter elementary school but re-appear many years later in terms of completed schooling attainment or other long-term effects (Almond, Currie, and Duque 2018; Garces, Thomas, and Currie 2002).

Figures 3 to 5 show that benefit eligibility had no impact on health outcomes at any age, except for a small and borderline significant increase in the hospitalization rate of two-year-old children. However, this result does not survive a multiple hypothesis

[14] The upper confidence interval estimate for Catalan grades is 0.065 standard deviation units; for English grades, is -0.124 standard deviation units; and for GPA is 0.065 standard deviation units.

testing correction.[15] Overall, we can rule out that the results presented in Section 5.1 may hide any significant impact of the bonus for children at specific ages.

Recent literature has documented that boys' behavioral and educational outcomes are disproportionally affected by family disadvantage compared to girls' (Autor et al. 2019). Consistently, increases in income improve boys' educational and health outcomes to a larger extent than girls' (Milligan and Stabile 2011). It may well be that the baby bonus has a different impact for boys and girls. Table 7 stratifies the sample by child's sex and shows that none of the health or education impacts of the bonus are significant for boys (Panel A) or girls (Panel B).

Most of the literature reviewed in Almond, Currie, and Duque (2018) and Cooper and Stewart (2020) focuses on shocks that are either targeted to low-income families (Dahl and Lochner 2012; S. E. Black et al. 2014; Hoynes, Schanzenbach, and Almond 2016), show larger impacts for lower income samples (Akee et al. 2010; Cesarini et al. 2016; Løken, Mogstad, and Wiswall 2012), or only have positive impacts for the lower income sample (Milligan and Stabile 2011). We explore whether our main results may hide any significant impact of the bonus for children from different family backgrounds in Table 8.

We partition the data into children from low and high socioeconomic backgrounds. Low-income status is defined by the family having a yearly income below €18,000 in Columns (1) and (2), by the family residing in a region with a yearly income below the median in column (3), and by having neither parent with more than high school education in columns (4) to (7). Again, we do not find consistent evidence that benefit

[15] We computed p-values using the Romano and Wolf correction procedure separately on each primary outcome to take into account that we were testing multiple hypotheses for each child's age. For the two-year-old hospitalization rate, we observe that, even though in our main regression the coefficient was statistically significant at the 95% level, the same does not hold after the Romano-Wolf correction (p-value = 0.3663).

eligibility had any significant impact on children's health and educational outcomes , even in households of socioeconomic status.

6. Mechanisms

Previous literature has identified different channels through which income can influence children's outcomes, either by affecting the availability of parental time and money resources (Akee et al. 2010) or by improving parenting and parental mental health through stress reduction (Milligan and Stabile 2011).

For instance, parents may choose to increase or decrease their time investments in children in response to increased income. If leisure (or childcare time) is a normal good, parents may decrease their labor supply in order to spend more time with their children, potentially improving children's outcomes (Guryan, Hurst, and Kearney 2008; Fiorini and Keane 2014; Agostinelli and Sorrenti 2018).

Also, income transfers may improve parental emotional wellbeing (Milligan and Stabile 2011; Evans and Garthwaite 2014), leading to reduced family conflict and couple divorces or separations (Akee et al. 2018). Finally, if children are normal goods, then increased income may also increase the demand for additional children (Becker 1960; D. Black et al. 2013; González 2013). If parental investments per child decrease with family size (Becker and Lewis 1973; Price 2008), children in larger families may display worse outcomes.[16]

Even though we fail to find any significant impact of increased income on any of our child health and education outcomes, we explore whether the baby check impacted any of the potential mechanisms. Understanding whether and how parental outcomes

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[16] Most of the papers in this literature find no impact of increased family size on child quality in developed countries (S. E. Black, Devereux, and Salvanes 2005) but negative effects on schooling outcomes in developing countries (J. Lee 2008; Kugler and Kumar 2017).

were affected by the policy change may be relevant for the effective design of policy. Learning about potential negative work incentives of universal cash policies, such as the baby check, will also contribute to the debate regarding the potential negative impacts of universal basic income policies on labor supply (Hoynes and Rothstein 2019; Jones and Marinescu 2018).

To study potential parental behavioral changes as a result of the policy, we use data from the Spanish Labor Force Survey (*Encuesta de Población Activa*) for 2006-2017. The survey interviews a representative sample of the Spanish population of about 160,000 individuals each quarter. We use this supplemental dataset to study maternal labor supply, maternal partnership and divorce status, and subsequent fertility.

Table 9 presents the results from estimating Equation (1) (as presented in note 5) for our four mechanism variables measured from ages 0 to 8. None of the estimates are significantly different from zero. Given the survey nature of the data, our estimates are less precise. However, we can rule out behavioral responses larger than +/-2 percentage points. In particular, we are able to rule out increases in subsequent fertility larger than 2 percentage points (7 percent), increases in partnership status larger than 1.7 percentage points (1.9 percent),[17] reductions in maternal labor supply larger than 0.6 percentage points (1.2 percent), and reductions in divorced status larger 1.3 percentage points (28 percent).[18]

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[17] A priori income should increase subsequent fertility and partnership status. The upper confidence interval for subsequent fertility is 7 percent ((-0.006+1.96*0.012)/0.247), for partnership status, 1.9 percent ((0.002+1.96*0.008)/0.901). So we reject increases in subsequent fertility larger than 7 percent and increases in partnership status larger than 1.9 percent as a result of the baby bonus.

[18] Conversely, income is assumed to reduce maternal labour supply and divorced status. The lower confidence interval for maternal labour supply is -1.2 percent ((0.021-1.96*0.014)/0.500). We can reject reductions in labour supply larger than 1.2 percent. The lower confidence interval for divorced status is -28 percent ((0.002-1.96*0.006)/0.049).

Figure 6 shows differentiated impacts of the baby bonus on parental outcomes by age of the child. Coincident with previous studies investigating the impact of the Cherokee casino opening (Akee et al. 2010) and the Norwegian childcare subsidies (Black et al. 2014), we do not find that benefit eligibility had a significant impact on our mechanism variables, including labor force participation, in the short or long term (Panels A, B, C, and E).[19] The only exception comes from marital status.

In line with previous literature documenting an improvement in parental relationships within the household once Casino payments begin (Akee et al. 2018), we find that benefit eligibility indeed led to a lower probability of divorce during the two years following childbirth, although the effect is short-lived (Panel C). However, we also show that eligible mothers are no less likely to be living with a partner (Panel D in Figure 6), which suggests that time investments in the child were not drastically affected.

Finally, similarly to previous analyses by Cesarini et al. (2016) on the impact of lottery wins, Tables A.6 and A.7 in the Appendix use the Spanish Household Budget Survey (2008) and the Spanish EU-SILC (2006-2016) to show that neither household expenditures in child-related goods and services, healthcare, tobacco, and electricity, nor childcare or schooling arrangements of children under 8 years of age were affected by baby bonus eligibility.

7. Comparison to Previous Causal Estimates

We have provided credible causal estimates for the impact of an income shock on a variety of children's outcomes using administrative data. In this section, we use a

[19] González (2013) finds that eligible mothers took longer to return to work the year after birth, following a regression discontinuity design. Our coefficient for the first year after birth is indeed negative, but statistically indistinguishable from 0, which may stem from our stricter specification that uses 2006 births as controls.

comparative benchmark for our estimates and contrast them with the effect sizes found in the relevant papers investigating a causal relationship between income shocks and children's outcomes in developed countries.

Column 1 in Table 10 presents the papers selected for our benchmarking, in chronological order. We select papers included in the latest literature review studies analyzing the impact of household financial resources on any of the child outcomes analyzed here, which look at transfers that are directly received by the household during childhood, independently of whether child outcomes are measured contemporaneously to the income shock or with a lag (see Almond et al. (2018) and the meta-analysis of Cooper and Stewart (2013, 2020)).

Column 2 provides information on the main features of the policy used as an exogenous source of income variation. In order to maximize the comparability of our estimates to previous work, we restrict the sample to studies set in Europe and North America estimating causal effects based on a natural, policy, or randomized control trial experiments, excluding descriptive papers using cross-sectional methods.[20]

According to the latest estimates in the most recent literature review by Cooper and Stewart (2020), all of the studies that satisfied our inclusion criteria examining health and student outcomes at any point in childhood report positive effects on at least one of the outcomes.

[20] We exclude a few papers studying near-cash programs supplying land or food aid, such as papers analyzing the Georgia's Cherokee Land Lottery (Bleakley and Ferrie 2016) or the US food stamp program (Hoynes, Schanzenbach, and Almond 2016), and changes in broad economic circumstances that may impact not only family resources but also livelihoods, consumption, and parental employment (see for instance Løken 2010 and Løken, Mogstad, and Wiswall 2012). We also exclude a large literature on the impact of family income on health at birth, as the Spanish baby bonus was not anticipated and was introduced retrospectively, which rules out any effect on birth outcomes. There is one paper exploiting a baby bonus policy similar to ours in the Australian context, Lynch et al. (2019), in which the bonus gave rise to strategic birth delays in order to qualify for the cash (see Borra, González, and Sevilla 2019).

With the exception of Cesarini et al.'s (2016) study of Swedish lottery winners, which finds no effect on children's educational outcomes from lottery cash payments, the majority of papers report positive income effects on cognitive outcomes, with impacts ranging between 0.05 and 0.37 standard deviation units per $1,000 increase in annual income.[21]

The evidence is more mixed for health outcomes. The study by Aizer et al. (2016) investigating the impacts of the Mothers' Pension program in the early 20th century in the US – a period when no other welfare programs were available – is the only one showing clear improvements in child health. Other studies either find no effects, such as the analysis by Milligan and Stabile (2011) of the rollout of Canadian child benefit programs, or find both positive and negative effects on child health outcomes. For example, Cesarini et al. (2016) find increases in child hospitalization rates as well as a decrease in child obesity after the lottery win.[22]

Column 3 in Table 10 presents the outcomes analyzed in these studies, where outcomes that are directly comparable to ours are highlighted in bold. Heterogeneity in the outcome measured does not seem to be a plausible reason behind the differences

[21] Notice that this top 0.37 sd corresponds to boys in a selected low-education sample of Milligan and Stabile's (2011) study of Canadian child benefit programs; a paper that reports no significant impacts for the main sample. Our own calculations, considering the fact that some income increases lasted for just a few years and could not be considered permanent income changes, range between 0.03 and 0.60 standard deviation units. We calculate these ranges by computing the magnitudes that correspond to $1,000 annual income increases, given that the estimates provided in the papers are the result of increases in income indicated in the relevant row in Column 5 of Table 1. The largest difference between our magnitudes and the effect sizes reported by Cooper and Stewart (2020) comes from the estimates in Black et al.'s (2014) study of Norwegian childcare subsidies. They do recognize that the size of their estimates needs to be interpreted as coming from a permanent increase in family income, rather than a one-off shock to income.

[22] For instance, children of families winning the lottery by an amount equivalent to a $1,000 annual income have a 12 percent lower likelihood of being obese but a 3 percent increased likelihood of being hospitalized within two and five years of the lottery win. Sizes computed from Table 8 in Cesarini et al. (2016).

between our insignificant impacts and the previously reported causal estimates of income changes. Educational outcome measures, such as the likelihood of grade retention and standardized math and English test scores and GPA considered in Table 7, have previously been investigated by Milligan and Stabile (2011), Dahl and Lochner (2012, 2017), Black et al. (2014), or Cesarini et al. (2016). Similarly, Milligan and Stabile (2011), Aizer et al. (2016), and Cesarini et al. (2016) study health outcome measures analyzed here such as standardized weight, height, and BMI, the probability of being obese, as well as overall and respiratory hospitalization rates (also shown in bold).

