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

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Abstract

We estimate the impact on child health of the unanticipated introduction of the Australian Baby Bonus, a one-time $3,000 unconditional cash transfer given at birth. With population-level administrative data from South Australia and a regression discontinuity design, we find that eligible infants had fewer hospital presentations by age one for preventable, acute, and severe problems. Our auxiliary analyses using nationally-representative data suggest that parents increased spending on food and groceries, experienced less financial stress and hardship, and improved physical and mental health. We calculate that 34% of the payout was recouped within the first year due to lower healthcare costs.

JEL: I12, I14, I18, I32, I38

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

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1 Introduction

It is well established that poverty in early childhood has large negative consequences on educational attainment, labour market outcomes, health, criminality in adulthood, and longevity (Almond and Currie, 2011; Almond, Currie and Duque, 2017, 2018). A key moment in a child's life when poverty is particularly harmful is at birth, a moment when parents experience a sharp rise in stress that is only exacerbated by the intense financial pressures associated the arrival of a newborn in the household. Unconditional cash transfers promise a simple way to alleviate those financial pressures with little concern for take-up, albeit with large upfront public costs.

Several governments worldwide (e.g., Australia, Québec, Singapore, Spain, France, Poland) have opted to pay baby bonuses, a one-off cash transfer offered to families at the birth of a child with the aim of alleviating the perceived financial pressures of raising a child (Parr and Guest, 2011; McDonald, 2006*a*,*b*). Baby bonuses are administered through the tax and welfare system, easy to adjust when needed, and easy to cancel in times of fiscal austerity. Their small scale and one-off nature are not intended to change permanent income or life-cycle consumption, saving, and investment behaviors. Yet a large enough one-off cash transfer can help overcome the major initial expenses after the birth of child that could crowd out other expenditures. 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, these transfers also entail the risk that parents use the cash to consume nonessential or even risky goods that may result in unintended negative externalities for children (Currie and Gahvari, 2008).

In this paper, we estimate the effect of family income at birth on child health care utilization and child health in early childhood. Our setting offers a unique opportunity to implement a highly credible research design using detailed population-level hospital records on child health along with detailed parental behaviors as mechanisms. To answer our research question, we estimate regression discontinuity models of the effect of being born just before versus just after the implementation of the Australian Baby Bonus (ABB)—an unconditional cash transfer paid at birth—on children's health care utilization and health status in the first five years of life. We use population-level perinatal records, birth records, emergency department and inpatient services records in all public hospitals in South Australia. Our data are exceptionally rich and allow us to analyze the entire spectrum of presenting problems for children from birth onward. They allow us to identify longitudinally and in depth the nature of the hospitalization down to the diagnosis, the urgency and severity of the health problem, and whether the presentation was planned, such as with a referral or for elective care.[1]

[1]This level of detail allows us to distinguish between areas of health that can be affected directly by a windfall cash payment (e.g., money spent on extra health care to treat a preexisting condition, which is not fully covered by universal health care), or indirectly (e.g., money spent on alcohol, which increases trauma presentations at the

The Baby Bonus was an unconditional cash transfer initially amounting to \$3,000[2] (US\$2,400), a small yet economically meaningful windfall income for families at a time when Australia was the only OECD country other than the United States without paid parental leave legislation. It represented 2.5 times the weekly median disposable household income in 2004 and 5.3 times the weekly disposable household income of families in the lowest income decile. The Baby Bonus was discontinued in 2014, shortly after the introduction of paid parental leave. Almost all eligible households (over 95%) claimed the payment (see Drago et al., 2011, p. 383) and received it within 14 days of the birth of the child. For identification, we exploit the unexpected introduction of the Baby Bonus shortly before its implementation on July 1, 2004, which is also the birth date eligibility threshold.

We find that the Baby Bonus led to a decline of 0.098 standard deviations in our index of health care utilization for treated babies in their first year of life—an economically meaningful effect in a country with universal access to health care and a high standard of living. Our main finding also suggests that the Baby Bonus led to improvements in the health status of treated babies because our index captures presentations for urgent, acute, and severe problems. In particular, we show that the overall decline in health care utilization is driven by a decline in presentations for urgent, acute, and severe problems that require hospital admissions and overnight stays. This finding is important because it is unbiased by parental sorting because those outcomes are not substituable by out-of-hospital care and are free of charge for all Australian residents.

Our findings are robust to several sensitivity checks, with our main estimate ranging from -0.05 to -0.31 and averaging at -0.11 standard deviations in health care utilization. Our preferred specification excludes all births within seven days of the birth date eligibility threshold to address identification concerns related to birth shifting.[3] Our results are robust to using alternative donuts (from 5 to 15 days), using alternative data-driven optimal-bandwidths (CER-, MSE-, asymmetric CER- and half CER-optimal bandwidths) and researcher-chosen bandwidths (60- and 90-day bandwidths), considering selective pregnancy terminations, conducting permutation tests with placebo thresholds (+/– 90 days) and placebo years prior to the policy, as well as multiple hypothesis testing corrections for the familywise error rate (Romano and Wolf, 2005*a*,*b*, 2016; Holm, 1979), and to alternative ways of clustering standard errors across both MSE- and CER-optimal bandwidths.

Back-of-the-envelope calculations indicate that the Baby Bonus represented a large return on investment due to reduced hospital care utilization. We find that 34% of the initial payout of the

emergency department).

[2]All amounts are in Australian dollars unless otherwise noted.

