

DISCUSSION PAPER SERIES

IZA DP No. 14693

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ABSTRACT

Child Health and Parental Responses to an Unconditional Cash Transfer at Birth

We estimate the impact on child health of the unanticipated introduction of the Australian Baby Bonus, a \$3,000 one-off unconditional cash transfer at birth. Using regression discontinuity methods and linked administrative data from South Australia, we find that treated babies had fewer preventable, acute, and urgent hospital presentations—medical care available without co-payments—in the first two years of life. The payment later increased demand for elective care, which requires planning, medical referrals, and often co-payments. Our effects are strongest for disadvantaged families. Our findings suggest that up to 34% of the payout were recouped within the first year.

JEL Classification: I14, I38

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

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1 Introduction

Many governments worldwide (Australia, Québec, Singapore, Spain) have opted to pay families a one-off cash transfer for the birth of a child with the aims of alleviating the perceived financial pressures of raising a child and improving equity (McDonald, 2006*a,b*; Parr and Guest, 2011). These so-called Baby Bonuses are usually not means-tested, making them a cheap policy to administer and easy to adjust when needed. Baby Bonuses are also popular, because they are not tied to a specific expenditure category, which gives households the freedom to spend the one-off transfer according to their preferences and needs without government interference. However, this freedom also entails risks. The cash transfer may be used in ways useless or even harmful for the affected newborn child. Baby Bonuses have been evaluated along many dimensions, yet little is known about their effects on children's health outcomes.

In this study, we evaluate the impact of the Australian Baby Bonus (ABB) on children's health in the first five years of life. The ABB was an unconditional cash transfer of initially A\$3,000 (US\$2,400), a non-trivial windfall income at a time when Australia was the only OECD country other than the United States without compulsory parental pay legislation. The ABB lump sum was economically meaningful. It represented 2.5 times the weekly median disposable household income of households with a newborn in 2004 and 5.3 times the weekly disposable household income of families in the lowest income decile. Almost all eligible households claimed the payment and received it within 14 days of the birth of the child.

To identify the causal effect of the ABB, we exploit its unanticipated introduction. We compare the short- and long-term health outcomes of children born just before and after 1 July 2004, the eligibility cutoff date. The strict cut-off date allows us to use a sharp regression discontinuity approach. The analysis is conducted with high-quality, linked administrative data from the South Australian Early Childhood Data Project (ECDP), one of the most comprehensive child population-based administrative research databases worldwide (see Nuske et al., 2016, for an overview). The ECDP links, among others, population-level perinatal records (from all hospitals) with birth records, emergency department visits, inpatient and outpatient visits in all public hospitals of South Australia, and social security payments. We study outcomes that represent predominantly urgent need for health care that require hospital emergency or inpatient care. The detailed nature of our data allows us to identify the nature of the hospitalisation (diagnosis) and whether it was acute (e.g. urgent admission) or planned (referral for elective care).

We show that the introduction of the ABB in the state of South Australia was as good as random. Its introduction did not cause substantial problems of strategic birth-shifting or maternity ward bottlenecks reported elsewhere (Borra, González and Sevilla, 2016; Gans and Leigh, 2009). To make the eligibility cut-off, families may negotiate with obstetricians to postpone a scheduled

birth or pregnant mothers may delay the natural birth of the baby in other ways. This could be problematic as shifted babies, which stay longer in utero, would be heavier and thus healthier at birth. This in turn would lead to an overestimate of the benefits of the Baby Bonus. We demonstrate that strategic birth shifting was a minor concern in the South Australian context. We estimate that only 49 births were potentially shifted from the last week of June to the first week of July 2004. This shift lay within possible week-to-week variations in the number of births experienced by South Australian hospitals within any given year. With 40 maternity wards in South Australia, it implied that only every sixth hospital would have had one additional birth per day in the first week of the ABB introduction. To deal concerns about potential residual manipulation, we use data-driven methods to select the optimal bandwidth and donut (excluding potentially shifted births) around the cutoff date. All models are estimated with local linear regression models and hypothesis testing is based on robust bias-corrected inference methods, following the recent literature (Calonico, Cattaneo and Farrell, 2018, 2020; Calonico, Cattaneo and Titiunik, 2014*b*; Calonico et al., 2019; Cattaneo, Idrobo and Titiunik, 2019).

We find that the ABB had a significant impact on hospital presentations of babies in their first and second year of life. Our evidence points towards an improvement in the health status of treated babies. We find: i) a reduction in urgent and acute health problems that require hospital admissions and overnight stays, presentations that cannot be substituted by alternative forms of care (in particular respiratory infections); ii) a reduction in potentially preventable pediatric hospitalizations, in particular bronchiolitis (a respiratory health problem that usually leads to asthma in children); iii) an increase in elective hospital care which require medical referrals in the second year of life; iv) the magnitude of our effects is larger for children from disadvantaged backgrounds; and v) importantly, no robust evidence that the ABB led to more accidents. Our findings are consistent with the hypothesis that the ABB caused parents to invest in their child's health during the first two years of life, especially for families from disadvantaged backgrounds for whom the Baby Bonus produced a larger income shock.

Back-of-the-envelope calculations suggest that up to 34% of the initial payout of the Baby Bonus were recouped in the first year of life of the child. The economic gains due to reduced hospital care utilization are sizable. While our simple calculations do not account for dynamic effects of the Baby Bonus at later ages, recent findings in the medical literature suggest that early detection and prevention of respiratory problems could lead to even higher savings.¹ Thus, positive income shocks early in life may reduce the economic burden to society through medical expenditure savings in the longer run.

¹Indeed, recent evidence 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, Lemanske Jr and Gern, 2010, for an overview).

Our findings are important because Baby Bonuses as a policy tool have caused controversy. Baby Bonuses may improve children's outcomes if parents spend the bonus on "child-centered goods like books, quality day care or preschool programs, better dependent healthcare, or to move to a better neighborhood" (Dahl and Lochner, 2012, p.1931). Yet, Baby Bonuses may have unintended consequences – just like any other unconditional cash transfer (UCTs) paid to families. 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). Importantly, the Baby Bonus did not change permanent income, in contrast to many other unconditional cash transfers 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, but is designed to buffer short-term financial and emotional stressors triggered by the birth of a child.

Although there is no reliable consumption data available, we propose that the ABB was used both as indirect and direct investment in the health of the child. First, mothers may have used the additional cash transfer to postpone reentry into the labor market, which was found for the Spanish Baby Bonus (González, 2013). This would have delayed the newborn's enrollment in child care centres, a breeding ground for infections and transmissions. An alternative explanation is that households used the cash transfer for better home heating. Our experimental time period is characterized by winter months. Better heating has been associated with lower risks of respiratory infections in children with asthma (Howden-Chapman et al., 2008). Both could explain a reduction in the demand for urgent health care due to respiratory health problems. Both behaviors can be interpreted as indirect investments in the health of the child. Finally, the ABB – if used as precautionary savings, as suggested in Gaitz and Schurer (2017) – could have been used for treating a chronic condition that is not life threatening. This is consistent with our finding that in the second year of life demand for elective care, which requires long-term planning and a medical referral, increased. Both elective care and medical referrals from specialists trigger co-payments, which could have been covered with the bonus payment. We interpret demand for elective care as a direct investment in the health of the child.

Our findings contribute to an emerging literature on the effectiveness of government social assistance schemes to improve child health, human capital and well-being including Baby Bonus payments (Borra et al., 2021; Deutscher and Breunig, 2018; Gaitz and Schurer, 2017; González, 2013), earned-income tax credits (Currie and Almond, 2011; Dahl and Lochner, 2012; Duncan GJ, 2011; Hoynes, Schanzenbach and Almond, 2016; Hoynes, Miller and Simon, 2015; Milligan and Stabile, 2011), and food stamp programs (Almond, Hoynes and Schanzenbach, 2011). Baby Bonuses have been studied widely, but most of the previous work has focused on the unintended consequences of their introduction (Gans and Leigh, 2009) or their cancellation (Borra, González and Sevilla, 2016,

2019), or both (González and Trommlerová, 2021). Borra, González and Sevilla (2019) provide the first and only evidence that birth shifting induced by the cancellation of the Baby Bonus in Spain led to worse health outcomes for shifted infants. Surprisingly little attention has been paid to evaluating the “cash component” effects of Baby Bonus payments on human and health capital (two exceptions are Borra et al., 2021; Gaitz and Schurer, 2017). Our finding that children in their first years of life experience fewer acute health problems due to respiratory disease is new evidence that Baby Bonus payments may not be wasted investment.

Our findings are also relevant in the context of a broader literature that investigates the causal impact of household material resources and children’s health outcomes (Almond, Currie and Duque, 2018; Case, Lubotsky and Paxson, 2002; Case, Lee and Paxson, 2008; Cesarini et al., 2016; Currie, Shields and Price, 2007; Currie, 2009; Currie and Almond, 2011; Currie and Stabile, 2003; Kuehnle, 2014; Propper, Rigg and Burgess, 2007; Yeung, Linver and Brooks-Gunn, 2002). Identifying the causal impact of household income on children’s health has been difficult because few compelling randomization experiments exist, with the exception of the “Baby’s First Years” randomized control trial, which is still in the field-work stage.² Previous solid evaluations exploited lottery winnings in combination with sophisticated linked administrative data (Cesarini et al., 2016) or used quasi-experimental approaches in combination with survey data (Kuehnle, 2014).

2 Related Literature

Baby bonuses are an extremely useful policy tool: they can be administered easily through the tax and transfer system; they can be implemented quickly when needed and cancelled during times of fiscal austerity. Their one-off nature does not change permanent income. This has the advantage that they do not change lifecycle consumption, saving, and investment paths. All they do is take away immediate financial pressures associated with the birth of a child. This characteristic differentiates Baby Bonuses from other cash transfers that are more permanent in nature, such as the US American Earned Income Tax Credit (EITC) or the Canada Child Tax Benefit (CCTB).³

²The RCT Baby’s First Years is the first causal study to test the connections between poverty reduction and brain development among very young children. Since 2018, the trial recruited one thousand low-income mothers and their newborns in several ethnically and geographically diverse communities. Mothers receive either (1) US\$333 each month (or \$4,000 each year), or (2) \$20 each month (\$240 each year), for the first 40 months of the children’s lives with the first payments occurring shortly after the baby’s birth. First study results will be available after the trial is concluded in 2020.

³THE EITC provides substantial support to low- and moderate-income working parents. It is a financial incentive for parents to work. Families with one child can obtain a maximum credit of up to \$3,584, while the maximum credit for families with three or more children is \$6,660 in 2020. 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 behaviors

Baby bonuses have the disadvantage that their spending is not tied to a specific category, and so they may be wasted, misspent or harmfully spent from the perspective of the child.

Most of what we know about Baby Bonus policies comes from the Spanish and the Australian experience. The main insights from the previous literature is that Baby Bonuses can increase fertility (González, 2013; González and Trommlerová, 2021; Sinclair, Boymal and De Silva, 2012) and childbearing intentions of women from lower-income households (Risse, 2010), a key objective of Baby Bonuses. The introduction of the Spanish Baby Bonus has been shown to enable mothers to stay home longer after the birth of a child (González, 2013). Baby Bonuses seem to have little to no longer-term impacts on children's human capital formation (Borra et al., 2021; Deutscher and Breunig, 2018; Gaitz and Schurer, 2017).

Most previous work focuses on the unintended consequences of the introduction (Gans and Leigh, 2009) or the cancellation of Baby Bonuses (Borra, González and Sevilla, 2016, 2019). González and Trommlerová (2021) focus on both. The announcement of a birth-cut off date for eligibility to the bonus creates incentives for parents to shift the birth date of a baby in utero, potentially harming the unborn child. An early study of the Australian Baby Bonus demonstrated that overall 1,000 births may have been postponed to after the 1 July 2004 cut-off date, which represents less than 0.4 percent of all births in Australia in the year 2004, or 2 percent of all June-July births. Similar findings have been produced for Spain. Borra, González and Sevilla (2019) and Borra, González and Sevilla (2016) estimate that the cancellation of the Baby Bonus led to 2,000 births potentially shifted forward from January to December, amounting to 6 percent of all January 2011 births. These studies argue that the shift did not happen due to an increase in C-Sections, but occurred to full-term babies and babies that weighed at least 2,500 grams.

Gans and Leigh (2009) find that babies born in the first 1-2 weeks following the introduction of the Baby Bonus were 75g heavier at birth and were 3% more likely to weigh more than 4,000 gram. There was no statistically significant effect of the ABB on infant mortality. They argued that the fall in births in June was due to a fall in Cesarean section and induction, while half of the increase in July was due to C-Section. The authors state that: "we have identified a very significant disruption to normal operating procedures for maternity hospitals and staff in Australia. This disruption appeared to impact both planned and unplanned birth procedures" (p. 263). To understand whether the number of shifted birth led to significant disruptions, it is worthwhile to put this number into perspective. We calculated that if all births were shifted in the first week

(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. The CCTB is also paid to families with children, but eligibility does not depend on work status of the carer. Families could claim up to CAN\$1,228 per child per annum in the mid-2000s. Milligan and Stabile (2011) find that a 1,000 benefit increase paid to families with dependent children aged 0-17 affected mainly educational outcomes and mental health but had no impact on child physical health. For a full review of this literature, see (see Currie and Almond, 2011, for a review).

of July 2004, then this would have implied 143 additional births per day. Overall, there are 550 maternity wards in Australia at the time of the Baby Bonus introduction. If every ward had shown the same likelihood of shifting a birth from June to July, then this would have meant that every fourth hospital would have recorded one additional birth per day in the first week of July, which can hardly be interpreted as a significant disruption.

González (2013) shows that the introduction of the Spanish Baby Bonus had positive impacts on fertility. Parts of this fertility increase were explained by a reduction in abortions. The bonus helped eligible mothers to stay longer out of the labour force after birth. Borra, González and Sevilla (2016) shows that the cancellation of the Spanish Baby Bonus led to an increase in infant hospitalisations. Borra, González and Sevilla (2019), which extends Borra, González and Sevilla (2016), shows the negative consequences of the cancellation of the Baby Bonus on specific health diagnoses using ICD-10 classifications. González and Trommlerová (2021) test for symmetry in the transitory effects of the introduction and cancellations of the Baby Bonus (mainly because of a short term decrease in abortions), and show that the impact on fertility of cancelling the payment was stronger in magnitude than the introduction of the payment.

Most previous papers are thus concerned with the cancellation of the Baby Bonus, which may have led to a series of unintended consequences. A significant number of babies were harmed by a potentially forced, premature birth when parents attempted to shift their baby's birth to an earlier date, to be eligible to the Baby Bonus. In this previous literature, the main treatment is not the reduction in income, but the early life health shock induced by birth shifting that resulted from fear of losing the cash transfer.

We argue that one-off government transfers should be evaluated with the child's welfare being at the centre of the analysis. Cash transfers can impact child health through two channels (Mayer, 1997; Milligan and Stabile, 2011; Yeung, Linver and Brooks-Gunn, 2002): the "resources channel", which is the direct impact of additional income which allows carers to purchase more goods and services, and the "family process channel", which is the indirect impact of income on psychological well-being of the family.

Children benefit directly through more income when parents use additional household resources to purchase child-centred goods (see Dahl and Lochner, 2012). In the context of child health in the first five years of life such goods would involve high-quality health care, day care, food, shelter and clothing (Milligan and Stabile, 2011). The direct income channel has been widely tested in previous work (e.g. Case, Lubotsky and Paxson, 2002; Case, Lee and Paxson, 2008; Cesarini et al., 2016; Currie, Shields and Price, 2007; Currie, 2009; Currie and Almond, 2011; Currie and Stabile, 2003; Kuehnle, 2014; Mayer, 1997; Milligan and Stabile, 2011; Propper, Rigg and Burgess, 2007; Yeung, Linver and Brooks-Gunn, 2002). 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) between 1986 and 1994 leads to a significant 19% increase in two- and five-year hospitalization rates of children after the lottery win. Similar effects are found for hospitalisations due to respiratory illness and external causes, although those estimates are not statistically significant. No effects were found for adults. It is hard to understand why large exogenous increases in household income would lead to increased hospitalisations for children for respiratory illness in a country with universal health coverage. 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 finding is in stark contrast to Kuehnle (2014), who exploits 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.

The family-process channel has also been tested in the literature. Income shocks may improve children's 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 (2019) 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 international literature from the perspective that Baby Bonus cash transfers are different to lottery wins or permanent increases in household income such as the EITC or the CCTB. They are emergency cash injections into households that can be easily administered and that are unlikely to change long-term consumption and saving behaviors. We produce causal estimate of the impact of such a Baby Bonus on children's early-life health outcomes in Australia, a country with universal access to health care and a high standard of living. We can answer the question of whether the ABB affected children's health outcomes early in life, shortly after the bonus was paid out, which aspects of health were affected and within which time frame. Our data allow us to study the mechanisms that explain the impact. We distinguish between areas of health that can be affected indirectly by a windfall cash payment (e.g. money spent on alcohol, which increases trauma presentations at the emergency department) or directly (e.g. money spent on extra

⁴Furthermore, 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.

health care to treat a pre-existing conditions which would increase elective outpatient care demand for services not fully covered by universal health care).

3 Institutional Background

The Australian Baby Bonus (ABB) was an A\$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 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. 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 program underwent important structural changes, which included subsequent increases to A\$4,000 and A\$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 A\$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

⁵The 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 in response to the announcement could not have been born before February 2005, assuming full-term gestation of 37 weeks and over.

⁶Own 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 A\$61,663 (or A\$1,186 per week). The mean household disposable income for households in the bottom decile of the income distribution was A\$29,661 (or A\$570 per week). The sample comprises 142 out of 161 households which had a newborn in 2004 and were interviewed in Wave 4 of HILDA.

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 A\$3,000 for all households (Deutscher and Breunig, 2018). The Maternity Allowance was a subsidy of A\$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. Our own calculations using social security payments confirm previous findings that almost all eligible households (94%) received the payment (Drago et al., 2011). The median household received the payment within 14 days of the birth of the child, while 90% received it within 49 days. Thus, the payment was immediately effective. Providing the same level of help to all parents, the policy was more favorable to low- and middle-income households. According to Deutscher and Breunig (2018), 75 percent of births in June 2004 would have been better off under the new policy.

