THE CAUSAL EFFECT OF AN INCOME SHOCK ON CHILDREN'S HUMAN CAPITAL*

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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 little impact of the cash transfer on children's health and academic performance and can bound our estimates to a reasonably tight interval around zero. We do not find an effect on household structure, maternal employment, parental time or money investments in children. There is evidence that the benefit increased household expenditure on big ticket items. However, this increase in material resources did not enhance early childhood development in the Spanish context, characterized by a generous social safety-net system. Our results contribute to our understanding of which interventions are effective at improving children's health and human capital formation.

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I. INTRODUCTION

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; Black et al. 2020). Cash transfers to families with children can be an effective tool to mitigate growing socio-economic inequalities stemming from gaps in early childhood investments and a vast literature has documented that parental wealth is highly associated with better child outcomes starting early-on in a child's life (Brooks-Gunn and Duncan 1997; Almond, Currie, and Duque 2018; Cooper and Stewart 2021). Highly targeted (usually conditional on parental employment) schemes characteristic of the American welfare support system since the mid mid-1990's (Aizer, Hoynes, and Lleras-Muney, 2022), while cheaper, can be complicated to administer and risk not reaching the targeted population because of the complexity associated with claiming benefits. For example, 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. No strings attached may also create disincentives for work. Both reasons may explain why the recent initiative from the Biden's administration to increase the – unconditional and universal - child allowance in the aftermath of the COVID crisis has stalled. In this paper, we exploit the unexpected introduction of a one-off unconditional and 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.

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 nor the timing of conceptions or change in-utero investments. Also, benefit take-up was close to full, and the policy change monotonously increased income for all eligible families, independently of income, earnings, or labor force status.

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-to-mid-childhood 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 Carneiro, Løken, and Salvanes' (2015) and 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 did 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 bound our estimates to a reasonably tight interval around zero, so that we can reject positive effects of the magnitudes reported in most previous studies using similarly sized income shocks (Duncan, Morris, and Rodrigues 2011; S. E. Black et al. 2014; Aizer et al. 2016; de Gendre et al. 2021; A. C. Barr, Eggleston, and Smith 2019). 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 outcomes in any of the subsamples.

Using a variety of representative large-scale Spanish surveys we fail to find statistically significant long run effects on subsequent fertility, a mother's probability of being partnered and her propensity to work, and parental time investments and market childcare use. We find that the cash transfer facilitates spending on big-ticket items such as major home repairs and household appliances.

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 and hospitalization rates, which have been extensively used in previous work. Out of the nine 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, the age of the child at measurement, the socio-economic characteristics of the targeted population, or the size of the income shock cannot consistently explain the lack of positive marginal effects implied by our causal estimates. In particular, four of the papers report positive impacts of increases in income on cognitive development of children during middle childhood (Milligan and Stabile 2011; Duncan, Morris, and Rodrigues 2011; Dahl and Lochner 2012; 2017; A. C. Barr, Eggleston, and Smith 2019). 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; Duncan, Morris, and Rodrigues 2011; S. E. Black et al. 2014; Dahl and Lochner 2017; A. C. Barr, Eggleston, and Smith 2019).

Our re-scaling comparison exercise also allows us 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 (Duncan, Morris, and Rodrigues 2011; S. E. Black et al. 2014; Aizer et al. 2016; de Gendre et al. 2021; A. C. Barr, Eggleston, and Smith 2019).

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, conditional cash transfers, and childcare subsidy programs finding positive effects from small income shocks.

The Spanish safety net may also help explain why our estimated effects are close to zero whereas similar one-off cash transfers interventions find larger impacts of income on health and development outcomes (de Gendre et al. 2021; A. Barr et al. 2022). Similar to other European countries, and unlike the US and Australia, access to health care, early childcare, and maternity leave is universal in Spain (Farré and González 2019; González and Trommlerová 2021). Liquidity constraints are thus unlikely to have important effects on access to health care or early childhood care. Time constraints are also less pressing in Spain compared to countries with no universal maternity leave and universal childcare systems, such as United States and Australia before 2011. The literature has also documented an increase in big-ticket items spending from other cash transfer programs such as the EITC, particularly expenditure associated to private transportation (Goodman-Bacon and MacGranahan 2008; Despard et al. 2015). A. Barr et al. (2022) argue that a potential avenue by which the big-ticket items affect children's outcomes is by increasing the capacity of parents to maintain employment. These monetary investments seem to be of somewhat less importance in generating persistent increases in parental earnings in the Spanish context, where providing your own transportation is less critical and there is already a generous safety net.

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 (2021) for recent literature reviews). The universal and unconditional 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 conditional cash transfers or welfare to work experiments 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] Compared to most previous studies that evaluate income shocks during early life, the unanticipated policy change resulting

[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).

from the baby bonus allows us to confidently rule out other potential confounding factors such as conception and birth timing manipulation behaviors resulting from policy anticipation effects (Borra, González, and Sevilla 2019).

Using large administrative data we are also able to evaluate the causal 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). With a few exceptions (Milligan and Stabile 2011; Duncan, Morris, and Rodrigues 2011; Dahl and Lochner 2012; 2017), most studies do not analyze the effect of income shocks on children's outcomes during middle-childhood, as population-level data is more broadly available in the form of birth registries or adult labor registries. 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).

We also contribute to the recent debates in the literature of whether unconditional universal programs such as the baby check analyzed here may discourage recipients from seeking paid work (Hoynes and Rothstein 2019; Jones and Marinescu 2018). Previous targeted schemes such as the Earned Income Tax Credit (EITC) in the US and the welfare to work experiments in the US and Canada are usually contingent on work. We find no evidence of substitution effects leading to negative work incentives for mothers that were exposed to the bonus, in line with the lack of impact on health and educational outcomes. 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, found to have no impact on recipients' labor supply resulting from the additional cash (Jones and Marinescu 2018; Kangas, Jauhiainen, and Simanainen 2021).

The remainder of the paper is organized as follows. Section II describes the institutional framework. Section III lays out the identification strategy. Section IV introduces the data and provides key descriptive statistics. Section V reports our main results. Section VI presents evidence on the mechanisms, and Section VII concludes.

II. 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 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. Unlike Australia's baby bonus, none of them was cancelled or modified by the introduction of the baby-check that created a monotonous increase in income for all eligible families completely unrelated to labor force status or earnings.

The newly announced cash transfer was also sizably larger than most previous benefits. 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]

The benefit was highly publicized. The first subsidies were paid in November of 2007, and take-up was very high. González and Trommlerová (2021) use data from tax returns and social security records and document close to full take-up for the duration of the benefit.

[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).

III. 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 I 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.

III.A. Data Sources

Panel A in Table I describes the main features of the registers containing health outcomes, which include primary health problems, referrals to health specialists, 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 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 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

[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.

[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

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 I 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 diagnostic-assessment tests for the whole population of Andalusian 2nd graders, externally conducted by the Agency 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' 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.

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

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hospital (not the parents) at the time of birth, and contains the date and time of birth, as well as the doctor's signature.

with data on the children born in June and July 2007 to perform additional robustness checks.

Column 5 in Table I 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.

III.B. 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, and cognitive test scores (J. 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 II reports summary statistics for this sample.

There are two sets of primary health outcomes observed for children between the ages of 0 and 8 (see Panel A of Table II).[5] The first set of primary health outcomes refers to 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

[5] In Appendix table A.3 we also report results for anthropometric measures at age 4: height-for-age, weight-for-age, BMI z-scores, and 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.

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).

To understand if the number of health problems is capturing utilization, or actual changes in health, we explore the socioeconomic status gradients for primary health care utilization. Coincident with cross-sectional correlations reported by Cesarini et al (2016) for Sweden, low socioeconomic status is also associated to increased health problems and increased use of health services in Spain (see Figure A.1, panel A). This is not surprising given the universal coverage of Spain's national health system and therefore we would expect that increased household income reduces the number of health problems and healthcare visits.

The second 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 II 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.

Again, similarly to the primary care problems, we observe that higher income is associated with lower hospitalization rates (Figure A.1, panel B). Therefore, we would also expect that increased income reduces hospitalizations.

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 II). 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.[6] 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, managed by the Andalusian Evaluation Agency, while Catalonian grades are internal evaluations, managed by individual teachers, means that we need to study each region separately. In Andalusia, we also know whether students were retained in a grade. Panel B in Table II 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.

[6] 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.

IV. 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. In our main datasets we can exploit information about the exact day of birth (see Column 3 in Table I for details). Thus, we estimate the causal impacts of the policy change using a difference-in-discontinuity design similar to Carneiro, Løken, and Salvanes (2015), Grembi, Nannicini, and Troiano (2016) and Bertrand, Mogstad, and Mountjoy (2020).[7] In essence, this model implements an RD strategy using as additional controls children born on the same dates in an earlier year. Comparing children born before and after July 1st 2007 in a conventional RD design may capture the impact of the policy together with a date-of-birth effect.[8] If we can assume that this date-of-birth effect does not change across years, we can obtain the impact the policy change by using children born in an earlier year as controls (Carneiro, Løken, and Salvanes 2015). 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:

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[7] We show that our results are very similar if we follow a simple regression discontinuity design comparing outcomes for children born right after the cutoff to those born right before (see Figures A.2 and A.3 and Tables A.8-A.10). As we explain more in detail below, for some of our datasets, we only have information at the monthly level, and we thus compare children born in July vs. June in 2007 to those born in 2006 (see equation (2)).

[8] For instance, date of birth may affect children's outcomes through school starting age cutoff dates, because children who are older for their school cohort tend to perform better during primary schooling (Dhuey et al. 2019). Date of birth may also affect children's outcomes through differences in in-utero conditions: exposure to sunlight while in the womb protects against developing asthma later in life (Wernerfelt, Slusky, and Zeckhauser 2019).

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

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. We cluster standard errors by date of birth.

The exact date of birth is not available in the BIFAP data set that we use to study health outcomes for children aged 0 to 4. We therefore use information on month and year of birth instead and estimate the following equation:

$$Y_i = \alpha + \gamma_1 Reform_i + \gamma_2 Post_i + \beta Reform_i * Post_i + \varepsilon_i \quad (2)$$

where $Post_t$ is an indicator variable equal to 1 if the child is born in July and 0 if the child is born in June. This specification is closer to a differences-in-differences design. Still, we only use observations near the threshold as treatment and control groups which are more likely to share similar characteristics and trajectories.[9]

For the study of hospitalization rates, where the dependent variable is measured as a daily-aggregate, Equation 1 takes the form:

[9] Due to the smaller sample size, we also include specifications using 2- and 3-month windows around the cutoff when using survey instead of population data in our mechanisms analyses of Section VI.

$$Y_t = \alpha \ + \gamma_1 Reform_t + \gamma_2 Post_t + \beta Reform_t * Post_t + f(Date_t) * [\gamma_3 + \gamma_4\ Reform_t + \gamma_5 Post_t + \gamma_6 Reform_t * Post_t] + \varepsilon_t \quad (3)$$

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 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 all equations, β is our main parameter of interest and captures the difference 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 I 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 II). We estimate the model in Equation (1) with pre-determined child and family characteristics as outcome variables. With the exception of the probability of the child being a girl for 0 to 4 in the BIFAP Primary Health Care Data, none of the point estimates, plotted in Figure II, 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 V, 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. [10]

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[10] 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 no significant impact of being born after July 1st on education outcomes for Andalusian children in 2009 compared to children born in 2008.

V. RESULTS

V.A. Main Results

Health Outcomes

Table III summarizes the impact of the baby bonus on healthcare measures. Panel A reports results for primary care outcomes for 0 to 4 year-olds from estimating Equation (2) on BIFAP data, and Panel B reports results for primary care outcomes for 5 to 8 year-olds from estimating Equation (1) on BDCAP data. 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 -0.02 and +0.08 standard deviation units. In all cases, estimates are not statistically distinguishable from zero. With the exception of referrals for 5 to 8 year-old children, the precision of the estimated effects is high, we can bound the effect within ± 0.09 standard deviation units, and we can reject improvements in primary health outcomes larger than 0.08 standard deviation units.

Columns 5 to 8 in Table III 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 reasonably precise and we can reject health improvements that reduce the number of health problems by more than 0.09 standard deviation units.

The results of estimating Equation (3) for the hospitalization outcomes are reported in columns (1)-(5) of Table IV. 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 +0.01 and 0.03 percentage points, and, with the exception of hospitalizations due to injuries and psychological problems, we can rule out reductions in the likelihood of being hospitalized larger than 7 percent.[11]

Table A.3 and Figure A.4 in the Appendix also shows that we find no evidence of increased income affecting any of our anthropometric measures: height, weight, BMI, and the likelihood of being overweight or obese at 4 years of age. Tables A.4 and A.5 further 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 V 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. Our 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 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.