[3]See Section 4 and additional details in Appendix A). Our results are robust to using the 15-day donut recommended by Jacobson, Kogelnik and Royer (2021) for settings where births can be shifted both before and after the threshold (although setting like ours, as noted by Jacobson, Kogelnik and Royer (2021), the financial incentives tied to the birth date eligibility threshold of the cash payment create no incentives for parents to shift a birth earlier)

Australian Baby Bonus was recouped in the first year of life of eligible children. Positive income shocks early in life may reduce the economic burden to society through medical expenditure savings in the longer run. Our estimates also likely underestimate the true return on investment of the Baby Bonus because our calculations are based on hospitalizations for severe, acute, and urgent problems, and do not include less severe, but still costly, presentations. Better calculations in the future should also consider improvements along the entire spectrum of health problems, using for example, Medicare expenditures or primary care data, and consider dynamic effects at later ages.[4]

We explore the two categories of mechanisms hypothesized in past literature: the “resources channel”—the direct impact of additional income that allows carers to purchase more goods and services—and the “family process channel”—the indirect impact of income on the psychological well-being of the family, which allows parents to spend more time with children in productive activities (Mayer, 1997; Yeung, Linver and Brooks-Gunn, 2002; Milligan and Stabile, 2011). We show evidence of both mechanisms at play using our population-level administrative records and additionally using supplementary data from the Household, Income and Labour Dynamics in Australia (HILDA), Australia’s nationally-representative longitudinal household survey. The HILDA offers unique measures of household expenditures, financial stress and hardship, along with detailed measures of parental subjective physical and mental health, marital relationships, labor supply and child care intentions. Although our sample size is small, we find suggestive evidence supporting both the “resources channel” and the "family process channel". First, the Baby Bonus allowed families to increase expenditures on food and groceries and decreased the incidence of financial stress and financial hardship (resources channel).

Second, our findings using administrative records suggest that the Baby Bonus allowed parents to invest more in preventive health. We show that our main result is driven by a reduction in hospital presentations deemed "potentially preventable" by a doctor, in particular acute bronchiolitis, a respiratory health problem that is the most common type of potentially preventable pediatric presentation for infants and that can cause asthma in older children. To perform this analysis, we exploit a unique feature of our hospital records, which associates a binary indicator for a "potentially preventable pediatric problem" to each diagnosis code included in each presentation record. This indicator marks hospital presentations that a medical doctor deemed preventable if parents had taken appropriate actions, that is, parental behaviors that should have prevented babies’ conditions from becoming acute or severe and thus preventing hospital presentations for those problems. Typically the indicator is positive for vaccine-preventable conditions, acute conditions, and chronic conditions that parents could have prevented with appropriate use of

[4]For example through early detection and prevention of respiratory problems. There is recent evidence 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 of acute care (see Busse, Lemanske Jr. and Gern (2010) for an overview).

primary care. In addition, the magnitude of our effects is larger for children from disadvantaged backgrounds, which is consistent with the Baby Bonus alleviating income constraints. Finally, using the HILDA, we find that the Baby Bonus increased marital stability and improved parental self-assessed physical and mental health; we also find suggestive evidence of an increase in child care use for older children in the family but no discernible impact on maternal intended labor supply. Our findings combined suggest that parental investments play an important role in explaining the decline in health care utilization and the increase in health status of infants as a result of the introduction of the Baby Bonus.

This paper contributes to a strand of literature focused on baby bonus policies, which has largely centered on birth shifting as an unintended consequence of financial incentives associated with baby bonuses. Yet, surprisingly little is known about the impact of baby bonuses on children's health outcomes and their parents' responses. With the exception of Borra et al. (2021), past studies have paid little attention to the impact of receiving those payments on children's human and health capital, but have instead focused much attention on birth manipulation induced by financial incentives. Most of the evidence on baby bonus policies originates from the Spanish and Australian experiences. The main findings of this literature are that baby bonus policies can i) cause small increases in fertility (through abortions and conceptions) (Sinclair, Boymal and De Silva, 2012; González, 2013; González and Trommlerová, 2021) and childbearing intentions, especially for women from lower-income households (Risse, 2010); ii) allow mothers to stay home longer after the birth of a child (González, 2013); and iii) have none to modest medium-term impacts on children's human capital formation, especially for children from disadvantaged backgrounds (Deutscher and Breunig, 2018; Borra et al., 2021). The bulk of the literature has focused on the unintended consequences of announcing the introduction of baby bonus policies (Gans and Leigh, 2009), 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 Sapnish baby bonus led to worse health outcomes for shifted infants. The announcement of a birth threshold date for eligibility for the payment creates small incentives for parents to shift the birth date of a baby in utero, potentially harming the unborn child. Parents may gain from advancing the date of a birth by induction to benefit from a cash transfer before its cancellation date, or from postponing the date of a birth to become eligible for a cash transfer before its implementation date. Importantly however, while it is relatively easy to advance the date of a birth by induction, it is not so easy to postpone the birth of a child. This feature of our natural experiment allows us exclude shifted births and to focus on the impact of the cash injection alone on child health outcomes and their parents' responses.[5] To the best of our knowledge, no other study has focused on estimating the

[5] While Gans and Leigh (2009) show that some birth shifting occurred with the introduction of the Australian Baby Bonus, Deutscher and Breunig (2018) find precisely estimated no impact of birth shifting itself on children's health or educational outcomes in Australia.

impact of baby bonuses on children's health outcomes and to study parental behavioral change as mechanisms.

This study also contributes to the growing consensus that social safety nets and social programs in general and cash injections in particular should be viewed as an investment in children that have large positive returns (Bailey et al., 2020; Hoynes and Schanzenbach, 2018; Hoynes, Schanzenbach and Almond, 2016; Aizer, Hoynes and Lleras-Muney, 2022). Our findings contribute to a long-standing literature on the effectiveness of government social assistance, which has shifted its focus in recent years to investigate the impact of social programs on children's birth outcomes, health, human capital, and well-being into adulthood (Aizer et al., 2016; Barr, Eggleston and Smith, 2022). This newer evidence concerns baby bonus payments (Deutscher and Breunig, 2018; González, 2013; Borra et al., 2021; Cygan-Rehm and Karbownik, 2022), earned-income tax credits and cash transfers (Hoynes, Miller and Simon, 2015; Hoynes, Schanzenbach and Almond, 2016; Dahl and Lochner, 2012; Currie and Almond, 2011; Milligan and Stabile, 2011; Duncan, Morris and Rodrigues, 2011; Amarante et al., 2016; Barr, Eggleston and Smith, 2022), nutritional assistance programs (Almond, Hoynes and Schanzenbach, 2011; East, 2020; Barr and Smith, 2023) and paid maternity leave (Baker and Milligan, 2010; Rossin, 2011; Dustmann and Schönberg, 2012; Carneiro, Løken and Salvanes, 2015) among others. In particular, Barr, Eggleston and Smith (2022) shows that small cash injections at birth increase adult earnings by up to 2%, but this study is limited in the number of mechanisms it can explore. Our study is unique in providing evidence on how parental investments in response to cash transfers can play a major role in generating positive returns to cash transfer policies.

