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ABSTRACT

Baby Bonuses and Early-Life Health Outcomes: Using Regression Discontinuity to Evaluate the Causal Impact of an Unconditional Cash Transfer*

We use administrative data from South Australia to study the impact of an unconditional cash transfer on child health. We use the unanticipated introduction of the Australian Baby Bonus (ABB), a one-off payment of AU$3,000 (US$2,400) made to families with a newborn, to isolate its causal effect. The ABB reduces the number of potentially preventable hospitalizations and emergency department presentations for respiratory problems in the first year of life. Findings from survey data suggest that households spent the windfall income on electricity and private health insurance. There is no robust evidence that the ABB increased accidents or non-essential good consumption.

JEL Classification: I14, I38

Keywords: unconditional cash transfers, baby bonus, child respiratory health, health care utilization, regression discontinuity design, natural experiment, linked administrative data

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1. Introduction

The governments of the richest countries in the world pay a considerable amount in benefits to support families. In 2013, for which the latest data are available, the average OECD country spent 2.1 percent of its GDP on family benefits (OECD 2018). Public spending on family benefits is most often administered either in the form of regular instalments through maternity leave pay arrangements or income support for sole parents, or through subsidized services that benefit children. Yet, some OECD countries – for instance Australia, Canada (Québec), Singapore, and Spain – have opted to pay families a one-off Baby Bonus to alleviate the perceived financial pressures of raising a child and to improve equity (Parr and Guest 2011; McDonald 2006a; 2006b).

Because Baby Bonuses are usually not means tested and not tied to a specific expenditure category, they have the advantage that they are cheap to administer and households are free to spend them according to their preferences. Whether Baby Bonuses also benefit children is less clear. If parents spend the bonus on "child-centred goods like books, quality day care or preschool programs, better dependent healthcare, or to move to a better neighbourhood" (Dahl and Lochner 2012, p. 1931), then Baby Bonuses are likely to improve children's outcomes. However, Baby Bonuses – just as any other unconditional cash transfers (UCTs) paid to families – may have unintended consequences. Parents may use them to increase the consumption of non-essential or even risky goods that may result in negative externalities for children (Currie and Gahvari 2008). To date, we do not know whether parents spend Baby Bonuses on their children's needs and whether they are effective in improving children's developmental outcomes.[1]

This study will contribute to a small but emerging literature on the effectiveness of UCTs – in particular Baby Bonuses – in shaping children's health and human capital. We use a natural experiment resulting from the unanticipated introduction of the Australian Baby Bonus (ABB), an UCT of initially AU$3,000 (US$2,400) paid to every family that had a newborn on, or after, 1 July 2004. The unanticipated nature of the ABB and a random birth-date cut off allow us to exploit a regression discontinuity approach to compare the short- and long-term health outcomes of children born just before and after the date of the birth eligibility cut-off. The analysis is made possible through access to high-quality, linked administrative data from the South Australian Early

[1] There is however an extensive literature that shows how early-life circumstances – e.g. low birth weight, poverty, early-life health problems – affect children's long-term health, educational, and labor-market outcomes (Almond, Currie and Duque 2017).

Childhood Data Project (ECDP), which is one of the most comprehensive population-based administrative research databases worldwide (see Nuske et al. 2016 for an overview).

The ABB experiment is of substantial scientific value. It provided very low-income families with almost ten weeks of net pay after the birth of a child, a large cash injection at a time when Australia was the only OECD country other than the United States without compulsory parental pay legislation. Importantly, it did not change permanent income, in contrast to many other UCTs that have been evaluated such as the Earned Income Tax Credit (EITC) in the United States (see Currie and Almond 2011, for an overview). Resembling a lottery win, the ABB is not expected to alter long-term consumption patterns. It is designed to buffer short-term financial and emotional stressors triggered by the birth of a baby. Although for a small number of children their births were strategically shifted so that families could qualify for the AU$3,000 payment (see Gaitz and Schurer 2017; Deutscher and Breunig 2017; Gans and Leigh 2009)[2], its introduction was as good as random. Babies born in the first half of the year provide an ideal control group against which babies born in the second half of the year can be compared. To be as conservative as possible, we use as our benchmark sample babies born in May and June (excluding births in the last week of June to avoid births that could not be shifted strategically), and births in July and August (excluding births in the first week of July to avoid those that were shifted strategically). Children in both the control and treatment group were born during the colder months of the year.

Our findings suggest that the ABB was useful in improving children's respiratory health in the short-run. The ABB had an impact on child health in the first year of life, the year in which the lump sum was paid to the families, but it had no impact at later stages. It reduced emergency department presentations for respiratory illnesses by 29-45 percent. This treatment effect means that in the control group one in seven babies presented for respiratory illness in their first year of life, whereas in the treatment group it was one in ten babies. Similar findings were obtained for potentially preventable hospitalizations. The causal impact of the ABB is stronger for children from disadvantaged families. Robustness checks demonstrate that these findings are not sensitive to alternative treatment and sample definitions or to functional form assumptions. The economic gains due to reduced hospital care utilization are sizable. We find no robust evidence that the ABB led to more accidents.

[2] Birth shifting was also observed in Spain, where suspending the Baby Bonus lead to strategic premature births and thus worse health outcomes of newborns (Borra, González, and Sevilla 2016).

To better understand the potential mechanisms that produced the decline in respiratory problems, we analyse auxiliary data from the Household, Income, and Labor Dynamics in Australia (HILDA) survey and review a study analyzing the Australian Longitudinal Study of Children (LSAC). Both data sources provide in limited form information on household expenditures, and parental behaviour and wellbeing, respectively. We show that households with newborns, which benefitted from the ABB relative to comparable households which did not, spent more on utilities and private health insurance, and tended to spend less on health care and non-essential goods. Although the estimates based on the survey data are estimated imprecisely, they support the hypothesis that the ABB led to better heating and home environments and access to better and appropriate health care. Evidence presented in Gaitz and Schurer (2017) based on the LSAC data shows that the ABB did not change parental behavior and wellbeing, and maternal labor supply. The Baby Bonus did however improve a family's ability to raise AU$2,000 at short notice. Taken together, we propose that the ABB helped families to weather financial stress and create home environments that protected newborn respiratory health.

Our findings contribute to an emerging literature on the usefulness of government social assistance schemes to improve child health, human capital and well-being including Baby Bonuses (Gaitz and Schurer 2017; González 2017; Deutscher and Breunig 2017; González 2013), earned-income tax credits (Hoynes, Schanzenbach, and Almond 2016; Hoynes, Miller, and Simon 2015; Dahl and Lochner 2012; Duncan et al. 2011; Currie and Almond 2011; Milligan and Stabile 2011); and food stamp programs (Almond, Hoynes, and Schanzenbach 2011). Baby Bonuses have not been particularly successful in boosting children's human capital in the long run (Gaitz and Schurer 2017; Deutscher and Breunig 2017). Some evidence exists of a negative health impact of Baby Bonuses (Gaitz and Schurer 2017; González 2017). Therefore, our unambiguous findings on health improvements for children in their first year of life is new evidence that Baby Bonus payments may not be wasted investment.

Our findings are relevant in the context of a broader literature that investigates the causal impact of household material resources and children's health outcomes (Cesarini et al. 2016; Kuehnle 2014; Currie and Almond 2011; Currie 2009; Case, Lee, and Paxson 2008; Currie, Shields, and Price 2007; Propper, Rigg, and Burgess 2007; Currie and Stabile 2003; Case, Lubotsky, and Paxson 2002; Yeung, Linver, and Brooks-Gunn 2002). Identifying the causal impact of household income on children's health has been difficult because few compelling randomizations exist that allow for policy evaluations, at best coming from lottery winnings (e.g. Cesarini et al. 2016) or strong modelling assumptions (e.g. Kuehnle 2014). Yet, our positive findings in relationship to children's respiratory health are in line for instance with Kuehnle (2014). Exploiting regional variation in income to identify the causal impact of household income on children's health in the UK, Kuehnle (2014) finds that doubling household income reduces the probability of respiratory illness by 46 percent relative to the base probability.

Evidence that is more recent shows that wheezing episodes early in life with the common cold virus is a major risk factor for the later diagnosis of asthma at age six. Children with asthma are at high risk of developing complications later in life and are therefore in need for acute care (see Busse et al. 2010 for an overview). Thus, positive income shocks early in life may reduce the economic burden to society through medical expenditure savings in the longer run.

The remainder of the paper is structured as follows. Section I reviews the existing literature and discusses the potential mechanisms that produce the causal relationship between income and child health. Section II outlines the ABB policy environment. Section III describes the empirical strategy, and Section IV presents the data and descriptive statistics. Section V presents our results, robustness checks, and a heterogeneity analysis. In Section VI, we explore the mechanisms through which the ABB may have affected child health using auxiliary data. Section VII concludes. Supplementary material is presented in an appendix.

2. Cash Transfers and Children's Outcomes

Cash transfers are one of the most widely used policy levers to reduce socioeconomic inequalities in health and human capital. They provide a unique policy experiment to evaluate the impact of household material resources on children's outcomes. Although cash transfers are popular with governments because they are easy to implement, they are expensive and they may be harmful to children if parents spend the windfall income on risky goods (Currie and Gahvari 2008). Hence, their potential benefits and costs need to be carefully evaluated. In what follows, we will review the small evidence base on the effectiveness of cash transfers in high-income countries, with a focus on Baby Bonuses, on children's developmental outcomes.

The literature on the impact of government cash handouts on children's health outcomes shows contradicting evidence (see Currie and Almond 2011, for a review). Recent studies based on data from the United States (US) find a positive impact of cash transfers on health outcomes in the short and long run. Hoynes, Miller, and Simon (2015) study the impact of the EITC on birth weight, exploiting a sharp rise in the program pay-outs in the mid 1990s. They find that the EITC roll out reduced the prevalence of low birth weight, and increased mean birth weight. Exploring the mechanisms, they show that EITC affected infant health through an increase in prenatal care and a reduction of negative health behaviours (smoking). Dahl and Lochner (2012) furthermore show that the EITC did not only improve children's health but also their math and reading test scores, especially for children from the most disadvantaged backgrounds.

Furthermore, evidence from the largest US publicly funded cash benefit, the Food Stamp Program, also known as the Supplemental Nutrition Assistance Program (SNAP), shows positive impacts on children's lives. It provides families on low income with vouchers to spend in grocery stores, increasing household resources available to spend on food, acting as a near-cash transfer. Almond, Hoynes and Schanzenbach (2011) identify an increase in birth weight for births to mothers benefiting from Food Stamps during pregnancy, with larger effects at the lower end of the birth weight distribution, for black women and in the poorest counties. Hoynes, Schanzenbach and Almond (2016) find that access to Food Stamps during pregnancy and early childhood reduced the incidence of metabolic conditions (i.e. obesity, high blood pressure, diabetes, or heart attack) in adulthood.

Although cash transfers appear to positively affect children's health in the United States, evidence from other countries is less positive. Gonzalez (2017) demonstrates that an unconditional cash benefit in Spain of similar scope, structure and magnitude to the Australian Baby Bonus (ABB), had a negative effect on children's health outcomes as measured by healthcare utilization. Children whose parents benefited from the Spanish Baby Bonus (SBB) experienced 10 percent more hospitalizations by age five than comparable children whose parents did not receive it. However, she also identified in a previous study that the benefit increased maternal time at home after birth (González 2013). Hence, the SBB may have increased use of hospital services, because mothers have more time with the child and thus more time to seek and opportunity to detect health problems.

Gaitz and Schurer (2017) also find negative treatment effects of the ABB on siblings' physical health using a small sample of children from a nationally representative cohort study.[3] The authors explain that this negative impact on siblings' physical health may be the result of a change in parental perception of the child's health, since the finding is observed for a subjective, parent-assessed health outcome measure, but not for more objective health data. The ABB also had little impact on children's test scores (Deutscher and Breunig 2017) and siblings' cognitive and non-cognitive skills at age eight (Gaitz and Schurer 2017). Both studies argue the lack of impact of the ABB later in the life of children (age 8) in aggregate might be due to its non-targeted and non-permanent nature.

The mechanisms through which cash transfers may impact upon child health are straightforward. Yeung, Linver and Brooks-Gunn (2002) distinguish between the direct impact of income, which they refer to as the "resources channel", and the indirect impact of income, which they refer to as the "family process channel". The direct channel refers to parents using additional household resources to purchase goods from which children benefit including quality daycare or access to better health care (see Dahl and Lochner 2012). Exploiting lottery wins and administrative data from Sweden, Cesarini et al. (2016) show that a substantial lottery win of 1M Swedish Kroner (approx. US$110,000) leads to a 19% increase in overall and respiratory-problem hospitalization rates of children within five years after the lottery win. Although not discussed in that way by the authors, this could be evidence that available financial resources were used to finance previously unmet health care demand for children. This interpretation is consistent with the findings on the SBB that led to a 10% increase in hospitalization rates for children two to five years after the receipt of the bonus (González 2017).