[11] For hospitalization rates, we offer the size of confidence intervals as a percentage of the average rate in the population.

The 95 percent confidence intervals (-0.14 to 0.04 for Andalusia and -0.18 to 0.09 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 -0.16 to 0.03), and borderline significant for Catalonia (-0.125, 95 percent CI -0.27 to 0.02). 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 V 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. 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 deviation units for English. The corresponding 95 percent confidence intervals allow us to reject causal effects larger than 0.12 standard deviation units.

Table A.6 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).[12]

V.B. 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 III and IV 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 testing correction.[13] Overall, we can rule out that the results presented in Section V.A. may hide any significant impact of the bonus for children at specific ages. [14]

Recent literature has documented that boys' behavioral and educational outcomes are disproportionally affected by family disadvantage compared to girls' (Autor et al. 2019).

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[12] Figures A.5 and A.6 in the Appendix equally shows no effect on health problems and hospitalization by diagnosis, type of visit, and type of prescription in primary care.

[13] 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).

[14] Figure A.2 shows that the bonus had no impact on anthropometric measures at specific ages, either.

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 VI splits 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 (2021) 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 VII.

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

VI. MECHANISMS

Previous literature has identified different channels through which higher income at birth can influence children's later-life outcomes, either by directly affecting the availability of parental time investments and money resources spent on human-capital enhancing goods and services (Akee et al. 2010; González 2013) or by indirectly improving parenting and parental mental health through stress reduction (Milligan and Stabile 2011).

Direct effects include parental time and money investments and parents' demand for additional children. 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). Parental money investments in child-related goods such as food, clothing, and books, may also improve their children's human capital (Yeung, Linver, and Brooks-Gunn 2002; Milligan and Stabile 2011; Caucutt and Lochner 2020). Also, 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; González and Trommlerová 2021). If parental investments per child decrease with family size (Becker and Lewis 1973; Price 2008), children in larger families may display worse outcomes.[15]

Indirect effects relate to parenting and stress. 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 and a higher likelihood of living in partnerships (Akee et al. 2018).

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. Many conditional cash programs such as the EITC are based on eligibility criteria that create incentives for increased fertility, lone parenthood, and

[15] 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).

reduced labor income. Previous literature has found negligible impacts on fertility and larger impacts on marriage, while the evidence is mixed regarding labor supply (see Aizer Shari Eli Adriana Lleras-Muney et al. 2020; Low et al. 2018; Guryan, Hurst, and Kearney 2008; Kearney 2004). Learning about potential work, marriage, and fertility incentives of universal cash policies, such as the baby check, may contribute to the debate regarding the potential negative impacts of targeted policies and universal basic income policies on welfare dependency and labor supply (Hoynes and Rothstein 2019; Jones and Marinescu 2018). Understanding whether and how parental outcomes were affected by the policy change may also be relevant for the effective design of policy.

To study potential parental behavioral changes as a result of the policy, we first 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 indicators for parental time investments, demand for additional children, and family conflict, measured from ages 0 to 8.

Column (1) in Table VIII presents the results from estimating Equation (2) for maternal labor supply and, in line with previous studies investigating the impact of the Cherokee casino opening (Akee et al. 2010) and the Norwegian childcare subsidies (Black et al. 2014), shows that maternal time investments were not greatly affected by the policy change. The estimated impact of benefit eligibility is not significantly different from zero, and we can rule out relatively modest reductions in maternal labor supply (for instance, larger than 0.7 percentage points or 1.4 percent for our preferred specification in Panel A). Also, Panel A in Figure V plots the impact of the baby bonus on maternal employment by age of the child and also shows that benefit eligibility had no significant impact on maternal labor force participation, especially in the long term.[16] Finally, Table A.7 in the Appendix use the Spanish EU-SILC (2006-2016) to show that neither childcare or schooling arrangements of children under 8 years of age were affected by baby bonus eligibility.[17] In sum, availability of the baby bonus does not seem to have significantly affected time investment in affected children.

Columns 2 to 4 in Table VIII and Panels B to D in Figure V look at the impact of the bonus on family structure: subsequent fertility, divorce, and partnership status. Consistent with most previous analyses on the impact of welfare benefits on fertility (Moffitt 1998; Almond, Hoynes, and Schanzenbach 2011), we fail to find a robust and significant increase in the demand for additional children, as evidenced in Column 2 in Table VIII and Panel B in Figure V. When using our preferred specification in Panel A of Table VIII, we are able to rule out increases in subsequent fertility larger than 2 percentage points (7 percent) and, when looking at differentiated impacts by age of the child in Panel B in Figure V we find no significant short or long-term impacts of the bonus on subsequent fertility.[18] In sum, there is no evidence in support for changes in the demand for additional children as a result of benefit eligibility.

Our findings for marital status in Columns 3 and 4 of Table VIII, however, paint a slightly different picture. Even if none of the estimates is significantly different from zero

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[16] 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.

[17] Note, however, that we do not have enough power to reject small time investments in this survey, for instance, smaller than 10% of a standard deviation.

[18] Our fertility results are also consistent with González's (2013) and González and Trommlerova's (2021) documented reductions in abortions. As explained in González and Trommlerova (2021), less abortions in July 2007 would lead to more births in December 2007, outside of our one-, two-, and three-month window of analysis. Note that the overall increase in fertility as a result of the introduction of the bonus reported in González and Trommlerová (2021a) is perfectly congruent with our findings, given that mothers with children born before and after the cutoff were equally likely to increase subsequent fertility.

in our 1-month window, in line with previous literature documenting an improvement in parental relationships within the household once Casino payments begin (Akee et al. 2018), we do find a significant reduction in mothers' divorced status when increasing the power of our analyses in the 2- and 3- month windows in Column 3 of Table VIII.[19] Panel C in Figure V shows that the effect is short-lived though, and benefit eligibility indeed led to a lower probability of divorce during the two years following childbirth. The fact that eligible mothers are no less likely to be living with a partner (Column 4 in Table VIII and Panel D in Figure V) suggests that neither family conflict nor money and time investments in the child were drastically affected.

To gain a better understanding of potential changes in monetary investments in children as a result of the policy, we also look at data from the Spanish Household Budget Survey (2008). The survey interviews a representative sample of the Spanish population of about 24,000 households each year and collects information on monetary expenses according to the official European classification of expenditures. We look at total expenditure, child-related expenditure, and expenditure on big-ticket items, including kitchen appliances, large furniture, home repairs, and vehicle purchases and repairs (Despard et al. 2015).

Consistent with previous analysis on how EITC recipients spend their refunds (Goodman-Bacon and MacGranahan 2008; A. Barr et al. 2022), Table IX suggests that eligible families increased expenditure not only in necessities, such as food, but also in big ticket items, such as household appliances and home repairs and renovations. The estimated impacts are large, implying two-fold increases in home repairs, for instance. However, due to the much smaller sample-size used in this analysis we interpret the results with some caution. Taken together, these results along with the lack of significant

[19] The results for divorced status are much less precisely estimated given the lower proportion of divorced mothers in the sample (0.5%).

effects on maternal employment suggest that these monetary investments in big-ticket items did not facilitate parents' capacity to maintain employment, unlike the evidence provided by A. Barr et al. (2022) for the United States.

VII. 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 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 X 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, Currie, and Duque, 2018 and the meta-analysis of Cooper and Stewart (2013; 2021). We add to this list a couple of very recent papers looking at policy changes involving increases in income during infancy, but not later on: one study analyzing the impact of the Australian baby bonus on children's hospitalizations (de Gendre et al. 2021), and another study investigating the impact of EITC on children's cognitive test-scores for families with children born in December rather than January (A. C. Barr, Eggleston, and Smith 2019).

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 (2021), 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. 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. Only 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 – and the study by De Gendre et al. (2021) investigating the impacts of the Australian baby-bonus

[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. We also exclude the literature studying in-kind programs such as housing vouchers, universal early-life healthcare, or early childhood education programs (Heckman et al. 2010; Raj Chetty et al. 2016; Wüst 2022).

[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 (2021) 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.

show 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]

One obvious potential explanation for the differences between our insignificant impacts and the positive previously reported causal estimates of income changes 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 20-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 3 in Table X reveals that the €2,500 Spanish baby bonus corresponds to an annuitized income increase of about $175 in 2000 prices, or to about a 0.7 percent

[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).

[23] We use the US$ Purchasing Power Parities (OECD 2021) and the US Consumer Price Index (BLS 2021).. 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 IX in Appendix B Notes to Table IX.

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 0.2 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, Morris, and Rodrigues 2011), the Norwegian childcare subsides (S. E. Black et al. 2014), the U.S. Mothers' Pension Program (Aizer et al. 2016), the U.S. EITC (A. C. Barr, Eggleston, and Smith 2019), and the Australian baby bonus (de Gendre et al. 2021).

Figure VI plots our main estimates, re-scaled for a $1,000 yearly income increase, and compares them to the adequately re-scaled estimated effects from those studies concerning permanent income shocks lower than $500 annually (S. E. Black et al. 2014; Duncan, Morris, and Rodrigues 2011; de Gendre et al. 2021; A. C. Barr, Eggleston, and Smith 2019). With the exception of the estimated impact of income on cognitive achievement from the welfare to work experiments in Duncan, Morris, and Rodrigues (2011), whose positive effect is contained within our confidence intervals, our 95% confidence intervals allow us to rule out effect sizes of the magnitude described in most studies finding positive impacts. We can rule out increases in GPA, similar to those implied by S. E. Black et al. (2014) and Barr, Eggleston, and Smith (2019); increases in Math and English scores, implied by Barr, Eggleston, and Smith (2019); and reductions in hospitalization rates, implied by de Gendre et al. (2021). All in all, Figure VI shows that our marginal effects are significantly smaller than the previously reported causal estimates from similarly sized income shocks, suggesting that the magnitude of the modestly sized income shock analysed here may not help explain why we find significantly smaller effects.

Column 4 in Table X presents the outcomes analyzed in these studies, and the age of the child at measurement, where outcomes and age of the child that are directly comparable to ours are highlighted in bold. Heterogeneity in the outcome measured or the age of the child at measurement does not seem to be a plausible reason behind the differences between our insignificant impacts and the previously reported causal estimates of income changes either, given that there is considerable overlap between our outcomes and ages and those of previous work. Educational outcome measures, such as the likelihood of grade retention and standardized math and English test scores and GPA considered in Table V, have previously been investigated by Milligan and Stabile (2011), Dahl and Lochner (2012, 2017), Black et al. (2014), Cesarini et al. (2016), or Barr et al. (2019). Similarly, Milligan and Stabile (2011), Aizer et al. (2016), Cesarini et al. (2016) and de Gendre et al. (2021) study health outcome measures analyzed here such as standardized weight, height, and BMI, as well as overall and respiratory hospitalization rates (also shown in bold). Furthermore, Milligan and Stabile (2011), Duncan, Morris and Rodrigues (2011) and Dahl and Lochner (2012, 2017) study health and cognitive outcomes during middle childhood.

Column 5 in Table X 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 VII 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.

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 our study shares with the Swedish study is the availability of universal, publicly funded healthcare systems, free early childcare education from age 3, and relatively generous paternal leave in both Sweden and Spain. This type of institutional setting is in stark contrast with the U.S. context at the beginning of the 20th century of Aizer et al.'s (2016) analysis. However, Spain's public safety net might not be so different from the institutional contexts at the end of the 20th century in the U.S., as in Barr, Eggleston, and Smith (2019), or nowadays in Australia, as in de Gendre et al. (2021), and it clearly shares many features with the institutional settings in two countries like Norway and Canada for which previous literature has found large positive effects (S. E. Black et al. 2014; Milligan and Stabile 2011).

The other commonality between lotteries like the Swedish study 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 González (2013), or in the medium run as documented in Tables VIII and IX, and Appendix Table A.7). It is thus plausible that, in line with the findings of Hendren et al. (2020), 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 with in-kind transfers and most of the welfare-to-work experiments and childcare subsidy programs finding positive effects from income shocks (see also J. M. Currie 1995; J. Currie and Gahvari 2008).

It is finally possible that the fact that the benefit is received in a lump-sum payment as opposed to monthly installments, as, for instance, Biden's expanded Child Tax Credit (Parolin et al. 2021), may favor expenses in big-ticket items instead of more child-oriented investments, as shown in Table IX.

VIII. 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, parental separation, or mothers’ subsequent fertility. We did find suggestive evidence of increases in big-ticket item purchases as a result of the bonus. However, these investments did not translate into better employment outcomes for affected families, unlike the results documented by (A. Barr et al. 2022) for a similar cash transfer in the US.

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.