More generally, this study also contributes to improving our understanding of the channels through which household income matters for children's health and human capital outcomes (Currie and Almond, 2011; Almond, Currie and Duque, 2018; Cesarini et al., 2016; Kuehnle, 2014; Currie, 2009; Case, Lee and Paxson, 2008; Currie, Shields and Price, 2007; Propper, Rigg and Burgess, 2007; Currie and Stabile, 2003; Case, Lubotsky and Paxson, 2002; Yeung, Linver and Brooks-Gunn, 2002). Identifying the causal impact of household income on children's health has been difficult because few compelling randomization experiments exist, with the exception of the ongoing "Baby's First Years" randomized control trial (see e.g., Noble et al., 2021). Previous credible evaluations have exploited lottery winnings (Cesarini et al., 2016), instrumental variable approaches (Kuehnle, 2014) or natural experiments, such as welfare expansions (Duncan, Morris and Rodrigues, 2011), Earned Income Tax Credit expansions in the United States (see e.g., Hoynes, Miller and Simon, 2015; Dahl and Lochner, 2012), tax benefits such as the Canada Child Tax Benefit (Milligan and Stabile, 2011), and even casino windfalls (Akee et al., 2010). This strand of literature focuses on large shocks to household income that are more permanent in nature, which often affect working parents; surprisingly few studies focus on the impact of a small one-off unconditional cash transfer that do not change permanent income but simply buffer short-term financial stress. Jacob et al. (2022) and Pilkauskas et al. (2022) are two particularly relevant studies that investigate in-depths responses of disadvantaged households to a short-run cash injection in the United States during the COVID-19 crisis. Both studies find only suggestive evidence regarding material hardship and mental health. In contrast, our findings on child health and mechanisms indicate that even a small, one-off, and unconditional payment can have a meaningful impact on child health and their parents.

2 Institutional Background

The Australian Baby Bonus was an $3,000 unconditional and nontaxable lump sum offered to parents for each birth (or adoption of a child under two years) on or after July 1, 2004. The Australian Government announced it on May 11, 2004 in the new budget—just a short time period before its implementation. The primary intention of the policy was to boost fertility by absorbing part of the (perceived) costs associated with the birth of a child. The introduction of the Baby Bonus 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.[6]

The Baby Bonus 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 incomes. 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 Baby Bonus represented a one-time increase in the median disposable household income for families who had a baby born in 2004 of almost 5%.[7]

Between its introduction and abolition on March 1, 2014, the program underwent important structural changes, which included subsequent increases to $4,000 and $5,000 on July 1, 2006 and July 1, 2008, respectively. As of 2009, it became means-tested and thus from this point forward only accessible to families with incomes of $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 Baby Bonus in 13 fortnightly installments instead of an up-front payment, and it was progressively rolled out to the entire population.

[6] The reason is that babies born on or after July 1, 2004, were in utero on the day of announcement. The first babies conceived after May 11, 2004 in response to the announcement could not have been born before February 2005, assuming full-term gestation of 37 weeks and over.

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

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

The Baby Bonus replaced two family benefits, the Maternity Allowance and the First Child Tax Refund (also referred to as the "Baby Bonus" at the time). Therefore, the Baby Bonus did not represent a net increase of $3,000 for all households (Deutscher and Breunig, 2018). The Maternity Allowance was a subsidy of $843 per child as part of the Family Tax Benefits (FTB) available to mothers with modest incomes. The First Child Tax Refund was introduced for babies born on or after July 1, 2002. It allowed mothers leaving the workforce to claim income taxes paid in the year prior to the birth of the first child born between July 1, 2001 and June 30, 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 with 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 Baby Bonus 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 (over 95%) received the payment (see Drago et al., 2011, p. 383). 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. In relative terms, the policy was more favorable to low- and middle-income households. According to Deutscher and Breunig (2018), 75% families with babies born in June 2004 would have been financially better off under the new policy had it been in effect at that time.

The effect of the Baby Bonus 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, under which 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 prescriptions. 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). Although dental or other ancillary services are not covered, children are fully covered under Medicare as well and receive additional free services regarding dental care, immunizations, disability, autism, and vision impairment.[8]

3 Data

3.1 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 contain detailed demographic characteristics of mothers, fathers, and children, as well as detailed pregnancy histories of mothers (including maternal gestational health, smoking behavior during pregnancy, and past pregnancies). These data are primarily sourced from the Perinatal Statistics Collection and supplemented and validated by Births Registry data.

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 cover the universe of admissions to inpatient services (IS) in public hospitals from July 2001 to 2014. The EDDC data cover the universe of admissions to emergency departments (ED) in public hospitals from July 2003 to 2014 (Nuske et al., 2016; South Australian Emergency Department Activity Data Standards, Government of South Australia, 2014). Both datasets 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, whether it is a first admission,

[8]For more details, see https://www.servicesaustralia.gov.au/individuals/subjects/whos-covered-medicare/childrens-health-care. 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).

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.

3.2 Data Limitations

The administrative records used in this paper present both advantages and disadvantages. A first concern is that by focusing on inpatient services and emergency care, we only observe censored health outcomes due to selection into hospital care. Ideally we would have both health measures capturing underlying health status and additional measures capturing access to hospital care. To limit the risk that selection would bias our conclusions, we focus on acute and potentially acute health conditions leading to presentations at emergency departments or inpatient services through an emergency department admission. For those outcomes it is extremely unlikely that parents would use primary care before presenting to emergency care. We discuss our outcome variables in detail below and further discuss selection concerns in Section 7.2.