Column 4 in Table 10 shows the targeted population in each study, and suggests that differences in the specific population studied are not likely to limit the comparability of our findings. In contrast to the policy shock analyzed here, which is a universal program targeted to the entire population, most studies cover policies that are targeted to low-income families, such as the US Earned Income Tax Credit, the Canadian Child Tax Benefit program, and the Norwegian childcare subsidies, or analyze income shocks that target low-income households, e.g. Akee et al (2010) who look at families living on an Indian reservation that opened a casino. Previous studies find positive effects, whereas our results in Table 8 clearly show no significant improvements in health or education outcomes for the low-income sample in our population. In that sense, our results are more aligned with those of Cesarini et al (2016), who look at lottery winners.

Another potential explanation for the difference in results might be the magnitude of the income supplement. It is plausible that an income change due to a one-off baby bonus such as the one analyzed here may have not resulted in permanent income changes that altered expectations of future income, preventing parental inputs from changing in ways that foster children's outcomes (Dahl and Lochner 2012; Blau 1999). In order to compare shocks to annual income across different studies, we take into account that some of the earlier studies evaluating income support programs have income supplements paid in monthly installments over childhood, while other studies deal with one-off payments like the baby bonus analyzed here, and follow the standard practice in the literature and compute the annuitized equivalent income by annuitizing temporary income changes over a 17-year period at a real return of 2 percent in 2000 US$ prices (see Cessarini et al., 2016).[23] By annuitizing these supplements, we adopt a conservative position that assumes no diminishing marginal effects of income supplements.

Column 5 in Table 10 reveals that the €2,500 Spanish baby bonus corresponds to an annuitized income increase of about $200 in 2000 prices, or to about a 0.7 percent *permanent* increase in annuitized income for a family of median earnings ($26,388 in 2000 prices). A handful of studies analyze permanent income shocks lower than $1000, ranging from about 1.6 to 4 percent of the average annual income in the targeted population, similar to the income transfer generated by the baby bonus. These include causal analyses of welfare to work experiments in Canada and the U.S. (Duncan et al., 2011), the Norwegian childcare subsides (Black et al., 2014), and the U.S. Mothers' Pension Program (Aizer et al., 2016). These studies find large impacts on children's health and cognitive outcomes, suggesting that the magnitude of the modestly sized

[23] We use the US$ Purchasing Power Parities (OECD 2021) and the US Consumer Price Index (BLS 2021). We consider that the income increase is sustained over 17 years, a duration similar to the US EITC studied by Dahl and Lochner (2012). The 2 percent rate of return is a conservative assumption, given that higher rates would only reduce the equivalized annual income. See details on the calculations for each paper in Table 10 in Appendix B Notes to Table 10.

income shock analysed here may not help explain why we find significantly smaller effects.

One recent study looking at income shocks generated by lotteries fails to find consistent income effects on children's outcomes even when the permanent income shock generated by the lottery shock is over 10 orders of magnitude larger than the increase in annual income generated by Spanish baby bonus (Cesarini et al., 2016). One commonality between lotteries and universal income transfers like the Spanish baby bonus is that income supplements are not conditioned on household time use investments or expenditures. We show that the receipt of the baby bonus was not associated to changes to household expenditure on child human-capital enhancing goods or services for families affected by the policy (either in the short run as found in Gonzalez, 2013, or in the medium run as documented in Table 9 and Appendix Tables A.6 and A.7). It is thus plausible that universal income supplement policies like the Spanish baby bonus can only be effective if their receipt is conditioned on expenditure and investment behaviors that directly affect children’s outcomes, as is the case in most of the welfare-to-work experiments and childcare subsidy programs finding positive effects from income shocks.

8. Conclusions

We investigate the causal impact of an unconditional and universal cash transfer paid shortly after birth on children’s health and academic performance. Using rich administrative data from Spain, we exploit an unexpected policy change: On a national speech on July 3, 2007, the Spanish president announced a cash transfer of €2,500 that would be paid to the mothers of all children born from July 1, 2007 onward. We use a differences-in-discontinuity design, comparing the gap between children born in the months immediately surrounding the policy introduction (June vs. July 2007), relative to children born in the same months in the previous year. We show that there was no discontinuous jump in the daily number of births around the cutoff date in 2007 compared to 2006. We also do not find evidence that eligible families were different in terms of pre-determined observable characteristics, supporting the validity of our identification strategy.

We show that the child benefit did not have any significant impact on children's human capital and well-being up to age 8, as far as we can detect. We can follow children's health trajectories in the primary care and hospital systems, and report age-by-age estimates from birth to middle childhood. We show that the number of health problems diagnosed by their primary care physician, the number of specialist referrals, and their hospitalization rates were not affected by benefit eligibility. Similarly, we do not find any significant impact on children's test scores at ages seven or eight. The high quality of our administrative data allows us to rule out relatively small effects in both health and school outcomes.

In line with these results, we show evidence that the policy change did not have significant effects on the main mechanisms via which the benefit could have affected children's development and well-being, such as maternal labor force participation, household expenditure patterns, parental separation, or mothers' subsequent fertility.

The Spanish government spent almost €4.000 million in three years on this benefit.[24] The subsidy was highly controversial, in part because of its unconditional nature. Governments may find these types of policies appealing, given the low administrative costs and the simplicity of their design. For example, countries like Canada, Australia, Italy, and France have introduced similar benefits in recent years.

[24] The information can be found here (in Spanish, accessed on 15/06/2020): https://transparencia.aragon.es/sites/default/files/documents/boletin_obdear_n1.pdf

We aim to contribute to informing the debate about the types of policies that are more likely to be effective in improving children's development. Compared to most previous studies evaluating income shocks during early life, the nature of the policy shock allows us to unambiguously separate pure income effects from other substitution effects induced by the incentives created by the policy change (Almond, Currie, and Duque 2018; Heckman and Mosso 2014).

Our results suggest that a one-time cash transfer of the size analyzed here and paid after birth is unlikely to have a meaningful impact on children's health or educational performance in the medium term. All in all, the evidence suggests that policies targeting families from lower socioeconomic status or tied to a specific expenditure leading to a permanent and repeated cash transfer over time, might be more effective in improving children's health and educational performance.

These results should be interpreted in the context of a country that has several policies in place oriented to improving the welfare of children and the support of families. Spain has a universal, publicly-funded health care system and an educational system providing free infant and primary education starting at age 3. The state also grants a relatively generous paternal leave: sixteen weeks for mothers and fifteen days for fathers in 2007. Our results should be interpreted with caution when extrapolating to other contexts where family policies are less widespread or generous.

The analysis presented here is not enough to conclude that the Spanish child benefit was not overall effective. González and Trommlerová (2021) show that it did lead to a temporary increase in fertility, which was one of the goals of the policy. González and Trommlerová (2021b) also find that the benefit had positive effects on health at birth for the children of women who received the benefit before becoming pregnant (after the birth of a previous child).

Also, benefit receipt may have had positive effects on families' consumption, especially among the more liquidity constrained ones, which we may be unable to detect in our analysis due to the small sample size in our expenditure survey data. In addition, we cannot rule out (yet) effects on children's health or cognition that may remain latent for some time before re-emerging later in life, as suggested by Almond, Currie, and Duque (2018). Our results do suggest, however, that a one-time cash transfer paid shortly after birth of the size analyzed here may not be an effective way to promote child development.

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Table 1: Overview of Main Registers

(1) Register name (period)	(2) Unit of obs	(3) Data description	(4) Outcomes	(5) Other variables
Panel A. Health Data				
BIFAP (2006-2011)	Child	Primary care data (8.6% sample). Children aged 0 to 4.	Visits, health problems (ICPC-2), referrals, prescriptions (ATC), anthropometric measures	1 if female, height and weight at birth
BDCAP (2011-2015)	Child	Primary care data (10% random sample). Children aged 5 to 8	Health problems (ICPC-2), referrals	1 if female, 1 if very low-income category
HMS (2006-2015)	Hospital stay	Administrative data on 97% of all hospital stays. Children aged 0 to 8	Hospitalization rates by age and diagnosis (ICD-9)	
Vital Statistics (2006-2007)	Child	Administrative population data on all births in Spain	Number of births	
Panel B. Education Data				
Andalusian Diagnostic Tests-ADT (2013/14-2014/15)	Child	Administrative population data of standardized tests scores. Children aged 7	Repeater, Math and Language Test Scores in 2nd year.	School ID, 1 if male, 1 if single parent, 1 if both parents less than high school, 1 if both parents more than high school, 1 if at least one non-employed parent, 1 if at least one parent high skilled
Catalonian Grades-CG (2013/14-2015/16)	Child	Administrative subject and general grades data on 70% of public schools. Children aged 7 and 8	Math, Spanish, English, and Catalan Grades in 2nd year, and Average Grades in 3rd year.	School ID, 1 if male, 1 if single parent, 1 if both parents less than high school, 1 if both parents more than high school, 1 if at least one non-employed parent, 1 if at least one parent high skilled
Panel C. Parental Behaviour				
Spanish Labour Force Survey (2006-2018)	Mother	Representative survey data of Spanish population. Children aged 0 to 10.	Subsequent fertility, maternal labor supply, maternal human capital accumulation, and maternal partnership status.	
Household Budget Survey 2008	Household	Representative survey data of Spanish households. Children aged 1 and 2	Overall monetary expenditure, child-related, health, health insurance, alcohol/tobacco, and electricity expenditure	

Panel D. Time Use			
Life Conditions Survey (2006-2016)	Child	Representative survey data of Spanish population. Children aged 0 to 10.	Time spent at pre-school, primary or secondary school, extra-school childcare, and time spent with nanny, relatives or parents.

Note: This table provides background information about the registers from which our key variables are derived. Access to BDCAP and BIFAP datasets requires a special license. HMS and Life Conditions Survey are publicly available at www.ine.es. Andalusian Data on Diagnostic Tests provided by the Andalusian Agency of Educational Evaluation. Catalonian Data on Grades provided by the Catalan Statistical Institute. ICPC: International Classification of Primary Care. ATC: Anatomical Therapeutic Chemical Classification. ICD: International Classification of Diseases.

Table 2: Overview of Outcome Variables and Summary Statistics

Outcome	**Register**	**Def./units**	**mean**	**sd**	**Obs.**
Panel A. Healthcare Outcomes					
Anthropometric Measures (0-4)					
Height z-score	BIFAP	z-score	0.075	1.020	3,850
Weight z-score	BIFAP	z-score	0.279	1.012	3,921
BMI	BIFAP	z-score	0.349	1.074	3,836
Overweight	BIFAP	0/1	0.142	0.349	3,836
Obesity	BIFAP	0/1	0.033	0.178	3,836
Primary Health Care Data					
Health problems ages 0-4	BIFAP	Total number	23.402	15.269	12,062
Health problems ages 5-8	BDCAP	Total number	5.362	6.349	16,435
Referrals ages 0-4	BIFAP	Total number	1.508	2.699	12,062
Referrals ages 5-8	BDCAP	Total number	0.218	0.754	16,435
Prescription ages 0-4	BIFAP	Total number	38.214	37.664	12,062
Visits ages 0-4	BIFAP	Total number	42.931	26.869	12,062
Respiratory problems ages 0-4	BIFAP	Total number	8.999	7.921	12,062
Respiratory problems ages 5-8	BDCAP	Total number	1.645	2.631	16,435
Infections ages 0-4	BIFAP	Total number	11.587	9.236	12,062
Infections ages 5-8	BDCAP	Total number	2.072	2.972	16,435
Injuries ages 0-4	BIFAP	Total number	0.646	1.011	12,062
Injuries ages 5-8	BDCAP	Total number	0.325	0.751	16,435
Psychological problems ages 0-4	BIFAP	Total number	0.100	0.340	12,062
Psychological problems ages 5-8	BDCAP	Total number	0.078	0.321	16,435
Hospitalizations 0-8					
All stays	HMS	Hosp. rate	0.694	0.056	122
Respiratory disease	HMS	Hosp. rate	0.128	0.016	122
Infections	HMS	Hosp. rate	0.101	0.014	122
Injuries	HMS	Hosp. rate	0.035	0.006	122
Mental disorders	HMS	Hosp. rate	0.002	0.001	122
Panel B. Education Outcomes					
Mathematics (Andalusia)	ADT	Std. score	0.002	0.999	29,590
Mathematics (Catalonia)	CG	Std. grade	0.006	0.987	15,696
Spanish (Andalusia)	ADT	Std. score	0.003	0.994	29,632
Spanish (Catalonia)	CG	Std. grade	0.003	0.986	15,696
Repeater (Andalusia)	ADT	0/1	0.042	0.201	30,975
Std. Catalonian Catalan Grade	CG	Std. score	0.009	0.984	15,696
Std. Catalonian English Grade	CG	Std. score	0.009	0.991	15,696
Std. Catalonian GPA 3rd grade	CG	Std. score	0.001	0.987	15,696

Note: This table summarizes the outcome variables used in the analyses and their corresponding summary statistics. The first column lists the outcome; the second column, the corresponding register; the third column, the units in which the outcome is measured; the fourth and fifth columns show the summary statistics; and the final column shows the available observations for each outcome. BIFAP, BDCAP, and the Education sample are restricted to the months of June and July for both 2006 and 2007. HMS sample is also restricted to the date of births in the 30 days surrounding the cutoff date July 1st for both 2006 and 2007. Note that the hospitalization rates are analyzed at the date of birth level. All other outcomes are studied at the child level.