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. In line with the findings from Hendren et al. (2020),

[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

the evidence suggests that policies targeting families from lower socioeconomic status or tied to a specific expenditure might be more effective in improving children's health and educational performance. Also, in line with the results documented by (Amarante et al. 2016) and (Hoynes, Miller, and Simon 2015) it may also be possible that child benefits received during pregnancy that improve health outcomes at birth might prove more successful than later interventions. Finally, we cannot rule out that repeated cash transfer programs such as the recent Biden's Extended Child Tax Credit can be more effective than a one-off lump sum payment in the presence of diminishing marginal effects of income. In turn, cash transfer received more regularly might allow credit constrained families to purchase better food and face rent and mortgage payments in ways that promote better childhood environments (Parolin et al. 2021).

These results should be interpreted in the context of a country that has a number of 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 (2022) 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).

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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TABLES AND FIGURES

Table I: 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. Month and year of birth	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. Exact date of birth	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. Exact date of birth	Hospitalization rates by age and diagnosis (ICD-9)	
Vital Statistics (2006-2007)	Child	Administrative population data on all births in Spain. Exact date of birth	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. Exact date of birth	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. Exact date of birth	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. Month and year of birth	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. Month and year of birth	Overall monetary expenditure, child-related expenditure, and expenditure on big-ticket items, including kitchen appliances, large furniture, home repairs, and vehicle purchases and repairs

Panel D. Time Use			
Life Conditions Survey (2006-2016)	Child	Representative survey data of Spanish population. Children aged 0 to 10. Month and year of birth	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 II: Overview of Outcome Variables and Summary Statistics

Outcome	Register	Def./units	mean	sd	Obs.
Panel A. Healthcare Outcomes					
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 III. Effect on Primary Health Care Outcomes

	(1) Health Problems (number)	(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	0.074	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
Std. Coefficient	-0.009	0.027	0.010	0.007	-0.009	0.005	-0.021	-0.006
CI in sd units	(-0.08, 0.06)	(-0.04, 0.10)	(-0.06, 0.08)	(-0.06, 0.08)	(-0.08, 0.06)	(-0.06, 0.08)	(-0.09, 0.05)	(-0.07,0.06)
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
Std. Coefficient	0.079	-0.025			0.093	0.062	0.109	0.097
CI in sd units	(-0.04, 0.20)	(-0.16, 0.11)			(-0.03, 0.22)	(-0.07, 0.19)	(-0.02, 0.24)	(-0.02, 0.22)
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, we estimate equation (2); 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 IV. Effect on Hospitalizations

	(1) All Stays (hospitalization rate)	(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
Proportion	4.5%	12.5%	8.9%	5.7%	39.7%
CI in % units	(-5.9, 14.9)	(-5.8, 30.8)	(-6.6, 24.4)	(-22.3, 33.7)	(-10.5, 89.9)
Observations	122	122	122	122	122
Linear Trend	Yes	Yes	Yes	Yes	Yes

Note: This table shows the estimates from equation (3) 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 V. Effects on School Outcomes

	(1) Math (standardized)	(2) Spanish (standardized)	(3) Repeater (0/1)	(4) Catalan (standardized)	(5) English (standardized)	(6) GPA (standardized)
Panel A. Andalusia						
Effect	-0.048	-0.064	0.013			
	(0.046)	(0.050)	(0.008)			
CI	(-0.14, 0.04)	(-0.16, 0.03)	(-0.003, 0.03)			
Observations	29,590	29,632	30,975			
Proportion			4.2%			
Controls	No	No	No			
Linear Trend	Yes	Yes	Yes			
Panel B. Catalonia						
Effect	-0.042	-0.125*		-0.0766	-0.0311	-0.0824
	(0.070)	(0.075)		(0.0726)	(0.0796)	(0.0756)
	(-0.18, 0.09)	(-0.27, 0.02)		(-0.21, 0.06)	(-0.18, 0.12)	(-0.23, 0.06)
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 VI. Effect on Health and Schooling Outcomes by Gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Health Outcomes					Schooling Outcomes			
	Health Problems 0/4 (number)	Health Problems 5/8 (number)	Referrals 0/4 (number)	Referrals 5/8 (number)	Hospitalizations (hosp. rate)	Math in Andalusia (standardized)	Math in Catalonia (standardized)	Spanish in Andalusia (standardized)	Spanish in Catalonia (standardized)
Panel A. Boys									
Effect	0.084	0.624	0.046	0.003	0.100	-0.044	-0.119	-0.0722	-0.119
	(0.794)	(0.635)	(0.147)	(0.068)	(0.090)	(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	0.797/0.132				
Observations	6,206	8,428	6,206	8,428	122	15,329	6,137	15,349	6,142
Std. Coeff.	0.005	0.098	0.016	0.004	7.3%				
CI in sd units	(-0.09, 0.10)	(-0.09, 0.29)	(-0.08, 0.11)	(-0.17, 0.18)	(-4.5, 19.1,)	(-0.14, 0.05)	(-0.33, 0.09)	(-0.20, 0.05)	(-0.30, 0.07)
Controls	No	No	No	No	No	No	No	No	No
Panel B. Girls									
Effect	-0.277	0.328	0.130	-0.041	-0.039	-0.053	0.029	-0.034	-0.110
	(0.778)	(0.512)	(0.130)	(0.063)	(0.070)	(0.067)	(0.100)	(0.0636)	(0.103)
Mean/SD	22.662/14.848	5.408/6.337	1.346/2.486	0.211/0.751	0.636/0.109				
Observations	5,856	8,007	5,856	8,007	122	14,261	5,807	14,283	5,811
Std. Coeff.	-0.019	0.052	0.052	-0.055	0.3%				
CI in sd units	(-0.12, 0.08)	(-0.10, 0.21)	(-0.05, 0.15)	(-0.21, 0.10)	(-13.5, 14.1)	(-0.18, 0.08)	(-0.17, 0.22)	(-0.16, 0.09)	(-0.31, 0.09)
Controls	No	No	No	No	No	No	No	No	No

Note: This table shows the estimates from equations (1), (2), and (3) 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. For hospitalizations, the standardized coefficient and corresponding confidence intervals are given as proportions of the mean * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VII. Effect on Health and Schooling Outcomes by Socioeconomic Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Health Outcomes			Schooling Outcomes			
	Health Problems 5/8 (number)	Referrals 5/8 (number)	Hospitalizations (hosp.rate)	Math in Andalusia (standardized)	Math in Catalonia (standardized)	Spanish in Andalusia (standardized)	Spanish in Catalonia (standardized)
Panel A. Low Income							
Effect	1.078**	0.022	0.074	-0.047	-0.046	-0.093	-0.119
	(0.510)	(0.062)	(0.052)	(0.064)	(0.103)	(0.056)	(0.114)
Mean/SD	5.946/6.712	0.261/0.831	0.811/0.082	-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
Std. Coefficient	0.161**	0.026	9.1%				
CI in sd units	(0.01, 0.30)	(-0.12, 0.17)	(-3.4, 21.7,)	(-0.17, 0.08)	(-0.24, 0.15)	(-0.20, 0.02)	(-0.34, 0.10)
Controls	No	No	No	No	No	No	No
Panel B. High Income							
	-0.221	-0.108	-0.013	-0.026	0.042	-0.007	-0.043
	(0.770)	(0.093)	(0.042)	(0.055)	(0.097)	(0.063)	(0.111)
Mean/SD	5.259/6.080	0.204/0.705	0.568/0.057	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
Std. Coefficient	-0.036	-0.153	-2.0%				
CI in sd units	(-0.28, 0.21)	(-0.41, 0.10)	(-16.7, 12.2)	(-0.13, 0.08)	(-0.15, 0.23)	(-0.13, 0.11)	(-0.26, 0.17)
Controls	No	No	No	No	No	No	No

Note: This table shows the estimates from equations (1), (2), and (3) 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 mean at the province level 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. For hospitalizations, the standardized coefficient and corresponding confidence intervals are given as proportions of the mean. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table VIII. Mechanisms

	(1) Maternal labor supply	(2) Subsequent fertility	(3) Divorced	(4) Partnered
Panel A: 1 month				
Effect	0.021	-0.006	-0.002	-0.002
	(0.014)	(0.012)	(0.006)	(0.008)
Mean/SD	0.500/0.500	0.247/0.431	0.049/0.216	0.910/0.286
Observations	19,469	19,469	19,469	19,469
Proportion	4.2%	-2.4%	-4.1%	0.2%
CI in % units	(-1.3,9.7)	(-11.9,7.1)	(-28.0,19.9)	(-1.5,1.9)
Controls	No	No	No	No
Panel B: 2 months				
Effect	-0.014	0.017*	-0.011**	0.004
	(0.010)	(0.009)	(0.004)	(0.006)
Mean/SD	0.499/0.500	0.252/0.434	0.051/0.220	0.909/0.288
Observations	38,909	38,909	38,909	38,909
Proportion	-2.8%	6.7%	-21.6%	0.4%
CI in % units	(-6.8,1.2)	(-0.4,13.9)	(-37.3,-5.9)	(-0.9,0.4)
Controls	No	No	No	No
Panel C: 3 months				
Effect	-0.003	0.013*	-0.014***	0.007
	(0.008)	(0.007)	(0.004)	(0.005)
Mean/SD	0.500/0.500	0.254/0.435	0.051/0.220	0.907/0.291
Observations	58,146	58,146	58,146	58,146
Proportion	-0.6%	5.1%	-27.5%	0.8%
CI in % units	(-3.8,2.6)	(-0.4,10.6)	(-43.1,-11.8)	(-0.3,1.9)
Controls	No	No	No	No

Note: This table shows the estimates of the impact of the treatment (benefit eligibility) on subsequent fertility, maternal labor supply, paternal labor supply, and marital stability from equation (2). 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. Divorced mother is an indicator variable that measures if the mother is (or became) divorced, when the newborn was between 0 and 8 years old. Partnered mother is an indicator variable that measures if the mother is (or became) partnered, 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 (panel A), born between May and August (panel B), or between April and September (panel C) of 2006 and 2007 . Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table IX. Effects on expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Monetary	Child-related	Food	Communication	Expenditures on Big-Ticket Items				
VARIABLES	Expenditures	Expenditures	Expenditures	Expenditures	Any	Appliances	Furniture	Home Repairs	Vehicles
Panel A: 1month									
Effect	-0.0481	0.263	0.310*	0.428	0.612	0.821	0.643	1.213*	0.548
	(0.138)	(0.260)	(0.159)	(0.434)	(0.691)	(0.773)	(0.817)	(0.668)	(0.897)
CI	(-0.3, 0.2)	(-0.2 , 0.8)	(-0.0 , 0.6)	(-0.4 , 1.3)	(-0.7 , 1.9)	(-0.7, 2.3)	(-0.9, 2.2)	(-0.1, 2.5)	(-1.2, 2.3)
Observations	236	236	236	236	236	236	236	236	236
Panel B: 2 months									
Effect	0.0164	0.131	0.167	0.483*	0.970**	1.088**	0.798	0.781*	0.703
	(0.0950)	(0.176)	(0.107)	(0.272)	(0.468)	(0.512)	(0.577)	(0.461)	(0.585)
CI	(-0.2, 0.2)	(-0.2 ,0.5)	(-0.0, 0.4)	(-0.1, 1.0)	(0.5 , 1.9)	(0.1, 2.1)	(-0.3, 1.9)	(-0.1, 1.7)	(-0.4, 1.8)
Observations	488	488	488	488	488	488	488	488	488
Panel C: 3 months									
Effect	0.0513	0.231	0.163*	0.594***	0.686*	0.712*	0.749	0.983***	0.248
	(0.0772)	(0.147)	(0.0916)	(0.224)	(0.383)	(0.427)	(0.478)	(0.380)	(0.479)
CI	(-0.1, 0.2)	(-0.1, 0.5)	(-0.0 , 0.3)	(0.2, 1.0)	(-0.1, 1.4)	(-0.1, 1.5)	(-0.2, 1.7)	(0.2, 1.7)	(-0.7, 1.2)
Observations	710	710	710	710	710	710	710	710	710
Mean Y (in €)	30278	5025	4372	998.2	3549	228	604.7	224.1	2492
SD (in €)	15844	4655	2504	703.3	7252	504.9	1547	965.9	6785
Controls	No	No	No	No	No	No	No	No	No

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 2). The outcomes are household expenditure (in logarithms) in several categories: overall monetary expenditure, child-related, food, communications, big-ticket items which includes appliances, furniture, home repairs, and vehicles. 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, ...). Communications include phone purchases and internet and land-phone services. The category Big-Ticket Items includes the other four categories: Appliances, Furniture, Home repairs, and Vehicles. Appliances include big household appliances such as refrigerators, washing-machines, tumble dryers, and dishwashers. Furniture expenses include large household furniture items such as tables, beds, and wardrobes. Home repairs include both electrician, plumbing, and decorator services and products and materials directly purchased by households. Vehicle expenses include purchases of new or second-hand vehicles and vehicle repairs, including parts. The data source is survey data from the Household Budget Survey (2008). An observation is a household. The sample is households with children born in June and July (panel A), born between May and August (panel B), or between April and September (panel C) of 2006 and 2007 Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table X. Previous Studies on Financial Incentives and Child Outcomes in Developed Countries