Yet, many previous studies have argued that the mechanism through which income affects child health cannot be lack of access to health care. This conclusion is drawn from the observation that the income-gradient of health is observed both in countries without universal health coverage such as the United States (Case, Lee and Paxson 2008; Case, Lubotsky and Paxson 2002) and in countries with universal coverage such as Canada (Currie and Stabile 2003) and the UK (Kuehnle 2014). Surprisingly little evidence exists on the question whether parents use cash transfers directly to purchase child-centered goods, including access to health care.

[3] Gaitz and Schurer (2017) evaluate the impact of the ABB on the older siblings of the children who were born just before and after the introduction of the ABB.

Income shocks may also impact on child health indirectly because they affect parental emotional well-being and allow parents to spend more time with their children in productive activities. McLoyd (1990) suggests that income poverty is associated with poor parental health and high levels of maternal depression and stress. Hence, cash transfers may be effective in relieving these constraints. Currie, Shields and Price (2007), Propper, Rigg and Burgess (2007), and Khanam, Nghiem, and Connelly (2009) show that the income gradient in child health is mediated by maternal mental health both in the UK and Australia.[4] Mullins (2016) finds that welfare payments significantly improve parental welfare and the stability of spousal relationships. They also assist mothers in returning to work smoothly. Less stressed mothers are more likely to spend time with their children in productive activities.

We contribute to this previous literature by studying the impact of the ABB on hospital health care utilization, our proxy for child health. Australia has a universal health care system, which covers the use of hospitalization services. Poor families often use emergency departments visits to avoid co-payments associated with visits to general practitioners who regulate access to specialist and in-hospital care. Hence, we can test whether the cash benefit affected preventable paediatric hospitalizations, especially those which can directly be linked to parental behaviours (e.g. accidents) and exposure to low-quality housing or insufficient heating (e.g. respiratory problems) (see Howden-Chapman et al 2008 for arguments and evidence). We furthermore contribute to the literature by presenting evidence on the type of expenditures that households make with their cash benefit.

3. The institutional background of the Australian Baby Bonus

The Australian Baby Bonus (ABB) was an AU$3,000 unconditional and non-taxable lump sum offered to parents for each birth (or adoption of a child under two years) on or after 1 July 2004. The Australian Government announced it on 11 May 2004 in the new budget, hence just a short time period before its implementation. The primary intention of the ABB was to boost fertility by absorbing part of the (perceived) costs associated with the birth of a child. The ABB can therefore

[4] Furthermore, neither study finds that the income gradient in child health is changing with the age of the child. On the other hand, Case, Lee and Paxson (2008), find the relationship between income and child health strengthens with age, which would be evidence suggesting that one channel through which income affects child health outcomes is through access to beneficial goods.

be seen as a natural experiment for all births between July 2004 and December 2004. A short period of less than seven weeks between announcement and implementation left no room for a fertility response in the short run.[5]

The ABB was atypical and of much broader scope than previous policies. First, it was not means tested. Any family who had a newborn baby received the bonus independent of family size or parental employment status. Second, the cash benefit was a sizeable amount of money, especially for families living on low income. Conceptually, the lump sum was 2.5 times the weekly median disposable household income of households with a newborn in 2004, or 5.3 times the weekly disposable household income of families in the lowest income decile. Overall, the ABB represented a one-time increase in the median disposable household income for families who had a baby born in 2004 of almost 5 percent.[6]

Between its introduction and abolition on 1 March 2014, the programme underwent important structural changes, which included subsequent increases to AU$4,000 and AU$5,000 on 1 July 2006 and 1 July 2008, respectively. As of 2009, it became means-tested and thus from this point forward only accessible to families with incomes of AU$75,000 or less in the six months following the birth or adoption of a child. Additionally, from 2008, parents under 18 would receive the ABB in 13 fortnightly instalments instead of an up-front payment, and it was progressively rolled-out to the entire population.

Importantly, the ABB was introduced at a time when Australia was one of two OECD countries that had not yet legislated a compulsory parental leave payment scheme. This legislation was introduced as a further commitment to supporting families in 2011, in the form of the national Paid Parental Leave (PPL) program. The scheme offered up to 18 weeks' pay at the minimum wage, a much larger support than the ABB for eligible families.

The ABB replaced two family benefits, the Maternity Allowance and the First Child Tax Refund (referred to as the Baby Bonus at the time). Therefore, the ABB does not represent a net increase of AU$3,000 for all households (Deutscher and Breunig 2017). The Maternity Allowance was a

[5] The reason is that babies born on or after 1 July 2004 were in utero on the day of announcement. The first babies conceived after 11 May 2004, and thus as a consequence of the ABB, could not have been born before Feb 2005, assuming full-term gestation of 37 weeks plus.

[6] Own calculations based on Wave 4 of the Household, Income, and Labor Dynamics in Australia survey. The median disposable household income, for families who had a newborn between January and December 2004 was AU$61,663 (or AU$1,186 per week). The mean household disposable income for households in the bottom decile of the income distribution was AU$29,661 (or AU$570 per week). The sample comprises 142 out of 161 households which had a newborn in 2004.

subsidy of AU$843 per child as part of the Family Tax Benefits (FTB) available to mothers on modest income. The First Child Tax Refund was introduced for babies born on or after 1 July 2002. It allowed mothers leaving the workforce to claim back income taxes paid the year prior to the birth of the first child born between 1 July 2001 and 30 June 2004 (not necessarily the first-born child in the family). The amount was paid back over a five-year period (i.e. some mothers received money back until 2009). If mothers were returning to work prior to the fifth birthday of the child, the payable amount would be reduced proportionally to the income earned. This subsidy, which was much more generous to women on higher incomes, had low utilization rates probably because of its complex and delayed tax refund scheme (Drago et al. 2011; Gans and Leigh 2009). In stark contrast, the ABB was administratively simple and low-cost to obtain. To acquire the benefit, parents needed to lodge their claim within 26 weeks of the birth. Consequently, almost all eligible households claimed it (Drago et al. 2011). Providing the same level of help to all parents, the policy was more favourable to low- and middle-income households. According to Deutscher and Breunig (2017), 75 percent of births in June 2004 would have been better off under the new policy.

4. Empirical Strategy

We use a sharp regression discontinuity design to evaluate the causal impact of the ABB on health outcomes of children born just before and after July 1, 2004. Because of the randomness by which the ABB affected families, we can assume that the control and treatment groups are identical in all relevant observable and unobservable characteristics. We pay careful attention to demonstrating the validity of this assumption.

We estimate a local linear regression on either side of the threshold 1 July 2004, allowing for different levels and trends in health outcomes (see Lee and Lemieux 2010):

$$Y_{ia} = \alpha + \beta T_i + \gamma_1 D_i + \gamma_2 T_i \times D_i + \gamma_3 W_i + \varepsilon_{ia}. \qquad (1)$$

Let Y_{ia} be a health outcome measure of interest of individual i at age a. The indicator variable T_i is a binary variable taking the value 1 if birth happened on or after 1 July 2004 and the family received the ABB (treatment group), and 0 otherwise (control group). We control for the date of birth D_i (normalized to 0 at the threshold), and allow for a different trend before and after the implementation date ($T_i \times D_i$). We furthermore control for variations in health by birth day-of-the-week, including a vector of indicator variables W_i (Deutscher and Breunig 2017; Lee and Lemieux 2010; Angrist and Pischke 2008).[7]

Of main analysis interest is the estimate of β, which captures the potential discontinuity at the cut-off date. We test the null hypothesis that the ABB had no impact on child health outcomes, $H_0: \beta = 0$, against the alternative hypothesis that the ABB had an effect, $H_a: \beta \neq 0$.

To measure child health outcomes, we use the number of hospitalizations (emergency, inpatient). Healthcare utilization data are commonly used to construct proxies for health outcomes because they measure demand for care when required and are available in administrative data collections. Health care use data allow quantifying the economic (or monetary) burden of disease to governments and societies. However, hospitalizations are rare events. Thus, the outcome measure is highly right-skewed, because of a large number of zeroes in the data. Given the distribution of the data, we use a negative binomial model to estimate Eq. (1) (Cameron and Trivedi 2005).[8]

We consider for our eligible sample only births that occurred before January 2005 to ensure that mothers were already pregnant at the announcement date of the ABB. This sample-eligibility restriction ensures our estimates are not confounded by fertility behavioural changes and by the introduction of other benefits. Because of the seven-week lag period between announcement and implementation, some parents at the margin of being affected may have attempted to delay birth to July 2004, mostly by women who re-scheduled a planned caesarean section birth,[9] to ensure they would receive the bonus. Gans and Leigh (2009) estimated that about 1,000 births Australia-wide were moved, with about 75 percent of the shifting happening in the last week of June and

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[7] We add dummy variables for quarter of birth in a robustness check, in which we extend the estimation sample to children born over 182 days around the eligibility cut-off to control for seasonal variations in birth outcomes.

[8] In a robustness check we demonstrate that our results are not sensitive to the choice of functional form for the count data model (e.g. the use of a zero-inflated negative binomial model).

[9] There is little systematic evidence on how women are able to shift birth dates to a later date. In the context of the ABB, Dr Chris Tippett, the then president of the Royal Australia and New Zealand College of Obstetricians and Gynaecologists, stated in an interview with the Australian Broadcast Association: "We know that that 4 percent of babies deliver on the date that we best calculate and what I am saying is in fact the women who would be able to defer the deliveries – the women who would have had planned caesarean sections – often they're planned at, say 38 weeks, and one or two days…There'd be no harm in transferring those to 39 weeks and two days…I think I'm correct in saying that last time this occurred and people looked at the data more closely, it seemed likely that this effect was associated with people deferring things like caesarean sections" ABC 8 Nov 2007, Simon Santow "Mums 'delaying births' for maximum baby bonus".

first week of July.[10] Because these babies might be different from the rest of the cohort, this is a problem for our identification strategy. We will demonstrate that gaming around the threshold also happened in our South Australian data and those babies born seven days before and after July 1, 2004 are different from the rest of the 2004 birth cohort. These newborns will be excluded from the regression analysis (N=663).

5. Data

5.1. South Australian Early Childhood Data Project

We conduct the analysis using linked administrative data from the South Australian Early Childhood Data Project (SA ECDP), which is one of the most comprehensive population-based administrative research databases on children and families in Australia. It brings together more than 30 different government administrative data sources spanning every cohort of South Australian children born between 1999 and 2013 (see Nuske et al. 2016). Birth data is obtained from the Born Population dataset, a merge of the Births Register and The South Australian Perinatal Statistics Collection. Available variables include date of birth, gestation length at birth, child sex, birth weight and APGAR scores as well as mother, father and child demographics. Further information includes maternal gestational health and smoking behaviour during pregnancy. These data are primarily sources from the Perinatal Statistics Collection and supplemented and validated by Births Registry data. (Nuske et al. 2016).

Health outcome measures are derived from the Integrated South Australian Activity Collection (ISAAC) and the South Australian Emergency Department Data Collection (EDDC). Data are available for every birth cohort born from July 2001 to 2014 and July 2003 to 2014 respectively (Nuske et al. 2016). Routine data are available for analysis in the EDDC from 72 hospitals (including country hospitals), and thus cover all of our sample cohort children (Emergency Department Data Collection Reference Manual 2014). The data are collected by hospital staff and updated at the time of hospital separation. Specific information includes child demographics,

[10] In 2004, in total 254,200 babies were born in Australia. On 30 June 2004, 490 babies were born, making it one of the quietest days in neonatal units in three decades, while double this number of babies who were born on 1 and 2 July 2004 (978 and 902 respectively).Source: Australian Bureau of Statistics (ABS).

admission time and category, diagnoses and other clinical indicators, length of stay and nature of separation (i.e. discharge, transfer or death).

We proxy health outcomes using the number of emergency presentations a child had in her first year of life (0-1), or any other year up until age five (1-2; 2-3; 3-4; 4-5). Similar measures have been used elsewhere to proxy children's health outcomes when the analysis is conducted with (linked) administrative data (González 2017; Cesarini et al. 2016).[11] The data allow us to distinguish demand for health care by both diagnostic and problem groups, data provided by two different sources. For problem groups, upon presentation a medical officer (e.g. a triage nurse) classifies the patient according to the presenting problem (for instance respiratory, head trauma, etc.), a broad category which is consistent with diagnostic sub-categories based on the ICD-10-AM, the Australian modification of the International Statistical Classification of Diseases and Related Health Problems.