The effect of the ABB on children's health outcomes must be understood in the context of Australia's health care system and its funding arrangements. Australia is a healthy, rich and highly developed country with an advanced health care system that ranks high amongst OECD countries. Average life expectancy is high (82.6 years) and infant mortality (0.33%) is low in comparison to other OECD countries (OECD 2019). Australia has universal health insurance, where 100% of the resident population has access to core services and medication. The Medicare program, implemented in 1984, is tax-funded. It has three major parts: medical services, public hospitals and medicines. It covers the expenses of public hospital services (free treatment for patients in public hospitals) and visits to general physicians. The 'Pharmaceutical Benefits Scheme' provides subsidies for a variety of prescription medicines. Hence, the fundamental structure of the hospital and medical services has been established in a way to provide essential healthcare services to all Australians without experiencing financial hardship (Rana, Alam and Gow, 2020). Dental or

other ancillary services are not covered. Children are fully covered under Medicare as well, and receive additional free services regarding dental care, immunization, disability, autism, and vision impairment.⁷

4 Data

4.1 The South Australian Early Childhood Data Project

We conduct the analysis with 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, for details).

Birth and Perinatal Data We obtain birth-related data from the Born Population dataset, a merge of the Births Register and The South Australian Perinatal Statistics Collection covering the universe of children born in South Australia between 1991 and 2016. Available variables include date of birth, gestation length at birth, child sex, birth weight and several indicators of the child's health at birth such as APGAR scores and admissions to neonatal intensive care units. The data also contains detailed demographic characteristics of mothers, fathers and children, as well as detailed pregnancy histories of mothers including maternal gestational health, smoking behavior during pregnancy, past pregnancies. These data are primarily sources from the Perinatal Statistics Collection and supplemented and validated by Births Registry data (Nuske et al., 2016).

Hospital records: inpatient services and emergency department admissions Health outcome measures are derived from the Integrated South Australian Activity Collection (ISAAC) and the South Australian Emergency Department Data Collection (EDDC). The ISAAC data covers the universe of admissions to inpatient services in public hospitals from July 2001 to 2014. The EDDC data covers the universe of admissions to emergency departments in public hospitals from July 2003 to 2014 (Nuske et al., 2016; South Australian Emergency Department, 2014).

ISAAC and EDDC data contain details about each patient's admission, including their mode of transport to the hospital, whether they came with a referral, whether the visit was planned,

⁷See <https://www.servicesaustralia.gov.au/individuals/subjects/whos-covered-medicare/childrens-health-care> for more details. Australia also has a market for private health insurance. Individuals are encouraged through the tax system and premium rebates by the Government to purchase private health insurance. The main aim of public subsidies to purchase health insurance is to relieve pressure from the overburdened public hospital system, an aim that is generally accepted as not having been achieved (Rana, Alam and Gow, 2020).

whether it is a first admission, the severity of the patient's condition as assessed by a triage nurse, and diagnosis code(s) and other clinical indicators associated with the admission, length of stay and the nature of the separation (discharge, admission, transfer, death). The data are collected by hospital staff and updated at the time of hospital separation.

4.2 Are public hospital records sufficient to study child health and parental health investments?

An ideal set of health measures would combine health status measures recorded by different health care providers (pediatricians versus nurse practitioners and primary versus specialty care) and in different health settings (such as hospitals or ambulatory care settings) (of Medicine and Council, 2011). Using hospital records alone to assess the health of young children has both advantages and disadvantages.

As we rely on outcome measures of child health derived from hospital emergency and inpatient data, we could potentially be missing out on important aspects of child health. In Australia, only 15% of all child consultations take place in hospitals and predominantly in the first two years of life (Hayes et al., 2019). This means that over 85% of all health care consultations would take place in the primary care or community care sectors. Hospital emergency and inpatient data thus account only for a fraction of the total care that children receive in their first years of life.

By using hospital emergency data, our analysis focuses on acute and potentially acute health conditions. In South Australia, as much as elsewhere in Australia, parents are advised to take their babies or children to emergency departments if they become ill suddenly, or if they had an accident such as an assault, fall or burn, poisoning, allergic reactions, broken bones or breathing problems.⁸ By using hospital inpatient records, our analysis also capture elective care, as parents may be referred from a specialist for an inpatient or outpatient service.

A clear advantage of these data is that we can measure accurately a child's health status (disease, impairment, injury, and symptoms) through extensive testing, screening and medical diagnosis. Such accuracy avoids measurement error inherent in general practitioner or parental assessments. Another advantage is that we focus on health conditions that may have severe long-term consequences if left untreated. Thus, we focus on illnesses and injuries with the greatest burden of disease, which are of major policy relevance. In our South Australian population data, the most common diagnosis for children seeking both emergency and inpatient care are respiratory problems (27% in emergency care and 19% in inpatient care).

One could argue that changes in the demand of hospital presentations may reflect a substitution

⁸See <https://www.healthdirect.gov.au/hospital-emergency-departments> for more details.

effect. Substitution effects may occur where additional financial resources could be used to purchase more appropriate but more costly care (e.g. primary care). For instance, free emergency care may be used as substitute for costly primary care. This is not a relevant argument in the Australian health care setting, as both public hospital emergency care and primary care can be accessed without co-payments. Thus, cash transfers are not expected to lead to substitution effects. Substitution effects could also be a concern for inpatient care, depending on whether the inpatient service is the result of admission from emergency care, or admission by referral, which is a scheduled visit. If it is the former, again no costs would arise for the child's family. Thus, we would not expect the Baby Bonus to increase demand for inpatient services that are due to acute conditions.

This is different for planned inpatient services and in particular for elective care. The pathway to elective hospital care goes through a general practitioner (which can be free of charge), who refers the patient to a specialist, who then refers the patient to a hospital service. Specialist fees are usually not fully covered by Medicare. On average, patients pay two thirds out of pocket, and this is true among pediatricians (Freed and Allen, 2018). In South Australia, patients pay roughly 50% of the scheduled fee for pediatric outpatient service (Freed and Allen, 2018). The median initial consultation doctor fee for pediatric consultations is A\$320, and in South Australia A\$263 (based on 2014 data, the only data available). Thus, in South Australia families pay for each visit around A\$130 out-of-pocket. Thus, it is possible that the Baby Bonus directly affects the demand for elective inpatient procedures. In this case, we would expect the Baby Bonus to have to increase demand for such services.

Thus, any effect of the Baby Bonus on the demand for hospital emergency care for infants must be the result of indirect effects through consumption (e.g. better food), investment in the child's health through other means (e.g. house renovation), or a reduction in household stress (e.g. less immediate stress to return to the labour market).

Table 1: Pathways into Hospital Care and Related Costs

Type of Hospital Care (1)	GP (2)	Specialist (3)	Health Investment (4)	Costs (5)	Can the ABB Affect Directly? (6)
Emergency Inpatient services	No	No	No	No	No
admitted from ED	No	No	No	No	No
Elective procedure	Yes	Yes	Yes	Yes	Yes
PPPH	Yes	Yes	Yes	Yes	Yes

Note: GP refers to general practitioner, ED refers to Emergency Department, PPPH refers to potentially preventable pediatric hospitalizations.

Another concern could be that we only use public hospital data. We argue that this will not invalidate our findings. Private hospitals are less prevalent in South Australia than in other Australian states, where almost one in two hospitals are private (695 public versus 630 private hospitals). In 2004, South Australia had 99 hospitals, of which 76 were public and 23 private.⁹ Figure 1 shows that all private hospitals are located in the Greater Adelaide region, mainly in economically advantaged areas (Councils of Calvary Wakefield, Ashford, and St Andrews) and mainly in the vicinity of a public hospital. In five out of the 23 private hospitals, emergency department admissions are shared between private and public hospitals. This means patients always have the opportunity to seek care even in the absence of private health insurance.

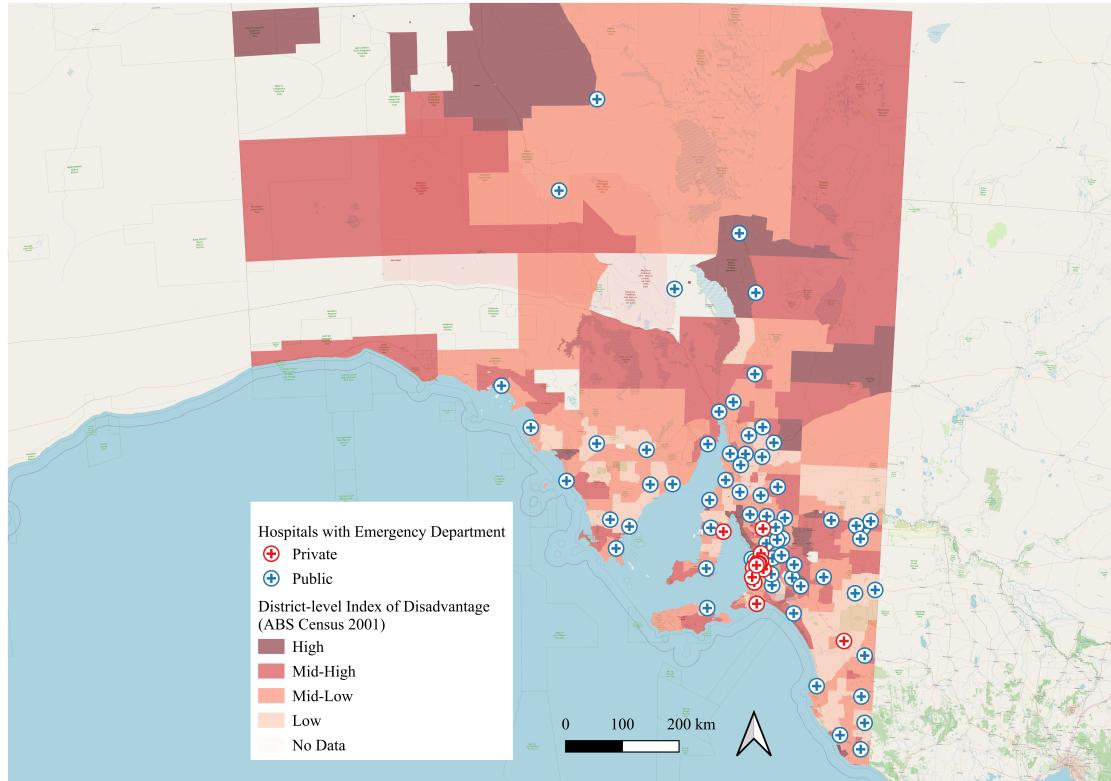
Thus, our analysis would miss out mainly on elective surgery and rehabilitation services for adult patients (2/3 elective surgeries and 80% of rehabilitation care are performed in private hospitals).

More importantly, young children are rarely treated in private hospitals (see Australian Institute of Health and Welfare, 2017, for reported statistics (Short AIHW 2017)). Overall, only one in seven children (15%) aged 0-4 will be treated in a private hospital as a private patient overall in Australia. Emergency care for children is almost exclusively provided in public hospitals. Around the time the Baby Bonus was implemented, private patient infants (age 0-4) made up around 1.5% of all hospital separations (AIHW 2017, Figure 4.2). There was not a single child with private health insurance (PHI) admitted to a private hospital for emergency surgery in this age-group (AIHW 2017, Figure 7.1). Around 1.5% of all hospital separations for medical care emergencies occurred in private hospitals, while over five times as many (8%) were treated in the public hospital sector in this age group (AIHW 2017, Figure 8.1). Other acute care hospitals admissions funded by PHI occurred almost never in private hospitals for children (<1% of all hospital separations), but about 7% of all hospital separations occurred in public hospitals (AIHW 2017, Figure 9.1). It is slightly more common to see children funded by PHI treated in private hospitals for non-emergency care such as non-emergency medical care or surgery (Around 2.5% of all separations in both cases), while between twice (4.3%) to three times (7.2%) as many were treated in public hospitals for non-emergency medical care and surgery, respectively (see AIHW 2017, Figure 8.2 and Figure 7.2, respectively).

Thus, the absence of private hospital data from South Australia will not affect our conclusions about the effects of the Baby Bonus for emergency care, and may only marginally affect our conclusions about elective care.

⁹For a complete list of hospitals in South Australia, see <https://data.sa.gov.au/data/dataset/sa-health-hospitals-locations> and <https://www.myhospitals.gov.au/browse-hospitals/sa/greater-adelaide/adelaide>. Today, South Australia has 100 hospitals.

Figure 1: Location of Public and Private Hospitals by District Level of Disadvantage in South Australia



Note: This map presents the universe of hospitals in South Australia with an Emergency Department, classified by district-level disadvantage measured in the 2001 Census. Public hospitals are observable in the Early Childhood Development Project data, and are shown with a blue hospital cross. Private hospitals are not in ECDP data, and are shown with a red hospital cross. Data source: SA Health, ABS Census 2001.

4.3 Outcomes and Variables of Interest

We construct several measures of child health and parental investments in child health, using children's hospital records. First, we construct dummy variables to capture whether a child used any hospital service, i.e. whether a child has any record in either or both the ISAAC and the EDDC data. We also construct a count variable for the number of admission records, and the age of first admission.

Second, we construct three outcomes associated with the severity and acuteness of a health problem: 1) having a record for a presentation qualified as an emergency by a triage nurse, in either dataset and both datasets; 2) having a record for a presentation which led to a hospital admission to an intensive care unit or to a ward, or transferred to another hospital or another service, and 3) having a record for a presentation which led to an overnight admission.

Third, we construct three outcomes associated with high parental investments in child health: 1) having a record for a planned or booked visit, 2) having a record for a presentation with referral

from a medical professional for specialist care, 3) having a record for an elective intervention (only in the inpatient services records).

Fourth, we also construct a unique measure of “potentially preventable pediatric hospitalization” based on the Potentially Avoidable Hospitalization (PAH) tool developed in New Zealand specifically for a pediatric population. In this tool, the definition of what is avoidable is based on a broad spectrum of factors influencing health from government policies and population-based health measures to appropriate access to primary care (see Anderson et al., 2012, for description). For instance, a child could be admitted for bronchiolitis, the first cause for emergency department visits for babies in their first year of life, but they could be admitted for a “potentially preventable” bronchiolitis depending on the severity of the symptoms.

Fifth and lastly, we characterize in detail the problem group for which children are admitted to hospitals, which is also indicative of parental investments in child health. We use the diagnosis codes available in the ISAAC data following the Australian modification of the International Statistical Classification of Diseases and Related Health Problems (ICD-10-AM), which we code at the level of the broad categories of the classification.¹⁰ The Emergency Department data allow us to distinguish admissions by both presenting problem and diagnostic group, which are provided by two different sources.

Problem groups are assigned upon presentation by a medical officer (e.g. a triage nurse) who classifies the patient according to the presenting problem (for instance respiratory, head trauma, etc.). Presenting problems are coded as broad categories that are consistent with diagnostic sub-categories based on the ICD-10-AM. Diagnosis codes are recorded by a medical officer upon separation, and appear in the hospital separation files according to which the hospital will be reimbursed. Each diagnosis is coded according to the ICD-10-AM. 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 infant which include in this order i) respiratory problems, ii) digestive problems, iii) infections, iv) skin problems, v) injuries, trauma and poisoning, and vi) externally caused health problems (generally accidents). We also include perinatal problems and clinical abnormalities as placebo outcomes, since these types of issues are entirely reimbursed by Medicare and should therefore not occur more or less often following the introduction of the Baby Bonus.

4.4 Summary Statistics

Table 2 presents summary statistics on our main outcome variables. Out of 35,236 babies born between 1 July 2003 and 1 July 2005, 45% of children have at least one presentation within their

¹⁰See International Classification of Diseases, 10th edition, Australian Modification (ICD-10-AM 10th Edition)

first year of life, 32% have at least one presentation at the ED, and 30% have at least one inpatient service. One in five children have at least one presentation to the ED for an urgent or acute problem, one in eight have a presentation which led to a hospital admission, and of these one in five stay overnight. Other types of visits are rarer: planned visits (1.7% at EDs and 2.4% at inpatient services), visits with a medical referral (5% at EDs and 9% at inpatient services) and visits for an elective procedure (5.7% in inpatient services). Overall, more than one in five children had a presentation that was potentially preventable, amounting to 19% of ED presentations and 9.4% of inpatient service presentations.¹¹

We present in Figure B.1 the distributions of hospital care utilization (potentially preventable pediatric hospitalizations, emergency department presentations) in the first year of life. We observe a high proportion of children with zero presentation, ranging from about 70 percent for our aggregate measure of emergency department presentations in Fig. B.1a (11,877 observations) to 90 percent for potentially preventable pediatric hospitalizations in Fig. B.1b (13,541 observations). We also show that the distribution of emergency department presentations for respiratory problems, that results from the discharge records (Fig. B.1c), is very similar to the distribution of emergency department presentations for respiratory problems that results from the records upon presentation (Fig. B.1d).

¹¹One could be concerned that the babies we observe in their first five years of their lives may be a non-random draw of the population, as some families may be internal or international migrants. According to the Australian Bureau of Statistics, in 2004 only 2,060 children aged 0 to 4 departed from South Australia. Under the assumption that this rate of departure applies uniformly, this would imply that out of the entire cohort of 17,200 babies born in 2004, we would predict that 412 would have left the state (2.4 percent, amounting to slightly more than one baby per day).¹² This can be considered a small number. Out-of-state migration would only pose a problem for statistical inference if outer-state migration is linked to the infant's health status, i.e. the unhealthiest babies leaving the state as a consequence of the Baby Bonus. This could happen if babies need specialist care that is not offered in South Australia. This is not likely to occur as South Australia offers all health care services and has a specialised Women and Children's public hospital.