A second concern is that we only use public hospital records, which might also provide a partial picture due to selection into public versus private hospital care sectors. In 2004, South Australia had 99 hospitals, of which 76 were public and 23 were private, including 18 that shared an emergency department with a sister public hospital. Importantly, young children are rarely treated in private hospitals 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), and there was no child with private health insurance admitted to a private hospital for emergency surgery in this age group (AIHW 2017, Figure 7.1).

3.3 Outcomes and Variables of Interest

In this paper we want to estimate the impact of the Baby Bonus on infant hospital health and parental investments in child health. In the absence of unambiguous measures of child health status, we construct a measure of the severity in infant health problems as a summary index of health care utilization for infants derived from hospital records.[9]

This index sums up hospital presentations for babies from birth until age 1 excluding birth-related problems.[10]. The summation proxies health care need and therefore health issues that

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[9]Measuring infant health beyond birth outcomes is more complex than measuring health for adults. The literature on the health production function for infants acknowledges this difficulty. Most commonly, infant health is based on proxies for morbidity such as diagnoses or health care utilization including hospitalization. See Corman, Dave and Reichman (2018) for a review of this literature.

[10]We also construct an analog index for presentations in each subsequent year of life until age 5, e.g., from age 1 until age 2, from age 2 until age 3, etc.

require care. We add up binary indicators for each hospital care visit: (1) emergency department only presentations; (2) inpatient services (without ED) presentations; (3) combined emergency department presentations and admission to inpatient services. To give more weight to presentations with greater health severity, we added counts of presentations at emergency department and inpatient services that were recorded as urgent, acute, or preventable, or presentations that required an overnight stay.[11] Table B.1 reports the full list of items that entered the index.[12]

This summary measure is, thus, increasing in health care use intensity and severity. To be able to interpret easily our index–higher values on the index indicate more severe health problems and more needs for hospital care–, we deliberately exclude health care visits for electives procedures or presentations with referrals because those are more likely to represent parental health investments to improve a pre-existing health condition. In further analyses, we consider each item separately as outcome variable and also present results regarding presentations for elective procedures and presentations with referral. We standardise this index measure to mean 0 and standard deviation 1 for all babies born between 1991 and 2016 to avoid taking a stand at this stage on the appropriate bandwidth to use in our empirical analyses.

Beyond our index of health care utilization, we also consider two additional sets of outcomes that proxy parental investments in child health. First, we construct three additional outcomes that require parental planning and additional co-payments (only available in the inpatient services records) : i) having a planned or scheduled visit; ii) having a presentation with referral for specialist care; and iii) having a record for an elective intervention. Second, we characterize in detail the type of presenting problem at inpatient services and at the emergency department of our infant patients. For inpatient records, we use diagnosis codes based on the International Statistical Classification of Diseases and Related Health Problems (ICD-10-AM), from which we extract the broad categories of presenting problem.[13] For emergency department records, we can distinguish admissions by presenting problem and diagnosis group according to two different

[11] Urgent and Acute as recorded by a triage nurse at the emergency department upon presentation, or by a doctor at the emergency department of inpatient service upon discharge. Preventable is defined as "potentially preventable pediatric" (PPP) presentations. These items are recorded in alignment with the Potentially Avoidable Hospitalization (PAH) tool, a classification system developed in New Zealand for infants to flag health care use that could have been avoided. This classification system is based on a broad spectrum of factors influencing health, in particular appropriate access to primary care (see Anderson et al. (2012) for a 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 and whether doctors consider that parents should have presented the child to a general practitioner (GP) before the bronchiolitis became acute. Potentially preventable hospitalizations typically cover vaccine-preventable conditions, acute conditions, and chronic conditions that parents could have prevented with appropriate primary care. Our index aggregates all our binary outcomes associated with hospital presentations for acute or severe problems.

[12] Using factor analysis to build our index yielded one strong first factor and similar factor loadings across items; the resulting index presented a strong Cronbach alpha of 0.86, with comparable and strong item-rest correlations across items. However, the continuous index presented large lumps for children with no presentations. For this reason, we preferred to construct on a summative scale.

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

sources: triage nurses upon presentation and medical doctor upon inspection or separation. Triage nurses classify the presenting patient according to the presenting problem (for instance, respiratory, head trauma), coded as broad categories that are consistent with diagnosis subcategories based on the ICD-10-AM. Upon inspection and separation, medical doctors update the records to classify presentations into diagnosis codes, which determine how the hospital will be reimbursed after separation. 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 infants 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). For each presenting problem, we construct a summative scale consisting of three dummies, one for whether an ED triage nurse classified a presentation as presenting this problem, and another two dummies for whether an emergency department doctor or a doctor at an inpatient service recorded a diagnosis associated with the presentation.

3.4 Summary Statistics

Table B.1 presents summary statistics on the health care summary index and the 11 individual items which define the index in the first year of life of all babies born in South Australia between July 1, 2003 and July 1, 2005. For this population ($N = 35,236$ babies), the average health index is 0.183 and the standard deviation (SD) is 0.16. 45% of children have at least one presentation within their 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 has at least one presentation to the ED for an urgent or acute problem, one in eight has a presentation that led to a hospital admission, and of these, one in five stays 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. Figure B.1 presents histograms of those different outcomes.

4 Empirical Strategy

4.1 Regression Discontinuity Design

The introduction of the Australian Baby Bonus on July 1, 2004, seems to naturally lend itself to a sharp regression discontinuity design. No babies born before July 1, 2004, received the Baby Bonus, while over 95% of all babies born after the birth date eligibility threshold received it. We exploit the sharp change in eligibility for the Baby Bonus based on dates of birth to evaluate the causal impact of the Baby Bonus on health outcomes of children in their first years of life. We compare the health outcomes of children born just before versus after July 1, 2004, by estimating the following equation for a child i with health Y_i upon reaching age 1 (or age 2, 3, 4, or 5):

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

where D_i is a dummy variable taking value 1 if the child is born on or after July 1, 2004, and 0 otherwise; R_i is the running variable corresponding to the child's date of birth centered around the birth date eligibility threshold, and $g(.)$ 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 equation (1) using local linear estimation and robust bias-corrected inference methods with CER-optimal bandwidth (Calonico, Cattaneo and Titiunik, 2014*b*; Calonico, Cattaneo and Farrell, 2018, 2020; Calonico et al., 2019).[14] We choose local linear estimation with a triangular kernel to give more weight to observations closest to the threshold, following Gelman and Imbens (2019), who warn against the use of global high-order polynomials because those often give too much weight to observations away from the threshold and bias estimates at the threshold. We use robust bias-corrected inference that are robust to bias arising from nonlinear conditional expectation functions of outcomes near the threshold (Calonico, Cattaneo and Titiunik, 2014*b*).