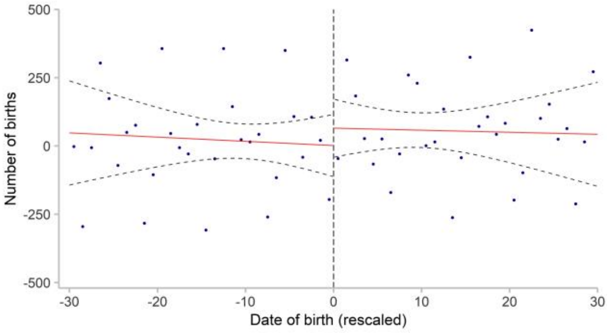
(1)	(2)	(3)	(4)	(5)
Publication	**Source of income variation**	**Annual size of income change (in 2000 US dollars)**	**Outcome (child's age at measurement in years)**	**Targeted to Low Income or Heterogeneity Analysis for Low-Income Sample**
Akee et al. (2010)	US Casino opening *Country:* US *Amount:* $4,000 Unconditional cash transfer for American Indian adults at age 21 (18 if high-school graduates)	$4,000 13% median annual income increase	-Years of education (21) -Prob of HS graduate (21)	Heterogeneity analysis for low-income sample
Milligan and Stabile (2011)	Child Tax Benefit plus National Child Benefit program. *Country:* Canada. *Amount:* Average increase of CAN$2,396 Unconditional cash transfer (dependent on earnings and family size)	$1,607 4% average income increase for the whole population 11% average annual income increase for low-income sample for which significant impacts found	*Cognitive outcomes* **-Ever repeated (4-10)** **-Math (4-10)** **-Peabody PPVT (4-10)** *Health outcomes* -Experience hunger -Good health **-Height (4-10)** **-Weight (4-10)** -Injured last 12m	Targeted to low income families
Duncan et al. (2011)	Random assignment to different MDRC programs *Country:* Canada. *Amount:* Average increase of CAN$1,000 and 2,200. Conditional cash transfers (dependent on employment or training enrolment)	$346 3.2% average income increase for the targeted population	**Cognitive test scores (4-10)**	Targeted to low income families
Dahl & Lochner (2012, 2017 revision)	US Earned Income Tax Credit expansion *Country:* US *Amount:* $1,129 Conditional cash transfer (dependent on employment)	$1,129 5% average annual income increase for the targeted population	*Cognitive outcomes* **Comb math-read (5-14)** **Reading rec (5-14)** **Reading compreh (5-14)** **Math (5-14)**	Targeted to low income families
Black et al. (2014)	Norway childcare subsidies *Country*: Norway *Amount*: Average increase of 10,000 NOK (Near-cash transfer (in-kind transfer)	$247 1.4% annual increase in income for families just before the cutoff	*Cognitive outcomes* **GPA** (13-16) Oral Exam Written Exam	Targeted to low income families
Aizer et al (2016)	Mothers' Pension program (1911-1935) *Country:* US *Amount:* $10-$30 monthly income of 1911-1930 (20% of manufacturing wages) Unconditional cash transfer for single mothers	$430 ($907) 1.7% (3.6%) annual increase in income for targeted families	*Health outcomes* Longevity **Weight** (25) **Height** (25) **BMI** (25)	Targeted to low income families

			Underweight (25) **Obese** (25) *Cog. outcomes* Edu attainment (25) Income (25)	
Cesarini, Lindqvist, Ostling, Wallace (2016)	Sweden lottery wins *Country:* Sweden *Amount:* 1M SEK ($140,000)	$5,355 44% annual increase in disposable income	*Cognitive outcomes* All 6 insignif. or negative -**Cog. Skills** (18) -Noncog. Skills (18) -**GPA** (16) -**English** (16) -**Swedish** (16) -**Math** (16) *Health outcomes* All 18 insignif. but **Obese** (18) **Hosp in 2 years (0-18)** **Hosp in 5 years (0-18)** **Resp. hosp. in 2 (0-18)** **Resp. hosp in 5 (0-18)**	Heterogeneity analysis for low income sample
Barr, Egleston, Smith (2021)	US Earned Income Tax Credit discontinuity for children born before and after January 1st. *Country:* US *Amount:* $1,300 Conditional cash transfer (dependent on employment)	$60 0.2% annual increase in disposable income	*Cognitive outcomes* **Test scores index (8-13)** **Math test scores (8-13)** **English test scores (8-13)** Ever suspended (8-13) HS graduation (18) Earnings (23-25)	Targeted to low income and estimated on low-income sample
De Gendre, Lynch, Meunier, Pilkington, Schurer (2021)	Australia Baby bonus *Country:* Australia. *Amount:* AU$3000. Unconditional universal cash transfer that replaced income-dependent benefits: AU$ 2,157 for low- and middle-income families, but lower (even negative) for higher income families	$90 0.2% of median annual disposable household income 0.5% of annual income for families in bottom decile	*Health Outcomes* **Hospitalizations (<1)** Hosp. in Emergency Dep (<1) Hosp. in Inpatient Serv. (<1) Preventable hosp. (<1) **Respiratory hosp. (<1)**	Heterogeneity analysis for low-income sample
Our paper	Spain Baby bonus *Country:* Spain. *Amount:* €2,500. Unconditional universal cash transfer	$175 0.7% annual increase for the median family 1.0% annual income increase for the bottom quartile	Cog. Outcomes Repeater (7) Math (7) Spanish (7) Catalan (7) English (7)	Heterogeneity analysis for low-income sample

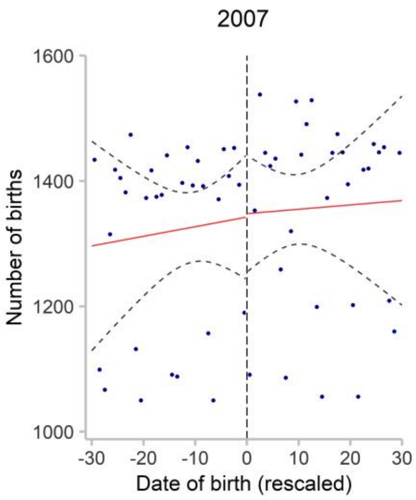
			GPA (8) *Health outcomes* Height (0-4) Weight (0-4) Visits (0-4) Health probs (0-4) (5-7) Referrals (0-4) (5-7) Prescriptions (0-4) Vaccines (0-4) Hospital. (0-8) Resp. hosp. (0-8) Infect. Hosp (0-8) Perinat hosp. (0-8)	

Note: 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; 2021). 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.

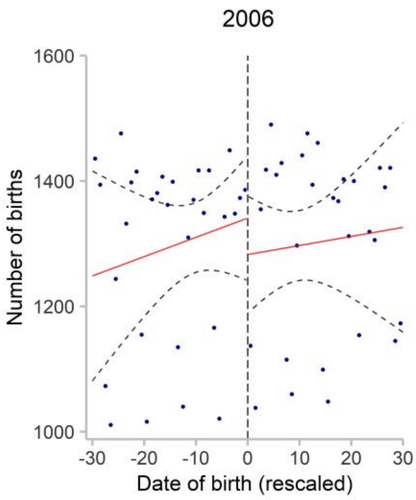
Figure I:
Number of Births
Panel A. Difference



Panel B. 2007



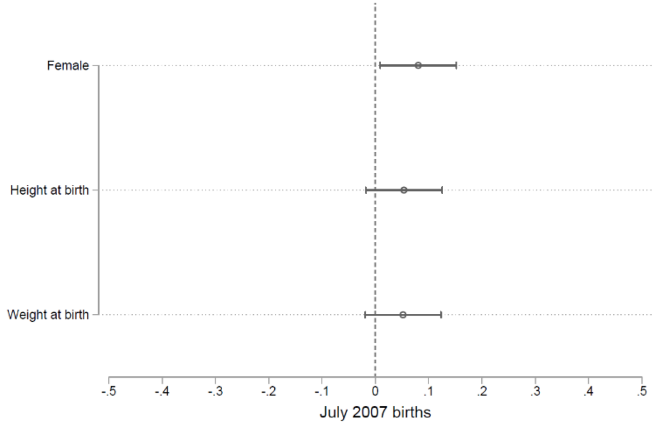
Panel C. 2006



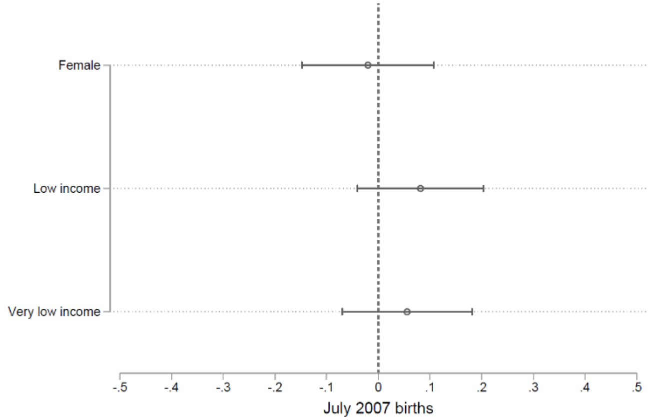
Note: Panel A shows the difference in the daily number of births between 2007 and 2006. Panels B and C show the daily number of births in 2007 and 2006, respectively. Linear fits (with 95% Confidence Intervals) are displayed on both sides of the threshold (July 1). The data source is 2006 and 2007 birth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

Figure II: Balance in Covariates

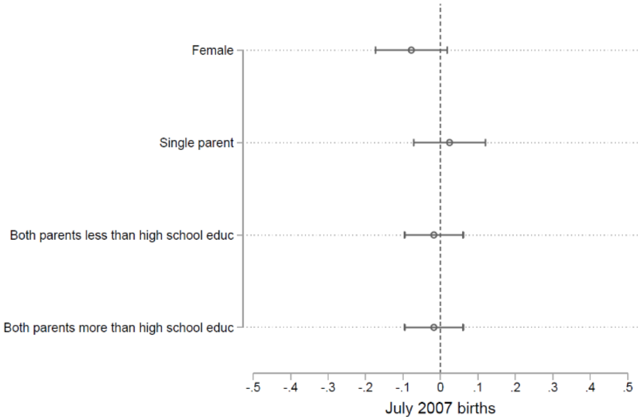
Panel A. Child characteristics. BIFAP Primary Health Care Data



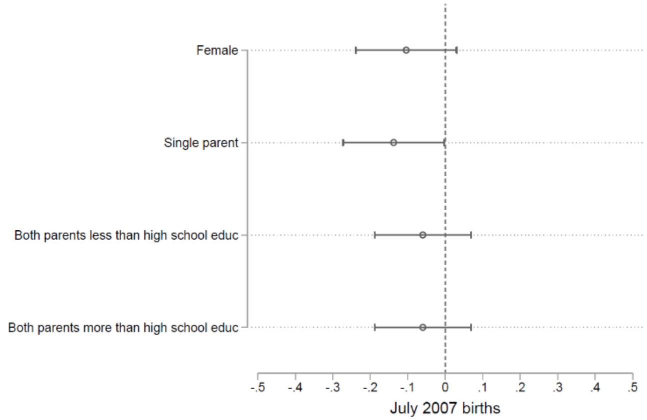
Panel B. Family Characteristics. BDCAP Primary Health Care Data



Panel C. Family Characteristics. Andalusian Educational Data



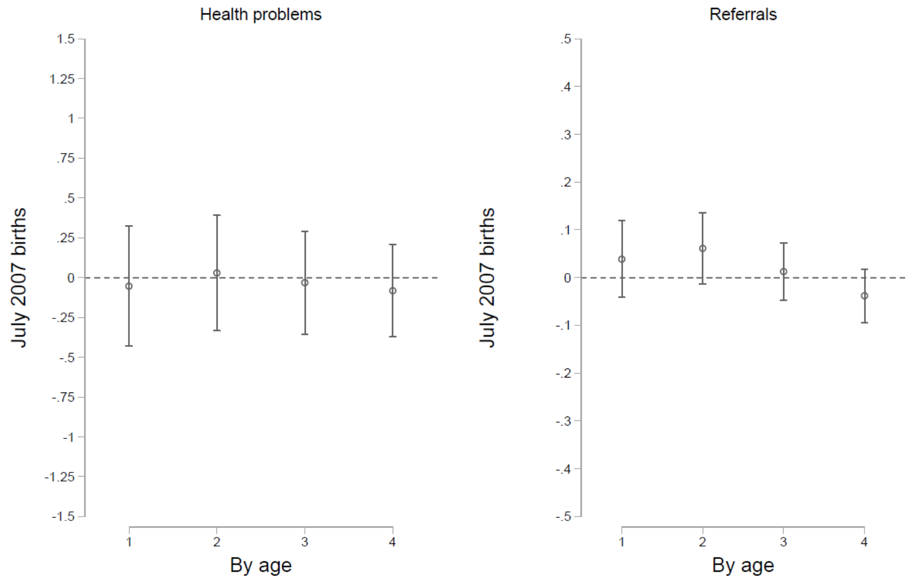
Panel D. Family Characteristics. Catalonian Educational Data



Note: Panel A plots the coefficients and 95% CI of estimating equation (2) on different placebo outcomes for health data (Panel A). Panels B, C, and D plot the difference in discontinuity 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.