Table 2: Summary Statistics of Hospitalizations at Ages 0-1

	Mean	Sd
Share of children with records before age 1:		
Hospital presentations:		
Any presentation, either ED or inpatient services	0.455	0.073
Any Emergency Department presentation	0.317	0.067
Any inpatient record	0.301	0.070
Presentation for urgent/acute problems:		
<i>Emergency Department:</i>		
Any presentation for urgent/acute problem	0.205	0.058
Any presentation with admission to ward	0.123	0.047
<i>Inpatient Services:</i>		
Any presentation for urgent/acute problem	0.157	0.053
Any presentation with admission to ward	0.025	0.025
Any presentation with overnight admission	0.195	0.062
Planned visits or presentations with medical referral:		
<i>Emergency Department:</i>		
Any planned visit	0.017	0.019
Any visit with medical referral	0.050	0.033
<i>Inpatient Services:</i>		
Any planned visit	0.024	0.022
Any visit with medical referral	0.089	0.042
Any visit for an elective intervention	0.057	0.034
Potentially Preventable Pediatric Hospital Presentations:		
Any potentially preventable presentation	0.221	0.059
Any potentially preventable presentation, ED	0.187	0.056
Any potentially preventable presentation, inpatient services	0.094	0.044

Note: This table presents descriptive statistics of the main outcome variables. The sample includes children with a birth record in South Australia, who were born in South Australia between 1 July 2003 and 1 July 2005. For all variables, the sample contains 35,236 observations.

5 Empirical Strategy

To evaluate the causal impact of the ABB on health outcomes of children in their first years of life, we use the sharp change in eligibility to the ABB based on dates of birth, and compare health outcomes of children born just before versus after 1 July 2004. We estimate the following equation, for a child i with health Y_i upon reaching age 1 (or age 2, 3, 4 or 5):

$$Y_i = \alpha + \beta D_i + \gamma g(R_i) + \varepsilon_i, \quad (1)$$

where D_i is a dummy variable taking value 1 if the child is born on or after 1 July 2004, and 0 otherwise; R_i is the running variable corresponding to the child's date of birth centred around the cutoff date, and $g(\cdot)$ is a linear function of the child's date of birth. Thus, β is our parameter of interest capturing the difference in health outcome Y_i between treated and control babies.

We estimate Eq. 1 using local linear estimation and robust bias-corrected inference methods, with CER-optimal bandwidth (Calonico, Cattaneo and Farrell, 2018, 2020; Calonico, Cattaneo and Titiunik, 2014b; Calonico et al., 2019). We choose local linear estimation with a triangular kernel to give more weight to observations closest to the cutoff date, following Gelman and Imbens (2019) who warn against the use of global high order polynomials. They argue that global high-order polynomials often give too much weight to observations away from the cutoff, which can bias estimates at the cutoff. We prefer robust bias-corrected inference over conventional inference methods, which are not robust to bias arising from non-linear conditional expectation functions of outcomes near the cutoff (Calonico, Cattaneo and Titiunik, 2014b). We choose the CER-optimal bandwidth following Calonico, Cattaneo and Farrell (2020), who demonstrate that the CER-optimal bandwidth is superior to the MSE-optimal bandwidth, which is invalid for inference purposes. Lastly, we cluster standard errors at the level of birth dates following Bartalotti and Brummet (2017).¹³

Recent theoretical developments in the literature on regression discontinuity designs recommend using data-driven methods relying on non-parametric estimation techniques (see in particular Cattaneo, Idrobo and Titiunik, 2019). One central reason why the methodological discussion on regression discontinuity designs is converging towards data-driven methods, is to allow optimal bandwidth selection and prevent bias in local estimates at the cutoff driven by observations that are further away from the discontinuity.

¹³We use the `rdrobust` Stata command (Calonico, Cattaneo and Titiunik, 2014a; Calonico et al., 2017, January 2020 update).

5.1 Validity of the regression discontinuity design

The introduction of the ABB on 1 July 2004 naturally lends itself to a RD research design for three reasons. First, the cutoff differentiates children born before 1 July 2004 whose parents do not receive any maternity bonus, from children born from 1 July 2004 whose parents unconditionally receive the Baby Bonus amounting to A\$3000; the RD is sharp in the sense that compliance with the treatment assignment is close to perfect; no eligible families based on their child's date of birth did not receive the payment (see Deutscher and Breunig, 2018, for a detailed discussion on the distribution of the payment using tax data). Second, the ABB was announced on 12 May 2004, only seven weeks prior to introduction, which limits the scope for manipulation of families into control and treatment groups. The announcement could not lead to immediate fertility effects at the cutoff. We show below that the ABB did not increase or decrease selective abortions either. Our data-driven bandwidth selection excludes children born around February 2005, which would be the first babies potentially conceived as a result of the announcement of the ABB in May 2004. Third, for women expecting to deliver their child around 1 July 2004, manipulating the exact delivery date is relatively complicated and dangerous for both the child's and the mother's health so we expect limited manipulation in child date of birth.

Our regression discontinuity design is valid if there is no manipulation in the running variable determining assignment to treatment and control groups, and no significant difference between control and treatment units at baseline. Thus, our main parameter of interest, β yields a causal estimate of the true effect of the Baby Bonus on the outcomes of babies just after versus just before the cutoff date if the two identification assumptions hold. We test both in the following.

5.1.1 Selective birth shifting

Previous studies have suggested that Baby Bonus policies create incentives for parents to shift their child's birth (see in particular Borra, González and Sevilla, 2016, 2019; Gans and Leigh, 2009). In the Australian context, Gans and Leigh (2009) show that approximately 1,000 births were potentially shifted. The shift was highly concentrated in the days immediately surrounding the implementation cutoff of 1 July 2004. As protocols governing maternity wards differ by states, we replicated the analysis of Gans and Leigh (2009) using South Australian birth records from 1991 to 2005 (see [Appendix A](#)). We show that birth-shifting occurred also within a seven-day window in South Australia. We calculate that 49 births were potentially shifted from the last week of June to the first week of July, corresponding to 14% of all births expected in the last week of June, or about 2 standard deviations of the average weekly birth variation that SA maternity wards have experienced in the past five years. With 40 maternity wards in South Australia, this means that about every sixth maternity ward would have had one additional birth per day. Although this

cannot be considered a substantial disruption of daily processes in maternity wards, as speculated in Gans and Leigh (2009), this does however suggest that shifted babies – now in the treatment group – were potentially healthier than non-shifted babies that are still in the control group.

To deal with selection into treatment by birth shifting, we use a "donut" regression discontinuity design that excludes potentially-shifted births around 1 July 2004. To choose the ideal donut size, we adapt the optimal-bandwidth selection method of Calonico, Cattaneo and Farrell (2020) to our data, to ensure that i) newborns and parents in the control and treatment groups are comparable based on pre-treatment observable characteristics, and that ii) the density of daily births is not statistically different before versus after the cutoff date. Using such methods, we conclude that a seven-day donut fully balances pre-treatment characteristics between treatment and control group and ensures a smooth density of births across the cut-off date. For more details, see [Appendix A](#).

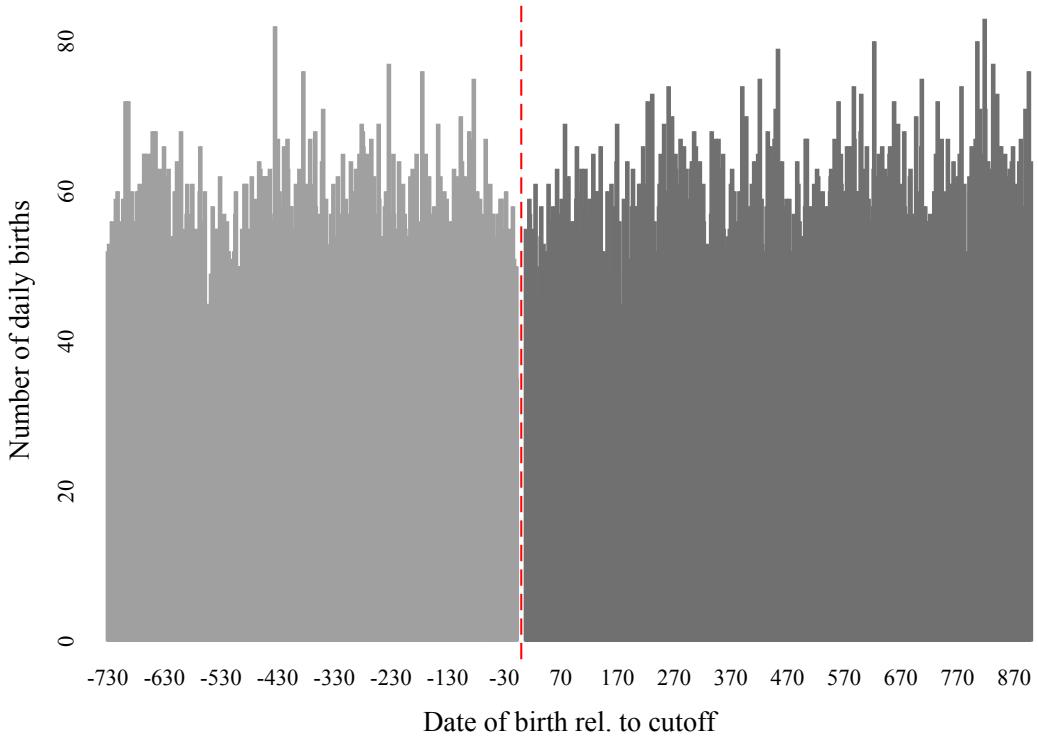
The seven-day "donut" is not only consistent with empirical evidence of birth-shifting, but also with the day-of-the-week variation in births. As hospitals tend to schedule planned birth deliveries during weekdays, there are always more deliveries on weekdays than on weekends. Weekend deliveries tend to be natural births, whereas weekday deliveries are a mix of natural and scheduled (C-section, induction) births. Thus, a donut of less than seven days would have caused a spurious imbalance on pre-determined observable characteristics between treatment and control groups.

We find strong evidence that our research design using a seven-day "donut" regression discontinuity approach is valid. Figure 2 presents graphical evidence that, after exclusion of births within seven days of 1 July 2004, there is no obvious change in the distribution of the number of daily births over the remainder of the year 2004.

Table 3 shows that the running variable is smoothly distributed at the cutoff date. This table presents the results of the local polynomial density test proposed by Cattaneo, Jansson and Ma (2018, 2020), which compares the distribution of the running variable to a Gaussian approximation, a density which we expect to be smooth at the cutoff date. The sample used is the universe of children born in South Australia between 1 July 2003 and 1 July 2005, excluding 93 children born abroad during this time, and all children born within seven days of 1 July 2004, the date of the introduction of the Australian Baby Bonus. Col. 1 indicates the local polynomial fit method and the bandwidth estimation method. Col. 2 and 3 indicate the estimated parameter for the CER-optimal bandwidth, and col. 4 and 5 indicate the number of observations used in the test on either side of the cutoff. Col. 6 presents the p-value of each density test. Across all three density tests, we cannot reject the null that there is not discontinuity in the running variable at the cutoff date, that is, that the running variable seems smoothly distributed at the cutoff date. Thus, we find no evidence of manipulation in the data based on Table 3.

Table B.1 provides additional evidence that our data is consistent with an absence of manipulation in dates of birth. Using the same sample as for Table 3, we run 100 nested binomial density

Figure 2: Number of daily births



Note: This figure presents a histogram of daily births around 1 July 2004, the cutoff date marking the introduction of the Australian Baby Bonus. Each bar corresponds to a date of birth. The sample underlying this figure excludes all births within 7 days around 1 July 2004, and is chosen by the local polynomial density test of Cattaneo, Jansson and Ma (2018, 2020), implemented in Stata using the command `rddensity` (January 2020 update).

tests, an alternative type of density test that is used for the local randomization regression discontinuity approach. For this alternative approach under more stringent assumptions, the binomial density test assesses whether the distribution of the running variable on both sides of the discontinuity is comparable to a binomial distribution of mean 0.5 in increasingly large windows around the cutoff date. We thus start by comparing the number of births eight days before versus eight days after the cutoff date, until 107 days before versus after the cutoff date. For simplicity, we only display a summary of our findings: across 100 nested tests, we rejected the null at the 10 percent level only three times, and at the 5 percent level only once. Thus, Tables 3 and B.1 together show strong evidence that, once we exclude births within seven days of the cutoff date, the data are consistent with an absence of manipulation in birth dates.

One could still be concerned about selection bias arising from residual shifted births beyond our seven-day donut. The main endogeneity concern with birth shifting is that either babies whose birth is postponed by one week are healthier, which would lead to fewer hospital presentations

Table 3: Density Test 1: Local Polynomial Density Estimation

Estimation Method (1)	Est. Bandwidth		Observations		Density Test
	Left (2)	Right (3)	Left (4)	Right (5)	p-val. (6)
Unrestricted, asymmetric bandwidth	75	97	3,128	4,086	0.16
Unrestricted, symmetric bandwidth	97	97	4,169	4,086	0.18
Restricted, symmetric bandwidth	107	107	4,748	4,651	0.23

Note: This table presents the results of three non-parametric density tests of the running variable around 1 July 2004, the cutoff date marking the introduction of the Australian Baby Bonus. These results implement the test proposed by Cattaneo, Jansson and Ma (2020) implemented through the Stata command `rddensity` (Cattaneo, Jansson and Ma, 2018). Col. 1 indicates the local polynomial fit method and the bandwidth estimation method. Col. 2 and 3 indicate the estimated bandwidth on either side of the cutoff (if applicable), and col. 4 and 5 indicate the number of observations used in the test on either side of the cutoff. Col. 6 presents the p-value of each density test comparing the distribution of births on each side of the cutoff to a Gaussian approximation. Large p-values indicate that the distribution of births on either side of the cutoff are not statistically different from one another. The sample used is the universe of children born in South Australia between 1 July 2003 and 1 July 2005, excluding 93 children born abroad during this time, and all children born within 7 days of 1 July 2004, the date of the introduction of the Australian Baby Bonus.

in the first year of life, or they born with worse health conditions, which we would lead to more hospital presentations in the first year of life. Table 4 shows that control and treated babies are not statistically different from one another in terms of birth outcomes or maternal characteristics. We present the results of 25 RD regressions on pre-treatment observable characteristics of children and their parents recorded in the Perinatal data and Birth records, and on outcomes at birth. For these balancing and falsification tests, we follow the recommendation of Cattaneo, Idrobo and Titiunik (2019) and run our preferred specification where outcome variables are the pre-determined characteristics and birth outcome variables. We reject the null of no RD treatment effect at the 5 percent level for two outcomes at birth: Apgar scores at 1 and at 5 minutes, which are correlated at 60%. Apgar scores range from 0 to 10, and the scale is designed such that healthy babies receive scores above 7. One minute after birth, babies born just after the cutoff date have 0.1 fewer Apgar points on average compared to control babies, amounting to a decline of 1.2% from the mean in the optimal bandwidth. Five minutes after birth, treated babies have 0.06 fewer Apgar points, a decline of 0.7% of the mean in the optimal bandwidth. We also find suggestive evidence (significant at the 10% level) that treated babies are 1 percentage point more likely to be born to single mothers compared to control babies.

Figure B.2 provides supporting graphical evidence. The figure presents RD-plots for 6 key indicators of birth shifting: gestational age in weeks, an indicator for pre-term birth, another indicator for whether the baby is placed in a special nursery at birth, Apgar scores at 1 minute and 5 minutes

after birth, and birth weight in grams. For this exercise, we first estimate the CER-optimal bandwidth for the outcome of interest using our main specification, then we plot the average outcome for each date of birth in the estimated bandwidth, the linear fit on each side of the discontinuity, and the 95% confidence interval. These RD-plots provide graphical evidence confirming that babies in the control group are not systematically different from treated babies based on 6 key characteristics associated with birth shifting and predictive of health status in the first year of life.

5.1.2 Selective Fertility

Another endogeneity concern is the presence of selection into treatment selective fertility decisions, including conception and abortion. Previous research on the Spanish Baby Bonus has shown that its announcement and cancellation led to selective conceptions and abortions (Borra, González and Sevilla, 2016, 2019; González, 2013). In our setting, the policy was announced on 16 May 2004, so selection into fertility would not be revealed at the time of implementation on 1 July 2004.

One could still be concerned about selective abortions for already pregnant women at the time of the announcement of the policy, who would be in the control group and would rather have another child later in the year to be in the treatment group. As discussed in Deutscher and Breunig (2018); Gans and Leigh (2009), the short-notice announcement of the policy left little scope for selective abortions because pregnant women whose planned delivery date would be just before 1 July 2004 would be at an advanced stage of their pregnancies, at which stage selective abortions are legally prohibited.

Table B.2 confirms that selective abortions are not a concern for our results. The introduction of the ABB did not induce changes in fertility. To demonstrate this point, we exploit the richness of our Perinatal Data containing the entire history of pregnancies of mothers to all babies born in South Australia between 1991 and 2016. The data contains births in all public and private hospitals and clinics of South Australia. It also includes information on past pregnancies of these mothers, including miscarriages, neonatal deaths and abortions along with associated dates. With this data, we estimate RD effects on the likelihood that the mothers of treated babies had significantly more past pregnancies, live births, miscarriages, or abortions compared to mothers of control babies. We find no significant differences between control and treated babies on any of these indicators of past pregnancies.

Table 4: Balancing Tests on Pre-Determined Characteristics

	Coef. Est.	Sd.err.	p-value	Bandwidth 1/2 length	N.Obs.		Mean
					(1)	(2)	
Child characteristics:							
Female	0.015	0.011	0.175	477	22,681	22,733	0.49
Baby weight	22.067	14.375	0.125	483	22,955	23,004	3353.33
Special Nursery	0.001	0.010	0.923	381	18,007	17,856	0.16
NICU	0.001	0.003	0.755	703	33,225	33,731	0.03
PICU	0.000	0.001	0.822	475	22,465	22,544	0.00
Neo-natal Death	-0.001	0.001	0.724	323	15,208	15,074	0.00
Apgar 1min	-0.107	0.046	0.020	297	13,852	13,729	8.14
Apgar 5min	-0.058	0.022	0.009	441	20,874	20,787	9.06
Gestational Age	0.057	0.062	0.354	336	15,824	15,682	38.80
Pre-term Birth	-0.004	0.007	0.606	670	31,671	32,140	0.15
Obsetric Complication	-0.024	0.014	0.100	294	13,725	13,617	0.35
C-Section	0.007	0.013	0.566	622	29,224	29,721	0.32
Private Hospital	0.012	0.013	0.334	387	18,366	18,187	0.30
Mother smoke	0.002	0.008	0.788	591	27,325	27,787	0.16
No. ante-natal visits	-0.056	0.090	0.535	325	14,058	13,840	10.40
Parental characteristics:							
<i>Mother age:</i>							
35+	-0.005	0.008	0.509	565	26,620	26,846	0.19
40+	-0.004	0.004	0.288	475	22,566	22,635	0.03
<i>Father occupation:</i>							
High skilled	0.007	0.012	0.554	472	21323	21216	0.326
Low skilled	0.009	0.012	0.458	558	25023	25088	0.56
<i>Mother marital status:</i>							
Never Married	0.011	0.006	<i>0.077</i>	620	29,180	29,639	0.10
Married	-0.006	0.008	0.464	503	23,821	23,910	0.89
Single	-0.004	0.003	0.115	425	20,117	20,039	0.01
<i>Mother race:</i>							
Caucasian	0.001	0.006	0.915	509	24,082	24,150	0.86
Asian	0.003	0.004	0.531	571	26,895	27,117	0.08
Aboriginal or TSI	-0.004	0.005	0.346	471	22,369	22,411	0.06

*Note: This table presents the results of balancing tests on pre-treatment characteristics of children and their parents based on birth and perinatal records. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004, and all births within 7 days of 1 July 2004. The correlation between Apgar scores at 1 and 5 min is 0.604 in this sample. p-values in **bold** indicate effects statistically significant at least at the 5% level, and in italic at the 10% level.*

6 Estimation Results

Our results indicate that the Australian Baby Bonus led to reduction in hospital presentations in the first year of life for urgent and acute health problems, presentations followed by a hospital admission and overnight stay, and potentially preventable hospitalizations. The Baby Bonus did not lead to increased, potentially costly elective care in the first year of life. These findings are consistent with an improvement in the health status of treated babies, potentially through increased parental investments in earlier stages of the child's life.