Table 3. Effects on anthropometric measures (age 4)

	(1) Height z-score	(2) Weight z-score	(3) BMI z-score	(4) Overweight	(5) Obesity
Effect	0.022	0.064	0.097	0.019	-0.001
	(0.066)	(0.065)	(0.070)	(0.023)	(0.012)
Observations	3,850	3,921	3,836	3,836	3,836
Controls	No	No	No	No	No
Mean/SD	0.075/1.02	0.279/1.012	0.349/1.074	0.142/0.349	0.033/0.178

Note: This table shows the estimates for the coefficient of the interaction term of being born after July 1st and belonging to the 2007 cohort (equation in footnote 5). We analyze the impact of the treatment (benefit eligibility) on different anthropometric measures 4 years after birth. Height-for-age, weight-for-age and BMI z-scores are calculated using the World Health Organization's (WHO) universally applicable growth standards for children aged zero to five years. A z-score of 0 represents the median of the gender- and age-specific reference population. Obesity and overweight are defined using the Stata command zbmicat, which allows for children (ages 2 to 18) to be categorized into thinness grades – normal weight, overweight, and obese – according to international body mass index (BMI) cutoffs defined by the Childhood Obesity Working Group of the International Obesity Taskforce. The data source is primary care administrative data from the BIFAP project. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Effect of child benefits on primary health care outcomes ages

	(1) Health Problems	(2) Referrals	(3) Visits	(4) Prescriptions	(5) Respiratory	(6) Infections	(7) Injuries	(8) Psychological
Panel A. Primary Healthcare Outcomes Ages 0-4. BIFAP								
Effect	-0.139	0.0741	0.264	0.258	-0.069	0.049	-0.021	-0.002
	(0.557)	(0.099)	(0.979)	(1.381)	(0.289)	(0.337)	(0.037)	(0.012)
Mean/SD	23.402/15.269	1.508/2.699	42.931/26.869	38.214/37.664	8.999/7.921	11.587/9.236	0.646/1.011	0.100/0.340
Observations	12,062	12,062	12,062	12,062	12,062	12,062	12,062	12,062
Controls	No	No	No	No	No	No	No	No
Panel B. Primary Healthcare Outcomes Ages 5-8. BDCAP								
Effect	0.499	-0.019			0.244	0.183	0.082	0.031
	(0.398)	(0.052)			(0.168)	(0.206)	(0.050)	(0.020)
Mean/SD	5.362/6.349	0.218/0.754			1.645/2.631	2.072/2.972	0.325/0.751	0.078/0.321
Observations	16,435	16,435			16,435	16,435	16,435	16,435
Controls	No	No			No	No	No	No
Linear Trend	Yes	Yes			Yes	Yes	Yes	Yes

Note: The table shows the estimates for the coefficient of the interaction term of being born after July 1st and belonging to the 2007 cohort. In Panel A, the specific equation is reported in note 5; outcomes are the number of health problems, referrals, visits, prescriptions, respiratory problems, infections, injuries, and psychological problems from ages 0 to 4. The data source is primary care administrative data from BIFAP project; an observation is a child; and the sample includes observations for children born in June and July of 2006 and 2007. In Panel B, we estimate Equation (1); outcomes are the number of health problems, referrals, respiratory problems, infections, injuries and psychological problems from ages 5 to 8. The data source is primary care administrative data from BDCAP project; an observation is a child; and the sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Robust standard errors in Panel A and clustered standard errors by date of birth in Panel B. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5. Effect of child benefits on hospitalizations

	(1) All Stays	(2) Respiratory	(3) Infections	(4) Injuries	(5) Psychological
Effect	0.031	0.016	0.009	0.002	0.031
	(0.037)	(0.012)	(0.008)	(0.005)	(0.020)
Mean/SD	0.694/0.056	0.128/0.016	0.101/0.014	0.035/0.006	0.078/0.321
Observations	122	122	122	122	122
Linear Trend	Yes	Yes	Yes	Yes	Yes

Note: This table shows the estimates from equation (2) for total hospitalization rates (number of hospital stays over number of births) and for hospitalization rates due to respiratory diseases, infections, injuries and mental disorders, from ages 0 to 8. The data source is the Hospital Morbidity Survey 2006-2015. An observation is a day (of birth). The sample includes observations for date of births in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6. Effects of the child benefit on school outcomes

	(1) Math	(2) Spanish	(3) Repeater	(4) Catalan	(5) English	(6) GPA
Panel A. Andalusian Diagnostic Tests						
Effect	-0.048	-0.064	0.013			
	(0.046)	(0.050)	(0.008)			
Observations	29,590	29,632	30,975			
Proportion			4.2%			
Controls	No	No	No			
Linear Trend	Yes	Yes	Yes			
Panel B. Catalan Grades						
Effect	-0.042	-0.125*		-0.0766	-0.0311	-0.0824
	(0.070)	(0.075)		(0.0726)	(0.0796)	(0.0756)
Observations	11,944	11,953		11,936	11,900	11,738
Controls	No	No		No	No	No
Linear Trend	Yes	Yes		Yes	Yes	Yes

Note: This table shows the estimates of equation (1) for education outcomes: for Andalusia, the outcomes are 2nd grade standardized test scores and an indicator variable that takes value of 1 if the student is a repeater; for Catalonia, the outcomes are the school subject grades and the overall GPA in 2nd grade. Scores (grades) are standardized to have a mean of zero and a standard deviation of 1 at the subject-cohort level. The data was provided by the regional governments. Each observation is a student. The sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7. Effect of child benefits on health and schooling outcomes ages by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Health Outcomes					Schooling Outcomes			
	Health Problems0/4	Health Problems 5/8	Referrals 0/4	Referrals 5/8	Hospitalizations	Math in Andalusia	Math in Catalonia	Spanish in Andalusia	Spanish in Catalonia
Panel A. Boys									
Effect	0.084	0.624	0.046	0.003	0.029	-0.044	-0.119	-0.0722	-0.119
	(0.794)	(0.635)	(0.147)	(0.068)	(0.024)	(0.050)	(0.109)	(0.0645)	(0.097)
Mean/SD	24.099/16.625	5.318/6.360	1.660/2.878	0.225/0.758	0.397/0.037				
Observations	6,206	8,428	6,206	8,428	122	15,329	6,137	15,349	6,142
Controls	No	No	No	No	No	No	No	No	No
Panel B. Girls									
Effect	-0.277	0.328	0.130	-0.041	0.001	0.029	0.029	-0.034	-0.110
	(0.778)	(0.512)	(0.130)	(0.063)	(0.021)	(0.100)	(0.100)	(0.0636)	(0.103)
Mean/SD	22.662/14.848	5.408/6.337	1.346/2.486	0.211/0.751	0.297/0.306				
Observations	5,856	8,007	5,856	8,007	122	5,807	5,807	14,283	5,811
Controls	No	No	No	No	No	No	No	No	No

Note: This table shows the estimates from equation (1) and (2) for the health and schooling outcomes indicated in the column headings, by gender. The sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Robust standard errors in Column 1. For the rest of outcomes, standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8. Effect of child benefits on health and schooling outcomes by socioeconomic status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Health Outcomes			Schooling Outcomes			
	Health Problems 5/8	Referrals 5/8	Hospitalizations	Math in Andalusia	Math in Catalonia	Spanish in Andalusia	Spanish in Catalonia
Panel A. Low Income							
Effect	1.078**	0.022	0.021	-0.047	-0.046	-0.093	-0.119
	(0.510)	(0.062)	(0.022)	(0.064)	(0.103)	(0.056)	(0.114)
Mean/SD	5.946/6.712	0.261/0.831	0.253/0.030	-0.143/1.064	-0.187/0.999	-0.169/1.049	-0.163/1.001
Observations	9,811	9,811	122	14,465	6,199	14,485	6,204
Controls	No	No	No	No	No	No	No
Panel B. High Income							
	-0.221	-0.108	0.009	-0.026	0.042	-0.007	-0.043
	(0.770)	(0.093)	(0.025)	(0.055)	(0.097)	(0.063)	(0.111)
Mean/SD	5.259/6.080	0.204/0.705	0.435/0.038	0.216/0.816	0.348/0.854	0.250/0.817	0.314/0.871
Observations	4,527	4,527	122	13,373	4,606	13,394	4,608
Controls	No	No	No	No	No	No	No

Note: This table shows the estimates from equation (1) and (2) for the health and schooling outcomes indicated in the column headings, by socio-economic status. Low-income status is defined by the family having a yearly income below €18,000 in Columns (1) and (2), by the family residing in a region with a yearly income below the median at the regional level (Comunidad Autónoma) in column (3), and by neither parent having more than high school education in columns (4) to (7). The sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9. Mechanisms

	(1)	(2)	(4)	(5)
	Subsequent fertility	Maternal labor supply	Divorced mother	Partnered mother
Effect	-0.006	0.021	-0.002	0.002
	(0.012)	(0.014)	(0.006)	(0.008)
Observations	19,469	19,469	19,469	19,469
Controls	No	No	No	No
Mean/SD	0.247/0.431	0.500/0.500	0.049/0.216	0.910/0.286

Note: This table shows the estimates of the impact of the treatment (benefit eligibility) on subsequent fertility, maternal labor supply, human capital accumulation, and marital stability. The specific equation is reported in footnote 5. Subsequent fertility is an indicator variable that takes value 1 if the mother had another child within 8 years after the birth. Maternal labor supply is an indicator variable that takes value 1 if the mother has performed any type of paid work when the newborn was between 0 and 8 years old. Maternity leave takes value of 1 if the mother was still employed, but not working due to maternity leave when the newborn was between 0 and 8 years old. Divorced and partnered mother are two indicator variables that measure if the mother is (or became) divorced or partnered, respectively, when the newborn was between 0 and 8 years old. The data source is the Spanish Labor Force Survey 2006-2018. An observation is a mother. The sample is mothers aged 16 to 50 of children born in June and July of 2006 and 2007. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10. Previous studies on financial incentives and child outcomes in developed countries