For diagnosis codes, we have data on the diagnosis recorded by a medical officer in the hospital separation files and according to which a hospital will be reimbursed. Each diagnosis is coded according to the International Classification of Diseases, 10th edition, Australian Modification (ICD-10-AM 10th Edition). As both sources of information have advantages and disadvantages, we consider both in the analysis.

We focus our analysis on the most common diagnoses, problems or complaints for young children and infants.[12] These diagnoses/problems include, in order of relevance, respiratory (Chapter X), infections (Chapter I), digestive (Chapter XI), injury-poisoning (Chapter XIX), eyes (Chapter VII), ears (Chapter VIII), skin (Chapter XII) and symptoms, signs and abnormal clinical and laboratory findings not elsewhere classified (Chapter XVIII).

From the inpatient dataset (ISAAC), we derive a measure of the number of admissions due to injuries as well as the number of potentially preventable paediatric hospitalizations. We calculated a measure ”potentially preventable paediatric hospitalization” based on the Potentially Avoidable Hospitalization (PAH) tool developed in New Zealand, specifically for a paediatric population. In this tool, the definition of what is avoidable is based on a broad spectrum of factors influencing

[11] The disadvantage of our proxy is that we may understate health problems that an individual has and the resulting economic burden to society. Preferably, we would like to use health care demand from general practitioners who are the gatekeepers for hospital and specialist care in Australia, non-hospital based specialist services, and parental observations. Such data are not available.

[12] Appendix 2 of the EDDC Reference Manual 2014 lists all specific hospital codes, which are being used in emergency departments in South Australia and Western Australia.

health from government policies and population-based health measures to appropriate access to primary care (see Anderson et al. 2012 for description).

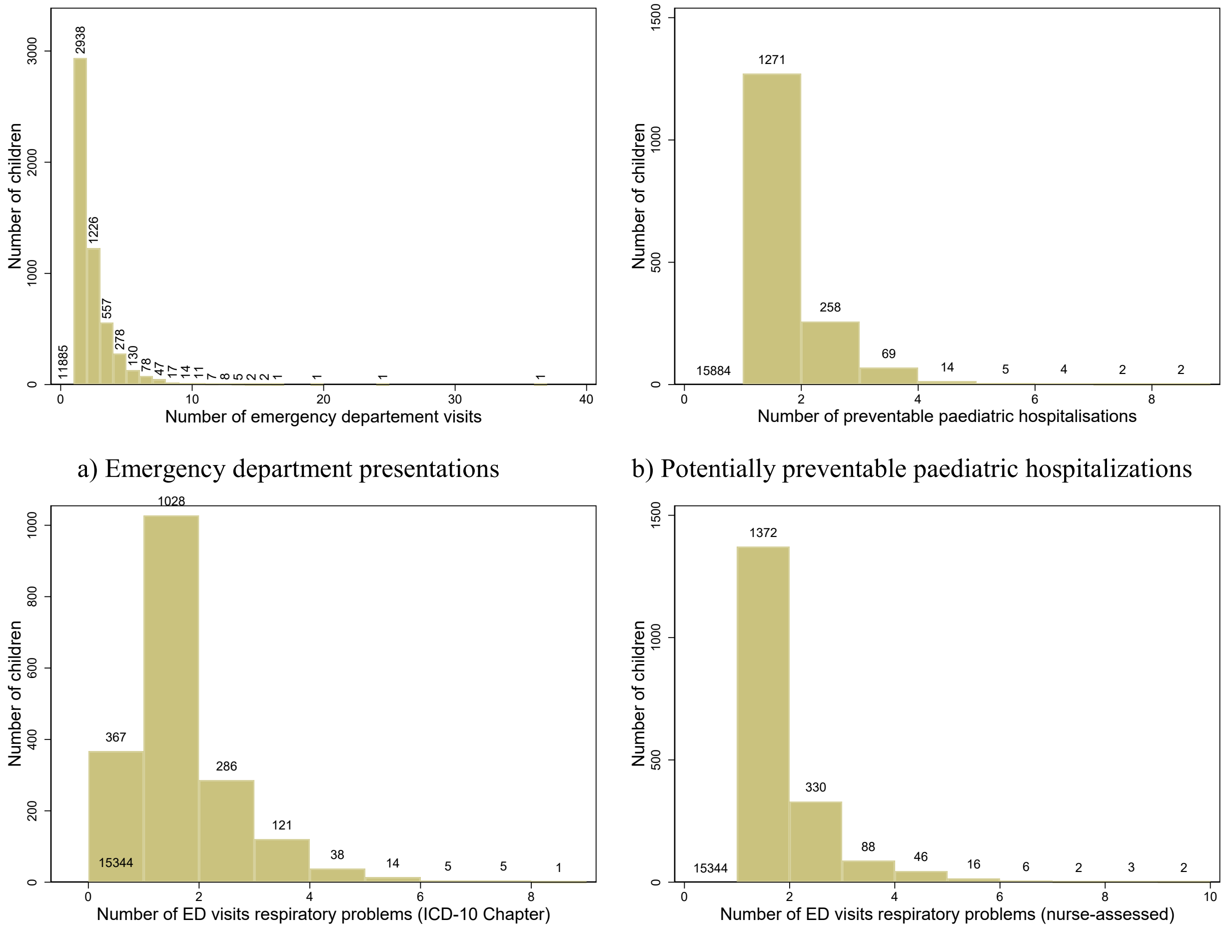
For many children with non-missing birth records, we could not find a hospitalization record. Missing values in the dataset represented about 30 percent for emergency department visits and 70 percent for potentially preventable paediatric hospitalizations. However, missing values in both data sets do not imply missing information (e.g. a lost record), but simply indicate that the child never used public hospital services in South Australia. We have coded missing values as zeros assuming that, if a child had a missing value in the Emergency Department dataset (EDDC), it meant the child did not have any contact with a hospital in South Australia. Similarly, for potentially preventable paediatric hospitalizations, we considered a missing value to be equivalent to the child not having had any hospitalization deemed preventable. Nevertheless, there could be missing values because the child left South Australia. Although we do not have data to rigorously test the validity of this assumption, we can exclude this case. According to interstate migration statistics provided by the Australian Bureau of Statistics, this number is likely to be small and therefore negligible.[13] Finally, we do not have access to private hospital data. Theoretically, it is therefore possible that we misclassified some children to have zero hospitalization even though they consulted private hospital services in South Australia.[14]

We present in Figure 1 the distributions of hospital care utilization (potentially preventable paediatric hospitalizations, emergency department presentations) in the first year of life for the 2004 birth cohort. We observe a high proportion of zeros, ranging from about 70 percent for our aggregate measure of emergency department presentations in Fig. 1a (11,885 observations) to 90 percent for potentially preventable paediatric hospitalizations in Fig. 1b (15,884 observations). We also show that the distribution of emergency department presentations for respiratory problems, that results from the discharge records (Fig. 1c), is almost identical to the distribution of emergency

[13] According to the Australian Bureau of Statistics, in 2004 2,060 children aged 0 to 4 departed from South Australia. Under the assumption that each age group has the same probability to leave, this would imply that 412 of about 17,200 babies born in 2004 would leave the state (2.4 percent). In our case, this would affect about 30 babies per month. See http://stat.data.abs.gov.au/Index.aspx?DataSetCode=ABS_DEM_QIM for details, accessed 17 April 2018. This would only pose a problem for our estimation if outer-state migration is linked to the infant's health status.

[14] In total, South Australia has 99 hospitals, of which 23 are private. Only three private hospitals have emergency departments, which are all located in Greater Adelaide (Calvary Wakefield, Ashford, and St Andrew's). As the EDDC data collection covers all the public ED in SA (72), the number of ED visits that we are missing because we do not have access to private hospital data is much likely very small in comparison and therefore we may not be dramatically skewing the data to the left by recoding all missing values. See https://data.sa.gov.au/data/dataset/sa-health-hospitals-locations and https://www.myhospitals.gov.au/browse-hospitals/sa/greater-adelaide/adelaide

department presentations for respiratory problems that results from the records upon presentation (Fig. 1d). The only difference is that there is a smaller number of zeros in the discharge records than in the presentation problem records (14,140 versus 15,344 observations).



a) Emergency department presentations

b) Potentially preventable paediatric hospitalizations

c) Respiratory problems recorded by medical officer in discharge files based on ICD-10 diagnosis code

d) Respiratory problems recorded at presentation by triage nurse

FIGURE 1. DISTRIBUTION OF THE NUMBER OF HOSPITALIZATIONS AGES 0-1

Notes: The data source for Potentially Preventable Paediatric Hospitalizations is the Integrated South Australian Activity Collection (ISAAC). The data source for ED presentations and Respiratory ED is the South Australian Emergency Department Data Collection (EDDC). Data are presented for the 2004 birth cohort, excluding 37 babies born overseas.

5.2. Sample selection

Ideally, we would like to compare outcomes of children born on 30 June 2004 to those of children born on 1 July 2004 to ensure both groups were exposed to the exact same policy environment. However, given the relatively small number of births per day in South Australia and the birth shifting around the eligibility cut-off date of the policy, this would compromise the statistical power and validity of our analysis. Striking a balance between a tightly defined treatment and control group, and a sample size that allows a statistical analysis, we restrict our analysis to all births that occurred either 28 or 56 days around the July 1 threshold. Ideally, the +/-28 days sample offers the best comparability as both treatment and control groups were born during the winter season in Australia (June, July). However, the sample size is relatively small to allow for a fully interacted model with controls. We therefore consider the +/-56 days sample as benchmark specification, which also includes May (late fall/autumn) and August (late winter) births. As a robustness check, we use additional sample sizes extending the birth window up to six months either side of the threshold date to cover the full year (17,200 babies).[15]

We are able to demonstrate that the +/-56 days sample does not dramatically differ from the full 2004 birth cohort, with one exception (see Table A1, Appendix for descriptive statistics). There are more babies born on any day in the full sample than in the benchmark sample. Babies in the +/-56 days sample are slightly more likely to have been born pre-term (9 versus 8 percent) and to attend a special care nursery (SCN) than the full 2004 birth cohort (17.8 versus 16.5 percent). They also spend more days in SCN (1.7 versus 2 nights). These suggest that babies in the benchmark sample are slightly less healthy at birth than the full 2004-birth cohort. Although these differences are statistically significant, it is important to note that the +/-56 days sample includes all births from late fall (May) to the end of winter (August). It is commonly recognised that babies born in the winter months have poorer health outcomes compared to their peers born in the summer months (Buckles and Hungerman 2013).

Once controlling for multiple hypothesis testing,[16] a necessity as we have 55 independent mean comparisons, the only remaining significant difference between the +/-56 days sample and the full

[15] Additionally, we excluded 37 babies born overseas, who are unlikely to have received the baby bonus.

[16] With 58 independent hypotheses tested, the chance of finding at least one significant difference – assuming a significance level of α=0.05 – is 95%. To reduce the chance of Type I errors due to multiple hypothesis testing, we adjust the p-values for each hypothesis test. There are different ways how p-values can be adjusted, each of which has advantages and disadvantages. All methods reduce Type I errors at the cost of increasing Type II errors, but may lead

2004-birth cohort (in 5 out of seven possible adjustment methods) is that there are slightly more babies born per day on average in the full sample (49.7 versus 48.3). As we will show in the next Section, the number of births per day is mainly driven by babies born within the first week of July. Table A2 (Appendix) reports the adjusted α's (<0.004 independent of the adjustment method) and explains in detail the adjustment methods used.

5.3. Birth shifting

The introduction of the ABB coincided with some strategic shifting of births in the South Australian data. Figure 2 depicts the number of births per day between 1 June and 31 July 2004. On average, there are almost 50 births per day. We observe a slight decline in the number of births in late June, with a particularly low number of births in the last three days of June (Monday, 28 June, Tuesday, 29 June and Wednesday, 30 June). In the same way, there is a peak in the number of births in early July. On Thursday 1 July and Friday 2 July, the number of births is 70 and 73 respectively, numbers far above the average of 49 (horizontal line).

Some of these differences may be due to the day-of-the-week effect. Exploring the data in seven-day bundles to control for this effect, we observe 290 births in the last week of June compared to 373 in the first week of July, a difference of 83 births per week. It is also worth noting that there are 115 births in the last three days of June compared to 188 in the first three days of July. Hence, the major contributor to the difference in the number of births between the last week of June and first week of July are births that occurred within three days of the July 1 cut-off.