6.1 Hospital presentations within the first year of life

Table 5 presents our main results on hospital presentations within the first year of life. Panel A shows results on the probability of having any recorded hospital presentations, overall and split by presentations at Emergency Department or Inpatient Services (admission after ED). We find that treated babies are significantly ($p < 0.05$) less likely to have presentations at Inpatient Services by -3.4 percentage point (ppt). This drop corresponds to a decline of 11 percent compared to the mean (0.30). In other words, while almost one in three babies were admitted for an Inpatient Service pre-treatment, only one in four babies were admitted post-treatment (0.30-0.034). Although we also find negative treatment effects for any hospital presentations (-1.3 ppt) and Emergency Department presentations (-2.4 ppt), but these estimates are not statistically significant. Figure B.3 presents graphical evidence of the estimation results using RD-plots, which confirm the drop in hospitalizations at the eligibility cutoff.¹⁴

Further evidence in favor of the hypothesis that parents are not just substituting other, potentially costly care for free hospital care is that treated babies are significantly less likely to be admitted for urgent and acute care needs (as classified by the triage nurse) to an Emergency Department ward, which requires observation, by 3.8 ppt (or 29 percent relative to the mean of 0.15) and being recorded as an emergency presentation to the Inpatient Services by 3.7 ppt (25 percent relative to the mean of 0.15). These acute episodes are also markers for poor infant health.

The interpretation of the previous results in terms of better infant health are furthermore corroborated by the evidence that the ABB reduced potentially preventable hospitalizations, which refer to health concerns that should have been dealt with earlier in lifecycle of the disease or where the

¹⁴ Alternatively, we estimate the impact of the ABB on the number of hospitalisations, using a count data model. Our conclusions remain unchanged (Table B.7), as the estimated coefficients have same sign, magnitude and significance as our main results. ABB treated babies have on average 0.12 fewer visits at the emergency department and 0.06 fewer presentations as inpatient within their first year of life. These effects represent a decline of 17 percent and 13.6 percent compared to the respective means of 0.69 and 0.44 visits, which are economically sizeable effects: following the Baby Bonus, 5 out of 10 children have a presentation at the ED before their first birthday instead of 7 out of 10 before the policy, and 3 out of 10 instead of 4 out of 10 have a presentation at an inpatient service within their first year of life.

disease could have been avoided altogether with the availability of better financial resources. Indeed, one of the criteria used in the New Zealand PAH tool to identify preventable hospitalizations for children is whether hospitalization could have been avoided in the presence of “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. As shown in Panel C, we find that treated babies have significantly fewer hospital presentations deemed “preventable” by hospital staff at inpatient services by -2.8 ppt. Relative to the mean of 8 percent, this implies a reduction of 35 percent. Thus, not only do treated babies have fewer hospital presentations, but they have fewer of the more acute and costlier presentations, and fewer preventable hospitalizations.

Finally, the ABB did not change planned hospital presentations which would have been associated with an increase in direct health investments made by parents. If anything, presentations that require planning (including planned re-admissions and reviews) and a medical referral from a specialist, decreased in the first year of life.

6.2 Treatment effects by diagnosis

So far, we have shown that demand for acute and urgent health care decreased significantly for babies who benefit from the ABB relative to babies who did not, suggesting that on average infant health improved. In this section we now explore the impact of the ABB on specific diagnoses, which allows us to better understand the mechanisms through which the ABB affected infant health. In Table 6, we present these estimation results.

We find, overall, that the ABB reduced hospitalizations that were classified as respiratory problems in the first year of life, but had no impact on other disease groups. In Table 6 we present estimation results for the 5 (PPP hospitalizations) and 10 (ED and inpatient services) most common problem/diagnosis groups. Across the board, we find economically and statistically significant treatment effects for bronchiolitis, the most common respiratory illness for infants, which is associated with asthma at later stages, and diagnoses of respiratory diseases according to both the triage nurse and ICD-10 coding. For instance, treated babies were 2.5 ppt less likely to have potentially preventable ED presentations due to bronchiolitis compared to control babies (Panel A). Relative to the mean of 0.06, this implies a reduction of 42 percent. In other words, non-treated babies had a chance of 1 in 20 to be admitted for bronchiolitis, even though it could have been prevented. For treated babies this chance is only 1 in 40. This reduction in the likelihood is consistent with the decrease in PPPHs, as the PAH tool identifies respiratory problems treated in hospital as “potentially avoidable” (Anderson et al., 2012, p.26).

The treatment effect is also large in magnitude for emergency department visits, independent

of whether the diagnosis was based on the presenting problem as recorded by the triage nurse (-3.8 ppt, or 32 percent relative to the mean of 0.12) or by the treating doctor who recorded the diagnosis in form of an ICD-10 code (-3.2 ppt, or 25 percent relative to the mean of 0.13). The ABB also reduced Inpatient service hospitalizations due to respiratory illness by 3.7 ppt, or by 62 percent relative to the mean of 0.06. This suggests that non-treated babies had a 1 in 20 chance to be admitted to an inpatient service for respiratory illness. For treated children this chance is only 1 in 45 (see Panel B). Figure 3 visualizes the large magnitude of the treatment effect of the ABB on respiratory hospitalizations.

One other result is worth discussing. We find no consistent evidence that the ABB may have increased accidents, injury and trauma, which could have resulted if households prone to domestic violence and maltreatment use the Baby Bonus to increase alcohol or drug consumption. We have three different outcome measures that could capture such hospitalizations: (1) Injuries, Trauma and Poisoning according to ICD-10-AM chapter classification that was used upon discharge to categorize the diagnose made by a doctor; (2) Injuries, Trauma and Poisoning as assessed by the triage nurse upon ED presentation; and (3) Externally caused problems as assessed by a doctor at Inpatient Services using an ICD-10-AM chapter classification upon discharge.

The results are mixed. When assessed by the triage nurse upon presentation at the ED, treated babies are 1 ppt ($p < 0.10$) more likely to be classified as injuries, trauma or poisoning. When assessed by a medical doctor using the ICD-10-AM chapter classification, our estimates consistently indicate that treated babies are not more likely to experience such presentations. We find precisely estimated null effects of the policy on presentations for injury, trauma and poisoning at both ED and inpatient services. Even more so, the ABB significantly reduced the probability of inpatient service presentations due to external causes by -1.2 ppt ($p < 0.05$), or 30% relative to the sample mean of 0.04. "External causes" is a diagnostic category in the ICD-10-AM chapter that is typically associated with accidents or problems caused by carers.

Our results therefore provide consistent evidence that the Baby Bonus had an overall positive impact of infant health, in part due to increased parental investments or behavioral responses with positive health returns.

6.3 Heterogeneity by socioeconomic background

The Baby Bonus presented a larger windfall payment relative to average household income for disadvantaged and financially constrained households. If the Baby Bonus was used to invest in children's health early in life, and if child health problems are generally more common in poorer households, then we would expect a stronger impact of the Baby Bonus on the child's health in the first years of life for poorer families. We are able to confirm this hypothesis with a heterogeneity

analysis by socioeconomic background. These additional analyses are presented in the Appendix (Table B.10).

We proxy socioeconomic background with information on the father's occupation (high skilled versus low skilled, Col. 1 and 3).¹⁵ We find that the reduction in hospital presentations due to the Baby Bonus are largely driven by children in families with low-skilled fathers. The coefficient estimates are larger in magnitude by a factor of 1.7-5.6 for all outcomes such as emergency department and inpatient presentations (Panels A and B), and presentations that are preventable (Panel D). For instance, children of low-skilled fathers are 4.2 ppt less likely to present to emergency departments (versus 2 ppt for high-skilled group) and are 5.1 ppt less likely to be admitted to a ward (versus -2.3 ppt for high-skilled group). Children of low versus high skilled fathers are 4.8 ppt less likely versus 0.8 ppt more likely to presenting to the emergency department for preventable problems. There are no significant differences for planned visits. Thus, the ABB seems to have facilitated better prevention of negative shocks to child health for families from disadvantaged backgrounds.

6.4 Robustness checks

6.4.1 Choice of bandwidth

One could be concerned that our estimation results are sensitive to the choice of bandwidth, which affects the size of the estimation sample. In our main specification, we use the symmetric CER-optimal bandwidth. Alternatively, one could use an asymmetric CER-optimal bandwidth or the MSE-optimal bandwidth. The MSE-optimal bandwidth is built on a larger neighborhoods to reduce variance in the whole neighborhood of the cutoff and achieve optimal point estimators; recent research has shown however that it leads to incorrect inference of RD treatment effects at the cutoff (Calonico, Cattaneo and Farrell, 2020). CER bandwidth methods shrink the MSE-bandwidth in order to minimize coverage error. This leads to smaller neighborhoods around the cutoff date, which may compromise statistical power. In data-sets such as ours, with only about 50 births per day, it is recommended to compare our results under alternative bandwidth selection methods.

In Table B.3 we show that our findings are robust to a variety of alternative bandwidth choices which widen the sample windows around the 1 July cutoff. While the smallest bandwidths of roughly half a year on each side of the cutoff are obtained with the symmetric CER-bandwidth method (spread: 142-299 days, median: 182 days), we obtain the largest bandwidth with the MSE-bandwidth method of around three quarters of a year (spread: 217-460, median: 286 days). The estimates are almost identical across the different specifications, in sign, magnitude, and sig-

¹⁵Father occupation is available in perinatal records and thus reflect disadvantage at birth. Being born to low-skilled fathers is an important markers of socioeconomic disadvantage in Australia and elsewhere.

nificance. The only difference is that the estimated coefficients tend to be larger in magnitude when using the MSE-bandwidth method and the estimates are more precisely estimated. For instance, the treatment effect of Inpatient Services changes from -0.034 ($p < .05$) to -0.044 ($p < 0.01$) for MSE-bandwidth and -0.047 ($p < 0.01$) for the asymmetric CER-bandwidth. The treatment effect of overnight admissions changes from -0.022 ($p < 0.10$) to -0.027 ($p < 0.05$) and -0.034 ($p < 0.01$), respectively. Thus, we conclude that our estimation results are not driven by an arbitrary choice of bandwidth.

6.4.2 Observations close to cutoff

Another concern could be that our results are driven by a small number of observations located near the cutoff. Table B.4 shows that our results are almost the same both in sign, magnitude and significance when we consider alternative samples excluding births within 5, 8, 12 and 15 days. Re-estimating our results including the seven-day donut births closest to the cutoff date of birth, does not change our conclusions either (see [Appendix A](#)).

6.4.3 Placebo cutoffs

Our results could be driven by other factors that coincide roughly with the 1 July cut-off date. For example, differences in birth outcomes across the 1 July cut-off could be due to seasonal variations. If this was true, then we should see statistically significant treatment effects also for randomly chosen cut-off dates in the vicinity of 1 July. To test whether the 1 July cutoff point can be treated as an exogenous assignment (Ganong and Jäger, 2018), we run a falsification exercise that assesses whether the coefficient estimate at the true cut-off point is extreme in the distribution of all coefficient estimates at alternative cut-off points. We re-run our main specification using 180 placebo cut-off points, ranging from 90 days before 1 July 2004 until 90 days after 1 July 2004. We then construct the percentile rank of our estimate at the cut-off date and compare it to the distribution of all the estimates obtained, and construct a randomization-based p-value as described in Ganong and Jäger (2018).

The resulting p-value should be interpreted with care. With 180 hypotheses to be tested, we expect to find at least nine significant RD effects at some placebo cut-off dates purely by chance. Thus, it will be hard for the permutation test to detect significant differences between the true cut-off date estimate and all other placebo cut-off date estimates. To guide the testing procedure, we suggest that small p-values indicate that the coefficient estimate at the true cut-off is more extreme than coefficient estimates at placebo cut-offs (in other words, a small rank number of the size of the coefficient estimate at the true cut-off date).

Table B.5 presents the results of these permutation tests, one per outcome. Cols. (1)-(3) and

(5) recall our benchmark estimates originally reported in Table 5; Col. (4) presents the rank of the coefficient estimate at the true cut-off compared to coefficient estimates at 180 alternative cut-offs; Col. (6) reports the randomization-based p-values of the cut-off permutation tests. The outcome of the permutation tests indicate that overall our main results seem rightfully attributed to the policy introduction at the cut-off date rather than to confounding factors such as seasonality. In five out of seven significant treatment effects of the ABB, we find that the permutation test unambiguously states that the benchmark estimate, based on the 1 July cut-off, is a reliable estimate. For these cases, p-values of the permutation test are around 0.1 and estimated ranks are around 10.¹⁶

6.4.4 Multiple hypothesis testing

As we test many hypotheses, it could be the case that the significant estimates reported above were produced simply by chance. In this section, we show that our conclusions are robust to using multiple-hypothesis corrected standard errors for statistical inference. In Table B.6, we present the original asymptotic p-values associated with our main results (Col. (1)), bootstrap p-values using 500 replications (Col. (2)), and step-down Romano-Wolf p-values correcting our original p-values using a family-wise error rate (Col. (3)) (Romano and Wolf, 2005, 2016).¹⁷ We find that all the statistically significant coefficient estimates in our main results remain statistically significant when we use both bootstrapped and step-down p-values: admissions to ward for acute or urgent problems, urgent inpatient services and PPPH presentations at inpatient services.

¹⁶There are two cases where the permutation test yields a more ambiguous finding. These are: Inpatient Services ($p = 0.696$) and Any PPPH at inpatient services ($p = 0.265$). As the estimation results for these two outcomes were consistent across all sensitivity checks, we consider the outcome of the low-powered permutation test as not problematic.

¹⁷The adjustment tests are implemented with the `rwolf` Stata command (Clarke, Romano and Wolf, 2020).

Table 5: The Effects of the Australian Baby Bonus on Hospital Presentations Within the First Year of Life

	Coef.	Sd.err.	p-value	Bandwidth	N.Obs.	Mean
	Est.			1/2 length		
	(1)	(2)	(3)	(4)	Left	Right
Panel A. Any presentation by hospital service:						
Any hospital service	-0.013	0.021	0.532	175	8,022	7,855
Emergency department	-0.024	0.020	0.230	152	6,906	6,764
Inpatient service	-0.034	0.017	0.040	215	9,832	9,611
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):						
<i>Emergency Department:</i>						
Emergency presentation	-0.017	0.018	0.369	172	7,833	7,667
Admission to ward	-0.038	0.013	0.004	173	7,891	7,772
<i>Inpatient Services:</i>						
Emergency presentation	-0.037	0.013	0.005	207	9,483	9,235
Admission to ward	0.002	0.007	0.761	206	9,426	9,202
Overnight admission	-0.022	0.012	0.074	299	13,994	13,856
Panel C. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):						
Any PPPH	-0.033	0.019	0.083	153	6,906	6,764
Any PPPH at ED	-0.032	0.018	0.071	142	6,418	6,320
Any PPPH at inpatient services	-0.028	0.010	0.005	215	9,832	9,611
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):						
<i>Emergency Department:</i>						
Planned visit	-0.010	0.005	0.074	147	6,619	6,497
Visit with med. referral	0.002	0.009	0.845	152	6,870	6,704
<i>Inpatient Services:</i>						
Planned visit	-0.001	0.004	0.882	289	13,535	13,442
Visit with med. referral	-0.010	0.010	0.308	198	9,108	8,874
Booked elective procedure	0.003	0.008	0.657	230	10,579	10,481

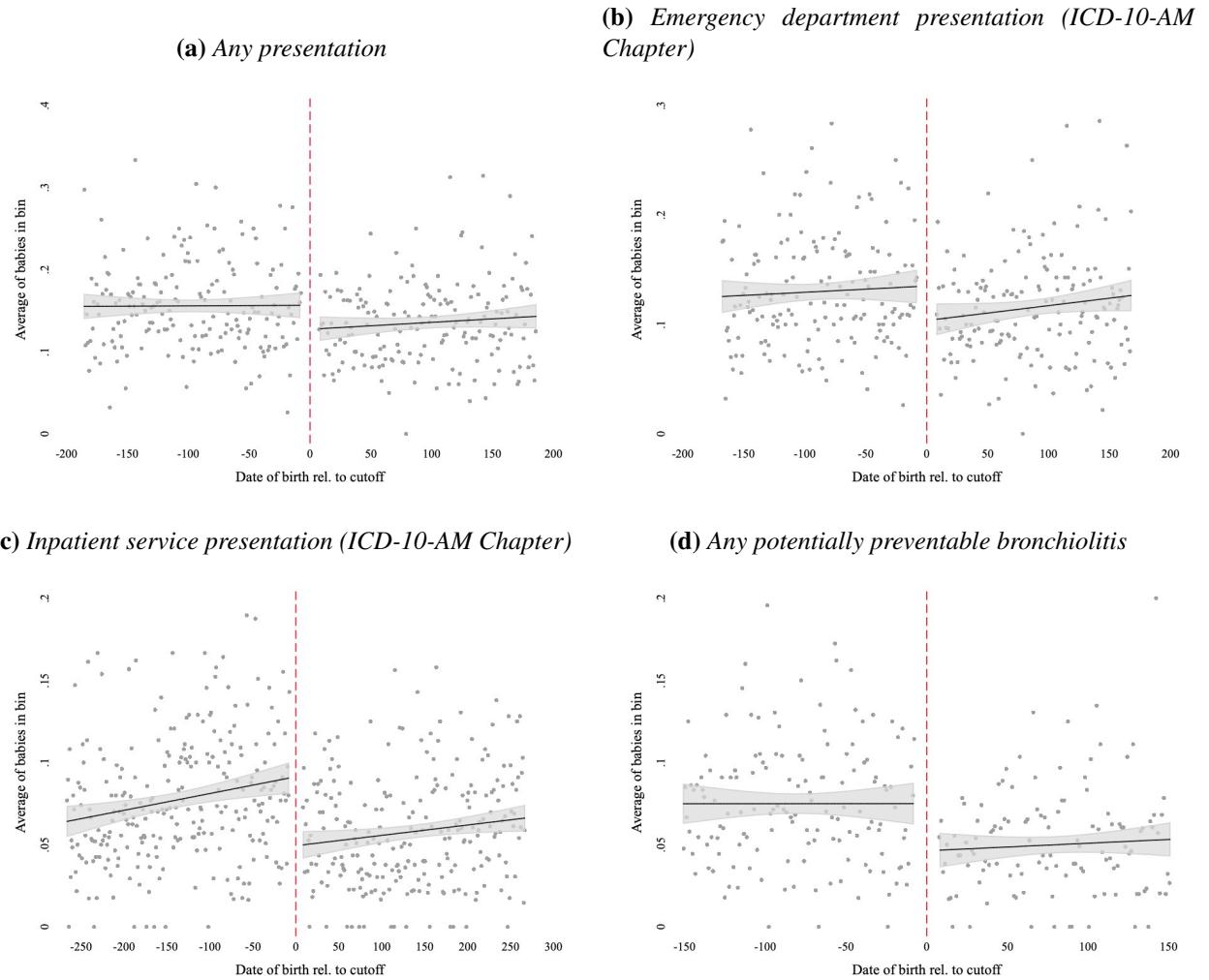
Note: This table presents our main results on hospital presentation outcomes. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.