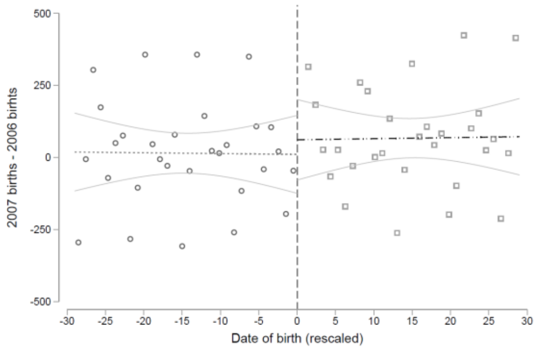
(1)	(2)	(3)	(4)	(5)
Publication	**Source of income variation**	**Outcome**	**Targeted to Low Income or Heterogeneity Analysis for Low-Income Sample**	**Annual size of income change (in 2000 US dollars)**
Akee et al. (2010)	US Casino experiment *Country:* US *Amount:* $4,000	-Years of education -Prob of HS graduate	Heterogeneity analysis for low-income sample	$4,000 13% median annual income increase
Milligan and Stabile (2011)	Child Tax Benefit plus National Child Benefit program. *Country:* Canada. *Amount:* Average increase of CAN$2,396	*Cognitive outcomes* **-Ever repeated** -**Math** **-Peabody PPVT** *Health outcomes* -Experience hunger -Good health -**Height** -**Weight** -Injured last 12m	Targeted to low income families	$1,607 4% average income increase for the whole population 11% average annual income increase for low-income sample for which significant impacts found
Duncan et al. (2011)	Random assignment to different MDRC programs *Country:* Canada. *Amount:* Average increase of CAN$1,000 and 2,200.	Cognitive test scores (not distinguished)	Targeted to low income families	$397 4% average income increase for the targeted population
Dahl & Lochner (2012, 2017 revision)	US Earned Income Tax Credit expansion *Country:* US *Amount:* $1,129	*Cognitive outcomes* **Comb math-read** **Reading rec** **Reading compreh** **Math**	Targeted to low income families	$1,129 5% average annual income increase for the targeted population
Black et al. (2014)	Norway childcare subsidies *Country*: Norway *Amount*: Average increase of 10,000 NOK (	*Cognitive outcomes* **GPA** Oral Exam Written Exam	Targeted to low income families	$283 1.6% annual increase in income for families just before the cutoff
Aizer et al (2016)	Mothers' Pension program (1911-1935) *Country:* US *Amount:* $10-$30 monthly income of 1911-1930 (20% of manufacturing wages)	*Health outcomes* Longevity **Weight** **Height** **BMI** Underweight **Obese** *Cog. outcomes* Edu attainment Income	Targeted to low income families	$492 ($1,000) 2% (4%) annual increase in income for targeted families

Cesarini, Lindqvist, Ostling, Wallace (2016)	Sweden lottery wins *Country:* Sweden *Amount:* 1M SEK ($140,000)	*Cognitive outcomes* All 6 insignif. but **-Cog. Skills** -Noncog. skills **-GPA** **-English** **-Swedish** **-Math** *Health outcomes* All 18 insignif. but **Obese** **Hosp in 2 years** **Hosp in 5 years** **Resp. hosp. in 2** **Resp. hosp in 5**	Heterogeneity analysis for low income sample	$6,127 50% annual increase in disposable income
Our paper	Spain Baby bonus *Country:* Spain. *Amount:* €2,500.	Cog. Outcomes Repeater Math Spanish Catalan English GPA *Health outcomes* Height (0-4) Weight (0-4) Visits (0-4) Health probs (0-4) (5-7) Referrals (0-4) (5-7) Prescriptions (0-4) Vaccines (0-4) Hospital. (0-8) Resp. hosp. (0-8) Infect. Hosp (0-8) Perinat hosp. (0-8)	Heterogeneity analysis for low-income sample	$200 0.7% annual increase for the median family 1.1% annual income increase for the bottom quartile

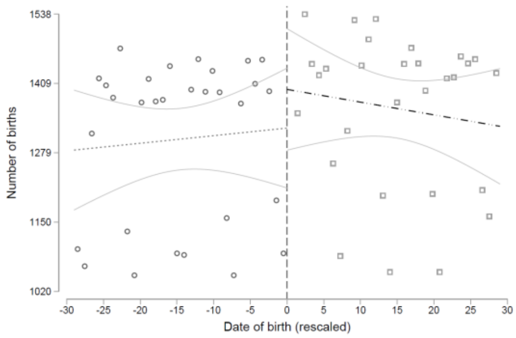
Notes: Papers are ordered chronologically. To compute comparable income changes in Column 7, we convert national currencies to US$ using Purchasing Power Parities (OECD 2020) for countries outside the US and then convert all dollar sums to 2000 prices using the US Consumer Price Index (BLS 2020). When income increases cannot be considered a permanent income change, we annuitize the one-off income change by considering a 17-year period at a 2% interest rate. In Columns 3 and 4, we calculate effect sizes as a percentage of the dependent variable's standard deviation that corresponds to $1,000 also in 2000 prices following Cooper and Stewart (2013, 2017). For indicator dependent variables, we calculate the percentage change in the likelihood of this indicator. We do not include the literature that examines in-utero impacts of cash transfers. See further notes in Appendix B Notes to Table 10.

Figure 1. Number of births

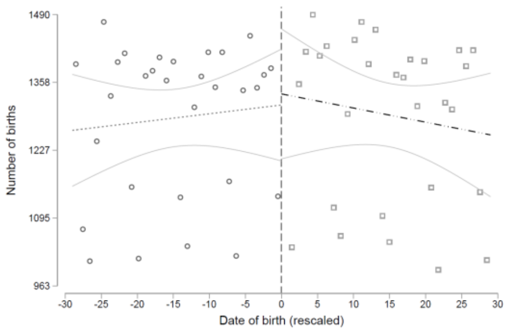
Panel A. Difference



Panel B. 2007



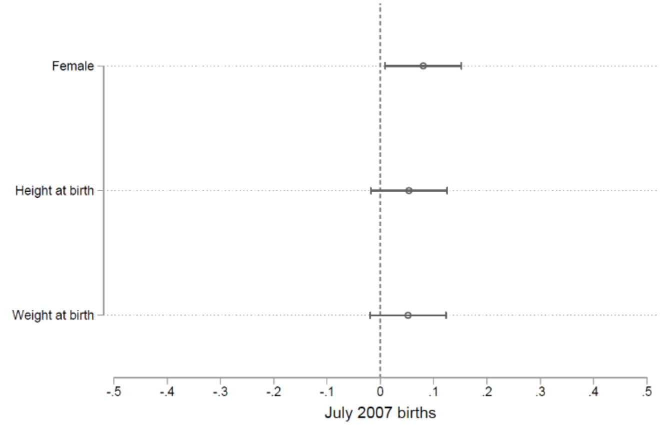
Panel C. 2006



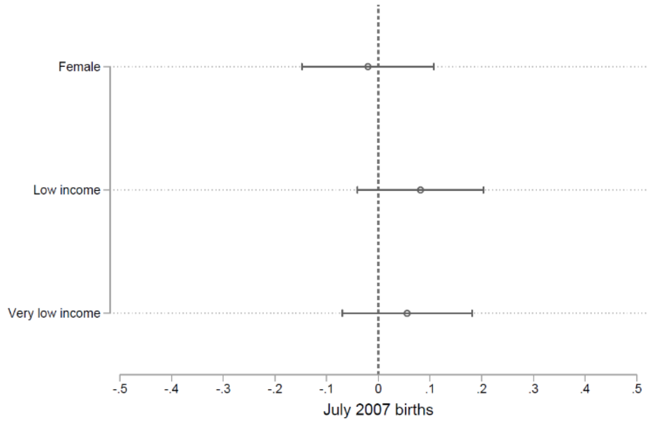
Note: Panel A shows the difference in the daily number of births between 2007 and 2006. Panels B and C show the daily number of births in 2007 and 2006, respectively. Linear fits (with 95% Confidence Intervals) are displayed on both sides of the threshold (July 1). The data source is 2006 and 2007 birth certificates from the Spanish National Institute of Statistics.

Figure 2: Balance in covariates

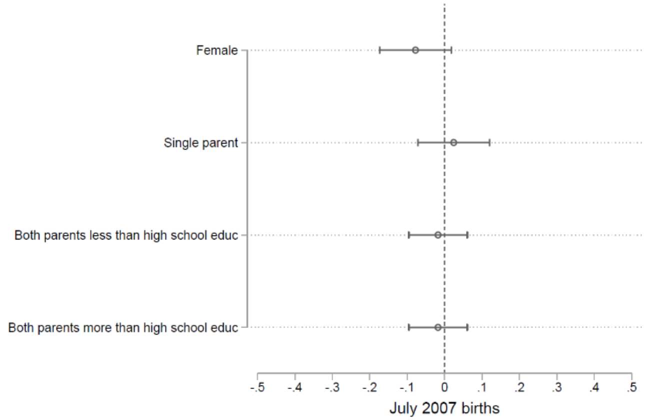
Panel A. Child characteristics. Primary Health Care Data



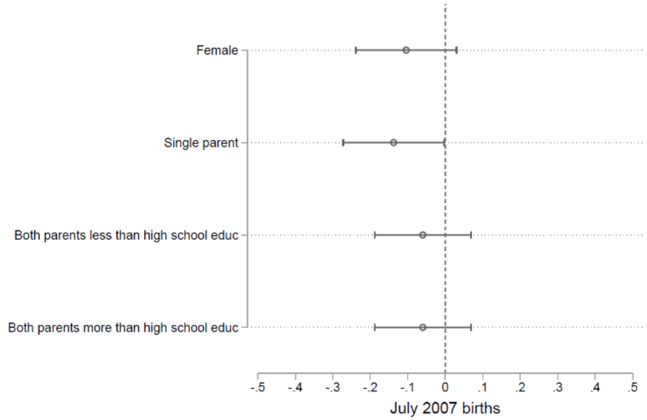
Panel B. Family Characteristics. Primary Health Care Data –



Panel C. Family Characteristics. Andalusian Educational Data –

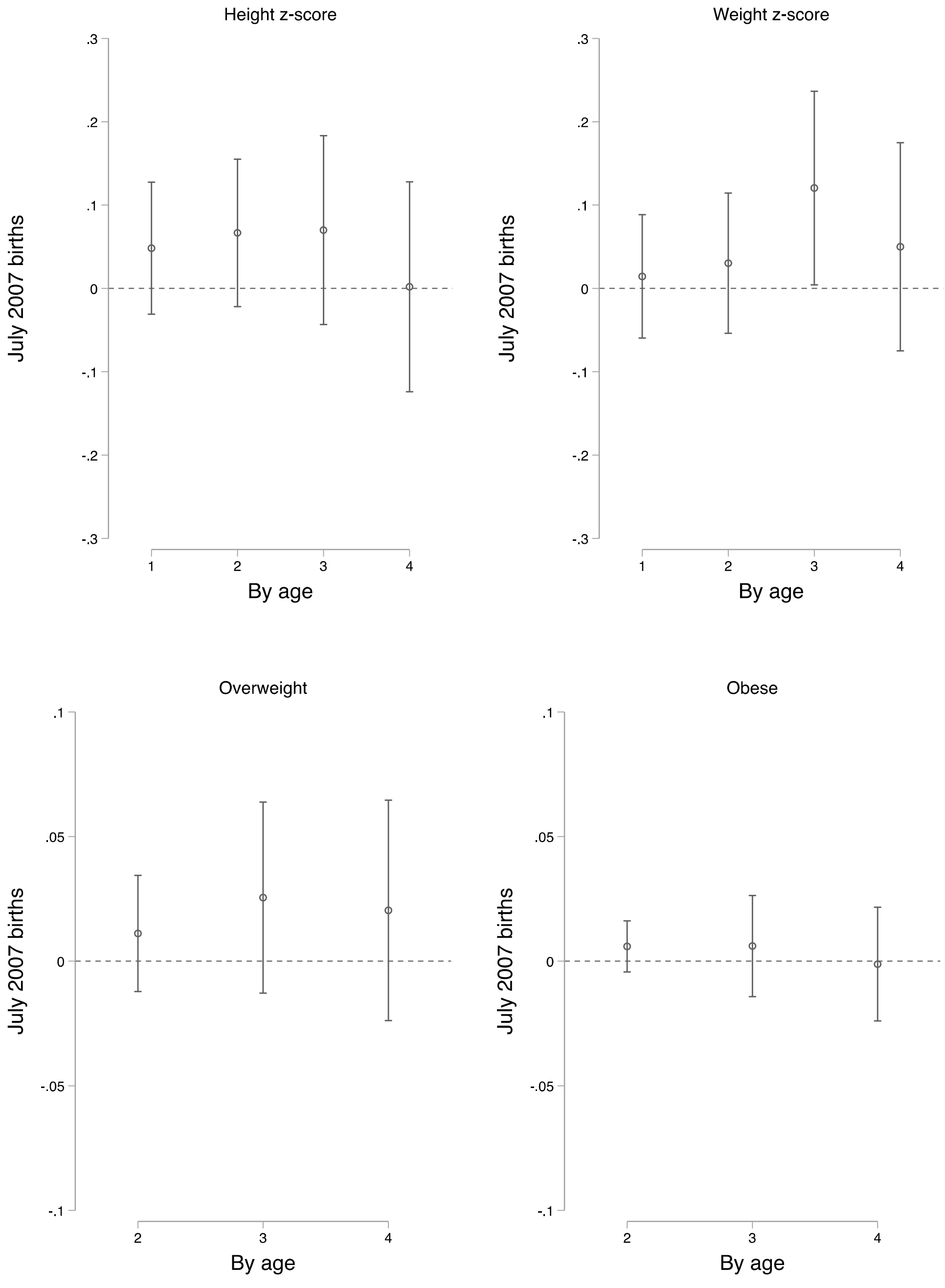


Panel D. Family Characteristics. Catalonian Educational Data –



Note: Panel A plots the coefficients and 95% CI of estimating equation reported in footnote 5 on different placebo outcomes for health data (Panel A). Panels B, C, and D plot the RDDiD estimates of the impact of treatment (benefit eligibility) on different placebo outcomes for health (Panel B) and educational data (Panel C and D) (equation (1)). The sample in all panels includes observations for children born in June and July of 2006 and 2007. The data source in Panel A is primary care data from BIFAP project. In Panel B, the data source is BDCAP. In panel C and D, the data source is administrative data provided by the regional governments. An observation is a child.