All these elements are clear evidence that there was indeed an introduction effect, with parents scheduling births for 1 July or shortly after in order to receive the ABB. Therefore, it seems reasonable to assume that the birth shifting is concentrated in these very last days of June and very first days of July. Gans and Leigh (2009) suggested that 75 percent of the birth shifting happened in the last week of June and first week of July. We will therefore exclude all births within a seven-day window on either side of July 1, 2004 from the estimation sample. The parents and children whose birth was shifted are likely to be systematically different from the children, which could have been shifted but were not and thus remained in the control group.

to a reduction in power of each test (see for a review Benjamini and Hochberg 1995 in psychology literature; List, Shaikh, and Xu 2016 in the experimental economics literature). We use a variety of standard methods to adjust the p-values of the 58 hypothesis tests to ensure our findings are not sensitive to one specific method.

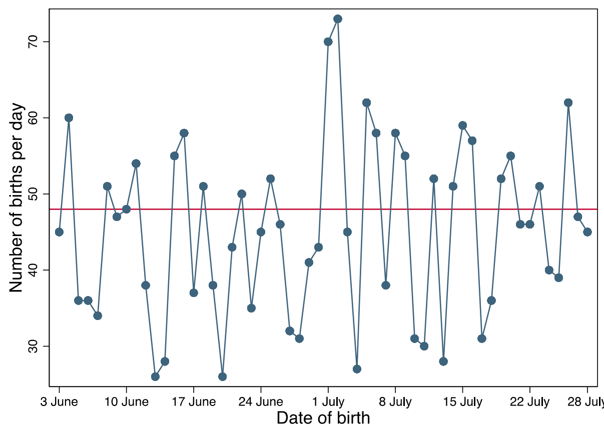


FIGURE 2. PATTERN OF THE DAILY NUMBER OF BIRTHS IN JUNE AND JULY 2004

Notes: The horizontal red line represents the average number of birth per day over the +/-28 days period around 1 July 2004. Each data point represents one birthday.

6. Estimation results

6.1. Validity of the regression discontinuity (RD) approach

The validity of our RD approach relies on the assumption that the treatment and control groups do not differ in relevant characteristics. To test this assumption, we compare the means of a large set of pre-treatment characteristics – including demographics, parental behaviour, pregnancy and birth outcomes – between treatment and control group, using regression analysis (see Pei et al 2018 for a justification of this approach). Table 1 presents the estimation results. Each row shows the estimated differences between treatment and control group and the significance level of this difference. Each column shows the estimated difference by alternative sample definitions: +/- 7 days around the July 1 cut-off (column (1)); +/- 28 days (column (2)); +/- 56 days (column (3)); and +/- 56 days minus births that occurred during +/- 7 days (column (4)).

We will discuss statistically significant differences at the 5 percent level. There are on average 15 additional births per day in the treatment group relative to the control group for the +/- 7 days sample (column (1)). Although this significant difference remains when widening the considered birth window, it drops significantly in size in the +/-56 days sample (2.4 births more). When dropping babies born in the last week of June and first week of July (column (4)), there is no significant difference in the number of births per day between the treatment and control group.

Both babies, and mothers of babies, who were potentially shifted into the first week of July look very different in terms of their birth experience from mothers of children who could have potentially been moved but remained in the control group (column (1)). As expected, mothers who gave birth in the first week of July were less likely to experience a spontaneous onset of labor and were more likely to be induced, which is consistent with birth shifting. Indeed, in an attempt to delay birth, more mothers than what would have usually been observed had planned a delivery of their baby on July 1, or after. Some of them failed to reach that date as they spontaneously entered labor while the others were induced as planned.

TABLE 1—BALANCE IN THE COVARIATES FOR DIFFERENT ESTIMATION SAMPLES (DAYS AROUND 1 JULY 2004)

	+/-7	**+/-28**	**+/-56**	**+/-56 excl. +/-7** [1]
	(1)	(2)	(3)	(4)
Number of births per day	15.382***	6.666***	2.400***	.402
	(.911)	(.426)	(.280)	(.279)
Mother age at birth	.435	.134	-.071	-.152
	(.458)	(.226)	(.157)	(.167)
Mother aged 35+ at birth	.016	.009	-.006	-.009
	(.031)	(.015)	(.011)	(.012)
Mother aged 40+ at birth	.013	.004	-.004	-.006
	(.016)	(.007)	(.005)	(.005)
Parents have high SES status[2]	-.088**	-.045**	-.009	.002
	(.036)	(.019)	(.013)	(.014)
Parents have low SES status[3]	.063	.056***	.024*	.018
	(.039)	(.020)	(.014)	(.015)
Private hospital	-.015	.013	.010	.014
	(.037)	(.019)	(.013)	(.014)
Never Married	-.011	.000	.006	.009
	(.026)	(.013)	(.009)	(.010)
Married	.009	.009	-.003	-.004
	(.027)	(.014)	(.010)	(.010)
Single other	.002	-.010**	-.004	-.005
	(.010)	(.005)	(.003)	(.004)
Caucasian	-.010	.010	.006	.008
	(.023)	(.011)	(.008)	(.009)

Asian	.007	.003	.004	.004
	(.018)	(.008)	(.006)	(.006)
Aboriginal	.002	-.013	-.010*	-.012*
	(.015)	(.008)	(.006)	(.006)
Number of antenatal visits	-.006	.025	-.024	-.017
	(.207)	(.118)	(.082)	(.089)
Smoking during pregnancy	.018	.005	-.004	-.007
	(.031)	(.016)	(.011)	(.012)
Obstetric complications	-.033	-.022	-.017	-.015
	(.038)	(.019)	(.013)	(.014)
Number of weeks of gestation	.540***	.188**	.065	.001
	(.188)	(.087)	(.061)	(.064)
Preterm birth	-.069***	-.013	-.008	.001
	(.025)	(.012)	(.008)	(.009)
Home birth	-.002	.000	-.001	-.001
	(.006)	(.003)	(.002)	(.002)
C-section	-.013	.035*	.017	.021
	(.037)	(.018)	(.013)	(.014)
Spontaneous labor	-.143***	-.062***	-.017	.001
	(.038)	(.020)	(.014)	(.015)
No labor	.044	.039***	.017	.013
	(.029)	(.015)	(.010)	(.011)
Induced labor	.100***	.024	-.000	-.014
	(.033)	(.018)	(.012)	(.013)
Labor complications	-.019	.018	.013	.018
	(.038)	(.019)	(.014)	(.015)
Female	.053	.032	.024*	.021
	(.039)	(.020)	(.014)	(.015)
Multiple births	-.025	-.004	.003	.007
	(.017)	(.007)	(.005)	(.005)
Baby weight	51.061	28.413	16.704	12.537
	(51.009)	(24.117)	(16.984)	(17.994)
Low birth weight	-.059***	-.004	-.000	.008
	(.022)	(.010)	(.007)	(.008)
Very low birth weight	-.004	-.007*	-.004	-.004
	(.010)	(.004)	(.003)	(.003)
Apgar score 1 min	.343***	-.061	-.038	-.093**
	(.113)	(.057)	(.040)	(.043)

Apgar score 5 min	.203***	-.021	-.012	-.043*
	(.072)	(.034)	(.023)	(.024)
Baby breathing (min)	-.123**	.046	.022	.044*
	(.052)	(.033)	(.022)	(.024)
Mortality 24h	-.008	-.004	-.002	-.001
	(.006)	(.002)	(.001)	(.001)
Special Nursery Care (SNC)	-.063**	.013	.005	.015
	(.030)	(.016)	(.011)	(.011)
Number of days in SNC	-1.595***	-.381	-.085	.127
	(.591)	(.282)	(.205)	(.218)
Neonatal intensive care unit (NICU)	-.036***	-.001	.001	.007
	(.014)	(.007)	(.005)	(.005)
Number of days in NICU	-.548**	-.201*	.050	.140
	(.229)	(.117)	(.106)	(.117)
Paediatric intensive care unit (PICU)	.000	-.000	-.000	-.000
	(.)	(.001)	(.001)	(.001)
Number of days in PICU	.000	.002	-.000	-.000
	(.)	(.006)	(.006)	(.007)
Number of observations	663	2,520	5,115	4,452

Notes: Estimates are from OLS estimation of the dependent variables specified in the row header on the treatment indicator, i.e. a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. Babies born overseas are excluded from the samples, as they are unlikely to have triggered the Australian Baby Bonus.
[1]Results for the sample including births in a 56-day window either side of 1 July 2004 excluding all births 7 days before and after 1 July 2004. [2, 3] refer to occupational classification, which is provided in the perinatal and birth record data. Parents high socioeconomic status (SES) refers to father working in managerial, professional and administrative occupations; parental low SES refers to father working as trades person, clerk, sales person, plant/machine operators, driver, or labourer, Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Babies born in the first week of July, compared to those born in the last week of June, were less likely to be born pre-term or of low birth-weight. These babies also had higher Apgar scores and needed less time to establish normal breathing. Finally, the probability of admission to, and time spent at a SCN or a NICU is also lower in the treatment group. These significant differences demonstrate that babies who stayed longer in the womb were less likely to experience poor health outcomes at birth. Interestingly, these babies are also less likely to be born to fathers from more advantaged socio-economic backgrounds (SES) as defined by father's occupational classification[17]. This means that families on medium or lower income, for which AU$3,000 represents a non-trivial amount, were more likely to be successful in delaying the birth.[18]

Most of the differences in birth-related outcomes and SES disappear or become smaller when widening the birth window of both the treatment and control group (columns (2)-(3)). When excluding births around seven days of the cut-off date, we observe significant differences in four out of forty-three covariates (column 4).

After adjusting for multiple hypothesis testing, there are no remaining statistically significant differences in pre-treatment covariates between the treatment and control group independent of the adjustment method.[19] Pre-treatment variables are balanced between both groups, a finding that supports our identification assumptions.[20] We thus have certainty that the introduction of the ABB was as good as random.

6.2.Effect of the ABB on children's health outcomes

Having shown that there are no discernible differences between treatment and control group babies, we have established the minimum condition for obtaining unbiased estimates of the treatment effect as defined in the statistical model (Eq. (1)). In Figure 3 we demonstrate the existence of a discontinuity in key health care utilization measures (ages 0-1) around the 1 July 2004 threshold in the raw data.[21] We observe a discontinuity in health care utilization at the threshold for all considered outcome variables. In every case, babies who benefitted from the ABB have lower counts.

This is particularly true for potentially preventable paediatric hospitalizations (PPPH) (Fig. 3a) and the number of emergency department (ED) presentations for respiratory problems, as recorded using ICD-10-AM codes at separation (Fig. 3c) and presenting problem (Fig. 3d). Albeit present

[17]Occupational classification data is provided in the perinatal and birth record data. High-skilled occupation refers to managerial, professional and administrative occupations; low-skilled occupation refers to trades persons, clerks, Sales persons; Lowest occupation refers to plant/machine operators, drivers, and laborers.

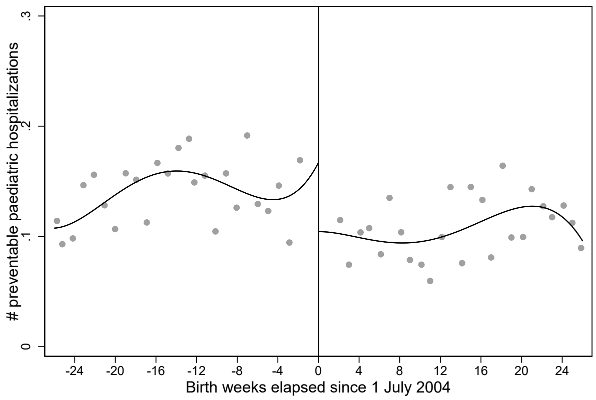
[18] Furthermore, we find no difference in the proportion of women giving birth in a private hospital. Private health insurance, which is associated with higher incomes, was not a determinant of shifting a child's birth date.

[19] Table A3 (Appendix) reports the adjusted α's.

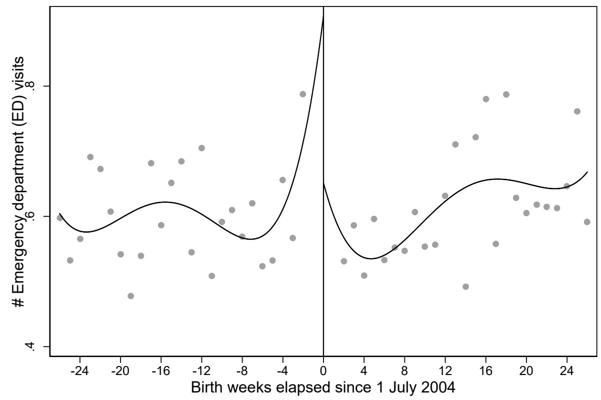
[20] This conclusion is based on the assumption that there are no important remaining differences in unobservable characteristics, an assumption we cannot test for.