Table 6: The Effects of the Australian Baby Bonus on Medical Diagnostics Within the First Year of Life

	Coef. Est.	Sd.err.	p-value	Bandwidth 1/2 length	N.Obs.		Mean					
					(1)	(2)						
Panel A. Potentially Preventable Pediatric Hospital Presentations:												
<i>Emergency Department:</i>												
Bronchiolitis	-0.025	0.010	0.012	152	6,870	6,704	0.06					
Gastroenteritis	-0.024	0.013	<i>0.061</i>	94	4,047	3,969	0.06					
Laryngitis	-0.002	0.006	0.759	127	5,674	5,622	0.01					
Otitis media	-0.002	0.005	0.706	136	6,056	5,978	0.01					
Respiratory infection	-0.013	0.010	0.223	121	5,357	5,280	0.05					
<i>Inpatient services:</i>												
Bronchiolitis	-0.013	0.008	0.105	160	7,272	7,126	0.04					
Gastroenteritis	-0.007	0.005	0.141	208	9,507	9,292	0.02					
Laryngitis	0.000	0.002	0.849	221	10,151	9,965	0.00					
Otitis media	0.001	0.001	0.369	274	12,657	12,665	0.00					
Respiratory infection	-0.003	0.004	0.453	271	12,540	12,537	0.01					
Panel B. Presentations by ICD-10-AM diagnostic chapter and Presenting problem:												
<i>Emergency Department ICD-10-AM Chapter:</i>												
Respiratory problems	-0.032	0.012	0.010	168	7,666	7,518	0.13					
Injuries, Trauma and Poisoning	0.004	0.007	0.595	182	8,316	8,069	0.05					
Infections	-0.012	0.013	0.365	134	5,958	5,882	0.09					
Digestive problems	-0.011	0.007	0.123	156	7,072	6,898	0.03					
Skin problems	0.001	0.006	0.854	164	7,491	7,290	0.02					
Unspecified problems	-0.019	0.012	0.098	160	7,228	7,077	0.11					
Nervous system problems	0.001	0.001	0.205	170	7,731	7,587	0.00					
Eyes and Ears-related problems:	-0.001	0.006	0.916	147	6,619	6,497	0.02					
<i>Emergency Department Triage Nurse:</i>												
Respiratory problems	-0.038	0.012	0.001	164	7,438	7,252	0.12					
Injuries, Trauma and Poisoning	0.011	0.006	0.059	163	7,376	7,221	0.03					
Infections	-0.008	0.010	0.456	157	7,114	6,933	0.09					
Digestive problems	-0.026	0.016	0.106	88	3,791	3,725	0.09					
Skin problems	0.008	0.008	0.346	141	6,360	6,285	0.05					
Unspecified problems	-0.011	0.015	0.462	151	6,828	6,671	0.08					
Nervous system problems	-0.001	0.003	0.822	153	6,957	6,811	0.01					
Eyes and Ears-related problems:	-0.009	0.008	0.286	130	5,768	5,662	0.03					
<i>Inpatient services: ICD-10-AM Chapter</i>												
Respiratory problems	-0.037	0.008	0.000	267	12,362	12,371	0.06					
Injuries, Trauma and Poisoning	-0.003	0.003	0.345	298	13,960	13,821	0.01					
Infections	-0.006	0.006	0.319	206	9,483	9,235	0.03					
Digestive problems	-0.005	0.005	0.290	218	9,973	9,790	0.01					
Skin problems	0.004	0.002	0.070	155	7,072	6,898	0.00					
Externally caused problems	-0.012	0.006	0.047	302	14,151	14,032	0.04					
Unspecified problems	-0.001	0.006	0.925	205	9,426	9,202	0.02					
Nervous system problems	-0.001	0.001	0.549	264	12,146	12,133	0.00					
Eyes and Ears-related problems:	0.003	0.003	0.400	169	7,731	7,587	0.01					

Note: This table presents the results of regressions of the effect of the Australian Baby Bonus on hospital presentations of babies within their first year of life. Each line corresponds to a separate regression using our main specification, where outcomes are specific diagnostics. We use local linear estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.

Figure 3: RD Effects of the ABB on Hospital Presentations for Respiratory Problems In The First Year of Life



Note: These RDPlots present graphical evidence that the introduction of the ABB reduced hospital presentations for respiratory problems in the first year of life for treated babies. On every figure, the x-axis corresponds to birth dates, the red dashed line indicates the introduction date of the Baby Bonus (1 July 2004), and the y-axis to the pre-treatment characteristic of interest. These plots are produced with the *rdplot* Stata command designed by Calonico, Cattaneo and Titiunik (2015); Calonico et al. (2017), which implements a data-driven procedure to optimally select the number of bins. We select the number of bins on each side of the cutoff using the evenly-spaced, variance-mimicking (ESMV) procedure and use for each outcome variable the CER-optimal bandwidth estimated using the *rdrobust* Stata command (Calonico, Cattaneo and Farrell, 2018, 2020; Calonico et al., 2017). The line represents the local linear fit, using a triangular kernel. The grey shaded areas represent the 95% confidence interval around the mean of each bin. We exclude births within 7 days of the cutoff date.

7 Longer-term impact of the Baby Bonus

So far we have shown that the ABB led to a decline in hospital care utilization for preventable and acute, respiratory problems within the first year of life. Our evidence is consistent with additional or more adequate parental investments in child health that occur outside the hospital sector, either in the primary care sector or in the home environment. We now assess what happens in the follow-up years until age 5 (See Appendix, Tables [B.8](#) and [B.9](#)).

Overall, we find that the risk of health care utilization for preventable and urgent care is also lower for treated than for control babies in their second year of life (Table [B.8](#)). Although, the estimates are imprecise, they are relatively large in magnitude. For instance, in both age groups, treated babies are around 2 ppt less likely to present to an emergency department. Treated babies are 2.2 ppt less likely to present for preventable problems at emergency departments in the second year only. No further effects are detected at later ages.

The lower risk of presentations for preventable problems is driven by lower presentation risk for preventable otitis media (glue ear) (-1 ppt)¹⁸ and respiratory infections (-2.6 ppt) in the second year of life (Table [B.9](#)). Treated babies are also less likely to present for injuries, trauma and poisoning ED presentations (-2.6 ppt), unspecified problems (-2.3 ppt) and eyes and ear problems (-1 ppt).

However, treated babies are significantly more likely to have planned visits (1.7 ppt) or booked elective procedures (1.6 ppt) at inpatient services in the second year of life. They are also more likely to present for planned visits at the emergency department (0.8 ppt) within their third year of life. It is these type of presentations which require a medical referral from a specialist and which incur co-payments.

We find precisely estimated null effects on any other subsequent diagnostics up until age 5. These findings suggest that the decline in hospital presentations that we found in the first year of life were not associated with later additional hospital care utilization compensating for lack of early care or early under-detection of true health problems, but rather these findings are consistent with treated babies receiving better or more adequate hospital care. Furthermore, our findings suggests that Baby Bonus households may have used the baby Bonus money to increase elective care which requires higher co-payments, which can be interpreted as a health investment.

¹⁸We consistently find that treated babies experience have a 0.1 ppt smaller probability to present at inpatient services for eyes and ears related problems.

8 Economic relevance of the Baby Bonus

Now that we have established that treated babies have fewer urgent and acute hospital presentations in the first year of life which are not associated with additional, compensatory emergency presentations in subsequent years, we can make back-of-the envelop calculations of the overall budgetary savings for the central planner. We focus on the cohort of babies born in South Australia in the year 2004 to ignore fertility effects induced by the policy following its introduction, and to avoid extrapolating our estimates to the national context.

The ABB cost \$3,000 per child. The ABB however substituted for other maternity benefits, so its net costs are only \$2,157 (see Deutscher and Breunig, 2018, Section 2.3). The overall cost of the policy could therefore amount to approximately \$3,000 or \$2,157 times the number of babies born between 1 July 2004 and 31 December 2004. With total births in 2004 amounting to roughly 17,200, we assume that births in the second semester of 2004 represented 8,600 children. Thus, the ABB costs corresponded to an increased investment in children ranging between \$18,550,200 and \$25,800,000.

We focus on urgent/severe and preventable hospital presentations in the first year only to avoid estimating a return on investment which could fail at discerning necessary from unnecessary hospital presentations. For example, had we found evidence consistent with adverse health outcomes for treated babies — e.g. decline hospital care in the first year of life at the expense of additional hospital care later in life — we would have incorporated these intertemporal effects in our accounting exercise. In the absence of these longer-run effects, we focus on our main results in Table 5. We found that treated babies were 3.8 ppt less likely to be admitted to a ward for urgent care and were 2.2 ppt less likely to be admitted for an overnight stay in Year 1. They were also 3.2 ppt less likely to present at an emergency department for a preventable problem.

We derive the average costs for ED presentations, admissions to wards and overnight stays (calculated at the average number of days admitted) from the National Hospital Cost Data Collection.¹⁹ The average costs of admissions to each service are:

1. Admission to a ward: \$6,111; Hence the total cost is $-0.038 \times 17,200 \times \$6,111 = \$3,994,150$;
2. Overnight stay paediatric: \$5,513; Hence the total cost is $-0.022 \times 17,200 \times \$5,513 = \$2,086,119$;
3. Presentation for preventable problems at ED: \$507; Hence the total cost is $-0.032 \times 17,200 \times \$507 = \$279,053$;

¹⁹ url<https://www.ihsa.gov.au/sites/g/files/net636/f/publications/nhcdround18.pdf>.

4. Total cost savings in Year 1: \$6,359,322

Given the payout costs of the ABB, ranging between \$18,550,200 and \$25,800,000 in 2004, we calculate that between 24% ($\$6,359,322/\$25,800,000$) and 34% ($\$6,359,322/\$18,550,200$) of the immediate costs of the ABB were recouped immediately through a reduction in acute and preventable hospitalisations in the first year of life of affected children.

The calculated cost savings are likely to underestimate true savings, since we only focus on presentations that are clearly considered avoidable by medical professionals and that require immediate attention due to their urgency. We did not include in these calculations the human capital benefits of better health early in life, or the longer-term health and human capital benefits of parental health investments (which we observed in Year 2). Although these health investments are costly, they are likely to bring benefits in the future. We thus consider these cost savings as a lower bound.

9 Conclusions

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, Currie and Duque, 2017, 2018). Baby bonuses aimed at improving living conditions and increasing purchasing power of households with newborn 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 Baby bonuses to improve children's health outcomes, and the mechanisms through which such an unconditional cash transfer may influence child health.

We contribute to this literature by evaluating the impact of the Australian Baby Bonus (ABB), an unconditional and unanticipated cash transfer paid to families with a newborn child, on health outcomes of children up until age 5. Using high quality linked administrative data and a regression discontinuity approach, we document that the introduction of the ABB did not lead to a large degree of birth shifting, which was reported elsewhere (Gans and Leigh, 2009). In fact, we estimate that only 49 baby births were shifted from last week of June to first week of July to make the cutoff date. South Australian hospitals frequently experience such week-to-week variations (the standard deviation of weekly births was 22 between 1999 and 2004). With 40 maternity wards, this meant that only every sixth hospital would have experienced one additional birth per day in July 2004. As a consequence, the 400 babies born in the first week of July were slightly heavier at birth. We deal with strategic birth shifting by excluding a donut of seven days around the 1 July 2004 cutoff.

Our RDD estimate demonstrate that the ABB reduced emergency department presentations and inpatient services utilization, mostly for respiratory problems in the first two years of life. Importantly, we find that while the ABB led to a decline in presentations for urgent and acute problems

and for potentially preventable pediatric hospitalizations in the first year of life, it did not lead to an increase in presentations for injuries, trauma or poisoning. These findings are consistent with parents of treated babies increasing early investments in child health, or providing more adequate care. We further show that these findings are driven by socioeconomically disadvantaged families. We show through a battery of robustness checks and falsification exercises that our findings are robust to our model assumptions and sample definitions. We can rule out that the findings are driven by strategic birth shifting, fertility decisions (including abortion), and migration.

Through a simple accounting exercise, we show that our estimates translate into economically meaningful budgetary savings. At the intensive margin alone, the ABB reduces the share of potentially preventable pediatric hospitalizations from one in four to one in five infants in the first year of life. Similarly, the ABB reduces emergency department presentations for respiratory problems from one in seven infants to one in ten in the first year of life, a reduction of more than 50 percent. 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 Propper and Rigg, 2006; Wickham et al., 2016). 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-centered goods (Dahl and Lochner, 2012). Importantly, we find no evidence that the ABB caused an increase in accident or trauma-related emergency department visits, which would have suggested that some families may have spent the ABB on risky rather than child-centered consumption goods. We can only speculate on the items on which households spent the ABB, but there are two likely items that are consistent with our findings. On the one hand, the ABB may have been spent on better home environments such as more and consistent heating. RCT evidence from New Zealand suggests that more effective heating can improve respiratory health of children with asthma (Howden-Chapman et al., 2008). We find that the ABB reduced urgent care presentations for respiratory problems, and in particular bronchiolitis, which is an antecedent of asthma. The babies in our sample were born during the colder winter months, so better heated home environments would have made an important contribution to child health. On the other hand, the ABB may have been spent on elective care for pre-existing conditions. As we have shown, elective care in Australia require more co-payments for specialist visits and referrals. We demonstrate that elective care demand did increase for ABB-treated households in the second year of the child's life.

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 more 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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Appendix A Data Appendix

A.1 Birth shifting in South Australia

In this appendix, we give a detailed account of birth shifting in South Australia following the announcement on 16 May 2004 of the Baby Bonus introduced on 1 July 2004. We first provide graphical evidence on the incidence of birth shifting around 1 July 2004. Next, we replicate the findings of Gans and Leigh (2009) to our setting. Finally, we show the impact of birth shifting on the identifying assumptions of our regression discontinuity design, and discuss how birth shifting within 7 days of the introduction of the ABB affects our findings.

A.1.1 Graphical evidence on birth shifting in South Australia

Gans and Leigh (2009) and Deutscher and Breunig (2018) provide convincing evidence that a small group of families were able to delay the delivery date of their child so to receive the ABB. Based on accounting exercises, both studies suggest that i) birth shifting concerned approximately 1,000 births (0.4% of the 254,200 children born in Australia in 2004)²⁰, ii) the vast majority of birth shifting occurred within the days contiguous to 1 July 2004, and iii) birth shifting mostly took place for women who could re-schedule a planned Cesarean section birth.²¹

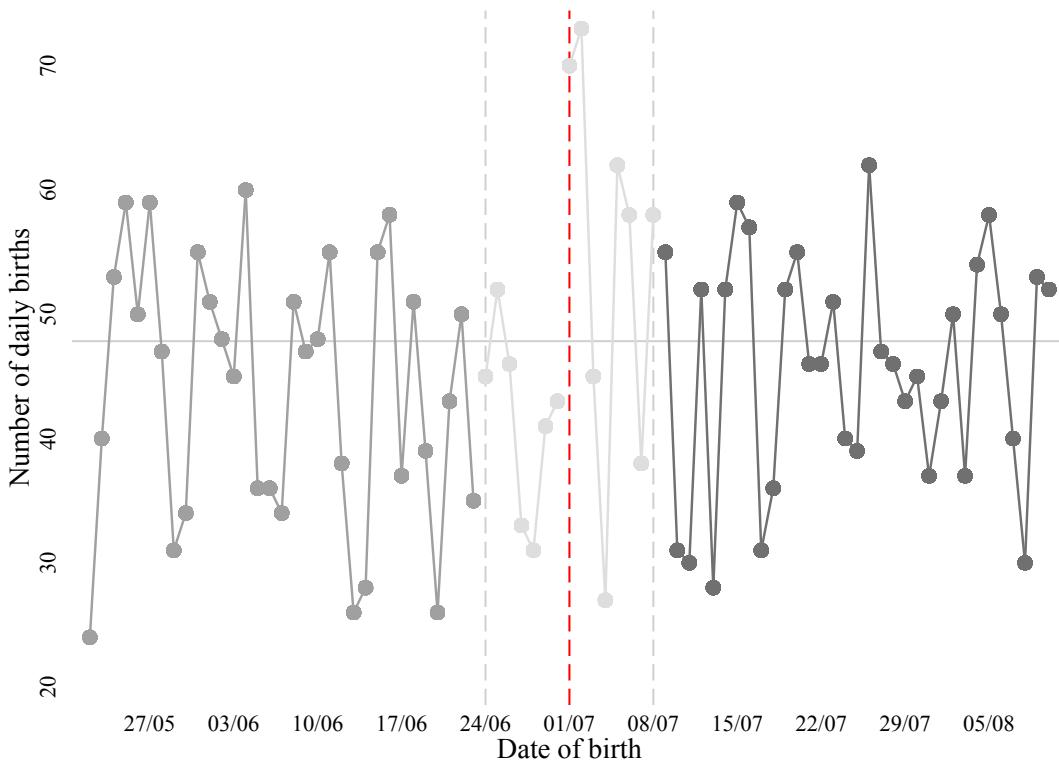
Figure A.1 suggests that birth shifting did take place in South Australia in the days closest to the cutoff date of 1 July 2004. On average, there are 46 births per day. We observe a slight decline in the number of births in late June, with a particularly low number of births in the last three days of 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). Thus, a first graphical inspection suggests that excluding the sample of births within 3 to 4 days around the cutoff could be sufficient to exclude birth shifting. Yet, graphical inspection is not sufficient

²⁰In 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).