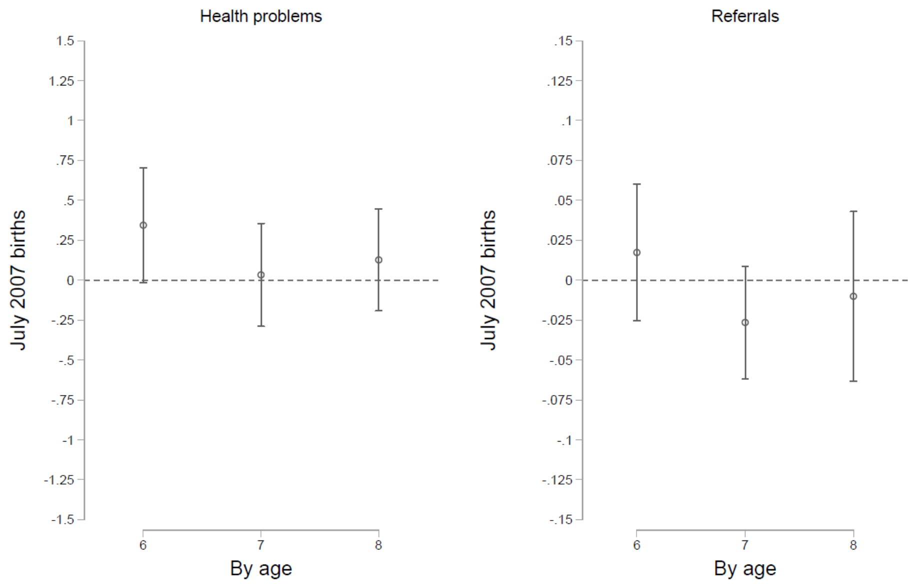
Figure III:
Primary Care Outcomes by Age

Panel A: Ages 0 to 4



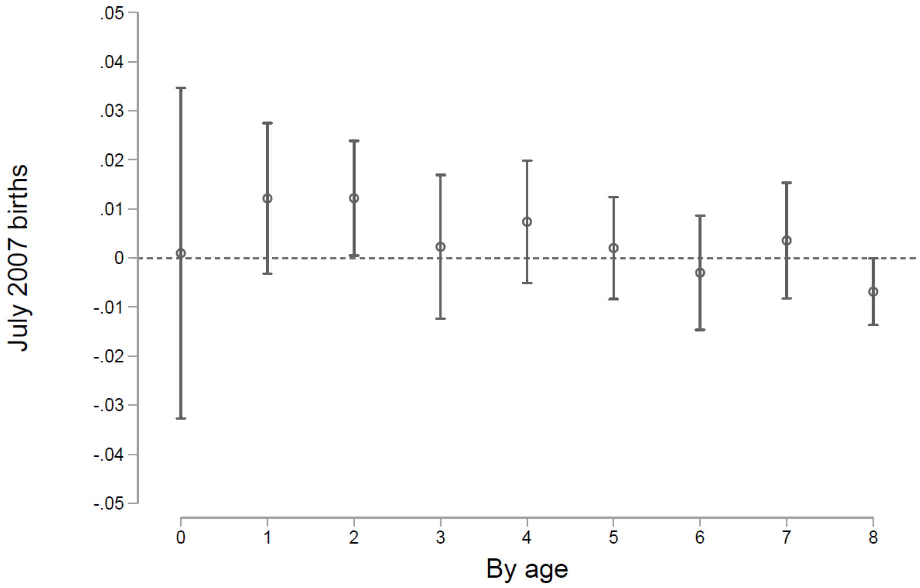
Note: These figures plot the coefficients and 95% CI of the impact of the treatment (benefit eligibility) on number of health problems and referrals by age (0 to 4) (equation (2)). The data source is primary care administrative data from the BIFAP. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007.

Panel B: Ages 5 to 8



Note: These figures plot the difference-in-discontinuity 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.

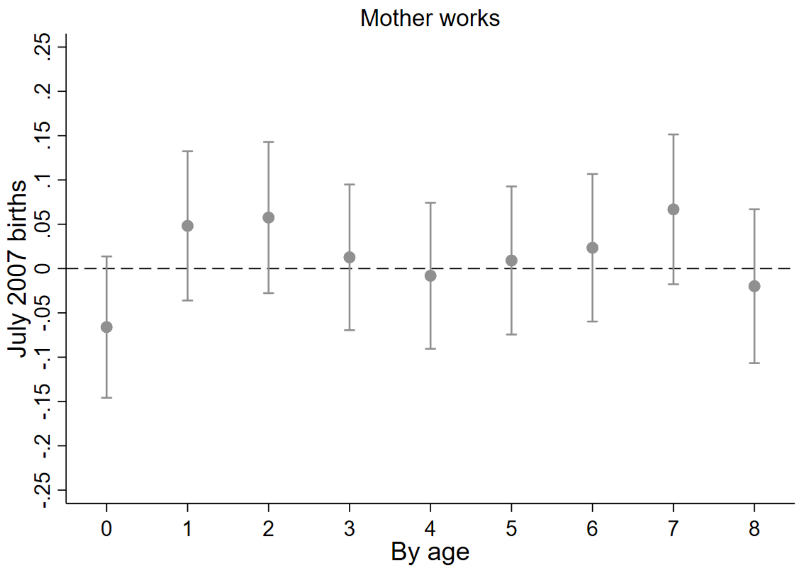
Figure IV:
Hospitalization Effects by Age



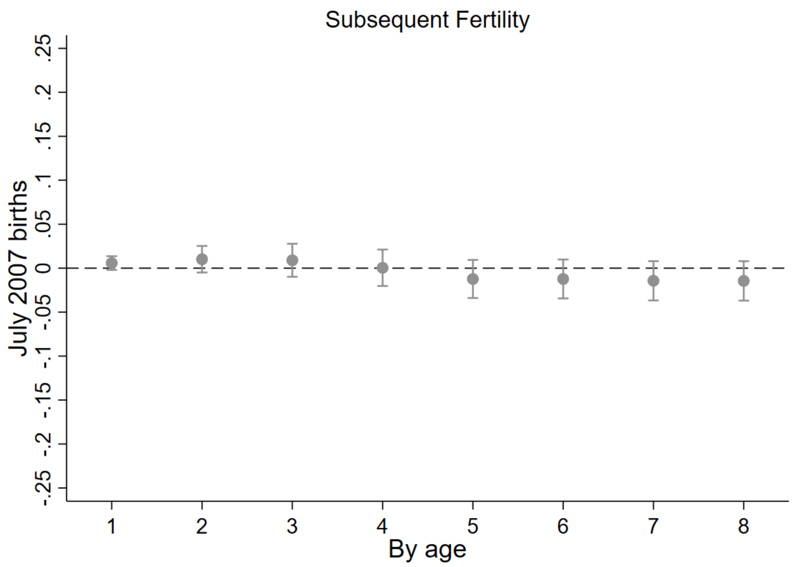
Note: This figure plots the difference-in-discontinuity coefficients and 95% CI of the impact of the treatment (benefit eligibility) on hospitalization rates (number of daily hospital stays over number of daily births) by age (equation (3)). The data source is the Hospital Morbidity Survey 2006-2015. An observation is a day (of birth). The sample includes observations for children born in June and July of 2006 and 2007.