Calonico, Cattaneo and Farrell (2020) argue that the MSE-optimal bandwidth is optimal for point estimation and the CER-optimal bandwidth is optimal for inference purposes. Our preferred specification focuses on the CER-optimal bandwidth in this context because the MSE-optimal bandwidth is larger, and could potentially include babies born in different seasons which could affect their health care needs in the first year of life. Table B.10 indicates that our main effects on health care utilization and its subitems are robust to alternative choices of bandwidths, and Section 7.1.4 presents falsification exercises regarding seasonalities.

Last, even though our running variable is discrete, we cluster standard errors at the level of birth dates in our preferred specification following Bartalotti and Brummet (2017) and Abadie et al. (2020, 2017). They recommend clustering at the experimental level in settings such as ours where treatment assignment is correlated within clusters. Nonetheless, we show in Table B.7 that this choice of clustering standard errors does not affect our conclusions (under either MSE- and CER-optimal bandwidths).[15]

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

[15] We also perform all our validity checks and analyses under a parametric specification following Lee and Card (2008) and find similar results that are available upon request.

4.2 Birth Shifting and Regression Discontinuity "Donut" Design

Our main parameter of interest yields a causal estimate of the true effect of the Baby Bonus on the health outcomes of babies if two assumptions hold: i) there is no manipulation in the running variable determining assignment to treatment and control groups, and ii) there are no significant differences between control and treatment babies at baseline.

4.2.1 Evidence on 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). The Australian government announced the introduction of the Baby Bonus on May 12, 2004, only seven weeks prior to July 1, 2004, which could not lead to immediate conception effects at the threshold, and did not lead to selective abortions (see Section 7). Yet the announcement period did allow some families to shift the birth of their baby.

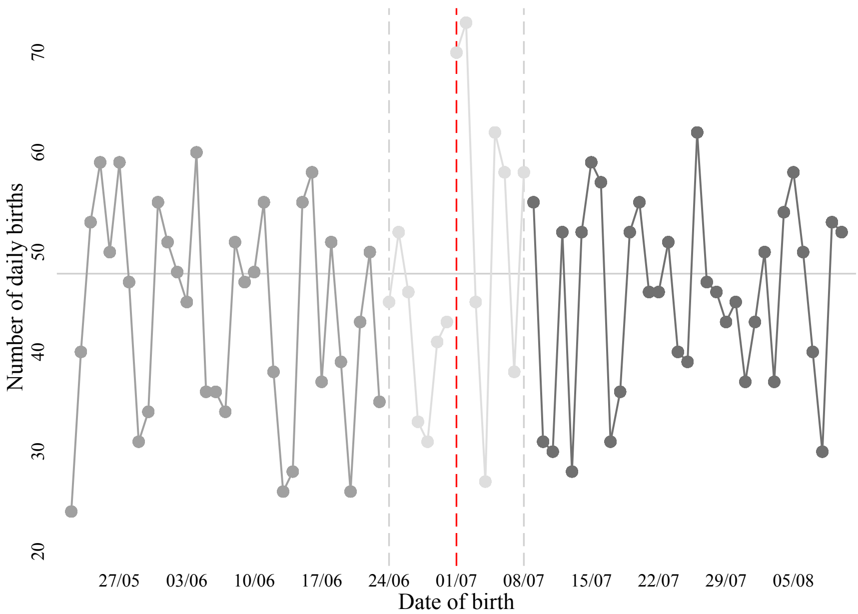
Figure 1 presents the number of daily births in South Australia within 30 days around July 1, 2004. This figure indicates common birth seasonalities: we see peaks on weekdays, when most births take place, and valleys on weekends, when few births take place. July 1, 2004, was a Thursday, so we would expect a peak on this day, but the peak is even higher than we should expect. The three points immediately before July 1, 2004, are also weekdays, but we clearly see fewer births occurring on those days. The third and fourth points to the right of the threshold are Saturday and Sunday, which is why birth shifting falls from there onward.

We confirm in our sample the key finding from Gans and Leigh (2009) that birth shifting was highly concentrated in the days immediately surrounding the birth date eligibility threshold of July 1, 2004.[16] 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 South Australian maternity wards have experienced in the previous 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 can hardly be considered a substantial disruption of daily processes in maternity wards, it does however suggest that babies born at a later date were potentially healthier than nonshifted babies (who stayed in the control group).

We therefore implement a "donut" regression discontinuity design that excludes potentially shifted births around July 1, 2004.

[16] In Appendix A we replicate the analysis of Gans and Leigh (2009) in our sample of South Australian birth records from 1991 to 2005; our results could differ because hospital guidelines and maternity ward protocols differ by state, and Gans and Leigh (2009) use data from several Australian states but not South Australia.

Figure 1: Daily Number of Births in South Australia Within 30 Days of Thursday, July 1, 2004



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

4.2.2 Choice of donut design

We conduct several analyses to determine that seven days is the appropriate donut in our context. Gans and Leigh (2009)'s original analysis and our replication indicate that the vast majority of birth shifting took place within seven days of the birth date eligibility threshold. Because of the day-of-the-week seasonalities in births, a seven-day donut ensures an even number of week and weekend days around Thursday, July 1, 2004; a shorter donut or a slightly larger donut could have caused a spurious imbalance on predetermined observable characteristics between treatment and control groups. Last, we also use a data-driven method based on nested nonparametric density tests (Cattaneo, Jansson and Ma, 2020) from which we also conclude that a seven-day donut yields a sample of balanced pretreatment characteristics between treatment and control groups, while ensuring a smooth density of births across the threshold. Our birth records dataset do not allow us to estimate the optimal donut window following Jacobson, Kogelnik and Royer (2021), because birth records start in 1999, which would only give us a few years prior to July 1, 2004 to estimate flexibly the extent of birth shifting around that date. However, we follow their advice to consider excluding all births up to 15 days around the threshold. We show in Table B.8 that our results are remarkably robust to excluding all births within 5, 8, 12, and 15 days from the threshold.