[21] The figures were plotted using the rdrobust command in Stata that implements statistical inference and graphical procedures for regression discontinuity designs employing local polynomial and partitioning methods (see Calonico et al 2014; 2017). We allow for a flexible, non-parametric approximation of the non-linear relationship with a quartic polynomial. Each dots represents the average number of hospitalizations per birth week.

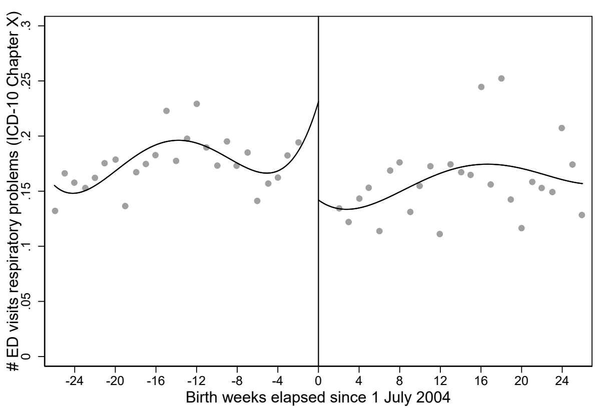
in the fitted line, the discontinuity around the July 1 threshold is less visible in the raw data for the aggregated measure of ED presentations (Fig. 3b). These figures demonstrate that children born just after the introduction of the ABB appear to be healthier in the first year of life than babies born just before the cut-off. We find no discontinuities around July 1 for any of the other disease categories (figures available upon request).



a) # potentially preventable pediatric hospitalizations

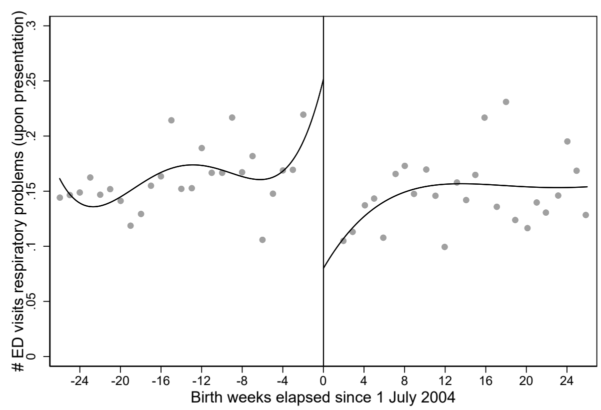


b) # visits in the emergency departments (ED)



c) # ED visits respiratory problems

(ICD-10 Chapter X ; separation files)



d) # ED visits due to respiratory problems

(upon presentation)

FIGURE 3. IMPACT OF THE ABB ON NUMBER OF HOSPITALIZATIONS BETWEEN AGE 0 AND 1

Notes: The data source for Potentially preventable Paediatric Hospitalizations is the Integrated South Australian Activity Collection (ISAAC). The data source for ED presentations and Respiratory ED is the South Australian Emergency Department Data Collection (EDDC). Data is presented for the 2004 birth cohort, excluding babies born overseas who are unlikely to have triggered the Baby Bonus, and births 7 days before and after 1 July 2004. Each dot represents the average number of presentations for babies born in one week. Figures were plotted with – rdrobust – command in Stata employing a local polynomial and partitioning methods (see Calonico et al 2014; 2017). We allow for a flexible, non-parametric approximation of the non-linear relationship with a quartic polynomial.

In what follows, we will test whether the negative treatment effect of the ABB on health care utilization is statistically significant and persists when estimating the full model as outlined in Eq. (1). We report in Table 2 the estimations results for both the +/-28 (N=1,862) and +/-56 (N=4,461) samples. Reported are marginal effects calculated from a negative binomial regression model.

As observed in Figure 3, the estimated treatment effects of the ABB are negative, independent of the sample definitions. This means the availability of additional financial resources did not lead to increased healthcare utilization. Rather, it reduced hospital care use. In the +/-28 days sample, the reduction in the total number of PPPHs in the first year of life is statistically significant at the 5 percent level. However, the treatment effect is not precisely estimated in the +/-56 days sample, although it is of similar magnitude. Hence, the ABB led to a reduction in the number of PPPHs by 0.04 visits, falling from an average of 0.14 visits in the control group to 0.10 visits in the treatment group. This drop represents a reduction of 30 percent relative to the sample mean. In other words, thanks to the ABB, the share of hospitalizations that could potentially be avoided is one in ten instead of one in seven babies. In 2004, the average cost of an admitted emergency visit in South Australia was about AU$800[22], therefore, the economic interpretation of this estimate is a saving of about AU$560,000 (= 0.04 × 17,500 × 800).

TABLE 2—IMPACT OF THE ABB ON HOSPITAL CARE UTILIZATION AGES 0 to 1

	Days around 1 July 2004	
	+/-28	+/-56
	(1)	(2)
Preventable Paediatric Hospitalizations	-.040**	-.042
	(.020)	(.033)
Emergency Department Presentations	-.124**	-.152*
	(.059)	(.088)
Number of observations	1,862	4,461

Notes: The coefficients are the marginal effects from negative binomial estimation models. The dependent variable is specified in the row headers. The treatment indicator is a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. All specifications exclude births 7 days before and after 1 July 2004 (N=633) as well as babies born overseas, who are unlikely to have triggered the Baby Bonus (N=37). Controls include date of birth interacted with treatment (+/-56 days sample) and day of the week (both +/-56 days and +/- 28 days sample). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[22] Data taken from the National Hospital Cost Data Collection Australian Public Hospitals Cost Report 2013-2014 Round 18, published by the Independent Hospital Pricing Authority. Report for the year 2003-2004 could not be found. Emergency department average cost per admitted-presentation is AU$863 (Table 14 p.27). A lower estimate has been used to account for inflation. https://www.ihpa.gov.au/sites/g/files/net636/f/publications/nhcdc-round18.pdf

In the New Zealand PAH tool, one of the criteria used to identify a hospitalization as "potentially avoidable" is set as follows:

> *"Government policies which ensured adequate socioeconomic resources were available to families with children (e.g. income support, childcare, assistance for solo parents returning to workforce)."* (Anderson et al. 2012, p. 28).

The ABB falls into this category. Our estimates suggest that transfers to families with newborns may have led to more appropriate use of healthcare services and therefore to a reduction in unnecessary hospitalizations.

We also observe a sizeable effect for our aggregate measure of ED presentations. The effect is significant at the 5 percent level in the +/-28 days sample and at the 10 percent level in the +/-56 days sample. Again, we are confident in saying that the financial support brought by the ABB had a positive impact on child health, reducing the number of ED presentations in the first year of life. On average, a child in the treatment group had between 0.12 (+/-28 days) and 0.15 (+/-56 days) less ED presentations due to the ABB. Given that the average is about 0.6 presentations in the control group, this represents a 20-25 percent reduction. In simple words, this means that because of the ABB only one in two children present at the ED instead of two out of three. This is an economically sizeable effect. Indeed, given that in 2004 the average cost of an ED presentation in South Australia was about AU$500[23], this represents roughly a saving of AU$1,330,000 (= 0.152 × 17,500 × 500). These conclusions do not change when choosing a different functional form of the estimation model that allows for modelling explicitly the determinants of zero-observations (Table A4, Appendix).

Table 3 reports the average treatment effect of the ABB separately for each problem group, defined by the diagnosis recorded in the discharge files (columns (1) and (2)) and the problem recorded upon presentation (columns (3) and (4)). We obtain a significant treatment effect only for respiratory problems, independent of its definition (Chapter X, presenting problem).

[23] Data taken from the National Hospital Cost Data Collection Australian Public Hospitals Cost Report 2013-2014 Round 18, published by the Independent Hospital Pricing Authority. Report for the year 2003-2004 could not be found. Emergency department average cost per presentation is AU$614 (Table 13 p.27). A lower estimate has been used to account for inflation. https://www.ihpa.gov.au/sites/g/files/net636/f/publications/nhcdc-round18.pdf

TABLE 3—IMPACT OF THE ABB ON EMERGENCY DEPARTMENT PRESENTATIONS AGES 0 TOs 1, BY PROBLEM GROUP

	IDC-10 AM		Presenting problem	
	+/-28	+/-56	+/-28	+/-56
	(1)	(2)	(3)	(4)
Infections	-.020	-.026	-.006	-.009
	(.018)	(.028)	(.016)	(.024)
Eyes and Oral	-.005	-.009	-.005	-.014
	(.008)	(.010)	(.008)	(.010)
Respiratory	-.045**	-.071*	-.065***	-.106***
	(.023)	(.038)	(.025)	(.038)
Digestive	-.008	-.012	-.021	-.026
	(.011)	(.016)	(.018)	(.028)
Skin	-.007	-.017	-.002	-.019
	(.009)	(.013)	(.011)	(.016)
Injury (Trauma)/Poisoning	.002	.020	.012	.034**
	(.009)	(.016)	(.008)	(.015)
Perinatal problems	-.008	-.009		
	(.007)	(.014)		
N	1,862	4,461	1,862	4,461

Notes: The coefficients are the marginal effects from negative binomial estimations models. The dependent variables is specified in the row headers. The treatment indicator is a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. All specifications exclude births 7 days before and after 1 July 2004 (N=633) as well as babies born overseas (N=37), who are unlikely to have triggered the Baby Bonus. Controls for the date of birth and day of the week are included in the +/-56 days sample. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

For instance, the average number of ED presentations for respiratory conditions is reduced by 0.07 presentations in the treatment group compared to the control group (column (2)). This means the ABB reduces the share of children going to the ED for respiratory problems in the first year of life from one in six to roughly one in 13, a reduction of 52 percent relative to the mean. This reduction in the number of ED presentations for respiratory problems is also consistent with the decrease in PPPHs. Indeed, the PAH tool identifies respiratory problems treated in hospital as "potentially avoidable" (Anderson et al. 2012, p. 26).

Additionally, we observe in all specifications a small absolute increase in the number of ED presentations for accident-related problems (trauma/injury/poisoning). In relative terms, this increase implies a doubling of the rate at which infants present at the ED (from one in 40 to one in

20). However, the impact of the ABB is only statistically significant in the +/- 56 days sample and when defining the outcome through the presenting problem (column (4)). We will show in our robustness checks that this impact estimate is not robust.

Since the ABB was only paid out after the birth of a child, it could not have affected birth outcomes. We need to show therefore that the ABB had no impact on ED presentations due to perinatal problems, which indicate whether the child was treated for problems occurring during birth. Table 3 shows a precise zero estimate of the ABB on perinatal problems, which is what we hypothesized.

Finally, we further explored whether the ABB had a long-term impact, i.e. at later stages of childhood. We therefore estimated the treatment effect of the ABB on the number of PPPH and ED presentations in every year of life up until age 5 (Table A5, Appendix). None of the estimates is statistically significant at conventional levels, leading us to the conclusion that additional financial resources available through the ABB in the first year of life were used in the first year of life. One explanation is that the amount of AU$3,000 is not large enough for families to be able to save up for later years or does not affect permanent income which would allow consumption choices to change in every year.

6.3. Robustness checks

In Table 4 we present robustness checks for key outcomes in the first year of life using the +/-56 days sample for brevity. First, we demonstrate that the treatment effects are sensitive to the exclusion of the children born in the last week of June and first week of July (column (2), N=633), but are not sensitive to excluding strategically shifted babies by up to 10 days around the threshold (column (3), N=270). The impact of the ABB is estimated to be of smaller magnitude or is no longer statistically significant when including the children who were born because of systematic birth shifting. One explanation for this finding is that although the potentially shifted babies may have had better health at birth, they may have had more health problems in the first year of life that required hospital attention. This interpretation is consistent with the idea that the potentially shifted babies were postponed births delivered by Caesarean section. A recent meta analysis based on 13 studies has shown that C-section babies have an increased risk of developing chronic respiratory problems during childhood (Keag, Norman and Stock 2018). If we had ignored selective birth shifting in our analysis, we would have underestimated the potential health benefits of the ABB.

Second, the AU$3,000 of the ABB is unlikely to have changed how a child with severe health problems at birth will be managed in the healthcare system. We therefore excluded children who were transferred at birth for congenital abnormalities to a special care unit (column (4), N=132) and children who experienced complications during delivery (column (5), N=54). The estimates are very similar to the baseline analysis and have the same level of significance.