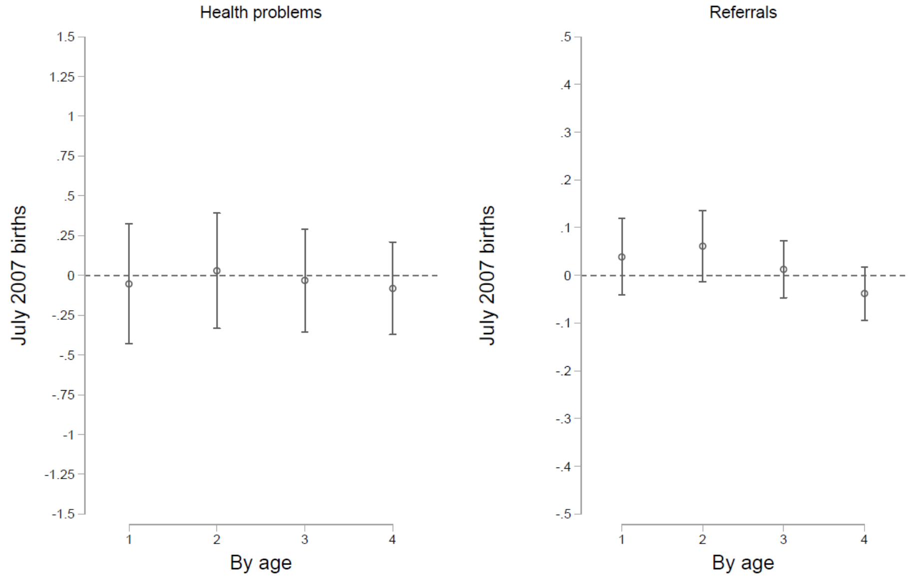
Figure 3: Anthropometric measures by age



Note: These figures plot the coefficients and 95% CI of the impact of the treatment (benefit eligibility) on different anthropometric measures by age. The specific equation is reported in note 5. Height-for-age and weight-for-age z-scores are calculated using the World Health Organization's (WHO) universally applicable growth standards for children aged zero to four years. A z-score of 0 represents the median of the gender- and age-specific reference population. Obesity and overweight are defined using the Stata command *zbmicat*, which allows for children (ages 2 to 18) to be categorized into thinness grades – normal weight, overweight, and obese – according to international body mass index (BMI) cutoffs defined by the Childhood Obesity Working Group of the International Obesity Taskforce. The data source is primary care administrative data from the BIFAP project. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007.

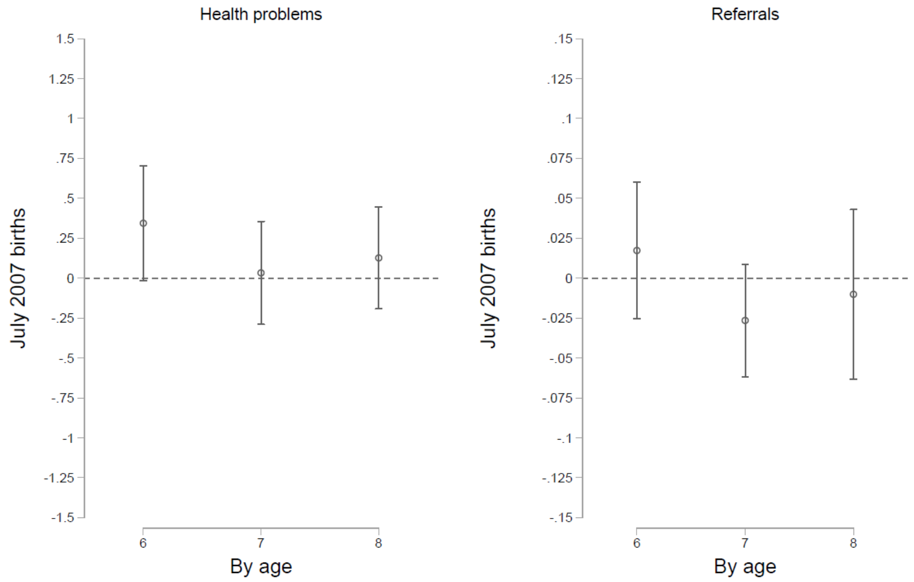
Figure 4. Primary care outcomes by age

Panel A: Ages 0 to 4



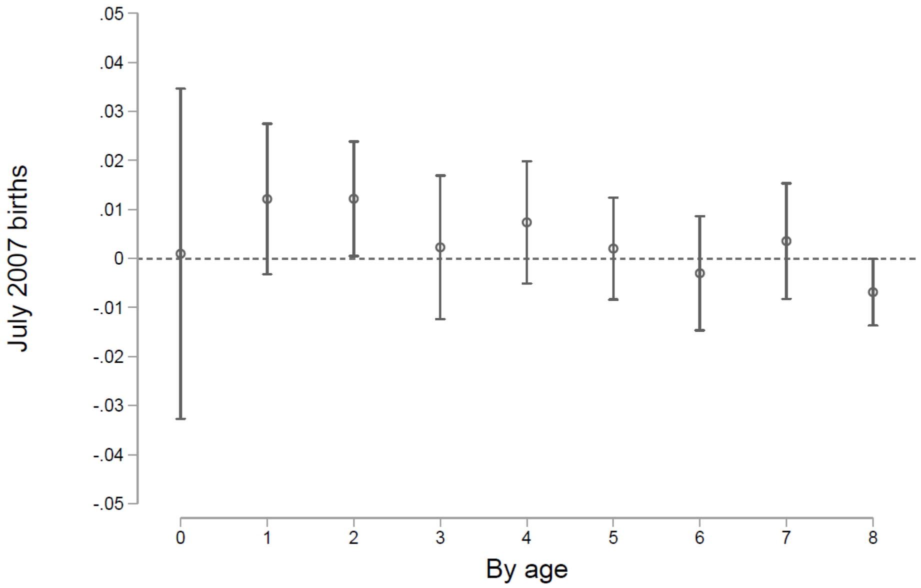
Note: These figures plot the coefficients and 95% CI of the impact of the treatment (benefit eligibility) on number of health problems and referrals by age (0 to 4). The specific equation is reported in footnote 5. The data source is primary care administrative data from the BIFAP. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007.

Panel B: Ages 5 to 8



Note: These figures plot the RDDiD coefficients and 95% CI of the impact of the treatment (benefit eligibility) on number of health problems and referrals by age (5 to 8) (equation (1)). The data source is primary care administrative data from the BDCAP. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007.

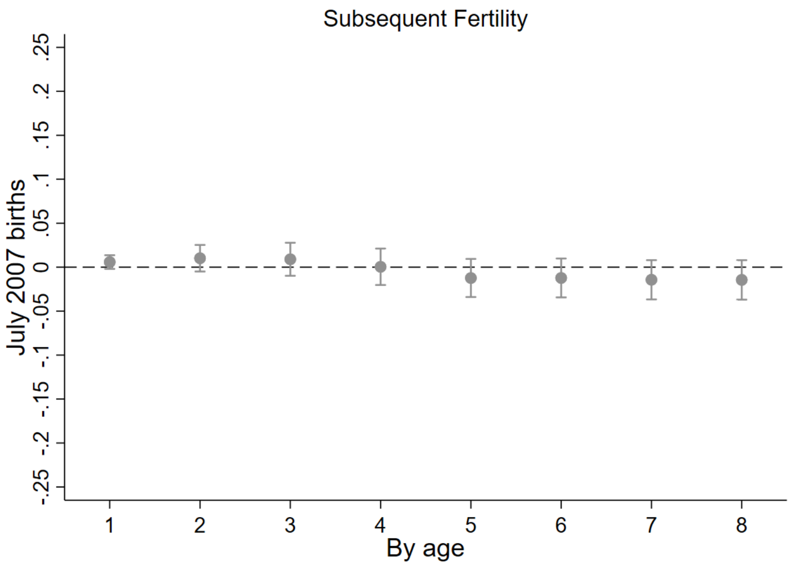
Figure 5. Hospitalization effects by age



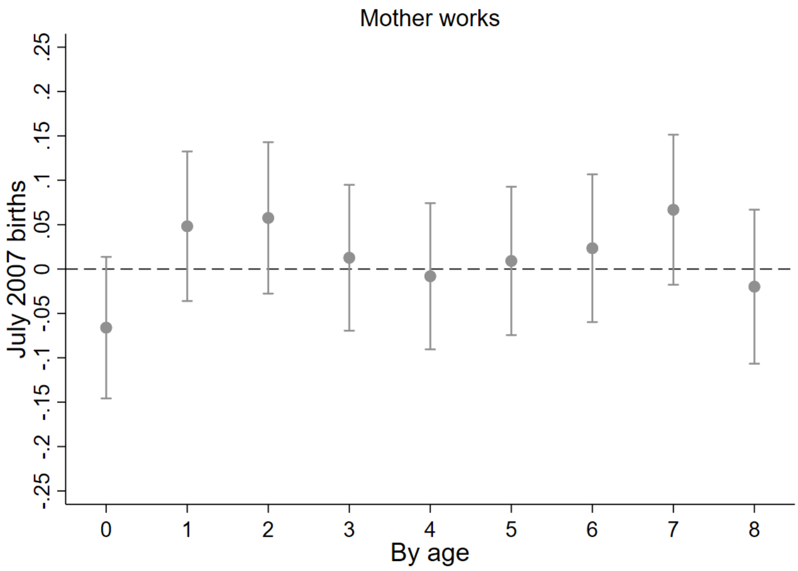
Note: This figure plots the RDDiD coefficients and 95% CI of the impact of the treatment (benefit eligibility) on hospitalization rates (number of daily hospital stays over number of daily births) by age (equation (1)). The data source is the Hospital Morbidity Survey 2006-2015. An observation is a day (of birth). The sample includes observations for children born in June and July of 2006 and 2007.