4.3 Validity of the Regression Discontinuity Design with a Seven-Day "Donut"

Density of the Running Variable Figure 1 shows graphically that removing the light gray central area removes the concerning window of births for identification; Figure B.2 shows that after excluding births within seven days of July 1, 2004, there is no obvious change in the distribution of the number of daily births over the remainder of the period 2002 to 2006. Beyond graphical evidence, Table 1 presents the result of nonparametric density tests on the running variable, which indicate that the running variable is smoothly distributed at the threshold (Cattaneo, Jansson and Ma, 2020, 2018). Column (6) presents the *p*-value of each density test. Across five out of six density tests, we cannot reject the null that there is not discontinuity in the running variable at the threshold; in only one test do we find a marginally significant *p*-value.

Table 1: Results of Local Polynomial Density Test

	Est. Bandwidth		Observations		Density Test
Estimation Method (1)	Left (2)	Right (3)	Left (4)	Right (5)	*p*-val. (6)
Models with symmetric bandwidth:					
Restricted, linear	184	184	8,455	8,217	0.777
Restricted, 2nd-order polynomial	361	361	17,019	16,940	0.069
Unrestricted, linear	106	106	4,678	4,613	0.308
Unrestricted, 2nd-order polynomial	97	97	4,169	4,086	0.180
Models with asymmetric bandwidth:					
Unrestricted, linear	114	166	5,059	7,348	0.325
Unrestricted, 2nd-order polynomial	73	117	2,990	5,114	0.156

Note: This table presents the results of three nonparametric density tests of the running variable around July 1, 2004. We conduct Cattaneo, Jansson and Ma (2020)'s test using the Stata command `rddensity` (Cattaneo, Jansson and Ma, 2018). Column (1) indicates the local polynomial fit method and the bandwidth estimation method. Columns (2) and (3) indicate the estimated bandwidth on either side of the threshold (if applicable), and columns (4) and (5) indicate the number of observations used in the test on either side of the threshold. Column (6) presents the p-value of each density test comparing the distribution of births on each side of the threshold to a Gaussian approximation. Large p-values indicate that the distribution of births on either side of the threshold are not statistically different from one another. The sample used is the universe of children born in South Australia between July 1, 2003, and July 1, 2005, excluding 93 children born abroad during this time, and all children born within seven days of July 1, 2004.

Table B.2 provides additional evidence on the continuity of the running variable at the threshold based on binomial density tests that are used in the local randomization regression discontinuity approach. We run 100 nested binomial density tests, in which we compare the number of births from eight days on each side of the threshold to 107 days on each side of the threshold. Across 100 nested tests, we rejected the null at the 10% level only three times and at the 5% level only once. Thus, we find overall strong evidence that the running variable is continuous at the threshold in our seven-day donut design.

Continuity of Predetermined Characteristics 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 in the first year of life, or they are born with worse health conditions, which would lead to more hospital presentations in the first year of life. However we show in Table 2 that control and treated babies are not statistically different from one another in terms of pretreatment characteristics.

We perform 14 balancing tests on pretreatment observable characteristics of children and their parents recorded in the perinatal data and birth records. We run our preferred specification where outcome variables are the predetermined characteristics and bandwidths are optimally chosen for each outcome (Cattaneo, Idrobo and Titiunik, 2019). We find precisely estimated zero association for almost all predetermined characteristics. For only one predetermined characteristic do we reject the null at the 10% level: we find that treated babies are 1 percentage point more likely to be born to single mothers compared to control babies. Importantly, our tests are high powered—we would be able to detect differences in the share of births to single mothers between control and treated babies as small as 1.68 percentage point at the 1% level (with 80% statistical power). Given the power of our tests, it is not surprising that we find this marginally significant association. We show in Table B.8 that our results are robust to excluding more observations close to the threshold where manipulation could, in principle, still occur.

Continuity of Birth Outcomes We present additional evidence in Table 3 that control and treated babies do not differ in 11 distinct birth outcomes, especially birth outcomes which would indicate endogenous postponing of births. We only find a small statistically significant imbalance in the Apgar score at 1 minute ($p = 0.092$). One minute after birth, babies born just after the birth date eligibility threshold are 1.9 percentage point less likely to have an Apgar score strictly above 7 compared to control babies, amounting to a decline of 2.5% from the pre-threshold mean in the optimal bandwidth. Five minutes after birth, we cannot detect any imbalances in Apgar scores between control and treated babies suggesting that these discrepancies have dissipated. Figure 2 provides supporting graphical evidence. These RD-plots provide graphical evidence suggesting that babies in the control group are not systematically different from treated babies based on six key characteristics associated with birth shifting and predictive of health status in the first year of life: gestational age in weeks, whether the baby was born prematurely (before 37 weeks), whether the baby required additional medical care (admitted to Special care Nursery), Apgar scores at 1 and 5 minutes after birth, and birth weight in grams.

Placebo Outcomes around the Threshold in Prepolicy Years To provide further evidence on the validity of our regression discontinuity design, Table 4 presents results on placebo outcomes for births around July 1, 2002, and July 1, 2003, respectively, following Cattaneo, Idrobo and Titiunik (2019) and used in e.g., Carneiro, Løken and Salvanes (2015) and Borra et al. (2021).