TABLE 4—ROBUSTNESS CHECKS

	Baseline		**Exclude individuals**			**Extended samples**[a]	
	Excl. +/- 7 days	Include +/- 7 days	Excl. +/-10	Excl. Cong. abnorm.	Excl. birth complic.	+/-112 Days	+/- 182 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prev. paediatric hosp.	-.042	.001	-.040	-.038	-.046	-.049**	-.059***
	(.033)	(.025)	(.037)	(.033)	(.033)	(.023)	(.017)
Emergency Depart.	-.152*	-.056	-.130	-.151*	-.157*	-.142**	-.084*
	(.088)	(.066)	(.098)	(.088)	(.088)	(.062)	(.046)
Respiratory problems							
ICD-10 AM Chap. X	-.071*	-.037	-.065	-.066*	-.067*	-.056**	-.040**
	(.038)	(.028)	(.043)	(.038)	(.038)	(.028)	(.020)
Presenting Problem	-.106***	-.064**	-.074*	-.111***	-.101***	-.077***	-.049**
	(.038)	(.028)	(.043)	(.039)	(.039)	(.027)	(.019)
Injury (trauma), Poisoning							
ICD-10 AM Chap. XIX	-.008	.006	-.015	-.008	-.009	-.005	-.004
	(.013)	(.010)	(.016)	(.013)	(.013)	(.008)	(.005)
Presenting problem	.034**	.011	.037**	.035**	.034**	.020**	.002
	(.015)	(.011)	(.017)	(.015)	(.015)	(.010)	(.007)
Number of observations	4,461	5,124	4,191	4,329	4,407	9,799	16,444

Notes: [a] Regression models include additional controls for winter birth (June, July, August) and summer birth (December January, February). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Third, extending the birth sample to +/-112 and +/-182 days born around the 1 July 2004 threshold does not alter out conclusions either (columns (6)-(7)). Although the magnitude of the estimated marginal effects is in most cases slightly smaller, we obtain similar and more precisely estimated treatment effects. The only difference is that in those extended samples, the marginally significant impact on accidents in the home (trauma/poisoning) is no longer statistically significant, suggestion that the impact of the ABB on accidents in the home is not robust.

Finally, we conduct placebo tests in which we re-estimate the baseline model on the years 2005-2013 (see Table A6, Appendix). In some of these years other important family assistance or tax reform were implemented on 1 July (2005, 2009), and in two years the Baby Bonus was raised by AU$1,000 on 1 July (2006, 2008). We cannot conduct the placebo test before 2004 as the hospitalization data were collected only from 2003 onward. We find no discernible pattern of a significant impact of the ABB on health care utilization in the years, when no other reform took place. We find tentative evidence that raising the ABB by AU$1,000 had beneficial health impacts, especially on respiratory health (2006, 2008).

6.4. Heterogeneity of treatment effect by socioeconomic status

As described in Section II., the AU$3,000 amount of the ABB represents 2.5 times the median weekly household disposable income, but 5.3 times the mean income for households in the bottom decile of the household income distribution. Hence, we expect the cash transfer to have a greater impact on higher need families and their children. We have no income data, but in Table 6 we present the treatment effect of the ABB separately for children of married (column (2)) and single (column (3)) mothers, and for families where the father works in high-skilled occupations (column (4)) versus low-skilled occupations (column (5)). High skilled occupations refer to managerial, professional and administrative tasks. Low-skilled occupations refer to trade, service, plant and operational workers. Fathers' occupations were reported in the birth records. Because the samples are small, we conduct the analysis on the extended sample (+/-182 days).

The treatment effect of the ABB is consistently larger in absolute values for babies from higher needs families (single mothers, low-skilled fathers) than for babies from more privileged families (married mothers, high-skilled fathers) across all outcomes. The heterogeneity is particularly striking for ED presentations due to respiratory problems, for which we find no or only a very small treatment effect for babies from more privileged backgrounds.

TABLE 6—HETEROGENEITY IN THE IMPACT OF THE ABB

	BL +/-182[a] (1)	Married (2)	Single (3)	Professional (4)	Low-skill (5)
Preventable paediatric hosp.	-.059***	-.054***	-.095	-.050**	-.046**
	(.017)	(.017)	(.061)	(.024)	(.022)
Emergency department visits	-.084*	-.067	-.193	-.020	-.097
	(.046)	(.047)	(.179)	(.071)	(.062)
ED presentations respiratory problems					
ICD-10 Chapter X	-.040**	-.035*	-.063	.002	-.060**
	(.020)	(.020)	(.074)	(.030)	(.026)
Presenting problem present.	-.049**	-.035*	-.148*	-.013	-.064**
	(.019)	(.020)	(.076)	(.028)	(.026)
ED presentations Injury (trauma)/poisoning problems					
ICD-10 Chapter XIX	-.004	-.000	-.022	-.001	-.007
	(.005)	(.005)	(.017)	(.009)	(.008)
Presenting problem present.	.002	-.002	.029	.002	.001
	(.007)	(.007)	(.026)	(.012)	(.009)
Number of observations	16,444	14,369	1,871	5,288	8,880

Notes: The coefficients are the marginal effects from negative binomial estimation models. The treatment indicator is a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. Controls include date of birth (interacted with treatment indicator), day of the week, and indicator variables for winter birth (June, July, August) and summer birth (December, January, February). Married includes all individuals in de facto relationships. Single refers to never married. Occupations refer to father's occupation as recorded in birth certificate. High skilled refers to professional, management, or administrative jobs. Low skilled refers to tradespersons, clerks, salespersons, plant, machine operators. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

For instance, the negative treatment effect of the ABB on respiratory problems (presenting problem) is four times larger for babies of single mothers than for babies of married mothers (-0.15 versus -0.04), and almost five times larger for babies of low-skilled fathers than for babies of high-skilled fathers (-0.06 versus -0.01). This finding supports the hypothesis that families with lower incomes, which we approximate with information on the (presence of the) father, benefit more from the ABB.[24]

[24] We also have information on whether the father is unemployed, which is another proxy for availability of financial resources. The treatment effect of the ABB is largest in magnitude for families where the father is unemployed, but the estimates are very imprecise, as we have only 840 observations in the extended sample. These results are provided upon request.

6.5.Mechanisms that explain the link between the ABB and respirator health

Our administrative data do not allow us to study the mechanisms through which the ABB may have led to better respiratory health of infants in the first year of their lives. To gain a better understanding of the potential mechanisms, we analyze auxiliary data from both the Household, Income, and Labor Dynamics in Australia (HILDA) survey and review findings from previous research based on the Longitudinal Study of Australian Children (LSAC), two nationally representative surveys on households and children, respectively.

In 2005 (Wave 5), the HILDA collected information on household expenditures dating back to the financial year 2004-2005, the year in which the ABB was introduced. As part of self-completion questionnaire (SCQ), participants were asked to report their annual expenditures on a battery of household goods, including expenditures on essentials (e.g. utilities, groceries), non-essentials (e.g. alcohol, cigarettes), and health (service use, private health insurance). We use average household expenditures across all household members who responded.

The household questionnaire of HILDA also includes detailed information on new (or leaving) household members, their arrival date (month, year), and their age at arrival. In total, we find 161 unique households who recorded the arrival of a newborn or an adopted baby in 2004. Almost 50 percent of this group benefitted from the ABB. Only a small fraction of SCQ respondents refused to answer the module. Less than 6 percent of the sample did not respond to the SCQ. Appendix B describes the data and the methods used to construct the treatment indicator.

Because of the small sample size of about 120 households for which we have complete data, we cannot conduct our analysis with babies born between May and August 2004 only (Table B1). The sample size also limits the possibilities of our model specification. Depending on the outcome variable[25], we estimate linear (continuous outcomes), negative binomial (count outcomes), and Probit models (0,1 outcomes), in which the dependent variable is, respectively: (1) Logarithm of annual household expenditures (for variables with positive expenditures); (2) Annual household expenditures (for variables with zero or positive expenditures); and (3) Probability of positive expenditures (for variables that had a very large number of zeros). The key right-hand side variable of interest is a dummy variable that takes the value 1 if the household is in the

[25] A full set of outcome variables and their summary statistics by treatment status is presented in Table B2, Appendix.

treatment group (newborn between July-December 2004), and 0 otherwise (newborn between January-June 2004). Additionally, we include a set of household composition variables (number of children aged 0-4, 5-9, 10-14; number of adults) to control non-linearly for differences in consumption by household size and need.[26] For comparability, all treatment effects are discussed in terms of (log) percent difference relative to the sample mean.

Overall, we find little evidence that essential household expenditures differ between treatment and control households, independent of whether we include (Panel A) or exclude (Panel B) the June-July babies for which strategic shifting may have occurred. There is however tentative evidence that treated households spent between 17-21 percent more than control households on utilities (mainly on electricity). However, our estimates are imprecise because of small samples, with p-values of 0.06-0.14 (Table B4, Appendix). Importantly, treated households are 20 percent less likely to have had health care expenditures above the sample mean (AU$582), and spent about 18 percent less on health care services. They are 29-33 percent more likely to have spent money on private health insurance (p-values range between 0.10 and 0.15) and 55 percent more on private health insurance (Table B5, Appendix). There is no evidence that households benefitting from the ABB spent more on non-essential goods such as alcohol, cigarettes, holiday games/entertainment/hobbies, or home renovation (Table B6, Appendix). These estimates combined allow us to conclude that treated households are more likely to spend money on goods that benefit new-born children.

Additional insights on the mechanisms through which the ABB may have affected infant health can be gained from Gaitz and Schurer (2017), who used LSAC data to study the spillover effects of the ABB on the siblings of newborn babies. They show that the ABB did not impact upon parenting behaviors (investments, styles), parental wellbeing, and maternal labor supply. The only noticeable impact of the unconditional cash transfer was that parents who benefited from the ABB stated that they felt they were more likely to raise AU$2,000 at short notice. Thus, the ABB may have provided families with an emergency buffer.

[26] We are able to demonstrate that treatment and control groups do not differ in their household composition and in the disposable household income pre-or post treatment. The only difference is that treated households significantly differ in the amount of maternity payments, which represents the Baby Bonus. The difference between treatment and control group is roughly AU$3,000. Consistent with our description of the institutional background of the ABB in Section II., the treatment group received less in Government allowances and other parenting payments than the control group (on average AU$870). See Table B3 in the Appendix.

7. Discussion and conclusion

Early-life health, family, and income shocks can have a long-lasting impact on children's health and human capital development and their adulthood labor-market trajectories (Almond et al., 2017). Government transfers aimed at improving living conditions and increasing purchasing power of households with children are, at least in theory, a powerful lever by which public policy can assist vulnerable children to a better start in life. Yet, little is known about the effectiveness of such policy levers to improve children's health outcomes, and the mechanisms through which cash transfers may influence health.

We contribute to this literature by evaluating the impact of the Australian Baby Bonus (ABB), an unconditional and initially unanticipated cash transfer paid to families with a newborn child, on health outcomes of children. We are able to demonstrate that the introduction of the ABB on 1 July 2004 was as good as random, because the children born around the implementation date are similar in all relevant observable characteristics apart from the income shock after birth. Using high quality linked administrative data and a regression discontinuity approach, we find that the ABB reduced the number of emergency department presentations for respiratory problems and the number of potentially preventable paediatric hospitalizations in the first year of life. The effects sizes are economically meaningful. The ABB reduces the share of potentially preventable paediatric hospitalizations from one in seven to one in ten infants in the first year of life. Similarly, the ABB reduces the number of emergency department presentations for respiratory problems from one in six infants to one in 13 in the first year of life, a reduction of more than 50 percent. These findings are robust to our model assumptions and sample definitions. Our findings on respiratory illness echo those in Kuehnle (2014), who finds a negative treatment effect of family income on the incidence of respiratory illnesses of children in the UK (and no relationship on other chronic health conditions).

In our data, the health gains due to the ABB are particularly visible for infants from disadvantaged families, who are the ones most likely to suffer from respiratory disease (see Taylor-Robinson et al 2016; Propper and Rigg 2006). Hence, the ABB was effective in reducing income-related disadvantages in health for families who appear to have spent the additional financial resources on child-centred goods (Dahl and Lochner 2012). Although we find tentative evidence that the ABB may have caused an increase in accident or trauma-related emergency department visits for infants from disadvantaged backgrounds, suggesting that some families may have spent the ABB on risky rather than child-centred consumption goods, this evidence does not pass a series of robustness checks. This interpretation is strengthened by our analysis of auxiliary data on household expenditures. The ABB did not seem to have led to an increase in the purchase of non-essential household goods such as alcohol, cigarettes, holidays or hobbies. Rather, it increased household expenditures on utilities (electricity) and private health insurance. We do not know for which purpose more electricity was used in the households benefitting from the ABB. Yet, if it was used for heating and cooling, which is associated with a better regulation of room temperature and moisture, this could have led to better respiratory health of infants. Evidence from New Zealand suggests that more effective heating can improve respiratory health of children with asthma (Howden-Chapman et al 2008).