Figure V:
Channels: Maternal Time use and Family Conflict Effects by Age

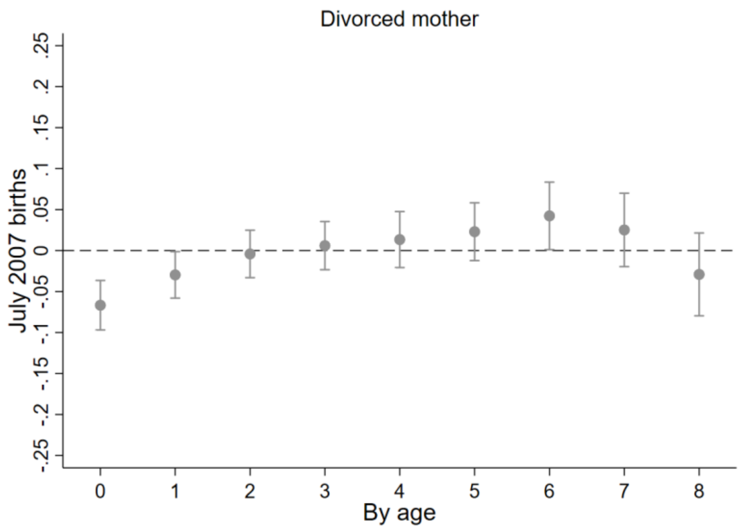
Panel A. Maternal labor supply



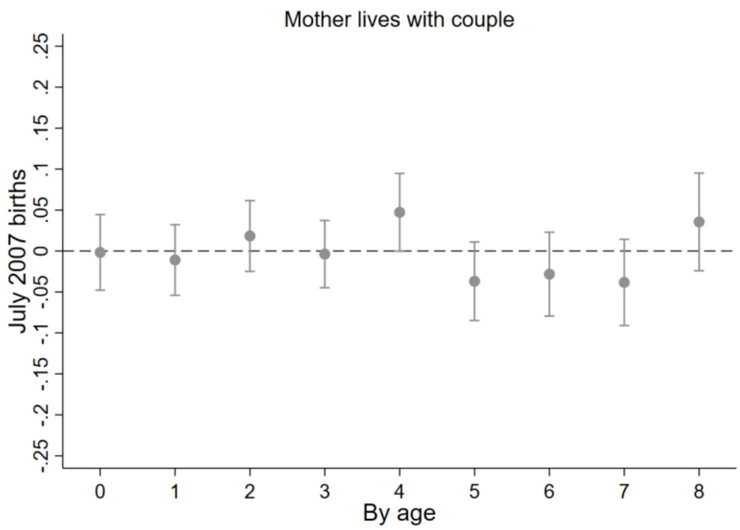
Panel B. Subsequent fertility



Panel C. Divorced mother

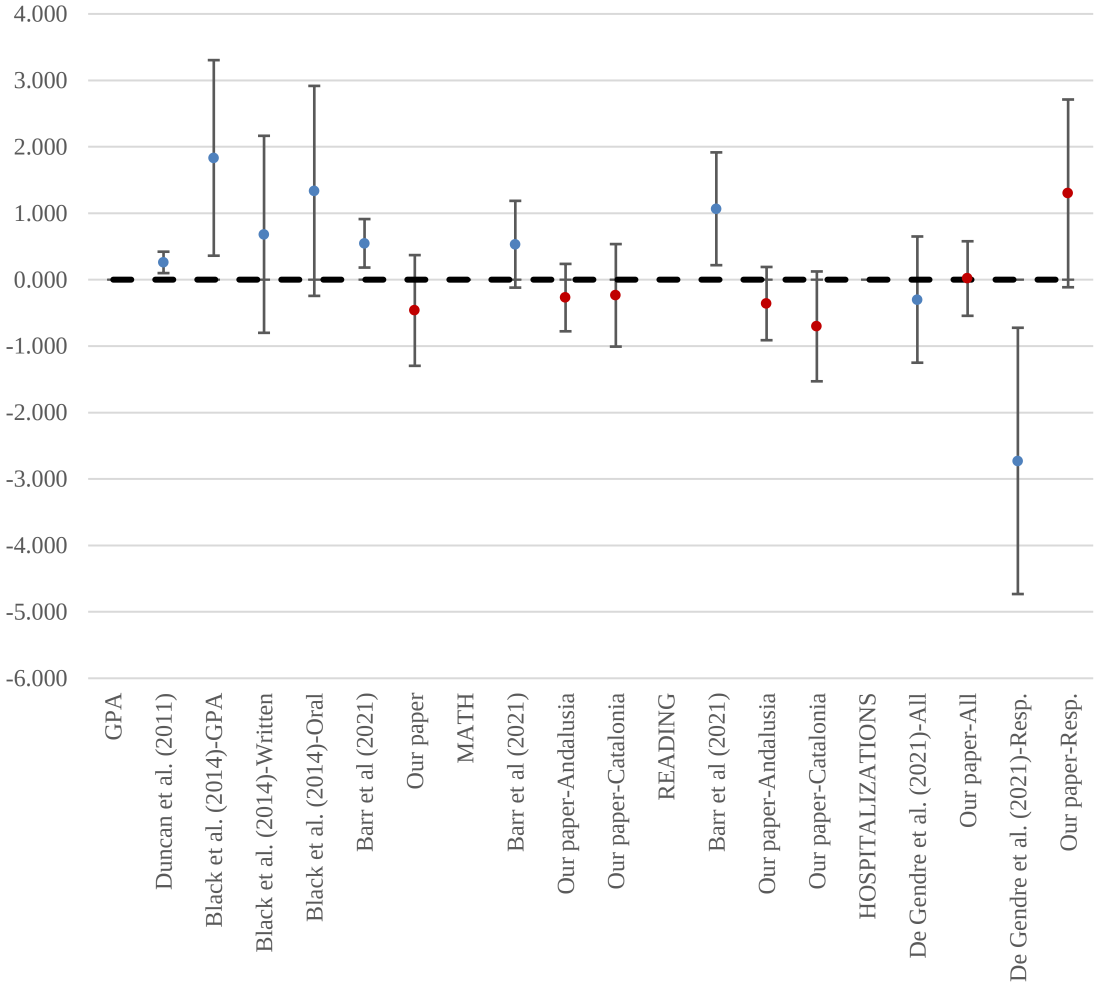


Panel D. Partnered mother



Note: These figures plot the coefficients and 95% CI for the interaction of being born in July with being born in 2007 (equation (2)). 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. Subsequent fertility is an indicator variable that takes value 1 if the mother had another child in the 8 years following the birth. 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.

Figure VI:
Comparison to Previous Effect Sizes in the Literature



Note: This figure compares our re-scaled estimates of the causal impact of a $1,000 yearly increase in income to the similarly re-scaled estimates in Duncan, Morris, and Rodrigues (2011); S. E. Black et al. (2014); de Gendre et al. (2021); and Barr, Eggleston, and Smith (2019). All coefficients are in standard deviation units and show the corresponding 95% CI.

APPENDIX A. TABLES AND FIGURES

Table A.1. 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
Std. Coefficient	-0.033	0.002	-0.050	-0.025	-0.068	-0.025
Controls	No	No	No	No	No	No
Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table shows the difference-in-discontinuity 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 A.2. 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 A.3. Effects on Anthropometric Measures (age 4)

	(1) Height (z-score)	(2) Weight (z-score)	(3) BMI (z-score)	(4) Overweight (0/1)	(5) Obesity (0/1)
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 2). 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 A.4. 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 A.5. 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 (3) 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 A.6. 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 A.7. Effects on parental time investments

	(1) Pre-school	(2) School	(3) Extra-school	(5) Nanny	(6) Relatives	(7) Parents
Panel A: 1 month						
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)
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
Observations	967	967	967	967	966	966
Std. Coefficient	0.081	-0.116	-0.278	0.175	0.137	-0.031
Panel B: 2 months						
Effect	-1.562	0.787	-0.341**	0.0961	-0.240	1.409
	(1.329)	(1.274)	(0.145)	(0.290)	(0.630)	(1.208)
Mean/SD	12.458/15.144	10.381/14.404	0.255/1.622	0.405/3.175	1.755/7.116	142.648/13.625
Observations	2037	2037	2037	2036	2033	2033
Std. Coefficient	-0.103	0.055	-0.210	0.030	-0.034	0.103
Panel C: 3 months						
Effect	-0.0480	-0.439	-0.197*	-0.130	0.309	0.567
	(1.069)	(1.036)	(0.116)	(0.258)	(0.521)	(0.974)
Mean/SD	12.144/14.879	10.545/14.469	0.266/1.620	0.443/3.485	1.858/7.265	142.601/13.528
Observations	3106	3106	3106	3105	3102	3102
Std. Coefficient	-0.003	-0.030	-0.122	-0.037	0.043	0.042
Controls	No	No	No	No	No	No

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 2). 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 (panel A), born between May and August (panel B), or between April and September (panel C) of 2006 and 2007 and whose mother is aged between 16 and 50. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8. Effect on Primary Health Care Outcomes Ages 5-8. BDCAP (RDD specification)

	(1) Health Problems (number)	(2) Referrals	(3) Respiratory	(4) Infections	(5) Injuries	(6) Psychological
Effect	0.560**	0.011	0.177	0.199	0.080**	0.008
	(0.220)	(0.035)	(0.108)	(0.123)	(0.036)	(0.015)
Mean/SD	4.140/5.423	0.162/0.621	1.288/2.240	1.610/2.531	0.247/0.649	0.058/0.276
Observations	8274	8274	8274	8274	8274	8274
Std. Coefficient	0.103	0.018	0.079	0.079	0.123	0.029

Note: This table shows RDD estimates in 2007 for the primary health care outcome health problems, referrals, visits, prescriptions, 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, 2007. The observations are weighted with a triangular kernel centered at the cutoff. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9. Effect on Hospitalizations (RDD specification)

	(1) All Stays (hospitilization rate)	(2) Respiratory disease	(3) Infections	(4) Injuries	(5) Psychological
Effect	0.066***	0.011	0.008*	0.004	0.001
	(0.024)	(0.010)	(0.005)	(0.004)	(0.001)
Mean/SD	0.692/0.062	0.137/0.026	0.097/0.013	0.034/0.007	0.001/0.001
Observations	59	59	59	59	59

Note: This table shows the RDD estimates 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, 2007. The observations are weighted with a triangular kernel centered at the cutoff. Standard errors are clustered by date of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

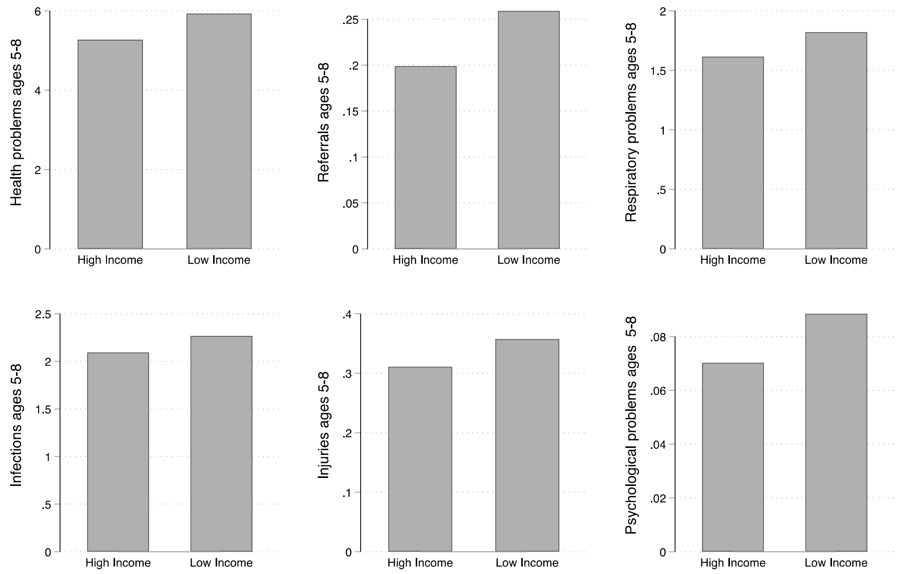
Table A.10. Effects on School Outcomes (RDD specifications)

	(1) Math (standardized)	(2) Spanish (standardized)	(3) Repeater (0/1)	(4) English (standardized)	(5) Catalan (standardized)	(6) GPA (standardized)
Panel A. Andalusia						
Effect	-0.061*	-0.036	0.001			
	(0.03)	(0.042)	(0.0065)			
Observations	14219	14234	14852			
Proportion			4.69 %			
Panel B. Catalonia						
Effect	0.025	-0.006		0.052	0.005	0.017
	(0.057)	(0.061)		(0.077)	(0.063)	(0.064)
Observations	5769	5772		5747	5763	5778

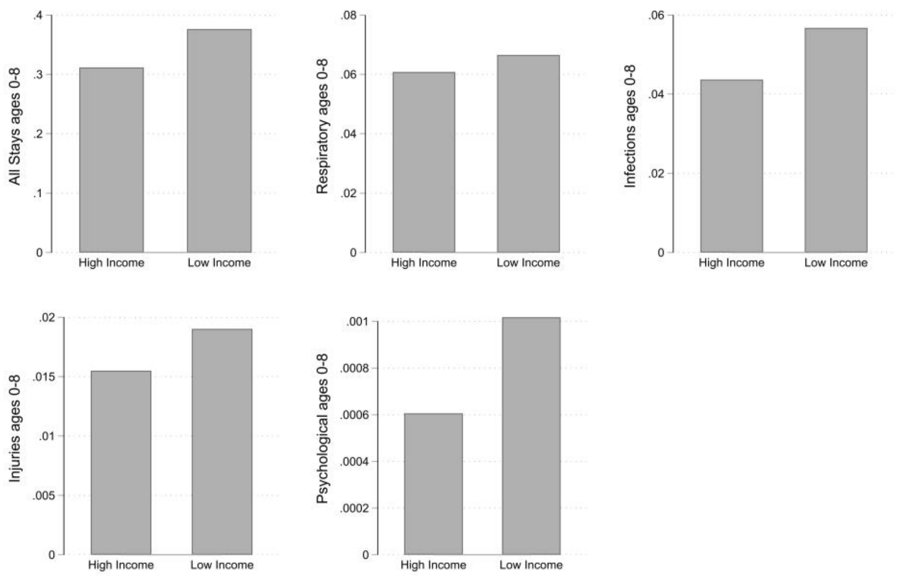
Note: This table shows the RDD estimates for education outcomes: for Andalusia, the outcomes are 2nd grade standardized test scores and an indicator variable that takes value of 1 if the student is a repeater; for Catalonia, the outcomes are the school subject grades and the overall GPA in 2nd grade. 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, 2007. The observations are weighted with a triangular kernel centered at the cutoff. Standard errors are clustered by date of birth. * p < 0.10, ** p < 0.05, *** p < 0.01

Figure A.1. Cross-sectional socio-economic status health gradients

Panel A. Primary care data (ages 5 to 8)



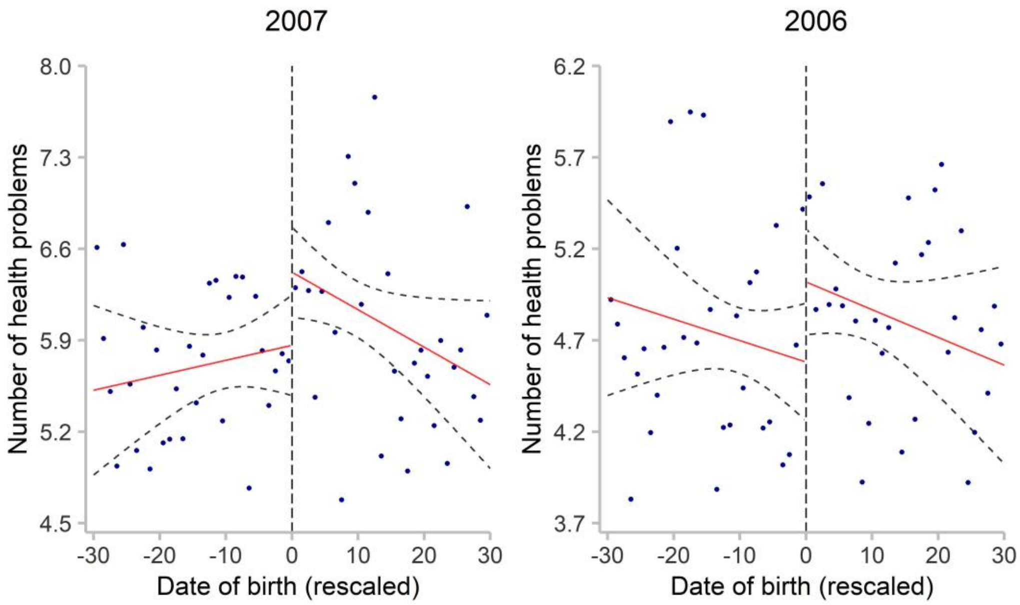
Panel B. Hospitalization rates (ages 0 to 8)