²¹There is no 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”.

to decide the exact number of days to exclude since other factors may influence the number of births, such as day-of-the-week effects. 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.

Figure A.1: Daily Number of Births in South Australia Within 30 Days of 1 July 2004



Note: This figure shows the number of daily births in South Australia between 1 June 2004 and 1 August 2004. The blue horizontal line indicates the average daily number of births over the period (47) and the red vertical line indicates the cutoff date, 1 July 2004. The grey area represents births within 7 days of the cutoff date, which are excluded from our estimation sample.

A.1.2 Quantifying the total extent of birth shifting

In this section, we replicate the methodology of Gans and Leigh (2009) in South Australia and Borra, González and Sevilla (2019) in order to quantify the total extent of birth shifting in South Australia induced by the announcement of the Baby Bonus.

For this purpose, we estimate birth shifting as the difference in the number of daily births just

after v. before 1 July 2004, against the yearly average difference in all previous years until 2004. We use the South Australia birth registry and perinatal data, covering all births between 1991 and 2016, and we estimate the following equations, proposed by Gans and Leigh (2009) and also used in Borra, González and Sevilla (2019):

$$\begin{aligned} Births_i = & \beta 1\{\text{Baby Bonus}\}_i + \gamma_1 1\{\text{Year}\}_i \times 1\{\text{Day of Week}\}_i + \gamma_2 1\{\text{Day of Year}\}_i \\ & + \gamma_3 1\{\text{Public Holiday}\}_i + \varepsilon_i \end{aligned} \quad (\text{A.1})$$

$$\begin{aligned} \ln(Births_i) = & \beta 1\{\text{Baby Bonus}\}_i + \gamma_1 1\{\text{Year}\}_i \times 1\{\text{Day of Week}\}_i + \gamma_2 1\{\text{Day of Year}\}_i \\ & + \gamma_3 1\{\text{Public Holiday}\}_i + \varepsilon_i \end{aligned} \quad (\text{A.2})$$

where the dependent variables are respectively the number of daily births in Eq. (A.1) and the log number of daily births in Eq. (A.2). Our parameter of interest is β which captures the effect of $1\{\text{Baby Bonus}\}_i$, a dummy variable marking births on or after the introduction of the Baby Bonus on 1 July 2004. Under the assumption that we can correctly account for pre-trends in births around 1 July 2004, β captures the causal impact of the introduction of the Baby Bonus on the timing of births around 1 July 2004. To accurately account for pre-trends, we include 4 control variables: $1\{\text{Year}\}_i$ an indicator for the year of birth, interacted with $1\{\text{Day of Week}\}_i$, an indicator for the day of the week of birth. We also include $1\{\text{Day of Year}\}_i$, a dummy variable to control for the day of the year, and $1\{\text{Public Holiday}\}_i$, a dummy variable for public holidays. These fixed effects in our regressions allow us to flexibly control for seasonality in births in year, week and time of year that occur due to regular birth scheduling. We ignore births after 2005, because the policy announcement on 16 May 2004 also announced future increases of the policy scheduled for 1 July 2006 and 1 July 2008, and is thus likely to have induced endogenous fertility decisions.

Table A.1 presents the results of this analysis both in daily births and in log daily births. Following Gans and Leigh (2009) and Borra, González and Sevilla (2019), we consider four windows of analysis: the first column of Table A.1 focuses on births within 7 days of 1 July yearly; the second column focuses on births within 14 days, the third column births within 21 days and the fourth column births within 28 days of 1 July yearly. Using the same method as Gans and Leigh (2009), we compute that within 7 days of 1 July 2004, around 50 births may have been shifted from the last days of June to the first days of July. These potentially shifted births correspond to around 14% of the births that would have been expected in the last days of June 2004. Columns (2) through (4) show that birth shifting did not seem to extend much beyond 7 days from the cutoff

date, since the number of potentially shifted births grows only slowly as we expand the window around the cutoff, and the share of potentially shifted births declines to 6.7%.

Table A.1: *The Effect of the Baby Bonus on Birth Shifting in South Australia*

Window (W)	+/- 7 days (1)	+/- 14 days (2)	+/- 21 days (3)	+/- 28 days (4)
<i>Panel A. Number of daily births</i>				
1.Baby Bonus	-14.22*** (3.85)	-8.45*** (3.19)	-7.18*** (2.36)	-6.45*** (2.01)
R2	0.76	0.68	0.63	0.61
N. shifted births	49.8	59.1	75.4	90.3
<i>Panel B. Log number of daily births</i>				
1.Baby Bonus	-0.26*** (0.08)	-0.15** (0.07)	-0.14*** (0.05)	-0.13*** (0.04)
R2	0.77	0.68	0.65	0.63
Share shifted births	13.9	7.8	7.2	6.7
Clusters	225	435	645	855
N Obs.	11,650	22,128	32,731	43,458

*Note: Daily births in June v. July within the relevant window, based on the universe of births in South Australia recorded between 1991 and 2005. All specifications include fixed effects for: day of year, public holiday, and year×day of week. We cluster standard errors at the level of the date of birth. Windows relative to 1 July are: births in +/- 7 days (Column 1); in +/- 14 days (Column 2); in +/- 21 days (Column 3) and in +/- 28 days (Column 4). Significance levels * $p < .10$, ** $p < 0.05$, *** $p < 0.01$. Calculations: 1) Number of shifted births in window W: $\beta * W / 2$; 2) Share of shifted births in window W: $\exp(\beta / 2) - 1$. See Gans and Leigh (2009) for details.*

Following Borra, González and Sevilla (2019), we also explore whether birth shifting was driven by private hospital and mothers with private health insurance. Our findings in Table A.2 indicate that birth shifting was only marginally more prevalent in private hospitals and for mothers treated as privately insured patient.

Lastly, Table A.3 presents the results of balancing tests on child and parental pre-determined characteristics without restricting our sample to births further than 7 days away from the cutoff date. In the absence of any donut, we find that babies born just after the cutoff date are born

Table A.2: The Effect of the Baby Bonus on Birth Shifting in South Australia

Window (W)	+/- 7 days		+/- 14 days		+/- 21 days		+/- 28 days	
	Public	Private	Public	Private	Public	Private	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A. Number of births, by type of hospital</i>								
1.June2004	-14.18***	-14.05***	-8.19**	-9.14***	-6.96***	-7.91***	-6.23***	-7.12***
	(3.91)	(3.89)	(3.22)	(3.22)	(2.36)	(2.41)	(2.01)	(2.05)
r2	0.76	0.77	0.67	0.68	0.63	0.64	0.61	0.62
N. Obs.	8,538	2,865	16,306	5,350	24,111	7,941	31,999	10,557
N. shifted births	49.6	49.2	57.3	64	73.1	83.1	87.2	100
<i>Panel B. Number of births, by mother patient status</i>								
1.June2004	-14.22***	-14.47***	-8.32**	-8.85***	-7.13***	-7.42***	-6.36***	-6.72***
	(3.95)	(3.76)	(3.23)	(3.21)	(2.36)	(2.42)	(2.01)	(2.09)
r2	0.76	0.76	0.67	0.68	0.64	0.63	0.61	0.61
N. Obs.	7,537	4,065	14,397	7,629	21,366	11,224	28,328	14,942
N. shifted births	49.8	50.6	58.2	61.9	74.9	77.9	89	94.1
Clusters	225		435		645		855	

Note: Daily births in June v. July within the relevant window, by hospital type and mother patient status, based on the universe of births in South Australia recorded between 1991 and 2005. All specifications include fixed effects for: day of year, public holiday, and year×day of week. We cluster standard errors at the level of the date of birth. Windows relative to 1 July are: births in +/- 7 days (Columns 1 and 2); in +/- 14 days (Columns 3 and 4); in +/- 21 days (Column 5 and 6) and in +/- 28 days (Column 7 and 8). Significance levels * $p < .10$, ** $p < 0.05$, *** $p < 0.01$. Calculations: 1) Number of shifted births in window W: $\beta * W/2$; 2) Share of shifted births in window W: $\exp(\beta/2) - 1$. See Gans and Leigh (2009) for details.

32g heavier than babies born just before the cutoff date, and are 0.11 weeks older in gestational age. These findings are consistent with shifting births by a few days from the end of June to the beginning of July. We also find that babies born just after the cutoff have 2.6 percentage points fewer complications compared to babies born just before the cutoff, which, in line with Gans and Leigh (2009), could indicate that birth shifting mostly occurred for scheduled and low-risk births.

Overall, our findings indicate that birth shifting was a small phenomenon in South Australia, largely confined to 7 days around the cutoff date, and that private hospitals or privately insured mothers did not entirely drive birth shifting. However, balancing tests show that, although small, birth shifting does impose that we adapt our estimation strategy. The next subsection discusses the optimal "donut" size in our data.

Table A.3: Balancing Tests on Pre-Determined Characteristics Without Donut

	Coef. Est.	Sd.err.	p-value	Bandwidth 1/2 length	N.Obs.		Mean			
					(1)	(2)	(3)	(4)	Left (5)	Right (6)
Child characteristics:										
Female	0.017	0.010	<i>0.081</i>	509	24,429	24,626	0.49			
Baby weight	32.066	15.364	0.037	383	18,422	18,385	3353.32			
Special Nursery	-0.005	0.010	0.599	412	19,797	19,828	0.16			
NICU	-0.002	0.003	0.654	700	33,410	34,054	0.03			
PICU	0.000	0.001	0.762	468	22,436	22,639	0.00			
Neo-natal death	-0.001	0.002	0.479	302	14,413	14,437	0.00			
Apgar 1min	-0.047	0.038	0.212	423	20,265	20,292	8.14			
Apgar 5min	-0.034	0.023	0.127	418	20,052	20,022	9.06			
Gestational age	0.114	0.067	<i>0.088</i>	293	14,016	14,048	38.80			
Pre-term birth	-0.006	0.007	0.370	721	34,378	35,063	0.15			
Obstetric complication	-0.026	0.015	<i>0.095</i>	274	12,969	13,115	0.35			
C-section birth	0.007	0.012	0.547	708	33,741	34,438	0.32			
Private hospital	0.010	0.012	0.379	399	19,149	19,144	0.30			
Mother smoke	-0.050	0.082	0.544	353	15,492	15,463	10.40			
Number of ante-natal visits	0.003	0.008	0.665	618	28,878	29,581	0.16			
Parental characteristics:										
<i>Mother age:</i>										
35+	-0.003	0.008	0.731	592	28,164	28,673	0.19			
40+	-0.002	0.004	0.668	503	24,129	24,355	0.03			
<i>Father occupation:</i>										
High skilled	-0.002	0.010	0.805	672	30,479	30,909	0.33			
Low skilled	0.015	0.010	0.145	670	30,428	30,851	0.56			
<i>Mother marital status:</i>										
Never Married	0.008	0.007	0.234	531	25,422	25,670	0.10			
Married	-0.004	0.008	0.618	451	21,684	21,775	0.89			
Single	-0.003	0.003	0.204	447	21,514	21,611	0.01			
<i>Mother race:</i>										
Caucasian	0.000	0.006	0.962	512	24,562	24,788	0.86			
Asian	0.003	0.004	0.469	565	26,879	27,285	0.08			
Aboriginal or TSI	-0.004	0.004	0.413	470	22,546	22,747	0.06			

Note: This table presents the results of balancing tests on pre-treatment characteristics of children and their parents based on birth and perinatal records, when we do not exclude births within 7 days of the cutoff date. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004. p-values in bold indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.

A.1.3 Quantifying the window of birth shifting: a data-driven choice of "donut"

We have now confirmed that the introduction of the Baby Bonus in July 2004 did induce some birth shifting in South Australia, albeit to a lower extent than shown in Gans and Leigh (2009). We now discuss which births to exclude from our regression discontinuity analyses.

To determine the size of the sample to exclude, we use the following procedure based on a density continuity test. The principle of the density test is to assess whether the density of births is similar before and after the cutoff. Cattaneo, Jansson and Ma (2020) propose a density test based on local polynomial smoothing of the empirical cumulative density function of the running variable at the cutoff. We perform a series of non-parametric density continuity test, where we progressively exclude days closest to the cutoff date until we cannot reject the null that control and treated are similarly distributed.

Table A.4 reports the results of these tests when we exclude 0 days (Panel A), 1 day (Panel B) and 5 days (Panel C). Each panel reports the result of the density test according to three estimation methods: i) line 1: unrestricted inference with two distinct estimated bandwidths ("U, 2-h"), ii) line 2: unrestricted inference with one common estimated bandwidth ("U, 1-h"), and iii) line 3: restricted inference with one common estimated bandwidth ("R, 1-h"). In column (1) and (2) we read the estimated optimal bandwidths according to the matching method, and in columns (3) and (4) the number of observations on each side of the cutoff until their respective bandwidth. In column (5) we read p-values of the density test following each method. Panel A shows that there is evidence of manipulation in the running variable in the days close to the cutoff. We can reject the null at the 10% level that units before and after the cutoff are similarly distributed in 2 out of 3 methods. Panel B shows that excluding 1 day on each side of the cutoff reduces substantially this manipulation, as the p-values sharply increase in all three methods. Yet, we can still reject the null at the 5% for 1 method out of 3. Panel C shows that once we exclude 5 days on each side of the cutoff, control and treated units are not statistically differently distributed.²² Thus, we conclude that the minimum "donut" required for our estimations is 5 days around the cutoff.

A.1.4 The impact of birth shifting for our findings: results without "donut" restriction

Table A.5 presents the main results in the full sample. Comparing these results to our results in Table 5 indicates how much our results would be biased if we had omitted our donut in our main specification; this bias arises because of selection into treatment on observables and unobservables characteristics that are correlated with health outcomes. This bias has an ambiguous sign. On the one hand, one could expect babies whose birth was shifted to have better health at birth, but

²²Binomial tests confirm the findings of Table A.4.

worse health later in life or additional health problems that required hospital attention. Gans and Leigh (2009) show in particular that shifted babies were more likely to be postponed vaginal births eventually delivered by Caesarean section, and there is some evidence that babies delivered by C-section experience worse health outcomes.²³ On the other hand, babies whose birth was shifted may belong to income-constrained families, who may be less likely to use hospital care in the child's first year of life. Our estimated treatment effects in Table A.5 are slightly larger in magnitude than when we exclude births within 7 days of the cutoff (Table 5), and slightly smaller for PPPH presentations; these findings indicate that birth shifting events induced a small, negative bias on the true treatment effect of the ABB on hospital presentations in general, and a small positive bias on preventable hospitalization. This sign of the bias would suggest that babies whose birth was shifted are less likely than other babies to be exposed to hospitals in their first year of life, either because they are healthier babies or because of omitted variables driving both birth shifting behavior and demand for hospital care. The positive bias we find for PPPH presentations suggests rather the latter: babies born closest to the cutoff date have more PPPH presentations than babies born slightly later. Overall, failing to exclude births closest to the cutoff date, we would have over-estimated the impact of the ABB.

²³For example, 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).