Finally, our findings contrast dramatically with the findings from two recent studies, which evaluated the impact of the Australian (ABB) and the Spanish Baby Bonus (SBB) on child health outcomes. Gaitz and Schurer (2017) find a robust negative treatment effect of the ABB on child health. However, Gaitz and Schurer (2017) study the impact of the ABB on the older siblings (between ages four and eight) of newborns who triggered the ABB and relies mainly on a parental report of health. Parent-reported measures present limitations, they are subjective and parents from different SES may systematically differ in the way they perceive and assess health (Currie 2009).

In a recent study on the impact of the SBB on children's health, González (2017) finds an increase in emergency department healthcare utilization, with stronger effects between ages two and five. These findings are contrary to ours in two ways. First, we find a reduction in ED presentations due to the ABB in the first year of life. Second, we find no impact of the ABB at later ages. The results from Australia may differ from the Spanish Baby Bonus experiment because the Baby Bonus may have had a different impact on maternal labor supply in both countries. In Spain, the SBB reduced maternal labor supply (González 2013), and a paid parental leave scheme existed at the introduction of the SBB. Combined, these two family benefit policies provided mothers with more time in the home and with their infants. This available time resource may have prompted mothers to seek hospital care more often. Seeking advice from health care specialists may also increase the probability of detecting problems, which implies a worsening of objective health. Although the ABB offered a relatively large increase in disposable household income for families with a new-born baby of almost 5 percent for the median household, it did not lead to a significant reduction in maternal labor supply after birth (Gaitz and Schurer 2017).

We conclude that our results provide a balanced account of the potential benefits and risks of an unconditional cash transfer paid to families in one of the richest OECD countries in the absence of an official paid parental leave policy. Our findings are suggestive that targeting of such transfers towards families on low income may be both more effective and efficient. The ABB was abolished in 2014; we suggest this abolition may have been premature in light of the empirical evidence on its effectiveness, especially for disadvantaged families.

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FOR ONLINE PUBLICATION-APPENDIX

TABLE A1 — DESCRIPTIVE STATISTICS

	01/01/2004 – 31/12/2004					+/- 56 days	
VARIABLES	N	mean	sd	min	max	mean	p-value
Number of births per day	17209	49.688	10.437	17	76	48.246	0.000
Mother age at birth	17209	29.436	5.623	14	54	29.428	0.904
Mother aged 35+ at birth	17209	0.182	0.386	0	1	0.184	0.661
Mother aged 40+ at birth	17209	0.030	0.171	0	1	0.031	0.856
Father occup.: professional	17209	0.322	0.467	0	1	0.317	0.385
Father occup.: low skilled	17209	0.540	0.498	0	1	0.546	0.274
Private hospital	17174	0.339	0.473	0	1	0.335	0.490
Never Married	17209	0.114	0.318	0	1	0.119	0.208
Married	17209	0.873	0.333	0	1	0.866	0.077
Single other	17209	0.012	0.109	0	1	0.014	0.113
Caucasian	17174	0.909	0.287	0	1	0.911	0.683
Asian	17174	0.048	0.214	0	1	0.047	0.713
Aboriginal	17174	0.043	0.202	0	1	0.042	0.849
Number of antenatal visits	15876	10.719	2.849	0	30	10.696	0.521
Smoking during pregnancy	16980	0.192	0.394	0	1	0.192	0.855
Obstetric complications	17174	0.318	0.466	0	1	0.331	0.021
Number of weeks of gestation	17174	38.838	2.056	20	44	38.787	0.033
Preterm birth	17174	0.082	0.274	0	1	0.091	0.002
Home birth	17209	0.004	0.065	0	1	0.005	0.312
C-section	17174	0.324	0.468	0	1	0.320	0.430
Spontaneous labor	17174	0.558	0.497	0	1	0.563	0.421
No labor	17174	0.167	0.373	0	1	0.166	0.893
Induced labor	17174	0.275	0.447	0	1	0.271	0.433
Labor complications	17174	0.363	0.481	0	1	0.372	0.092
Female	17209	0.487	0.500	0	1	0.490	0.572
Multiple births	17209	0.033	0.179	0	1	0.032	0.594
Baby weight	17204	3365.701	591.244	210	6060	3364.691	0.884
Low birth weight	17204	0.065	0.246	0	1	0.070	0.081
Very low birth weight	17204	0.011	0.103	0	1	0.013	0.037
Apgar score 1 min	17148	8.045	1.433	0	10	8.030	0.379
Apgar score 5 min	17152	9.126	0.799	0	10	9.117	0.352
Baby breathing (min)	17099	1.158	0.753	1	20	1.160	0.816
Mortality 24h	17206	0.002	0.041	0	1	0.002	0.171
Special Nursery Care	17174	0.165	0.371	0	1	0.178	0.002
Number of days in SNC	17174	1.740	6.723	0	143	1.989	0.002
Neonatal intensive care unit	17174	0.028	0.165	0	1	0.030	0.471
Number of days in NICU	17174	0.368	3.985	0	125	0.397	0.522
Paediatric intensive care unit	17174	0.002	0.046	0	1	0.001	0.175
Number of days in PICU	17174	0.026	1.031	0	86	0.007	0.124
Preventable Paed. Hospital.	17209	0.124	0.444	0	9	0.120	0.448
ED presentations	17209	0.603	1.297	0	37	0.565	0.012
Chapter I Infections	17209	0.116	0.405	0	6	0.103	0.009
Chapter VII Eyes/VIII Ears	17209	0.017	0.142	0	3	0.013	0.036
Chapter X Respiratory	17209	0.173	0.556	0	10	0.161	0.063
Chapter XI Digestive	17209	0.030	0.210	0	4	0.033	0.293
Chapter XII Skin	17209	0.022	0.181	0	4	0.020	0.317
Chapter XIX Injury/Poisoning	17209	0.039	0.214	0	4	0.036	0.207

Chapter XVI Perinatal	17209	0.015	0.146	0	3	0.018	0.069
Presenting Prob: Respiratory	17209	0.154	0.535	0	10	0.148	0.300
Presenting Prob: Digestive	17209	0.105	0.406	0	11	0.092	0.009
Presenting Prob: Infection	17209	0.091	0.336	0	4	0.082	0.021
Presenting Prob: Trauma/Pois.	17209	0.032	0.189	0	3	0.030	0.429
Presenting Prob: ENT	17209	0.017	0.149	0	5	0.016	0.443
Presenting Prob: Skin	17209	0.042	0.233	0	4	0.038	0.106

Notes: Results are presented for the 2004 birth cohort, excluding babies born overseas who are unlikely to have triggered the Baby Bonus. p-values refer to null hypothesis of no difference in means between the largest estimation sample (84 days either side of 1 July 2004) and the full 2004 birth cohort or to a test of equal means of proportions in case of indicator variables (adding to the note of test of equal means). N represents the number of children.

MULTIPLE HYPOTHESIS TESTING ADJUSTMENT

We use both step-up and step-down approaches, which either control the false discovery rate — the proportion of false positives among the set of rejected hypotheses — or control the family-wise error rate, which is the probability that at least one true null hypothesis is rejected (Benjamini and Yekutieli 2001; Benjamini and Hochberg 1995; Hochberg and Benjamini 1990; Hochberg 1988). We use the STATA program – multproc – that implements these different methods. We consider an effect statistically significant if the majority of seven possible adjustment methods, including Bonferroni, step-up, and step down approaches, yield the same conclusion. We use as false discovery rate (FDR) a level of 0.05. Our conclusions are not sensitive to choosing a higher FDR of 0.10. A priori, it is not straightforward to decide which method is more appropriate without a more detailed discussion of the nature of the data and the hypotheses tested (Blakesley et al. 2009). To err on the conservative side, we consider the differences in means as statistically significant if the estimated significant remains for the majority of (four out of seven) the adjustment methods.

TABLE A2—ADJUSTED P-VALUES FOR MULTIPLE HYPOTHESIS TEST NO MEAN DIFFERENCES IN RELEVANT COVARIATES BETWEEN FULL 2004 BIRTH COHORT AND BASELINE SAMPLE (+/- 56 DAYS)

Method	Number of rejected null hypotheses	Adjusted p-value	Covariates
Holm	1	0.00093	Number of births per day
Krieger	4	0.00373	Number of births per day
			Preterm birth
			Special Nursery Care (SNC)
			Number of days in SNC
Liu 1	1	0.00097	Number of births per day
Liu 2	1	0.00094	Number of births per day
Yekutieli	1	0.00020	Number of births per day
Simes	4	0.00364	Number of births per day
			Preterm birth
			Special Nursery Care (SNC)
			Number of days in SCN
Bonferroni	1	0.00091	Number of births per day

Notes: p-value adjustment based on alpha=0.05 (critical value). We test for 55 independent hypotheses. Adjustment estimates obtained with Stata command – multproc.

TABLE A3—ADJUSTED P-VALUES FOR MULTIPLE HYPOTHESIS TEST OF NO MEAN DIFFERENCES IN PRETREATMENT VARIABLES BETWEEN TREATMENT AND CONTROL GROUPS (+/- 56 DAYS)

Method	Number of rejected null hypotheses	Adjusted p-value
Holm	0	0.00128
Krieger	0	0.00122
Liu 1	0	0.00131
Liu 2	0	0.00128
Yekutieli	0	0.00030
Simes	0	0.00128
Bonferroni	0	0.00128

Notes: p-value adjustment based on alpha=0.05 (critical value). We test for 39 independent hypotheses. Adjustment estimates obtained with Stata command – multproc.

TABLE A4—IMPACT OF THE ABB ON NUMBER OF HOSPITALIZATIONS USING A ZERO-INFLATED MODEL FOR DIFFERENT ESTIMATION SAMPLES (DAYS AROUND 1 JULY 2004)

	+/-28	+/-56	+/-28	+/-56
	Presenting problem		ICD-10 AM Chapter	
	(1)	(2)	(3)	(4)
Preventable Paediatric Hospitalizations	-.039**	-.041	-.038**	-.041
	(.019)	(.033)	(.019)	(.033)
Emergency department (ED) presentations	-.123**	-.153*	-.122**	-.153*
	(.058)	(.087)	(.058)	(.087)
Respiratory ED	-.065***	-.105***	-.046*	-.073*
	(.025)	(.038)	(.024)	(.039)
Digestive ED	-.019	-.023	-.009	-.015
	(.018)	(.027)	(.011)	(.016)
Infection ED	-.005	-.008	-.020	-.017
	(.016)	(.024)	(.018)	(.028)
Trauma, Poisoning ED	.013	.037**	.001	.016
	(.009)	(.015)	(.010)	(.016)
ENT – Oral ED	-.005	-.015	-.007	-.007
	(.008)	(.010)	(.008)	(.010)
Skin ED	-.002	-.020	-.008	-.021
	(.011)	(.016)	(.008)	(.013)
N	1,862	4,461	1,862	4,461

Notes: The coefficients are the marginal effects from the zero-inflated estimations of the dependent variables specified in the row headers on the treatment indicator, i.e. a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. All three specifications exclude births 7 days before and after 1 July 2004 as well as babies born overseas, who are unlikely to have triggered the Baby Bonus. No controls included in the first specification (+/-28 days). Controls for the date of birth and day of the week are included the specifications using +/-56 days. N represents the number of children in the sample. The data source for Preventable Paediatric Hospitalizations and Injuries is the Integrated South Australian Activity Collection (ISAAC). The data source for ED presentations to Other ED is the South Australian Emergency Department Data Collection (EDDC). Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