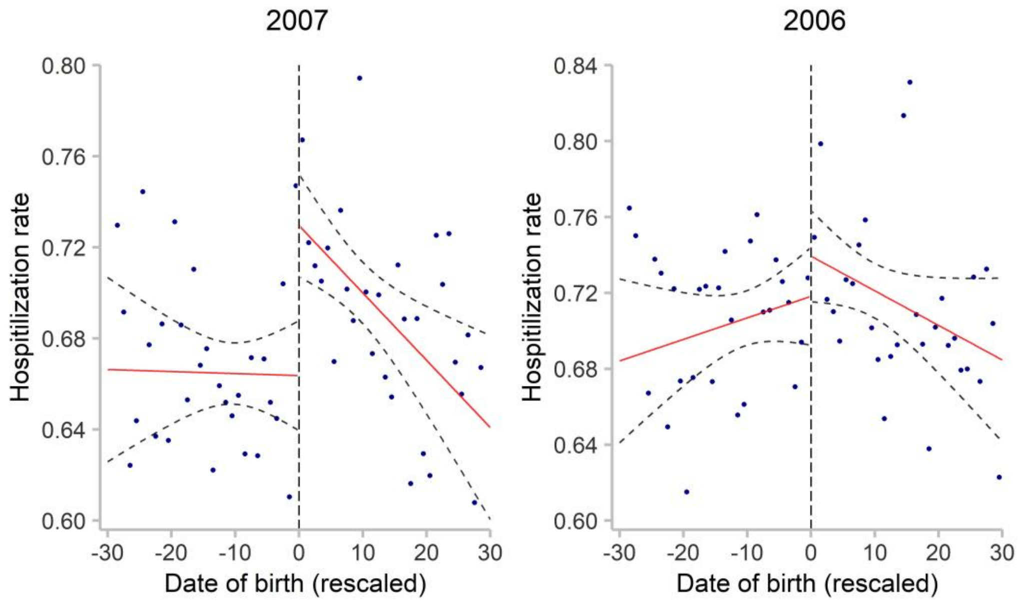
Note: These figures plot the socioeconomic status health gradient for primary care problems (panel A) and hospitalization rate (panel B). For the primary health care data, we plot health problems, referrals, respiratory problems, infections, injuries, and psychological problems from ages 5 to 8. Low-income status is defined by the family having a yearly income below €18,000. In panel (B), we plot total hospitalization rates, hospitalization rates due to respiratory diseases, infections, injuries, and mental disorders, from ages 0 to 8, by SES. Low-income is defined by the family residing in a region with a yearly income below the mean at the province level.

Figure A2. RDD comparing 2007 and 2006 (30 day bandwidth)

Panel A. Health Problems (ages 5 to 8)

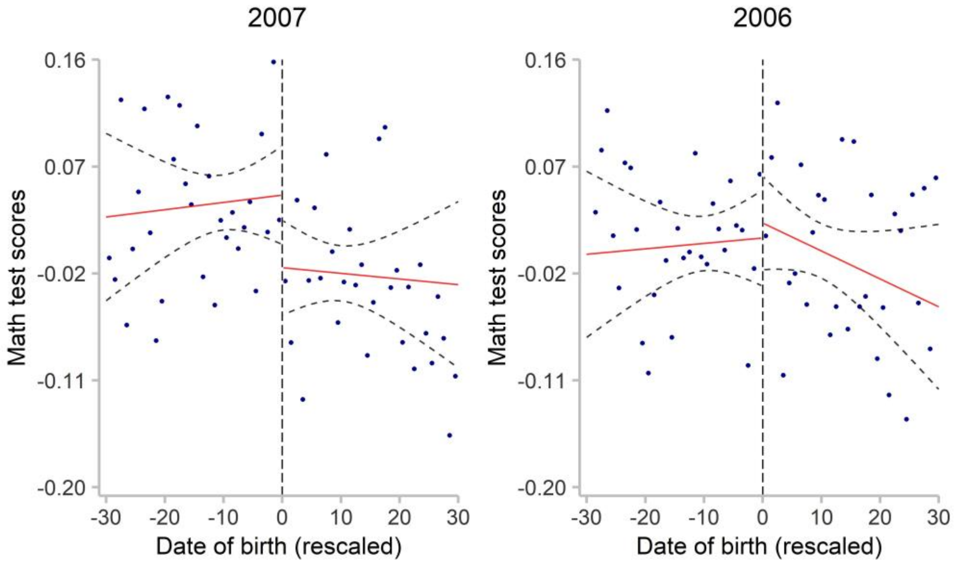


Panel B. Hospitalizations (ages 0 to 8)

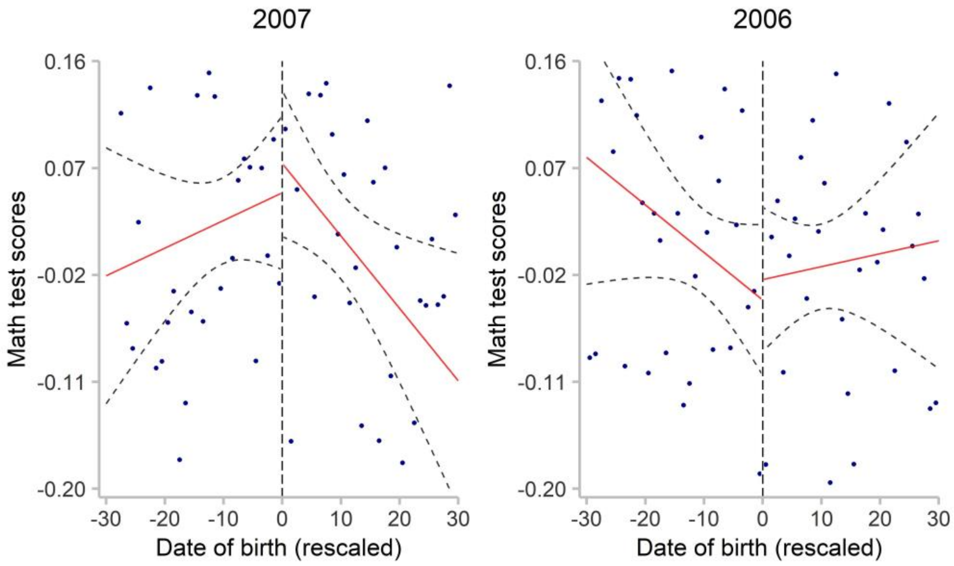


Panel C. Math Test Scores

Panel C1. Andalusia



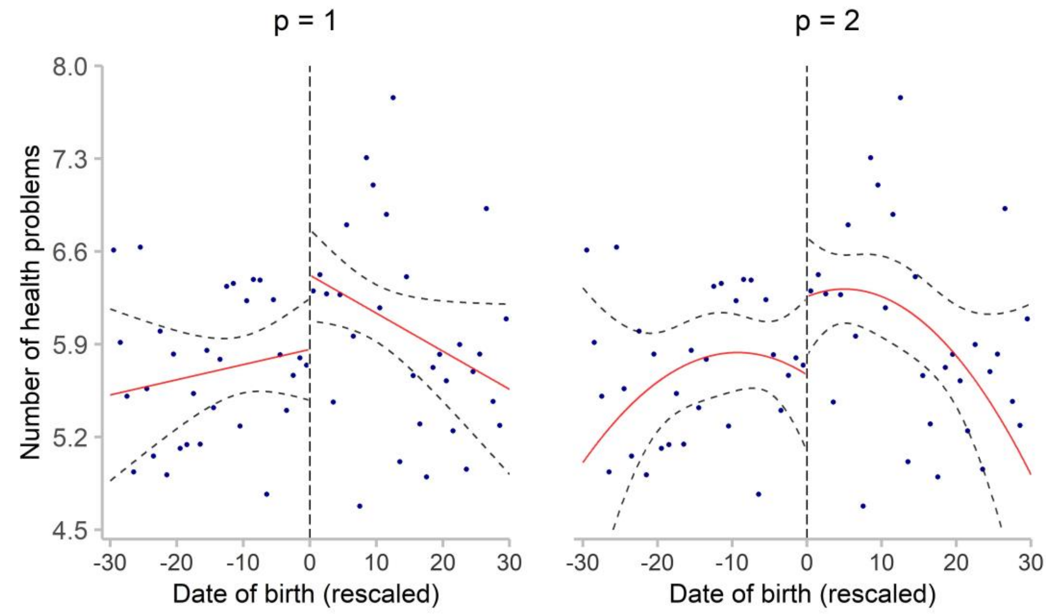
Panel C2. Catalonia



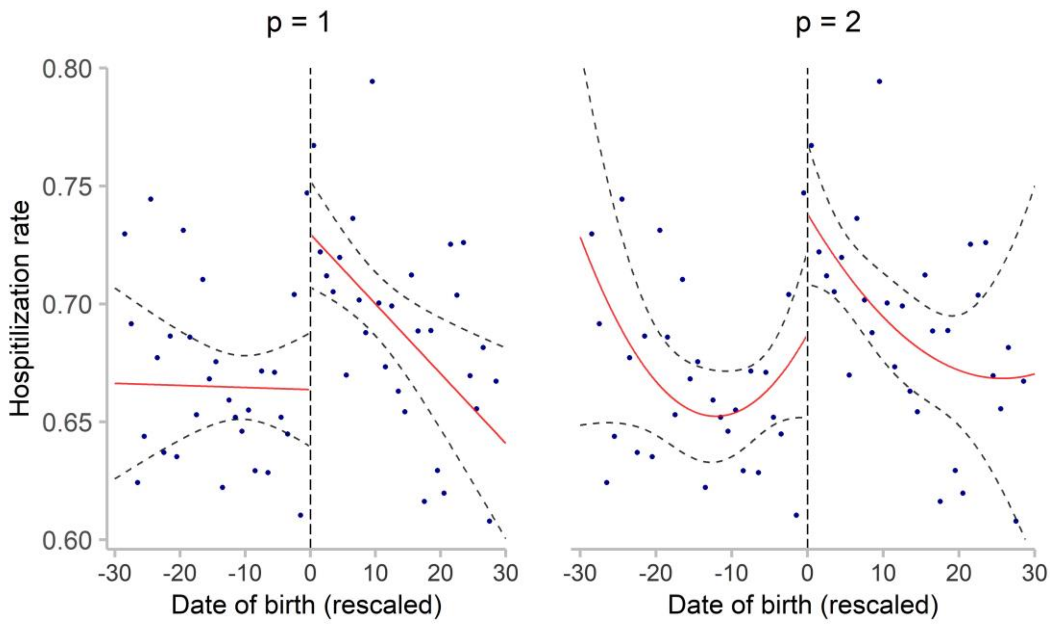
Note: These figures show the number of health problems (panel A) from ages 5 to 8, the hospitalization rate from ages 0 to 8 (panel B) aggregated by date of birth, and the math test scores for Andalusia (Panel C1) and Catalonia (Panel C2). The data source is primary care administrative data from BDCAP, 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.3. RDD for 2007 with Different Polynomial Specifications (30 day bandwith)

Panel A. Health Problems (ages 5 to 8)

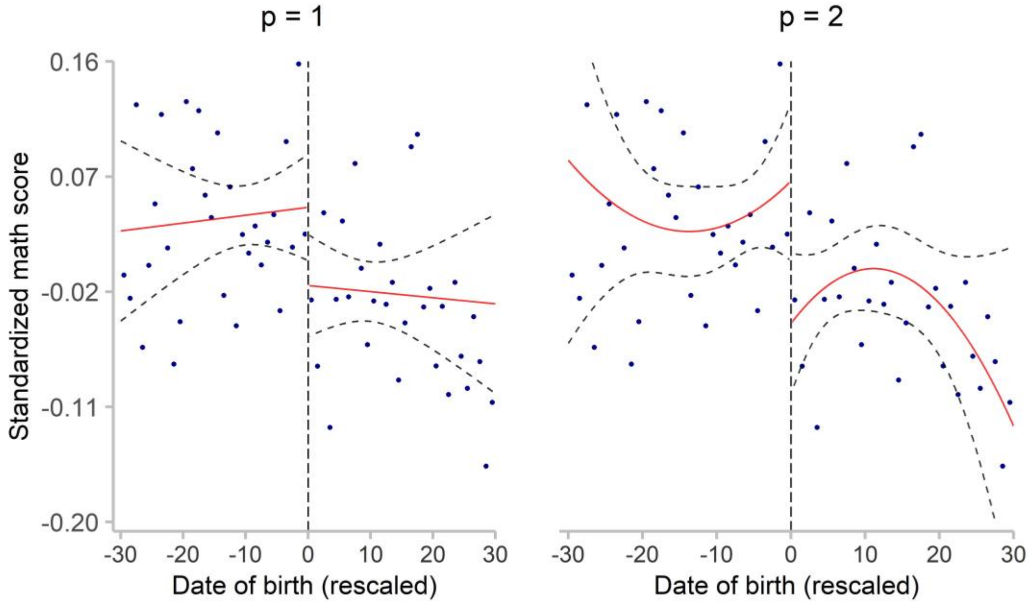


Panel B. Hospitalizations (ages 0 to 8)