Table 2: Continuity of Predetermined Characteristics at the Threshold

	Coef. Est.	Sd.err.	p-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child and Parental Predetermined Characteristics:							
Child is female	0.015	0.011	0.175	477	22,681	22,733	0.483
Birth in private hospital	0.012	0.013	0.334	387	18,366	18,187	0.341
No. of antenatal visits	−0.056	0.09	0.535	325	14,058	13,840	10.682
Mother smokes	0.002	0.008	0.788	591	27,325	27,787	0.205
Mother's age:							
35+	−0.005	0.008	0.509	565	26,620	26,846	0.180
40+	−0.004	0.004	0.288	475	22,566	22,635	0.031
Father's occupation:							
High skilled	0.007	0.012	0.554	472	21,323	21,216	0.332
Low skilled	0.009	0.012	0.458	558	25,023	25,088	0.557
Mother's marital status:							
Never Married	0.011	0.006	*0.077*	620	29,180	29,639	0.117
Married	−0.006	0.008	0.464	503	23,821	23,910	0.871
Single	−0.004	0.003	0.115	425	20,117	20,039	0.013
Mother's race:							
Caucasian	0.001	0.006	0.915	509	24,082	24,150	0.908
Asian	0.003	0.004	0.531	571	26,895	27,117	0.046
Aboriginal or TSI	−0.004	0.005	0.346	471	22,369	22,411	0.045

Note: This table presents the results of balancing tests on pretreatment 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 with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. The correlation between continuous Apgar scores at one and five minutes is 0.604 in this sample. p-values in italics indicate effects statistically significant at least at the 10% level.

The table focuses on outcomes from inpatient services measured in prepolicy years 2002 and 2003 as not all outcomes are available for those placebo years.[17] Table 4 demonstrates that there are no discontinuities in the use of inpatient services for children born around July 1, 2002 and July 1, 2003. In addition, we show in Table B.13 that there are also no discontinuities in birth outcomes and parental characteristics of those babies born around July 1, 2002 and July 1, 2003.

[17] Our emergency department records start in July 2003 and our inpatient services records start in July 2001.

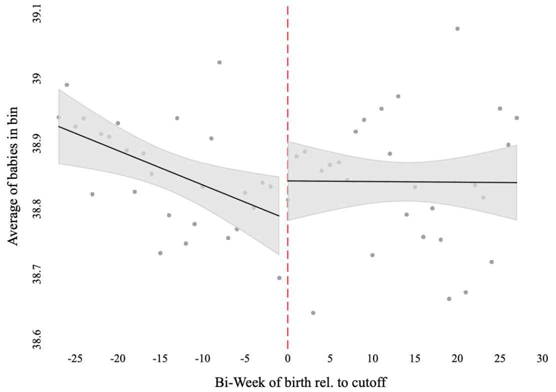
Table 3: Continuity of Child Birth Outcomes at the Threshold

	Coef. Est.	Sd.err.	p-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child birth outcomes:							
Baby weight	22.067	14.375	0.125	483	22,955	23,004	3349.67
Special Nursery	0.001	0.010	0.923	381	18,007	17,856	0.168
NICU	0.001	0.003	0.755	703	33,225	33,731	0.028
PICU	0.000	0.001	0.822	475	22,465	22,544	0.002
Neonatal death	−0.001	0.001	0.724	323	15,208	15,074	0.008
Apgar 1 min > 7	−0.019	0.011	*0.092*	431	20,382	20,208	0.759
Apgar 5 min > 7	−0.002	0.005	0.611	470	22,218	22,272	0.971
Gestational age	0.057	0.062	0.354	336	15,824	15,682	38.758
Preterm birth	−0.004	0.007	0.606	670	31,671	32,140	0.148
Obstetric complication	−0.024	0.014	0.100	294	13,725	13,617	0.316
C-Section	0.007	0.013	0.566	622	29,224	29,721	0.309

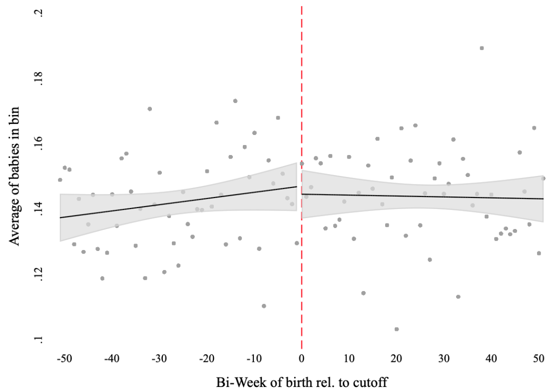
Note: This table presents the results of balancing tests on child birth outcomes based on birth and perinatal records. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. The correlation between continuous Apgar scores at one and five minutes is 0.604 in this sample. NICU and PICU refer to Neonatal Intensive care Unit and Pediatric Intensive care Unit. p-values in italics indicate effects statistically significant at least at the 10% level.