TABLE A5—IMPACT OF THE ABB ON NUMBER OF HOSPITALIZATIONS AT LATER AGES

	+/-28	+/-56
	(1)	(2)
PPPH age 0-1	-.040**	-.042
	(.020)	(.033)
PPPH age 1-2	-.002	-.026
	(.019)	(.030)
PPPH age 2-3	.001	.008
	(.016)	(.023)
PPPH age 3-4	-.003	-.007
	(.014)	(.021)
PPPH age 4-5	-.003	.000
	(.010)	(.016)
ED age 0-1	-.124**	-.152*
	(.059)	(.088)
ED age 1-2	-.042	-.112
	(.058)	(.089)
ED age 2-3	.035	.019
	(.046)	(.065)
ED age 3-4	.014	.020
	(.038)	(.061)
ED age 4-5	.023	.006
	(.031)	(.045)
ED respiratory (ICD-10) age 0-1	-.045*	-.074*
	(.024)	(.039)
ED respiratory (ICD-10) age 1-2	-.021	-.035
	(.024)	(.038)
ED respiratory (ICD-10) age 2-3	.019	.008
	(.019)	(.027)
ED respiratory (ICD-10) age 3-4	.030	.050*
	(.019)	(.028)
ED respiratory (ICD-10) age 4-5	.013	.011
	(.014)	(.020)
ED respiratory (upon present.) age 0-1	-.065***	-.106***
	(.025)	(.038)
ED respiratory (upon present.) age 1-2	-.015	-.035
	(.022)	(.034)
ED respiratory (upon present.) age 2-3	.012	.002
	(.018)	(.026)
ED respiratory (upon present.) age 3-4	.014	.039
	(.018)	(.026)
ED respiratory (upon present.) age 4-5	.011	.006
	(.013)	(.019)
Number of observations	1,862	4,461

Notes: The coefficients are the marginal effects from negative binomial estimation models. Each row is a separate regression model. The treatment indicator is a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. All three specifications exclude births 7 days before and after 1 July 2004 as well as babies born overseas, who are unlikely to have triggered the Baby Bonus. All models include day of the week controls, while the 56 days samples include date of birth controls (interacted with the treatment indicator). Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

TABLE A6—PLACEBO TEST

	Baseline 2004	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**
Prev. paediatric hosp.	-.042	-.016	.017	-.016	.008	-.022	-.013	-.004	-.001
	(.033)	(.028)	(.034)	(.031)	(.027)	(.027)	(.030)	(.026)	(.029)
ED visits	-.152*	.016	-.196**	.093	-.006	-.064	-.043	-.019	-.088
	(.088)	(.089)	(.090)	(.088)	(.077)	(.077)	(.082)	(.081)	(.083)
Respiratory problems									
ICD-10 Chapter X	-.074*	.017	-.050	.029	-.057*	-.042	.004	.001	-.026
	(.039)	(.036)	(.041)	(.040)	(.034)	(.035)	(.037)	(.034)	(.035)
Pres. problem	-.106***	.022	-.044	.049	-.047	-.040	.007	.039	-.019
	(.038)	(.034)	(.038)	(.039)	(.036)	(.036)	(.039)	(.036)	(.036)
Injury (trauma), poisoning									
ICD-10 Chapter XIX	.016	-.008	-.022	-.005	.016	.006	-.007	.021	-.010
	(.016)	(.016)	(.016)	(.015)	(.015)	(.014)	(.017)	(.017)	(.017)
Present. problem	.037**	.028*	-.024	.010	-.005	-.002	-.007	.024	-.019
	(.015)	(.015)	(.015)	(.014)	(.013)	(.013)	(.014)	(.016)	(.015)
Number of observations	4,461								

Notes: The coefficients are the marginal effects from negative binomial estimation models. The dependent variables are specified in the row headers. The treatment indicator is a dummy variable indicating if birth occurred on or after 1 July 2004 relative to before 1 July 2004. All three specifications exclude births 7 days before and after 1 July 2004 as well as babies born overseas, who are unlikely to have triggered the Baby Bonus. Data include all babies born within a 56 days window around 1 July 2004. All models include day of the week controls and date of birth controls (interacted with the treatment indicator). In 2006 and 2008 the Australian baby bonus payment increased by AU$1,000 on 1 July 2006 and 2008, respectively.

APPENDIX B

DESCRIPTION OF ANALYSIS OF AUXILIARY DATA

We use data from Waves 3, 4, and 5 to construct our treatment and control group. In each year, all households that are part of the Household, Income and Labor Dynamics in Australia (HILDA) Survey are asked whether a new household member has arrived or whether someone has exited. The so-called household roster describes the type of household member (e.g. new-born baby/adopted), and the month and year when the person arrived. The households are usually interviewed between September and March of the following year. By using this information from Years 2003, 2004, and 2005, we find 161 unique households in which a new-born baby arrived between January and December 2004 (Table B1).

TABLE B1—NUMBER OF NEWBORNS IN 2004 BY TREATMENT

Month	Control	Treatment	Total
January	17	0	17
February	21	0	21
March	10	0	10
April	16	0	16
May	13	0	13
June	10	0	10
July	0	10	10
August	0	17	17
September	0	16	16
October	0	11	11
November	0	12	12
December	0	8	8
Total	87	74	161

In 2005, the self-completion questionnaire of the HILDA included a module on annual household expenditures. Roughly 6 percent of the households in the treatment and control group did not complete the SCQ and only a tiny proportion of respondents refused to fill out the household expenditure component of the SCQ (usually between 3-5 individuals of our sample). There is no difference in the probability of returning a SCQ and responding to the question by treatment group status (results provided upon request). Because each eligible household member provides information on household expenditures, HILDA provides the average response of expenditure across each household. We provide mean values of these expenditures by category and control group status in Table B2. Column (3) provides the p-values of a test of equal means between treatment and control group.

TABLE B2—AVERAGE ANNUAL EXPENDITURES PER PERSON BY TREATMENT GROUP STATUS

	Control	Treatment	p-val
Panel A: Essentials			
Utilities (electricity, gas)	1533.2	1646.7	0.519
Groceries	9923.6	8985.6	0.151
Motor fuel	2178.8	2195.5	0.953
Meals eaten out	1173.6	1177.1	0.987
Clothing	1170.0	923.4	0.414
Panel B: Non-essentials			
Alcohol	1460.2	1071.1	0.13
Cigarettes	872.1	670.0	0.455
Holiday	1328.6	999.2	0.38
Hobbies, Gambling, Entertainment	602.4	634.1	0.795
Home renovation	1145.7	879.2	0.386
Alcohol expenditures>0	0.700	0.700	0.565
Cigarette expenditures >0	0.300	0.300	0.673
Holiday expenditures>0	0.700	0.600	0.414
Hobbies, games, entertainment expenditures>0	0.900	0.800	0.432
Home renovation expenditures>0	0.700	0.700	0.677
Panel C: Health			
Health expenditure > average	0.400	0.300	0.386
Health care	620.4	538.1	0.381
Private health insurance>0	0.500	0.700	0.051
Private health insurance	795.0	971.2	0.33

Note: Annual household expenditures are measured at the household. p-value refers to a t-test of equality of means between treatment and control group. Sample numbers vary by expenditure item, ranging between 121 and 127.

We are also able to demonstrate that the treatment and control groups do not differ in terms of household composition or pre-treatment disposable income (Table B3).

TABLE B3—HOUSEHOLD COMPOSITION, HOUSEHOLD DISPOSABLE INCOME, AND GOVERNMENT PAYMENTS

	Control	Treatment	p-val
Panel A: Household composition			
Number of children age 0-4	1.5	1.4	0.322
Number of children age 5-9	0.5	0.4	0.304
Number of children 10-14	0.2	0.2	0.965
Number of adults	2.1	2.1	0.467
Panel C:			
Disposable household income pre-treatment[a]	69739.8	75944.8	0.536
Panel B: Government payments[b]			
Pensions	1520.7	494.4	0.121
Parenting Payment	1707.4	1381.3	0.602
Allowances	981.2	439.2	0.173
FTB-A and FTB-B (including Baby Bonus)	3946.2	7571.8	0.000
Maternity Payments (Baby Bonus)	70.7	2959.8	0.000

[a] Disposable household (HH) income is refers to financial year 2003-2004 and thus is measured before potential treatment. [b] Government payments refer to financial year 2004-2005, and data is derived from HILDA Wave 5. The financial year in Australia operates from 1 July to 30 June each year. p-value refers to a two-sided t-test of equality of means between treatment and control groups.

The estimation results are presented in Tables B4, B5, and B6 and discussed in Section VI. Because of the small sample size, we extend acceptable significance levels to 0.15.

TABLE B4—MEAN ANNUAL HOUSEHOLD EXPENDITURES ON ESSENTIALS

	(1) Utilities (log %)	(2) Groceries (log %)	(3) Motor Fuel (log %)	(4) Meals outside (%)	(5) Clothing (%)
	Panel A: Full sample				
Treatment	0.209*	-0.016	0.070	0.101	-0.064
	(0.11)	(0.06)	(0.15)	(0.20)	(0.24)
Observations	123	127	121	127	126
	Panel B: Exclude June-July babies				
Treatment	0.174+	-0.008	0.134	0.257	0.005
	(0.12)	(0.06)	(0.15)	(0.21)	(0.27)
Observations	104	107	103	107	107

Note: Data taken from the Household, Income, and Labor Dynamics in Australia (HILDA) survey, waves 3-5. Treatment is defined as households in which a newborn arrived between July and December 2004, while the control group is defined as households where a newborn arrived between January and June 2004. Each column is a separate regression model of log of household expenditures (columns 1-3) on the treatment group indicator (0, 1). In columns 5 and 6, we use a negative binomial model to accommodate the high proportion of zeros in the outcome variable. All marginal effects are expressed in (log) percent change in annual household expenditure (divided by 100). Control variables include categorical variables for number of household members in four specific age groups (0-4; 5-9; 10-14; 15 and above). Standard errors in parentheses. Significance levels $^{+}$ $p < 0.15$, * $p < .10$, ** $p < 0.05$, *** $p < 0.01$

TABLE B5—ANNUAL HOUSEHOLD EXPENDITURES ON HEALTH CARE SERVICES AND PRIVATE HEALTH INSURANCE

	(1)	(2)	(3)	(4)
	Health care services		Private health insur.	
	Above avg. exp. (%)	Mean exp. (%)	Positive exp. (%)	Mean exp. (%)
Panel A: Full sample				
Treatment	-0.249	-0.206	0.294*	0.352
	(0.25)	(0.21)	(0.18)	(0.59)
Observations	112	124	123	126
Panel B: Exclude June-July babies				
Treatment	-0.210	-0.184	0.327*	0.554
	(0.28)	(0.22)	(0.19)	(0.63)
Observations	92	105	103	106

Note: Data taken from the Household, Income, and Labor Dynamics in Australia (HILDA) survey, waves 3-5. Treatment is defined as households in which a newborn arrived between July and December 2004, while the control group is defined as households where a newborn arrived between January and June 2004. Columns 1 and 3 report the marginal probability effects of treatment in terms of percent change (divided by 100), calculated from a probit model in which we estimate the probability of household health expenditures above the sample mean (AU$458) and positive household expenditures on private health insurance (sample mean 42 percent), respectively. Columns 2 and 4 report the marginal effects of treatment on the level of household expenditures on health and private health insurance, respectively (calculated from a negative binomial regression model to account for the skewness of the outcome data). Control variables include categorical variables for number of household members in four specific age groups (0-4; 5-9; 10-14; 15 and above). Standard errors in parentheses. Standard errors in parentheses. Significance levels $^{+}p < 0.15$, $^{*}p < .10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE B6—POSITIVE ANNUAL HOUSEHOLD EXPENDITURES ON NON-ESSENTIALS

	(1) Alcohol (%)	(2) Cigarettes (%)	(3) Holiday (%)	(4) Hobbies (%)	(5) Home (%)
			Panel A: Full sample		
Treatment	-0.054	-0.038	-0.164	-0.135^{+}	-0.094
	(0.12)	(0.31)	(0.14)	(0.09)	(0.13)
Observations	118	123	119	112	124
			Panel B: Exclude June-July babies		
Treatment	-0.064	-0.163	-0.230	-0.172^{+}	-0.096
	(0.14)	(0.33)	(0.17)	(0.10)	(0.14)
Observations	101	100	101	95	104

Note: Data taken from the Household, Income, and Labor Dynamics in Australia (HILDA) survey, waves 3-5. Treatment is defined as households in which a newborn arrived between July and December 2004, while the control group is defined as households where a newborn arrived between January and June 2004. Each column is a separate probit model of positive household expenditures on a treatment group indicator (0, 1). All marginal effects are expressed in percent change (divided by 100) in the probability of positive expenditures on non-essentials. Sample means for each outcome variable are, respectively: alcohol 72.5 percent, cigarettes 29.2 percent, holidays 66.1 percent, hobbies and entertainment 85.3, and home refurbishment 68.5 percent. Control variables include categorical variables for number of household members in four specific age groups (0-4; 5-9; 10-14; 15 and above). Standard errors in parentheses. Standard errors in parentheses. Significance levels $^{+}$ $p < 0.15$, * $p < .10$, ** $p < 0.05$, *** $p < 0.01$.