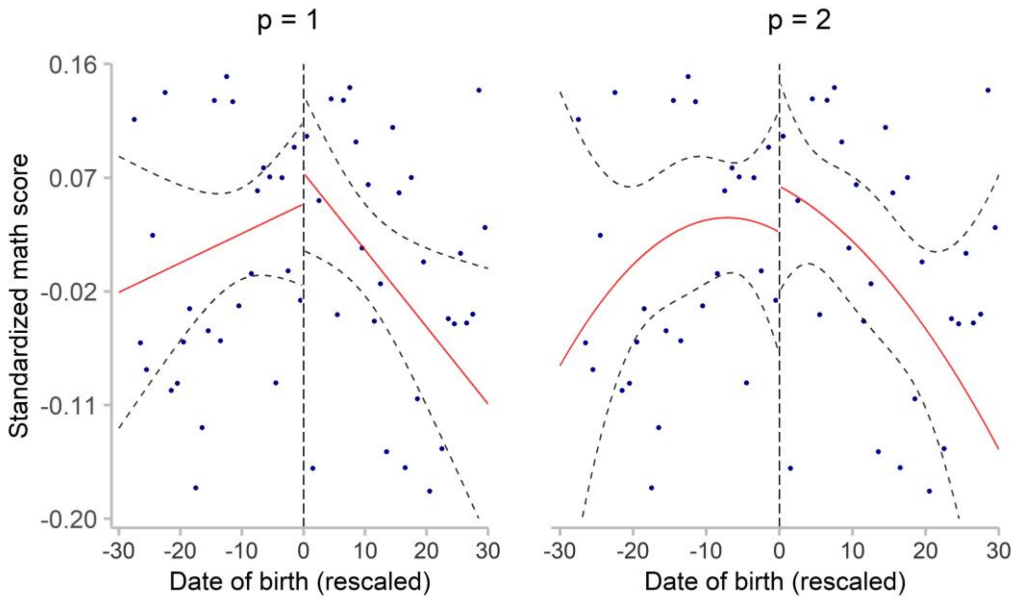


Panel C. Math Test Scores

Panel C1. Andalusia

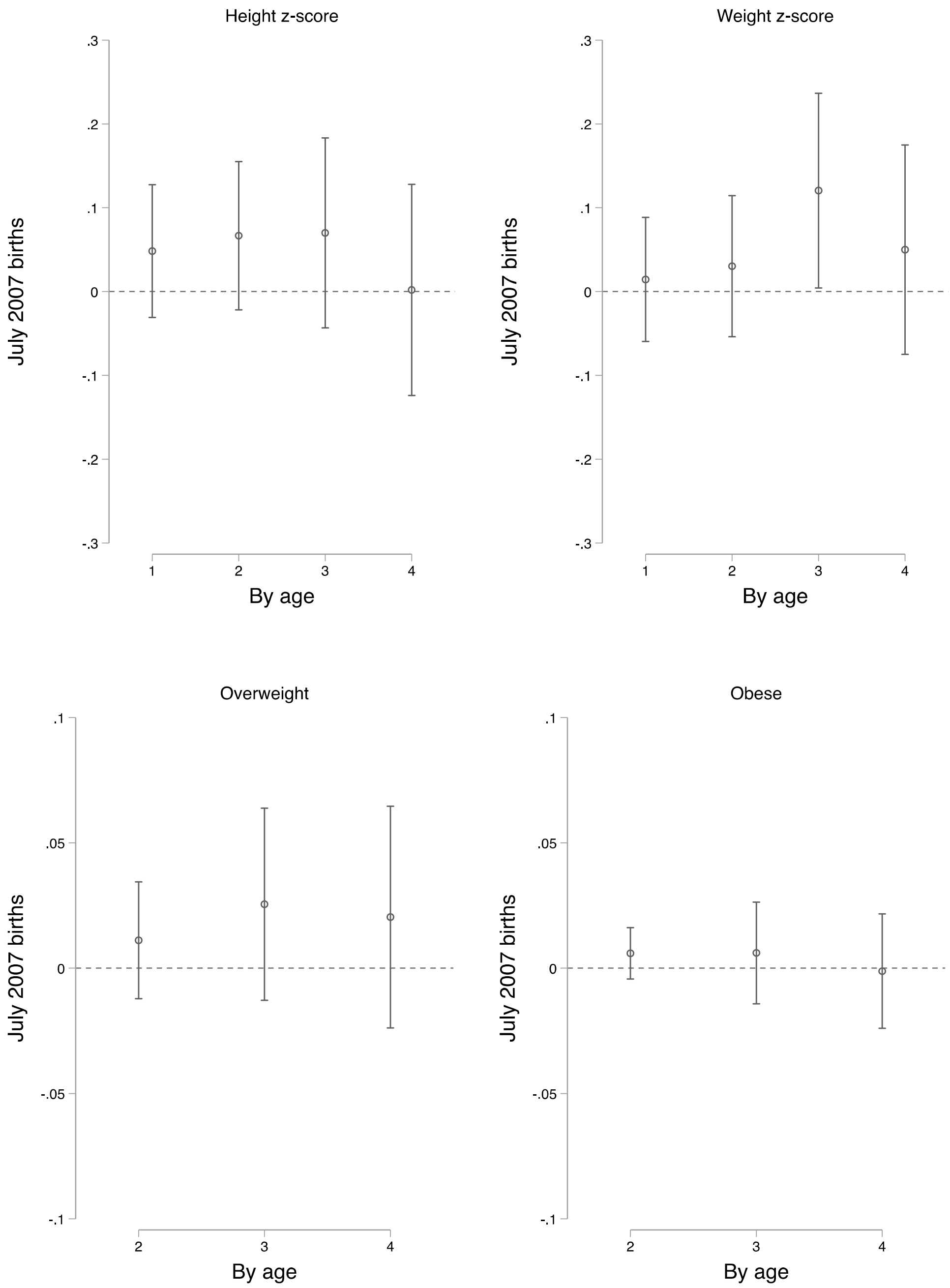


Panel C2. Catalonia



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), and the math test scores for Andalusia (Panel C1) and Catalonia (Panel C2) aggregated by date of birth. The data source is primary care administrative data from BDCAP, Hospital Morbidity Survey 2006-2015 and educational administrative data provided by the regional governments. The subplots contain polynomial fits of degree p (with 95% Confidence Intervals) for both sides of the threshold (July 1). All figures refer to 2007

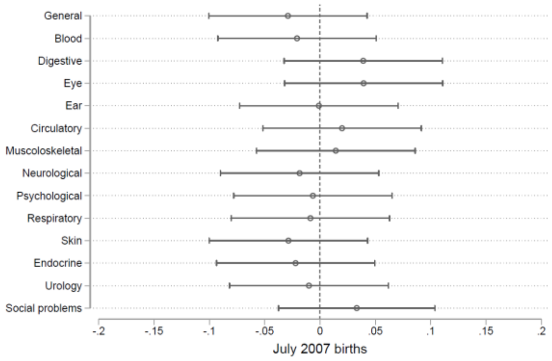
Figure A.4. Anthropometric Measures by Age



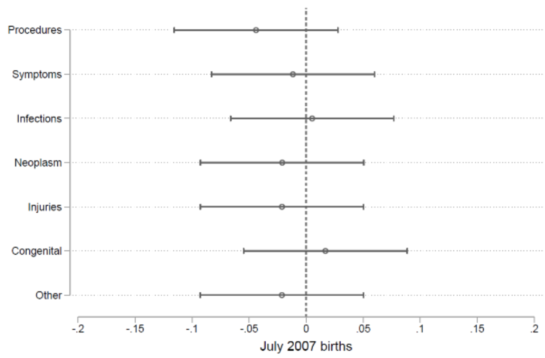
Note: These figures plot the coefficients and 95% CI of the impact of the treatment (benefit eligibility) on different anthropometric measures by age (equation 2). Height-for-age and weight-for-age z-scores are calculated using the World Health Organization's (WHO) universally applicable growth standards 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 A.5: Primary health care effects by diagnosis, type of visit and drug

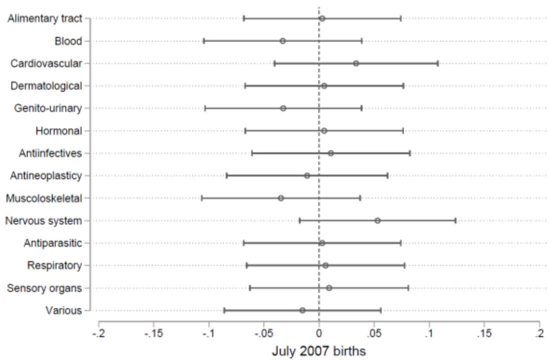
Panel A: By diagnosis



Panel B: By type of visit



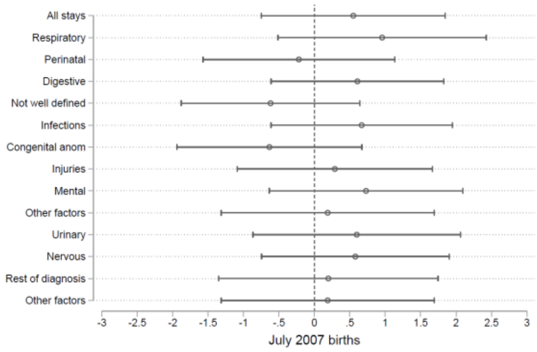
Panel C: By type of drug



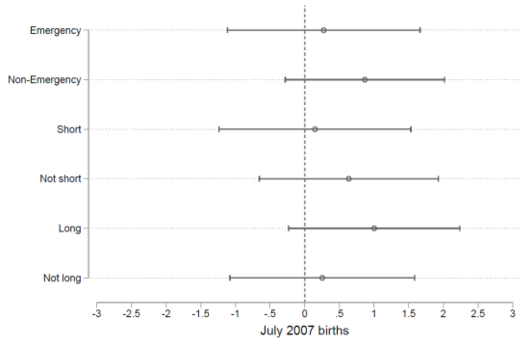
Notes: These figures plot the coefficients and 95% CI of the impact of the treatment (benefit eligibility) on health problems by diagnosis group (panel a), type of visit (panel b), and type of drug (panel c) (ages 0 to 4) (equation 2). The data source is primary care administrative data from the BIFAP project. An observation is a child. The sample includes observations for children born in June and July of 2006 and 2007.

Figure A.6: 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 Difference-in-discontinuity 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 (3)). 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.

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.

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 20-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 $346, that is, 3.2 percent of average annual 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 20-year period at a 2 percent interest rate. The computed equivalent annual income is $247. 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 1.4 percent increase when annuitized.

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 20-year period at a 2 percent interest rate. The computed equivalent annual income is $430.

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 $907 in 2000 prices over 20 years. A 20 percent increase in wages over 3 years translates to about a 3.6 percent annual increase in income.

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 20-year period at a real return of 2 percent to obtain an annual income increase of $5,355. 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 44 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).

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).

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.

Barr, Eggleston, and Smith (2021)

They use the discontinuity generated by being born before the end of the year in the eligibility rules for EITC in the US. Children born before the end of the year qualify for the whole of the tax credit while children born after do not. They report an average increase in income of about $1,300 for the average family. In page 7, they note that dollar figures are expressed in 2015 prices. The one-off increase in income amounts to $945 in 2000 prices. We annuitize that amount over a 20-year period at a real return of 2 percent to obtain an annual income increase of $60. In page 4, the authors explain that the direct increase in the net present value of lifetime family income is just 0.2%.

De Gendre, Lynch, Meunier, Pikington and Schurer (2021)

They use the introduction of a universal child benefit in Australia as exogenous increase in income. The baby bonus gave AU$3,000 to all new mothers in 2004. In page 9 they explain that the bonus replaced two income-related family benefits. For low-income non-working mothers the net gain would be AU$2,157 ($3,000-$843), while for high-income families the net sum could even be negative. This figure is about US$1,577.91 in 2004 prices according to PPP and US$1,438.41 in 2000 prices. We annuitize that amount over a 20-year period at a real return of 2 percent to obtain an annual income increase of $90. The authors say that median income is AU$61,663 (AU$29,661 for the bottom decile) in note 6. Thus the annual income increase of $90 in 2000 prices represents about 0.2% (90/41,120*100) of annual median disposable income and 0.5% (90/19,780*100) of annual disposable income for the bottom decile.

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 20-year period at a real return of 2 percent to obtain an annual income increase of $175. 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 1.0 percent increase in annual income for families in the bottom quartile.