Figure 6. Channels: Maternal time use and family conflict effects by age

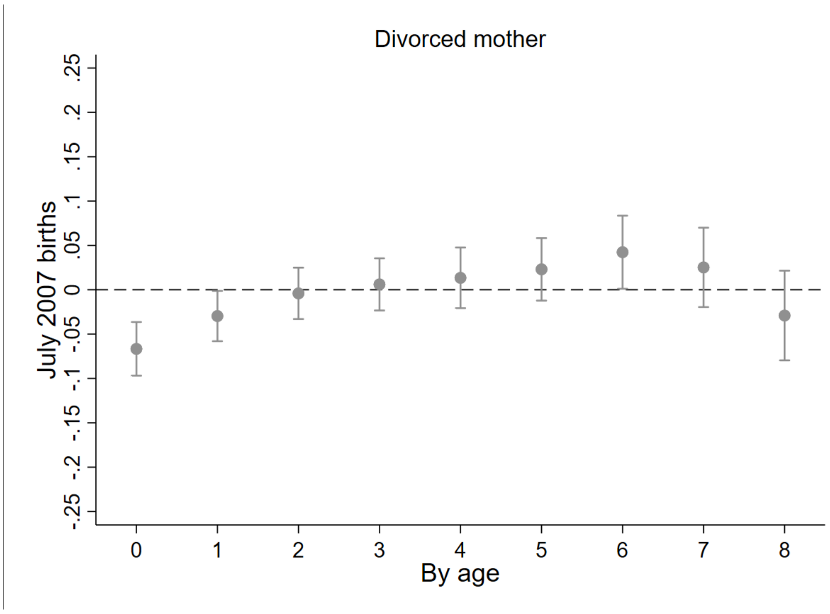
Panel A. Subsequent fertility



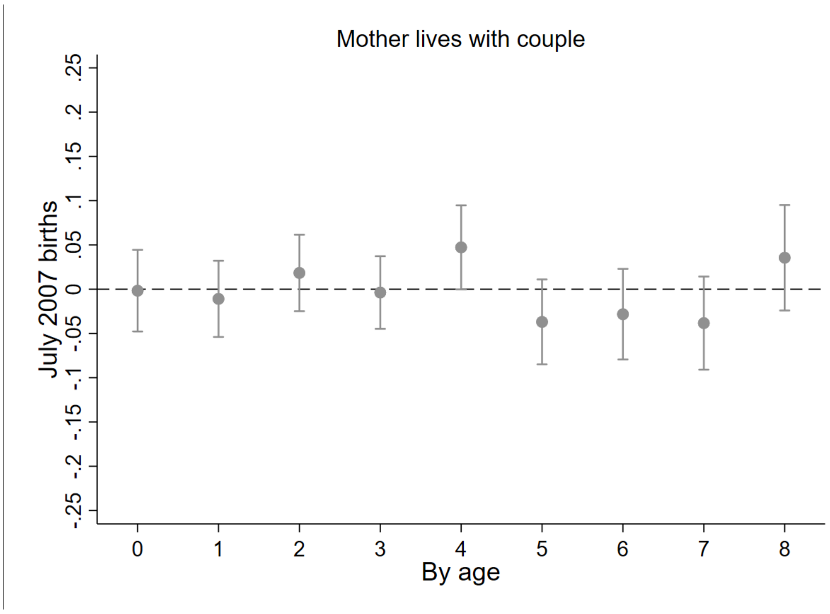
Panel B. Maternal labor supply



Panel C. Divorced mother



Panel E. Partnered mother



Note: These figures plot the coefficients and 95% CI for the interaction of being born in July with being born in 2007 (equation in footnote 5). Subsequent fertility is an indicator variable that takes value 1 if the mother had another child in the 8 years following the birth. Maternal labor supply is an indicator variable that takes value 1 if the mother performed any type of paid work by age of the child. Maternity leave takes value 1 if the mother was still employed, but not working due to maternity leave. Divorced and partnered mother are two indicator variables that measure if mother is divorced or partnered, respectively. The data source is the Spanish Labour Force Survey 2006-2018. The sample is mothers aged 16 to 50 of children born in June and July of 2006 and 2007.

Appendix A Tables and Figures

Table A1. Effect of child benefits on primary health care outcomes (placebo comparing 2006 with 2005)

	(1) Health Problems	(2) Referrals	(5) Respiratory	(6) Infections	(7) Injuries	(8) Psychological
Effect	-0.158	0.001	-0.097	-0.056	-0.042	-0.007
	(0.321)	(0.046)	(0.135)	(0.167)	(0.039)	(0.018)
Mean/SD	3.998/4.809	0.159/0.624	1.214/1.956	1.550/2.246	0.247/0.619	0.061/0.285
Observations	14,510	14,510	14,510	14,510	14,510	14,510
Controls	No	No	No	No	No	No
Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table shows the RDDiD estimates from equation (1), but comparing children born in July-June 2006 with those born in 2005. Outcomes are the number of health problems, referrals, respiratory problems, infections, injuries, and psychological problems, at ages 5 to 8. The data source is primary care administrative data from BDCAP project. An observation is a child; and the sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2005 and 2006. Standard errors clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2. Effects of the child benefit on school outcomes in Andalusia (placebo comparing 2009 to 2008)

	(1) Math	(2) Spanish	(3) Repeater
Effect	-0.039	0.014	-0.007
	(0.044)	(0.043)	(0.006)
Observations	28,508	28,507	29,292
Proportion			2.5%
Controls	No	No	No
Linear Trend	Yes	Yes	Yes

Note: This table shows the estimates of equation (1), but comparing children born in June-July 2009 with those born in 2008, for education outcomes in Andalusia. The outcomes are the scores obtained on standardized test applied on 2nd grade and an indicator variable that takes value 1 if the student is a repeater. Scores (grades) are standardized to have a mean of zero and a standard deviation of 1 at the subject-cohort level. The data were provided by the regional governments. Each observation is a student. The sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2008 and 2009. Standard errors are clustered by date of birth.
$^{*}p<0.10, ^{**}p<0.05, ^{***}p<0.01$

Table A3. Effect of child benefits on primary health care outcomes ages 5 to 8: Robustness to model specification

	(1) +/- 30 days	(2) +/- 30 days	(3) +/- 30 days	(4) +/- 60 days	(5) +/- 90 days
Health Problems	0.499	0.695	0.504	0.434	0.064
	(0.398)	(0.427)	(0.387)	(0.295)	(0.250)
Mean/SD	5.362/6.349	5.729/6.529	5.362/6.349	5.368/6.279	5.349/6.202
Referrals	-0.019	-0.016	-0.020	-0.018	-0.031
	(0.052)	(0.0567)	(0.053)	(0.036)	(0.029)
Mean/SD	0.218/0.754	0.243/0.794	0.218/0.754	0.214/0.746	0.216/0.746
Respiratory	0.244	0.328*	0.246	0.150	0.013
	(0.168)	(0.178)	(0.164)	(0.121)	(0.101)
Mean/SD	1.645/2.631	1.759/2.725	1.645/2.631	1.652/2.612	1.650/2.581
Infections	0.183	0.241	0.186	0.148	0.005
	(0.206)	(0.214)	(0.197)	(0.145)	(0.121)
Mean/SD	2.072/2.972	2.216/3.068	2.072/2.972	2.089/2.966	2.091/2.946
Injuries	0.082	0.093*	0.082*	0.059*	0.044
	(0.050)	(0.0554)	(0.048)	(0.034)	(0.027)
Mean/SD	0.325/0.751	0.346/0.779	0.325/0.751	0.326/0.749	0.325/0.747
Psychological	0.031	0.038*	0.031	0.017	0.012
	(0.020)	(0.0226)	(0.020)	(0.015)	(0.011)
Mean/SD	0.078/0.321	0.082/0.328	0.078/0.321	0.078/0.322	0.078/0.321
Observations	16,435	14,338	16,435	32,567	49,073
Controls	No	Yes	No	No	No
Linear Trend	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No

Note: This table shows the results from estimating Equation (1) for the number of health problems, referrals, respiratory problems, infections, injuries and psychological problems, from ages 5 to 8. In column (2), controls include: gender and a variable indicating if respondent comes from a very low income SES. The data source is primary care administrative data from BDCAP project. An observation is a child. The sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4. Effects on hospitalization rates ages 0-8: Robustness to model specification

	(1)	(2)	(3)	(4)
	+/- 30 days	+/- 30 days	+/- 60 days	+/- 90 days
All Stays	0.031	0.031	0.027	0.007
	(0.037)	(0.036)	(0.023)	(0.021)
Mean/SD	0.694/0.056	0.694/0.056	0.697/0.057	0.695/0.058
Respiratory diseases	0.016	0.016	0.004	-0.003
	(0.012)	(0.012)	(0.008)	(0.007)
Mean/SD	0.128/0.016	0.128/0.016	0.129/0.018	0.129/0.019
Infections	0.009	0.009	0.002	0.002
	(0.008)	(0.009)	(0.006)	(0.005)
Mean/SD	0.101/0.014	0.101/0.014	0.102/0.013	0.102/0.014
Injuries	0.002	0.002	0.003	-0.001
	(0.005)	(0.005)	(0.003)	(0.003)
Mean/SD	0.035/0.006	0.035/0.006	0.035/0.007	0.035/0.006
Mental disorders	0.001	0.001	0.001	0.001^{*}
	(0.001)	(0.001)	(0.001)	(0.001)
Mean/SD	0.002/0.001	0.002/0.001	0.002/0.001	0.002/0.001
Observations	122	122	242	362
Linear Trend	Yes	No	Yes	Yes
Quadratic Trend	No	Yes	No	No

Note: This table shows the results from estimating Equation (2) for total hospitalization rates (number of hospital stays over number of births) and for respiratory diseases, infections, injuries and mental disorders, from ages 0 to 8. The data source is the Hospital Morbidity Survey 2006-2015. An observation is a day (of birth). The sample includes observations for date of births in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5. Effects of the child benefit on school outcomes in 2nd grade: Robustness to model specification

	(1)	(2)	(3)	(4)	(5)
	+/- 30 days	+/- 30 days	+/- 30 days	+/- 60 days	+/- 90 days
			Panel A. Andalusia		
Math	-0.048	-0.0365	-0.048	-0.067**	-0.074***
	(0.046)	(0.0470)	(0.046)	(0.031)	(0.027)
	N = 29,590	N = 29,590	N = 29,590	N = 58,067	N = 80,006
Spanish	-0.064	-0.0422	-0.064	-0.087**	-0.083***
	(0.050)	(0.0485)	(0.050)	(0.033)	(0.028)
	N = 29,632	N = 29,632	N = 29,632	N = 58,138	N = 80,101
			Panel B. Catalonia		
Math	-0.042	-0.0323	-0.042	-0.043	-0.043
	(0.070)	(0.0651)	(0.072)	(0.053)	(0.043)
	N = 11,944	N = 11,944	N = 11,944	N = 23,665	N = 35,583
Spanish	-0.125*	-0.101	-0.125	-0.081	-0.031
	(0.075)	(0.0740)	(0.076)	(0.052)	(0.042)
	N = 11,953	N = 11,953	N = 11,953	N = 23,675	N = 35,579
Controls	No	Yes	No	No	No
Linear Trend	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No

Note: This table shows the results from estimating Equation (1) for Math and Spanish schooling outcomes: for Andalusia, the outcomes are 2nd grade standardized test scores; for Catalonia, the outcomes are the school subject grades obtained in 2nd grade. Scores (grades) are standardized to have a mean of 0 and a standard deviation of 1 at the subject-cohort level. Controls in column (2) include indicators for female students, students who have a single-parent, students whose parents both have less than high school education or more than high school education, students who have at least one parent who has a high-skill job, and at least one non-employed parent. The data were provided by the regional governments. Each observation is a student. The sample includes observations for children born in each reform window surrounding the cutoff date July 1st for both 2006 and 2007. Standard errors are clustered by date of birth. * p < 0.10, ** p < 0.05, *** p < 0.01

Table A6. Effects on expenditure

	(1) Monetary	(2) Child	(3) Health	(4) Health insurance	(5) Alcohol/Tobacco	(6) Electricity
Effect	356.8	162.1	203.5	-73.84	65.30	-13.91
	(4,008)	(1,168)	(395.5)	(199.7)	(237.6)	(107.2)
Observations	236	236	236	236	236	236
Controls	No	No	No	No	No	No
Mean/SD	30152/15180	4724/4444	782.4/1584	278.1/764.9	613.9/887.3	584.8/394.1