Table A.4: Non-Parametric Density Test for Alternative Donuts

Estimation Method	Est. Bandwidth		Observations		Density Test
	Left	Right	Left	Right	p-val.
	(2)	(3)	(4)	(5)	(6)
Panel A. Excluding 0 days					
Unrestricted, asymmetric bandwidth	68	88	3,078	4,086	0.08
Unrestricted, symmetric bandwidth	88	88	4,082	4,086	0.04
Restricted, symmetric bandwidth	176	176	8,313	8,216	0.68
Panel B. Excluding 1 day					
Unrestricted, asymmetric bandwidth	69	90	3,080	4,115	0.12
Unrestricted, symmetric bandwidth	90	90	4,129	4,115	0.07
Restricted, symmetric bandwidth	153	153	7,205	7,099	0.56
Panel C. Excluding 5 days					
Unrestricted, asymmetric bandwidth	73	95	3,087	4,093	0.16
Unrestricted, symmetric bandwidth	95	95	4,190	4,093	0.14
Restricted, symmetric bandwidth	118	118	5,336	5,264	0.29

*Note: This table presents the result of three density tests of the running variable, date of birth, for three alternative donuts (Panel A, 0 days; Panel B, 1 day; Panel C, 5 days) around 1 July 2004, the cutoff date marking the introduction of the Australian Baby Bonus. These results implement the `tesCattaneo, Jansson and Ma (2020)` implemented through the Stata command `rddensity` (Cattaneo, Jansson and Ma, 2018). Col. 1 indicates the local polynomial fit method and the bandwidth estimation method. Col. 2 and 3 indicate the estimated bandwidth on either side of the cutoff (if applicable), and col. 4 and 5 indicate the number of observations used in the test on either side of the cutoff. Col. 6 presents the p-value of each density test comparing the distribution of births on each side of the cutoff to a Gaussian approximation. Large p-values indicate that the distribution of births on either side of the cutoff are not statistically different from one another. The sample used is the universe of children born in South Australia between 1 July 2003 and 1 July 2005, excluding 93 children born abroad during this time. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.*

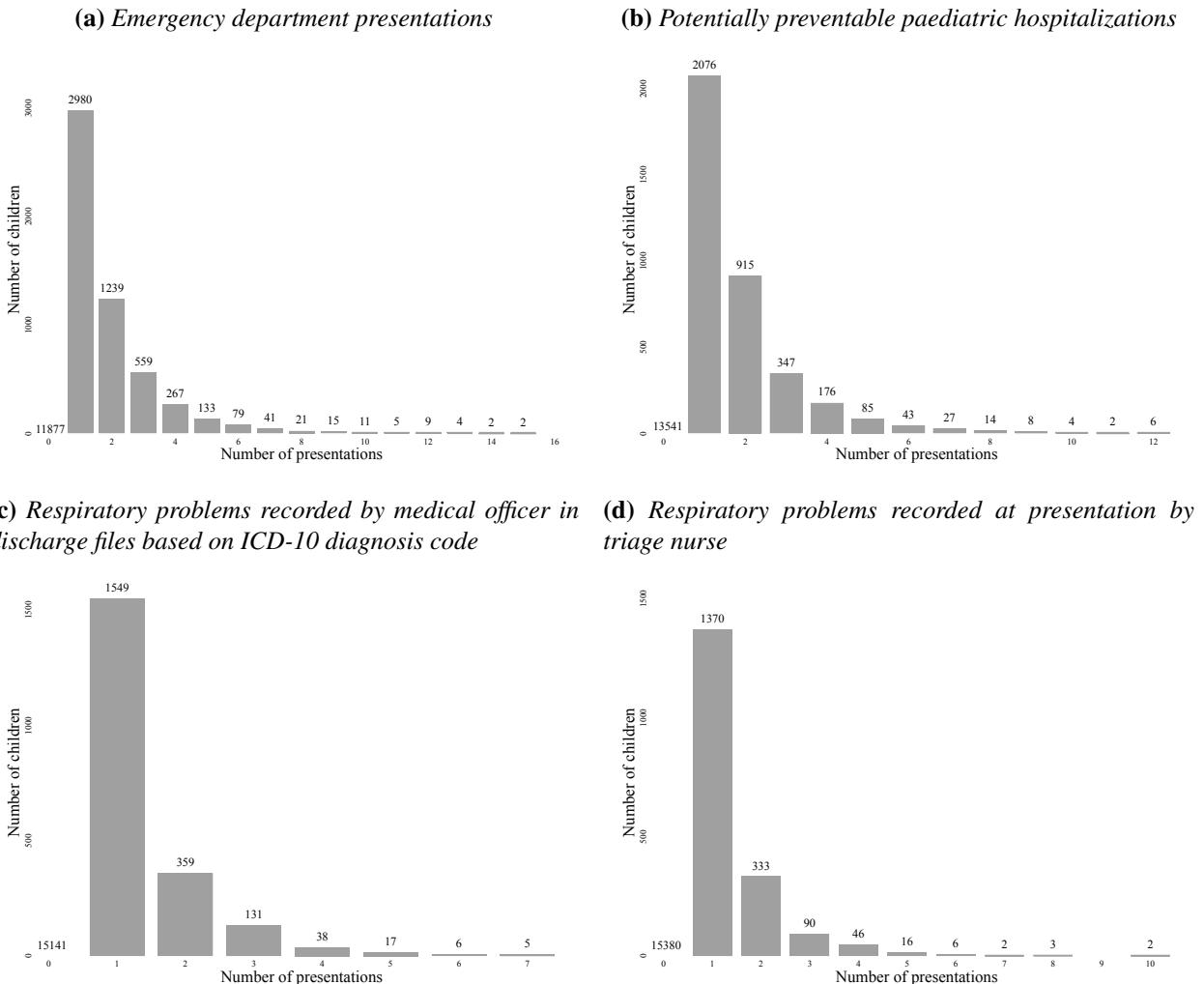
Table A.5: *The Effects of the Australian Baby Bonus on Hospital Presentations Within the First Year of Life Without Donut*

	Coef.	Sd.err.	p-value	Bandwidth 1/2 length	N.Obs.	Mean	
	Est.				Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Any presentation by hospital service:							
Any hospital service	-0.031	0.019	0.106	187	8,848	8,728	0.48
Emergency department	-0.024	0.016	0.123	163	7,667	7,652	0.36
Inpatient service	-0.039	0.016	0.014	224	10,524	10,518	0.30
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):							
<i>Emergency Department:</i>							
Emergency presentation	-0.021	0.015	0.156	184	8,691	8,602	0.24
Admission to ward	-0.028	0.011	0.013	196	9,237	9,148	0.13
<i>Inpatient Services:</i>							
Emergency presentation	-0.033	0.012	0.005	236	11,121	11,158	0.15
Admission to ward	0.004	0.007	0.497	176	8,313	8,286	0.02
Overnight admission	-0.026	0.013	0.038	279	13,191	13,314	0.20
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):							
<i>Emergency Department:</i>							
Planned visit	-0.005	0.005	0.270	160	7,519	7,508	0.01
Visit with med. referral	-0.002	0.007	0.835	172	8,147	8,147	0.05
<i>Inpatient Services:</i>							
Planned visit	-0.002	0.007	0.835	172	8,147	8,147	0.05
Visit with med. referral	-0.012	0.008	0.173	231	10,918	10,985	0.07
Booked elective procedure	0.002	0.006	0.737	242	11,433	11,419	0.05
Panel D. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):							
Any PPPH	-0.023	0.015	0.125	174	8,182	8,203	0.24
Any PPPH at ED	-0.029	0.014	0.037	152	7,161	7,135	0.21
Any PPPH at inpatient services	-0.015	0.010	0.109	193	9,138	9,040	0.08

Note: This table presents the effects of the ABB on hospital presentation of infants, when we do not exclude births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.

Appendix B Additional Tables and Figures

Figure B.1: Distribution of Hospitalizations at Ages 0-1



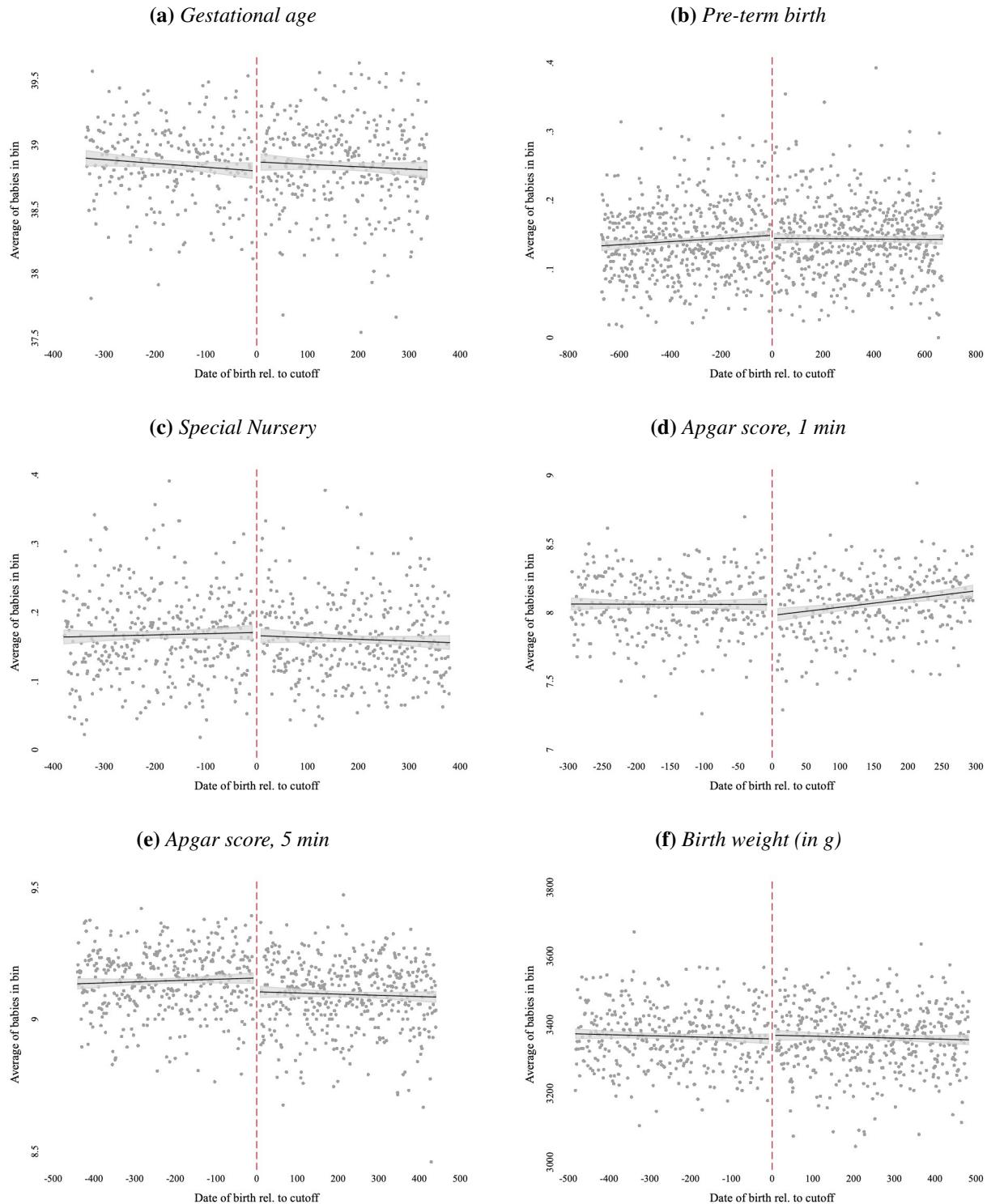
Source: Potentially Preventable Pediatric Hospitalizations: Integrated South Australian Activity Collection (ISAAC); ED Presentations and Respiratory ED: South Australian Emergency Department Data Collection (EDDC). Data are presented for the 2004 birth cohort, excluding 37 babies born overseas.

Table B.1: Density Test 2: Binomial Density Test

Window 1/2 Length	Furthest Day Away from Cutoff	Observations		Density Test p-val.
		Left	Right	
(1)	(2)	(3)	(4)	(5)
1	8	35	55	0.05
...
100	107	4,748	4,651	0.32
Share p-values < 0.1				0.03
Share p-values < 0.5				0.01

Note: This table presents the results of 100 nested binomial tests, performed using `rdwinselect`. In this procedure, we compare the number of daily births to a binomial distribution with mean 0.5. Col. 1 indicates the half-length of the sample evaluated, col. 2 the distance from the cutoff date of the furthest days in the sample evaluated. Col. 3 and 4 display the number of observations on each side of the cutoff in the sample evaluated, and col. 5 present p-values from the binomial test performed. Large p-values indicate that the distribution of births are not statistically different from a binomial distribution with mean 0.5. The sample used is the universe of children born in South Australia between 1 July 2003 and 1 July 2005, excluding 93 children born abroad during this time, and all children born within 7 days of 1 July 2004, the date of the introduction of the Australian Baby Bonus.

Figure B.2: RD Effects of the ABB and Balancing and Pre-Treatment Characteristics



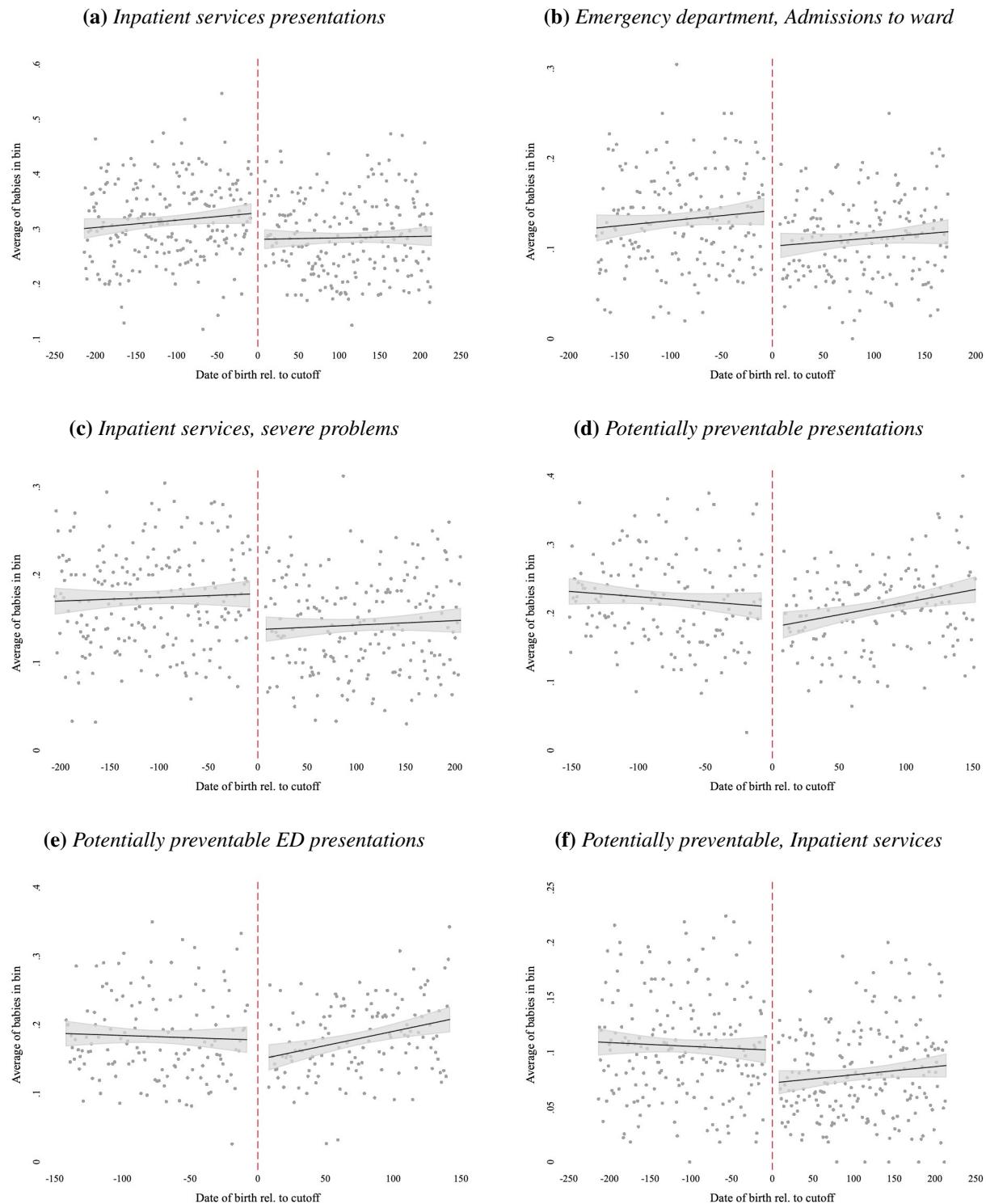
Note: These RDPlots present graphical evidence that the introduction of the ABB is not systematically associated with the pre-treatment characteristics and birth outcomes of babies. On every figure, the x-axis corresponds to birth dates, and the y-axis to the characteristic of interest. The red dashed line indicates the introduction date of the Baby Bonus (1 July 2004). These plots are produced with the `rdplot` Stata command (Calonico, Cattaneo and Titiunik, 2015; Calonico et al., 2017). We select the number of bins on each side of the cutoff using the evenly-spaced, variance-mimicking (ESMV) procedure and use for each outcome variable the CER-optimal bandwidth estimated using the `rdrobust` Stata command (Calonico, Cattaneo and Farrell, 2018, 2020; Calonico et al., 2017). The black line represents the local linear fit, using a triangular kernel. The grey shaded areas represent the 95% confidence interval around the mean of each bin. We exclude births within 7 days of the cutoff date.

Table B.2: *The Effects of the Australian Baby Bonus on Selective Abortions*

	Coef.	Sd.err.	p-value	Bandwidth	N.Obs.	Mean
	Est.			1/2 length		
	(1)	(2)	(3)	(4)	Left	Right
Past Pregnancies:						
Any past pregnancy	-0.015	0.016	0.337	313	14,639	14,529
Number of life births	-0.016	0.027	0.567	419	19,937	19,783
Any miscarriage	-0.011	0.011	0.311	439	20,859	20,742
Abortions:						
Any abortion	-0.006	0.007	0.340	671	31,792	32,261
Number of abortions	-0.007	0.010	0.508	664	31,378	31,841
Days since last abortion	2.316	31.659	0.942	395	8,946	8,554
						16,466

Note: This table presents additional balancing tests related to selective abortion and fertility decisions induced by the announcement of the Baby Bonus on 12 May 2004. Since announcement was so close to implementation of the policy on 1 July 2004, and since abortions are restricted in South Australia, we should not expect to find any significant differences between babies before and after the cutoff date in the distribution of pregnancy terminations among their mothers. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.

Figure B.3: RD Effects of the ABB on Hospital Presentations Within The First Year of Life



Note: These RDPlots present graphical evidence that the introduction of the ABB reduced hospital presentations within the first year of life for treated babies. On every figure, the x-axis corresponds to birth dates, and the y-axis to the outcome of interest. The red dashed line indicates the introduction date of the Baby Bonus (1 July 2004). These plots are produced with the *rdplot* Stata command (Calonico, Cattaneo and Titiunik, 2015; Calonico et al., 2017). We select the number of bins on each side of the cutoff using the evenly-spaced, variance-mimicking (ESMV) procedure and use for each outcome variable the CER-optimal bandwidth estimated using the *rdrobust* Stata command (Calonico, Cattaneo and Farrell, 2018, 2020; Calonico et al., 2017). The black line represents the local linear fit, using a triangular kernel. The grey shaded areas represent the 95% confidence interval around the mean of each bin. We exclude births within 7 days of the cutoff date.

Table B.3: Sensitivity of Main Results to Bandwidth Selection Method

Bandwidth Method:	CER-optimal, sym.		MSE-optimal, sym.		CER-optimal, asym.	
	Coef.	Sd.err.	Coef.	Sd.err.	Coef.	Sd.err.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Any presentation by hospital service:						
Any hospital service	-0.013	0.021	-0.020	0.020	-0.024	0.018
Emergency department	-0.024	0.020	-0.022	0.018	-0.010	0.016
Inpatient service	-0.034**	0.017	-0.044***	0.015	-0.047***	0.015
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):						
<i>Emergency Department:</i>						
Emergency presentation	-0.017	0.018	-0.020	0.016	-0.012	0.015
Admission to ward	-0.038***	0.013	-0.038***	0.012	-0.037***	0.011
<i>Inpatient Services:</i>						
Emergency presentation	-0.037***	0.013	-0.042***	0.012	-0.041***	0.012
Admission to ward	0.002	0.007	0.001	0.006	-0.001	0.005
Overnight admission	-0.022*	0.012	-0.027**	0.011	-0.034***	0.011
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):						
<i>Emergency Department:</i>						
Planned visit	-0.010*	0.005	-0.010**	0.005	-0.005	0.004
Visit with med. referral	0.002	0.009	-0.001	0.009	-0.003	0.007
<i>Inpatient Services:</i>						
Planned visit	-0.001	0.004	-0.005	0.004	-0.004	0.004
Visit with med. referral	-0.010	0.010	-0.016*	0.009	-0.012	0.008
Booked elective procedure	0.003	0.008	-0.001	0.007	0.000	0.006
Panel D. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):						
Any PPPH	-0.033*	0.019	-0.034**	0.017	-0.025	0.016
Any PPPH at ED	-0.032*	0.018	-0.030*	0.016	-0.019	0.015
Any PPPH at inpatient services	-0.028***	0.010	-0.034***	0.009	-0.027***	0.009

*Note: This table presents the effects of the ABB on hospital presentation of infants using three different bandwidth selection methods. Each row x panel represents a separate regression analysis, using our main specification - local linear estimation, robust bias-corrected inference and CER-optimal bandwidth, standard errors clustered at birth dates. For comparison purposes, Panel A recalls our main results. Panel B presents results under the MSE-optimal bandwidth, and Panel C under the two-sided CER-optimal bandwidth. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. *, **, and *** denote effects significance at the 10, 5 and 1% respectively.*

Table B.4: Sensitivity of Main Results to Observations Near the Cutoff

Exclude births within:	5 days		8 days		12 days		15 days	
	Coef.	Est.	Coef.	Est.	Coef.	Est.	Coef.	Est.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Any presentation by hospital service:								
Any hospital service	-0.017	0.020	-0.007	0.022	-0.010	0.022	0.005	0.024
Emergency department	-0.024	0.018	-0.019	0.020	-0.015	0.022	0.002	0.022
Inpatient service	-0.032*	0.017	-0.034**	0.017	-0.038**	0.017	-0.041**	0.018
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):								
<i>Emergency Department:</i>								
Emergency presentation	-0.015	0.017	-0.012	0.018	-0.008	0.020	0.007	0.021
Admission to ward	-0.035***	0.012	-0.037***	0.013	-0.036**	0.014	-0.027*	0.015
<i>Inpatient Services:</i>								
Emergency presentation	-0.032**	0.013	-0.037***	0.014	-0.039***	0.014	-0.036**	0.015
Admission to ward	0.006	0.008	0.001	0.007	-0.006	0.006	-0.010*	0.006
Overnight admission	-0.023*	0.012	-0.023*	0.013	-0.028**	0.013	-0.029**	0.013
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):								
<i>Emergency Department:</i>								
Planned visit	-0.007	0.005	-0.012**	0.005	-0.013**	0.006	-0.009	0.006
Visit with med. referral	0.003	0.009	0.001	0.010	-0.002	0.011	0.000	0.012
<i>Inpatient Services:</i>								
Planned visit	-0.001	0.004	0.002	0.005	0.009	0.006	0.003	0.006
Visit with med. referral	-0.010	0.009	-0.011	0.010	-0.015	0.011	-0.016	0.013
Booked elective procedure	0.003	0.007	0.003	0.008	0.001	0.008	-0.006	0.008
Panel D. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):								
Any PPPH	-0.029*	0.017	-0.027	0.019	-0.023	0.020	-0.005	0.020
Any PPPH at ED	-0.030*	0.016	-0.027	0.018	-0.021	0.018	-0.005	0.018
Any PPPH, inpatient services	-0.026***	0.010	-0.029***	0.010	-0.03***	0.011	-0.028**	0.012

*Note: This table presents the sensitivity of our main results to observations close to the cutoff, that is, sensitivity to our choice of "donut". In Panel A, results also exclude all births within 5 days of 1 July 2004; Panel B: 8 days; Panel C: 12 days; Panel D: 15 days. Each donut has the same number of weekend days on each side of the cutoff date. Each line corresponds to a separate regression using our main specification - local linear estimation, robust bias-corrected inference and CER-optimal bandwidth, standard errors clustered at birth dates. We exclude children born overseas. *, **, and *** denote effects significance at the 10, 5 and 1% respectively.*

Table B.5: Main Results and Placebo Cutoffs

	Coef. Est.	Sd.err. 1/2 length	Bandwidth at true cutoff	Est. rank	P-value		
				(4)	Asymp.	Rand.-based	
				(1)	(2)	(3)	(5)
Panel A. Any presentation by hospital service:							
Any hospital service	-0.013	0.021	175.328	91	0.532	1.006	
Emergency department	-0.024	0.020	152.106	5	0.230	0.055	
Inpatient service	-0.034	0.017	214.581	63	0.040	0.696	
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):							
<i>Emergency Department:</i>							
Emergency presentation	-0.017	0.018	171.878	37	0.369	0.409	
Admission to ward	-0.038	0.013	173.189	4	0.004	0.044	
<i>Inpatient Services:</i>							
Emergency presentation	-0.037	0.013	206.698	9	0.005	0.099	
Admission to ward	0.002	0.007	205.989	158	0.761	0.265	
Overnight admission	-0.022	0.012	298.843	83	0.074	0.917	
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):							
<i>Emergency Department:</i>							
Planned visit	-0.010	0.005	146.500	4	0.074	0.044	
Visit with med. referral	0.002	0.009	151.947	108	0.845	0.818	
<i>Inpatient Services:</i>							
Planned visit	-0.001	0.004	289.355	123	0.882	0.652	
Visit with med. referral	-0.010	0.010	198.265	94	0.308	0.972	
Booked elective procedure	0.003	0.008	230.225	162	0.657	0.221	
Panel D. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):							
Any PPPH	-0.033	0.019	152.639	10	0.083	0.110	
Any PPPH at ED	-0.032	0.018	142.116	11	0.071	0.122	
Any PPPH at inpatient services	-0.028	0.010	214.769	24	0.005	0.265	

Note: This table presents the results of permutation tests following Ganong and Jäger (2018). Each line corresponds to the result of a separate regression using our main specification at the true cutoff. We use local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. For each outcome variable, col. 5 presents the rank of the coefficient estimate at the true cutoff compared to coefficient estimates at 180 alternative cutoffs, ranging from -90 to +90 around the true cutoff. Col. 6 presents randomization-based p-values.

Table B.6: Robustness of Main Results to Multiple Hypothesis Testing

	P-Values		
	Original (1)	Bootstrap (2)	Stepdown (3)
Panel A. Any presentation by hospital service:			
Any hospital service	0.532	0.393	0.938
Emergency department	0.230	0.122	0.599
Inpatient service	0.040	0.004	0.096
Panel B. Any presentation deemed urgent/severe by hospital staff:			
<i>Emergency Department:</i>			
Emergency presentation	0.369	0.240	0.812
Admission to ward	0.004	0.002	0.014
<i>Inpatient Services:</i>			
Emergency presentation	0.005	0.002	0.020
Admission to ward	0.761	0.743	0.986
Overnight admission	0.074	0.040	0.214
Panel C. Planned visits/presentations with medical referral:			
<i>Emergency Department:</i>			
Planned visit	0.074	0.012	0.214
Visit with med. referral	0.845	0.800	0.986
<i>Inpatient Services:</i>			
Planned visit	0.882	0.882	0.986
Visit with med. referral	0.308	0.122	0.747
Booked elective procedure	0.657	0.573	0.984
Panel D. Potentially Preventable Pediatric Hospitalization:			
Any PPPH	0.083	0.018	0.214
Any PPPH at ED	0.071	0.016	0.208
Any PPPH at inpatient services	0.005	0.002	0.020

*Note: This table presents p-values of RD treatment effects of the ABB on hospital presentations of infants, under alternative inference methods. Our main specification uses local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. Col. 1 indicates p-values obtained from our main estimation method; col. 2 presents bootstrap p-values using 500 replications; col. 3 presents Romano-Wolf p-values corrected for familywise error rate (see Clarke, Romano and Wolf, 2020; Romano and Wolf, 2005, 2016). We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in italic indicate effects that are statistically significant at the 10% level.*

Table B.7: *The Effects of the Australian Baby Bonus on Number of Hospital Presentations Within the First Year of Life*

	Coef.	Sd.err.	p-value	Bandwidth	N.Obs.		Mean
	Est.			1/2 length	Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Any presentation by hospital service:							
Any hospital service	-0.150	0.086	0.082	146	6,561	6,453	1.13
Emergency department	-0.121	0.064	0.060	112	4,944	4,879	0.69
Inpatient service	-0.060	0.032	0.061	267	12,362	12,371	0.44
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):							
<i>Emergency Department:</i>							
Emergency presentation	-0.059	0.029	0.038	161	7,272	7,126	0.37
Admission to ward	-0.060	0.021	0.005	159	7,194	7,016	0.18
<i>Inpatient Services:</i>							
Emergency presentation	-0.059	0.022	0.008	215	9,857	9,677	0.21
Admission to ward	0.004	0.009	0.655	202	9,265	9,051	0.03
Overnight admission	-0.031	0.019	0.109	305	14,297	14,152	0.25
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):							
<i>Emergency Department:</i>							
Planned visit	-0.013	0.007	0.059	144	6,480	6,355	0.01
Visit with med. referral	0.000	0.012	0.971	146	6,619	6,497	0.06
<i>Inpatient Services:</i>							
Planned visit	0.000	0.007	0.997	265	12,245	12,269	0.02
Visit with med. referral	-0.023	0.021	0.274	191	8,704	8,505	0.10
Booked elective procedure	0.001	0.013	0.961	217	9,921	9,731	0.06
Panel D. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):							
Any PPPH	-0.110	0.041	0.007	143	6,480	6,355	0.43
Any PPPH at ED	-0.079	0.035	0.024	120	5,302	5,221	0.33
Any PPPH at inpatient services	-0.047	0.014	0.001	236	10,855	10,779	0.10

Note: This table presents results on the effects of the ABB on the number of hospital presentations within the first year of life. Each line corresponds to a separate regression using our main specification–local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus.

Table B.8: *The Effects of the Australian Baby Bonus on Hospital Presentations at Ages 1 to 5*

Child age:	1 to 2		2 to 3		3 to 4		4 to 5	
	Coef.	Est.	Sd.err.	Coef.	Est.	Sd.err.	Coef.	Est.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Any presentation by hospital service:								
Any hospital service	-0.006	0.031	0.004	0.019	-0.022*	0.013	0.010	0.009
Emergency department	-0.020	0.033	-0.001	0.021	-0.019	0.013	0.013	0.009
Inpatient service	0.007	0.011	0.001	0.008	-0.005	0.006	-0.002	0.006
Panel B. Any presentation deemed urgent/severe by hospital staff (by hospital service):								
<i>Emergency Department:</i>								
Emergency presentation	-0.030	0.022	-0.001	0.015	-0.004	0.009	0.007	0.006
Admission to ward	-0.007	0.013	0.004	0.009	-0.008	0.007	-0.001	0.006
<i>Inpatient Services:</i>								
Emergency presentation	0.002	0.011	0.000	0.008	-0.007	0.005	-0.002	0.005
Admission to ward	-0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.001
Overnight admission	-0.001	0.005	-0.001	0.004	0.001	0.003	0.001	0.002
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):								
<i>Emergency Department:</i>								
Planned visit	-0.007	0.005	0.008**	0.004	0.001	0.002	-0.001	0.001
Visit with med. referral	-0.004	0.008	0.003	0.007	0.001	0.003	0.001	0.004
<i>Inpatient Services:</i>								
Planned visit	0.017***	0.007	-0.001	0.004	0.000	0.004	0.002	0.003
Visit with med. referral	0.013	0.008	0.003	0.005	0.002	0.005	0.000	0.004
Booked elective procedure	0.016**	0.007	0.002	0.004	0.002	0.005	0.000	0.004
Panel D. Any presentations for Potentially Preventable Pediatric Hospitalization (by hospital service):								
Any PPPH	-0.017	0.022	0.014	0.016	-0.002	0.010	-0.002	0.008
Any PPPH at ED	-0.026	0.022	0.013	0.014	0.009	0.010	-0.003	0.008
Any PPPH, inpatient services	-0.002	0.012	-0.005	0.007	-0.009**	0.005	-0.001	0.004

Note: This table presents RD treatment effects of the Baby Bonus on later life hospital presentations. Each line \times Panel corresponds to a separate regression using our main specification, using local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. Panel A presents results in the 2nd year of life, Panel B the 3rd year, Panel C the 4th year and Panel D the 5th year of life. *, **, and *** denote effects significance at the 10, 5 and 1% respectively.

Table B.9: *The Effects of the Australian Baby Bonus on Diagnostics at Ages 1 to 5*

Child age:	1 to 2		2 to 3		3 to 4		4 to 5	
	Coef.	Est.	Coef.	Est.	Coef.	Est.	Coef.	Est.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Potentially Preventable Pediatric Hospital Presentations (combined ED/Inpatient):								
Bronchiolitis	-0.003	0.007	-0.002	0.002	0.000	0.001	0.000	0.000
Gastroenteritis	0.015	0.012	0.007	0.008	-0.001	0.005	-0.001	0.003
Laryngitis	0.002	0.008	0.004	0.004	-0.001	0.004	0.001	0.003
Otitis media	-0.011*	0.006	0.001	0.004	0.001	0.003	0.001	0.002
Respiratory infection	-0.026*	0.014	-0.003	0.009	0.004	0.006	0.004	0.003
Panel B. Presentations by ICD-10-AM diagnostic chapter and Presenting problem:								
<i>Emergency Department:</i>								
Respiratory problems	-0.015	0.016	0.004	0.011	0.003	0.006	0.003	0.004
Injuries, Trauma & Poisoning	-0.026*	0.016	-0.002	0.008	0.002	0.005	-0.002	0.005
Infections	-0.019	0.019	0.009	0.007	-0.006	0.006	0.004	0.003
Digestive problems	0.006	0.004	-0.005	0.004	-0.001	0.002	0.000	0.002
Skin problems	0.000	0.005	-0.001	0.003	0.000	0.002	0.000	0.002
Unspecified problems	-0.023*	0.012	0.000	0.007	-0.003	0.005	0.001	0.005
Nervous system problems	0.000	0.002	0.000	0.002	0.000	0.001	-0.001	0.001
Eyes and Ears problems	-0.011*	0.006	0.002	0.004	0.003	0.003	-0.001	0.003
<i>Inpatient Services:</i>								
Respiratory problems	-0.001	0.008	0.002	0.005	-0.002	0.003	0.002	0.003
Injuries, Trauma & Poisoning	-0.002	0.004	0.006*	0.003	-0.002	0.002	-0.001	0.002
Infections	0.005	0.006	-0.009*	0.005	0.000	0.002	-0.002	0.002
Digestive problems	0.003*	0.002	-0.001	0.002	-0.002	0.002	-0.002	0.002
Skin problems	-0.002	0.002	-0.001	0.001	0.000	0.001	0.001	0.001
Externally caused problems	0.003	0.003	0.003	0.002	-0.001	0.002	0.000	0.001
Unspecified problems	0.005	0.004	0.007***	0.002	-0.001	0.002	0.000	0.001
Nervous system problems	0.001	0.002	-0.002	0.002	-0.004**	0.002	-0.002*	0.001
Eyes and Ears problems	0.002	0.004	-0.002	0.003	0.002	0.002	0.000	0.002

Note: This table presents RD treatment effects of the Baby Bonus on diagnostic for presentations at ages 2 to 5. Each line x Panel corresponds to a separate regression using our main specification, using local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004, the cutoff date marking the implementation of the Australian Baby Bonus. Panel A presents results in the 2nd year of life, Panel B the 3rd year, Panel C the 4th year and Panel D the 5th year of life. *, **, and *** denote effects significance at the 10, 5 and 1% respectively.

Table B.10: *The Effects of the Australian Baby Bonus on Hospital Presentations by Socio-Economic Status*

Sample split:	Father occupation:			
	High skilled		Low skilled	
	Coef. Est.	Sd.err.	Coef. Est.	Sd.err.
	(1)	(2)	(3)	(4)
Panel A. Any presentation by hospital service:				
Any hospital service	0.031	0.038	-0.054*	0.028
Emergency department	0.020	0.041	-0.042*	0.025
Inpatient service	-0.011	0.024	-0.063**	0.024
Panel B. Any presentation deemed urgent/severe by hospital staff:				
<i>Emergency Department:</i>				
Emergency presentation	-0.028	0.029	-0.020	0.022
Admission to ward	-0.023	0.021	-0.051***	0.018
<i>Inpatient Services:</i>				
Emergency presentation	-0.025	0.020	-0.054***	0.018
Admission to ward	0.003	0.008	0.003	0.009
Overnight admission	0.016	0.026	-0.037*	0.021
Panel C. Any planned visits or presentations with referral :				
<i>Emergency Department:</i>				
Planned visit	0.003	0.010	-0.010	0.007
Visit with med. referral	0.008	0.015	-0.001	0.013
<i>Inpatient Services:</i>				
Planned visit	0.007	0.008	-0.005	0.007
Visit with med. referral	-0.009	0.014	-0.012	0.015
Booked elective procedure	0.003	0.011	0.004	0.012
Panel D. Any presentations for PPPH:				
Any PPPH	0.009	0.033	-0.049*	0.027
Any PPPH at ED	0.008	0.033	-0.048**	0.023
Any PPPH, inpatient services	-0.024	0.016	-0.028**	0.012

Note: This table presents RD treatment effects of the Baby Bonus on hospital presentations across SES at birth. Each line x Panel corresponds to a separate regression using our main specification, local polynomial estimation and robust bias-corrected inference methods, CER-optimal bandwidth with standard errors clustered at the level of birth dates. We exclude children born overseas, and all births within 7 days of 1 July 2004. *, **, and *** denote effects significance at the 10, 5 and 1% respectively.