Note: This table shows the estimates for the coefficient on the interaction term of being born after July 1st and belonging to the 2007 cohort (equation in footnote 5). The outcomes are household expenditure (in €) in several categories: overall monetary expenditure, child-related, health, health insurance, alcohol/tobacco, and electricity. Monetary expenditure includes the purchases actually effectuated by the household. Child-related expenses include baby food, children's clothes and shoes, furniture (includes cribs, highchairs, ...), kitchen appliances (includes baby bottles), household services (includes nanny), social security household services, toys, books (not aimed for school), paper and painting material, kindergarten expenses, hygiene-related items (diapers, lotion, baby scale, ...), and other baby-related items (strollers, carriers, pacifiers, ...). Health-related expenses include pharmaceutical and therapeutic products and all medical and hospital services, including doctor's appointments. Health insurance includes direct expenses in private health insurance. Alcohol/Tobacco includes expenses in alcoholic beverages, tobacco, and drugs. Electricity includes expenses on electricity. The data source is survey data from the Household Budget Survey (2008). The sample includes observations for households with children born in June and July of 2006 and 2007. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7. Effects on time use

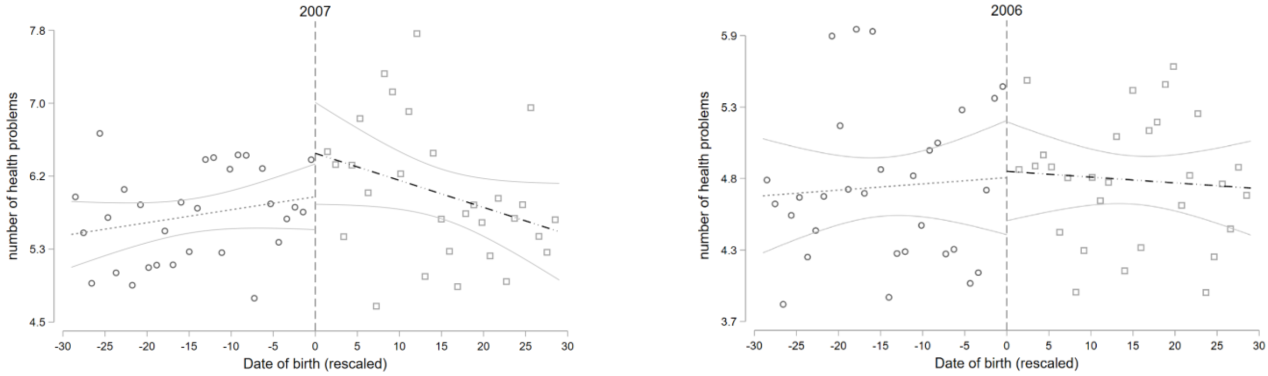
	(1) Pre-school	(2) School	(3) Extra-school	(5) Nanny	(6) Relatives	(7) Parents
Effect	1.242	-1.662	-0.501**	0.655	0.915	-0.425
	(1.962)	(1.846)	(0.233)	(0.497)	(0.854)	(1.772)
Observations	967	967	967	967	966	966
Controls	No	No	No	No	No	
Mean/SD	12.684/15.312	10.194/14.382	0.290/1.805	0.475/3.750	1.722/6.656	142.570/13.802

Note: This table shows the estimates for the coefficient on the interaction term of being born after July 1st and belonging to the 2007 cohort (equation in footnote 5). The outcomes are time use in hours (in one week) for children aged 12 years old or younger. Pre-school includes the usual number of hours in a week that the child spends in pre-school. Primary school represents the usual number of hours in a week that the child spends in (mandatory) school (this includes both primary and secondary school). Extra-school consists of the number of hours spent in childcare services/centres outside the school hours. Nanny includes number of hours the child spent under the care of professional caregivers (e.g. nanny). Relatives measures the number of hours in a week that the child spent under the care of other people, who are not the parents and who were also not remunerated (e.g. friends, grandparents, etc.). Finally, time spent with parents is calculated by subtracting to the total number of hours in a week (168) by all the measures mentioned above as well as time spent in other types of childcare that are not covered in the former categories. The data source is survey data from the Encuesta de Condiciones the Vida (2006-2016). The sample includes observations for children who were born in June and July of 2006 and 2007 and whose mother is aged between 16 and 50. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

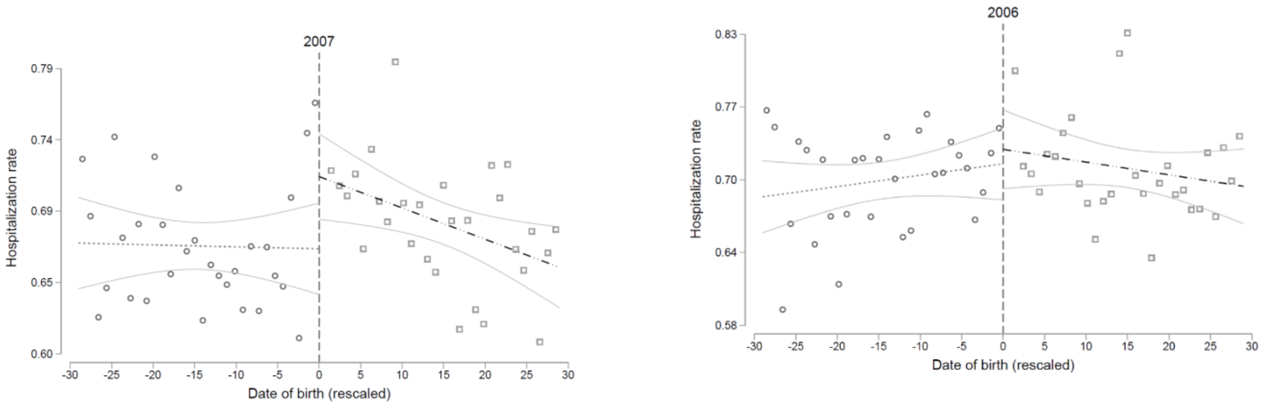
Appendix figures

Figure A1. RDD comparing 2007 and 2006 (30 day bandwith)

Panel A. Health Problems (ages 5 to 8)

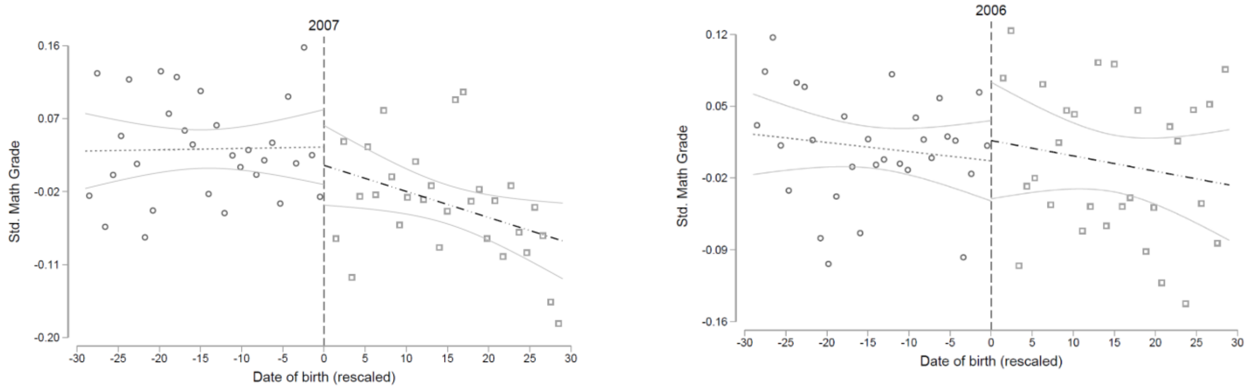


Panel B. Hospitalizations (ages 0 to 8)

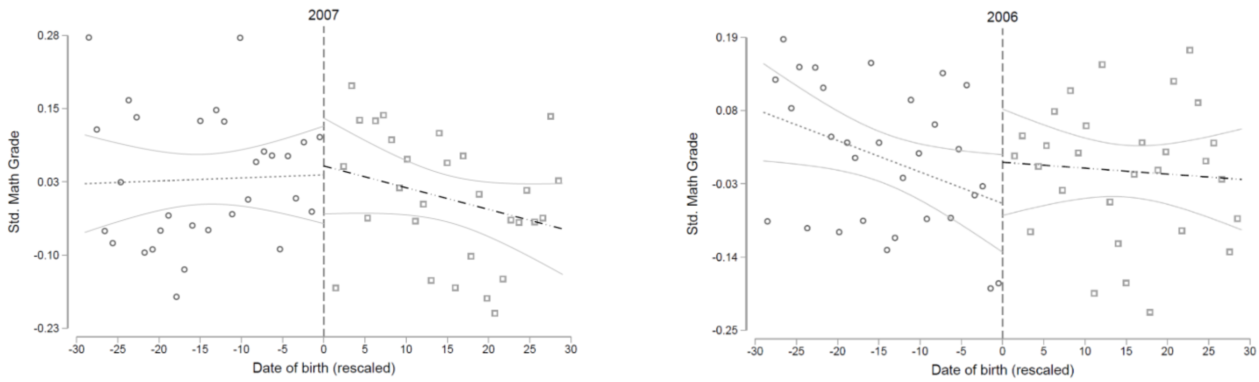


Panel C. Math Test Scores

Panel C1. Andalusia



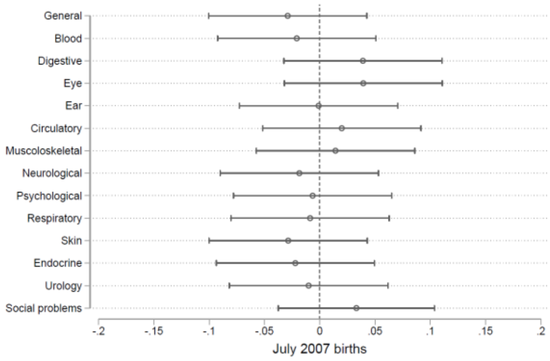
Panel C2. Catalonia



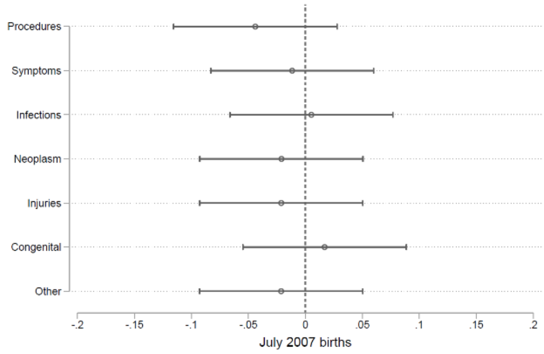
Note: These figures show the number of health problems (panel A) from ages 5 to 8, the hospitalization rate from ages 0 to 8 (panel B) aggregated by date of birth, and the math test scores for Andalusia (Panel C1) and Catalonia (Panel C2). The data source is primary care administrative data from BDCAP, Hospital Morbidity Survey 2006-2015 and educational administrative data provided by the regional governments. Linear fits (with 95% Confidence Intervals) are displayed on both sides of the threshold (July 1). The left figure always refers to 2007, while the right one to 2006.

Figure A.2: Primary health care effects by diagnosis, type of visit and drug

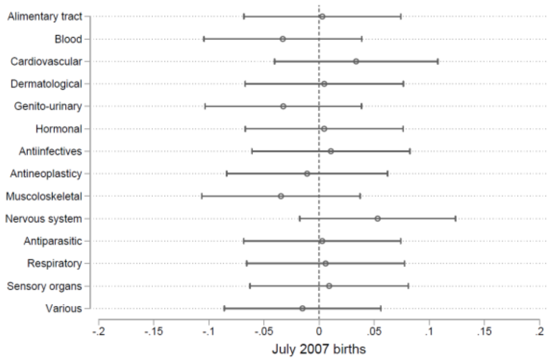
Panel A: By diagnosis



Panel B: By type of visit



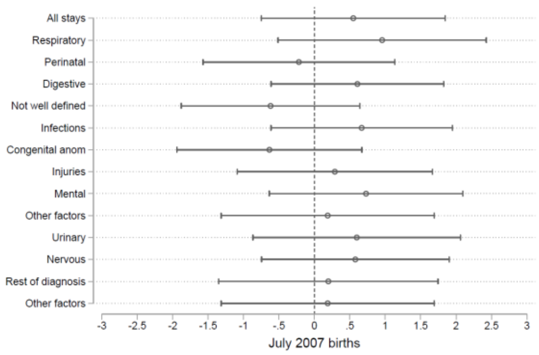
Panel C: By type of drug



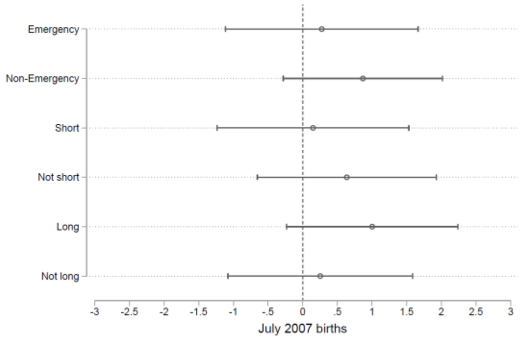
Notes: These figures plot the coefficients and 95% CI of the impact of the treatment (benefit eligibility) on health problems by diagnosis group (panel a), type of visit (panel b), and type of drug (panel c) (ages 0 to 4). The specific equation can be found in footnote 5. The data source is primary care administrative data from the BIFAP project. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007.

Figure A.4: Hospital effects by diagnosis, type of visit, and length of stay

Panel A. By diagnosis



Panel B. By type of visit and length of stay



Notes: These figures plot the RDDiD coefficients and 95% CI of the impact of the treatment (benefit eligibility) on hospitalization rates (number of hospital stays over number of births) by diagnosis group (panel a) and by type of visit and length of stay (panel b) (equation (1)). The data source is the Hospital Morbidity Survey 2006-2015. An observation is a day (of birth). The sample includes observations for children born in June and July of 2006 and 2007.

Appendix B Notes to Table 1

Akee, Copeland, Keeler et al. (2010)

They use the Casino openings as an exogenous increase in income. The youngest cohort is 9 when the Smoky Mountain begins. The casino opens after the fourth wave so the youngest treated child is 13 years old and the oldest is 15. They consider the increase in income to be permanent (page 88). We assume dollar figures are in 2000 prices. Therefore, the average increase in income is $4,000 as referenced in page 91. The median income pre-policy is $30,000, therefore, that figure amounts to about 13 percent of annual income. For column 3, we use the estimates on page 4. The marginal effect of 0.156 for $4,000 translates to 0.004 for $1,000. Given that the probability of graduating from high school is 0.62 in Table 1, the estimated impact is 6.3 percent. For column 4, we use the estimates in Table 5 for the 'Household previously in poverty columns. For years of education, 1.127 translates to 0.28 years. The standard deviation of years of schooling is not provided in the paper or the appendix and therefore this figure cannot be converted to units of a standard deviation. For probability of high school graduation, 0.391 translates to 0.097, a 16 percent increase.

Milligan and Stabile (2011)

We assume families consider the annual benefits as increased permanent income. All dollar values in this paper are transformed to 2004 CAN$ (page 187). They report that the average benefit in 2004 is CAN$2,174 (see page 187). That figure is US$1,763 in 2004 dollars and US$1,607 in 2000 dollars. Currie and Stabile (2003), using the same dataset, report average incomes of CAN$50,000 in 1998, that is, CAN$57,000 in 2004. Milligan and Stabile (2011) only find impacts for the 32 percent low income sample, between CAN$10,000 and CAN$25,000. So, the average income for the low-income family is about CAN$20,000. CAN$2,174 is about 11 percent of annual income. Therefore, the average benefit is about 4 percent of annual income. They estimate that an additional CAN$1,000 benefit in 2004 prices increases the probability of the child ever repeating by 2.7 percentage points (page 190), that is, 3.6 percentage points for a US$1,000 income increase in 2000 prices. Given that the probability of ever repeating is 0.029, the estimated figure implies an 125 percent increase in the likelihood of repeating for a US$1,000 income increase in 2000 prices. They also find that a CAN$1,000 income increase in 2004 prices increases math achievement of 6-10 year-olds by 6.9 percent of a standard deviation. This figure implies an increase of 9.3 percent of a standard deviation for a US$1,000 income increase. They also estimate different impacts for the low-income sample, differentiated by sex (see Table 3). For the physical health outcomes, we compute first the impact that corresponds to US$1,000 in 2000 dollars (CAN$739.2 in 2004 prices) and then calculate the impact in standard deviation units for height and weight or in percentage terms for indicator variables.

Duncan, Morris, and Rodrigues (2011)

They use the random assignment to welfare programs as exogenous change in income. The average increase in income due to the earnings supplement is $1,500 (p.1271). All monetary values are given in US$ at 2001 prices (p.1267). They say the average child is on welfare between 3 and 5 years on page 1275. We therefore calculate the present value of receiving $1,459.32 ($1,500 in 2000 prices) for four years and then annuitize the result by considering a 17-year period at a 2 percent interest rate. In Table 3, they say average income is $11,000 in 2001 prices, that is, $10,702 in 2000 prices. Therefore, the average annual income shock is the computed equivalent annual income which is between $231 and $508. They compute the impact for a $1,000 increase in income (for four years). On page 1275, they report an impact on the treated of about 0.06 of a standard deviation. These figures correspond to 0.26 of a standard deviation for a $1,000 annual increase in income.

Dahl and Lochner (2012, 2017)

They use changes in the Earned Income Tax Credit as exogenous changes in income. They provide all dollar figures in 2000 prices, so no conversion needed here. The median EITC payment is given in Table 1. The median pre-policy income ($23,463) is given on page 1937. The EITC represents an increase in income of 5 percent. All estimates provided in the paper correspond to $1,000 income increases so no need to adjust these either. Main results provided in Table 3. Heterogeneous impacts by mother's education, child's race, age, and gender appear in Table 6. They published a correction in 2017 with lower, but still significant, impacts.

Black, Devereux, Løken, and Salvanes (2014)

They use childcare subsidies as an exogenous change in income. The average subsidy is NOK10,000. This figure seems to be at current prices. Given that the data is for the years 1986-1992, we use 1990 PPP exchange rates with the dollar. The subsidy is $1,042.3 in 1990 prices and $1,373.27 in 2000 prices. The authors consider that income change should be interpreted as a permanent change (page 835), but, in fact, most families would have received the subsidy for an average of 3 years (when the child is 3 to 5, see discussion on day care centre rates on page 825). We therefore calculate the present value of receiving $1,373.27 for three years and then annuitize the result by considering a 17-year period at a 2 percent interest rate. The computed equivalent annual income is $283. On page 833, they say families just below the cutoff have just over 8 percent more yearly disposable income when the child is in childcare. That translates to a 1.65 percent increase when annuitized. On page 834, they report an impact on the treated of about 0.17 to. 0.50 of a standard deviation. These figures correspond to between 0.60 and 1.76 of a standard deviation for a $1,000 annual increase in income.

Aizer, Eli, Ferrie, and Lleras-Muney (2016)

They use the introduction of the Mothers' Pension program (1911-1935) as exogenous change in income. On page 939, they say the average transfer ranges from $10 to $30 per month. We consider, therefore, an average transfer of $20 monthly in 1919 dollars. That translates to $240 in annual income using 1919 prices and $2,389 annual income in 2000 prices. On page 940, they say the median duration of the program among recipients was 3 years. We therefore calculate the present value of receiving $2,389 for three years and then annuitize the result by considering a 17-year period at a 2 percent interest rate. The computed equivalent annual income is $492. However, this figure may understate the real value of the pension because they also explain on page 939 that the average monthly transfer was 20 percent of monthly manufacturing wages, that is, about $420 in 2000 prices using data from BLS. Using this other yardstick, the pension would translate to an annual income of about $1,000 in 2000 prices over 17 years. A 20 percent increase in wages over 3 years translates to about a 4 percent annual increase in income. Their main outcome is an impact of longevity of 1.2 years for an average of 72.44 years of age. We cannot translate this figure in sd units because the standard deviation of age is not provided in the summary statistics. For the rest of outcomes in Tables 6 and 7, we find the same problem, they only give the mean value and the percent effect. Therefore, we cannot calculate the impact in sd for the continuous variables (see summary statistics in Table S8 in the Appendix). Also, they emphasize estimated impacts without controls: only weight and BMI are significantly impacted, at the 10 percent level, when all controls are included. Only these impacts are included in the table.

Cesarini, Lindqvist, Östling, and Wallace (2016)

They use lottery prizes as an exogenous change in income. The average prize is about SEK15,000 as reported in Table V. However, they explain in page 704 that 90 percent of the identifying variation comes from large prizes: typically, SEK1,000,0000. On page 691, they say that monetary units are measured in 2010 prices, so SEK1M in 2010 prices is USD$110,877 in 2010 prices and USD$87,560 in 2000 prices. We annuitize that amount over a 17-year period at a real return of 2 percent to obtain an annual income increase of $6,127. The authors report an annual amount of $8,800 because, first, they use 2010 prices, and second, they do not translate monetary figures using purchasing power parity units but SEK/USD exchange rates. They give a median income of SEK144,000 in 2010 prices, which translates to $12,220 in 2000 prices. Therefore, the typical prize entails an annual increase in income of about 50 percent.

They consider 6 developmental outcomes: cognitive and non-cognitive skills (males 18-19), GPA, Swedish, English, and Math test scores (at the end of secondary 16 years of age). The impact is not significant unless stated in the table. All impacts in the paper (Table IX) correspond to SEK 1M in 2010 prices and are given in standard deviation units. We translate those to the impact of $1000 annually in 2000 prices by dividing by 6.127.

They consider 18 health outcomes, 3 relating to health at birth in the post-lottery children

sample and 15 relating to health of pre-lottery children: **BMI, overweight, and obesity** for males at 18-19 years of age, different indicators of drug consumption, and **hospitalization likelihoods within 2 and 5 years after the lottery win for all causes as well as respiratory disease and external problems** (see Table AXXIII for details on all outcomes measured). Only significant impacts are included in the table. All impacts relating to indicator variables in Tables VIII, AXXVII, and AXXVIII are standardized by dividing by 6,127 to calculate percentage point impacts for $1,000 annual dollars and then divided by means to compute percentage changes.
Interestingly, in Table AXXV, they report no significant impacts for lotteries under SEK2M ($10,710 annual 2000 dollars): neither for health nor for development outcomes, akin to our paper. The average prize is a bit under SEK15,000 ($1,313.4 in 2000 prices). Their significant results appear to come from about 300 observations (0.24 percent of the total sample) from lottery winners of SEK2M to SEK4M.

Our paper

We use the introduction of a universal child benefit in Spain as exogenous increase in income. The baby bonus gave €2,500 to all new mothers in 2007. This figure is about US$3,410 in 2007 prices according to PPP and US$2,833 in 2000 prices. We annuitize that amount over a 17-year period at a real return of 2 percent to obtain an annual income increase of $198. The median annual equivalized income in Spain was about 11,645€ in 2007 (Eurostat 2020). Assuming an equivalizing factor of 2 for the typical Spanish family with children, this figure is about €23,290 and US$31,774, in 2007 prices, and $26,388 in 2000 prices. The policy represents a 0.7 percent increase for the average household. The bottom quartile had €7,740 annual equivalized disposable income in 2007, that is €15,480 and $21,200 dollars in 2007 prices and $17,600 in 2000 prices. The policy represents a 1.1 percent increase in annual income for families in the bottom quartile.

We do not include studies like Gertler (2004), Fernald et al (2008, 2009) that analyze Mexico's *Oportunidades* program because it requires as conditions for participation meeting healthcare-focused requirements.