Figure 2: Continuity of Birth Outcomes at the Threshold

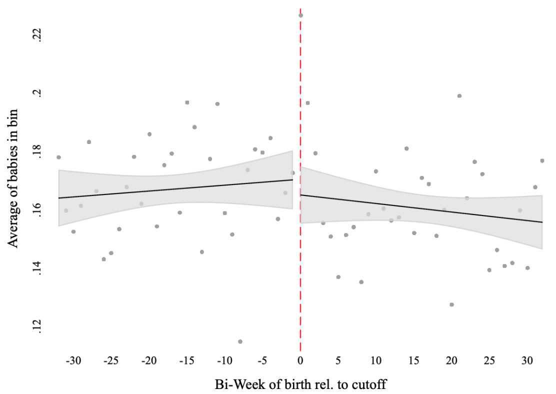
(a) Gestational age



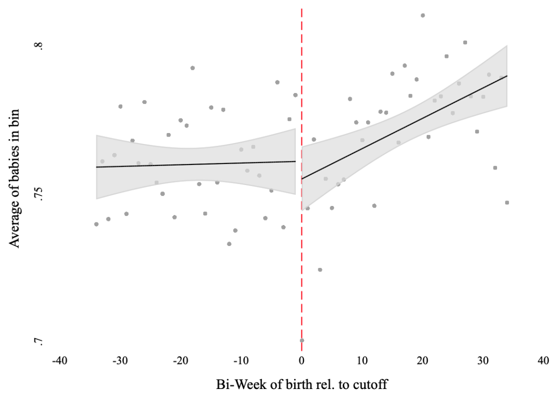
(b) Preterm birth



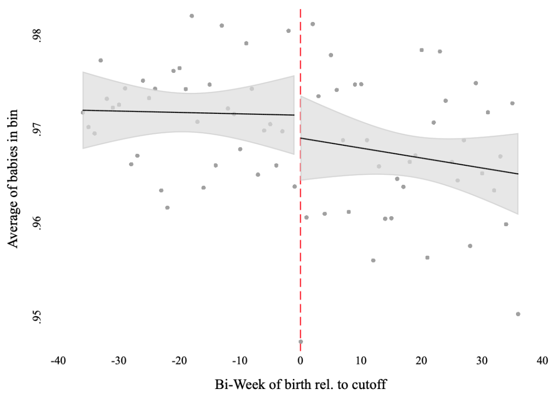
(c) Special Nursery



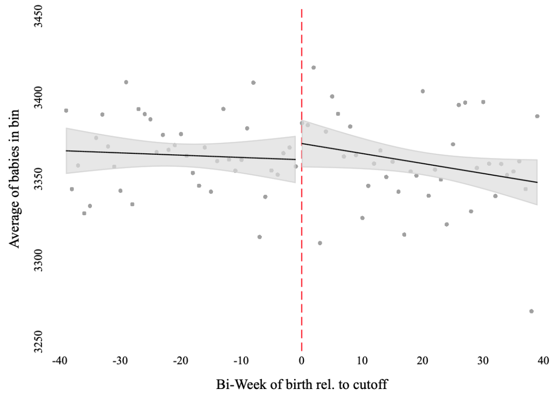
(d) Apgar score, one minute



(e) Apgar score, five minutes



(f) Birth weight (in g)



Note: These RDPlots present graphical evidence that the introduction of the Australian Baby Bonus is not systematically associated with birth outcomes. On every figure, the x-axis corresponds to birth dates aggregated in two-week windows around the threshold, and the y-axis to the birth outcome of interest. The red dashed line indicates the implementation date of the Baby Bonus (July 1, 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 threshold to be evenly spaced and variance-mimicking (ESMV), and we use for each outcome the CER-optimal bandwidth estimated using the* `rdrobust` *Stata command (Calonico et al., 2017; Calonico, Cattaneo and Farrell, 2018, 2020). The black line represents the local linear fit using a triangular kernel. The gray shaded areas represent the 95% confidence interval around the mean of each bin. We exclude births within seven days of the threshold.*

Table 4: Placebo Effects of the Australian Baby Bonus on Hospital Presentations in Prepolicy Years

	Placebo Outcomes in Prepolicy Years			
	July 1, 2002		July 1, 2003	
	Coef. Est.	Sd.err.	Coef. Est.	Sd.err.
	(1)	(2)	(3)	(4)
Health Care Utilization by Subitem (inpatient services only)				
Panel A. Any presentation by hospital service:				
Inpatient services	0.030	0.019	0.002	0.019
Panel B. Any presentation deemed urgent/severe by hospital service:				
Inpatient Services:				
Urgent, acute or severe problem	0.012	0.014	0.013	0.024
Admission to ward	−0.004	0.009	−0.001	0.007
Overnight admission	0.010	0.017	−0.015	0.020
Panel C. Any potentially preventable pediatric presentations:				
Any PPP presentation, inpatient services	−0.018	0.011	0.035**	0.017
Additional Items (not in Health Care Utilization Index)				
Panel D. Any planned visits or presentations with medical referral:				
Inpatient services:				
Planned visit	0.002	0.006	0.006	0.008
Visit with med. referral	0.012	0.014	−0.010	0.018
Booked elective procedure	0.018	0.012	−0.003	0.012

*Note: This table presents RD treatment effects of the Baby Bonus on hospital presentations in prepolicy years (2002 and 2003). Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude children born overseas in 2002 (resp. 2003) and all births within seven days of July 1, 2002 (resp. 2003). *, **, and *** denote effects significance at the 10%, 5% and 1% respectively.*

5 Main Results: Hospital Care Utilization in the First Year of Life

Our results indicate that the Australian Baby Bonus led to a 0.098 standard deviation reduction in hospital care utilization in the first year of life. Table 5 presents the main results on health care utilization within the first year of life. Figure 3 presents this result graphically.

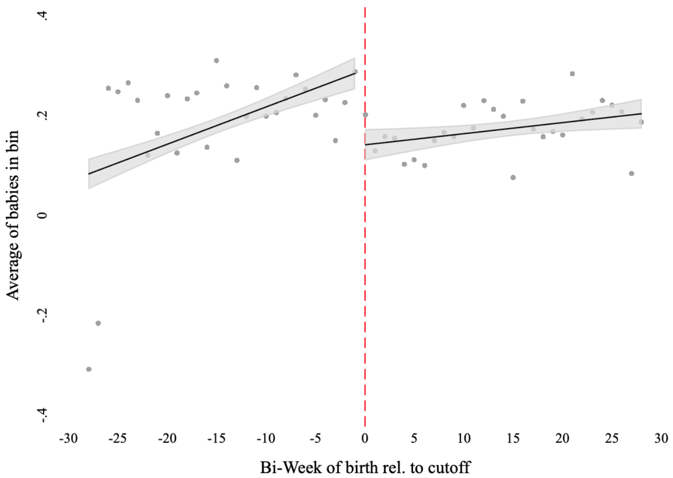
Table 5: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Health Care Utilization Index [std.]	−0.098	0.034	**0.004**	306	14,363	14,267	0.187

Note: This table presents our main results on our standardized index of health care utilization. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. p-values in ***bold*** *indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

Because the health care utilization index captures presentations for adverse outcomes, the above finding suggests that the Baby Bonus may be welfare enhancing. However, this result could have been explained by other factors. It is possible that the lump-sum cash injection of the Baby Bonus caused parents to sort out of the hospital sector. They may demand less hospital care for their children, while using the cash injection to (partially) fund demand for costly goods and services that are unrelated, or even adverse, to children's health. To better understand whether the treatment effect of the Baby Bonus on hospital care utilization can indeed be interpreted as suggestive of health improvements, we analyze the effect of the Baby Bonus on each item of our index separately which capture presentations for different types of presentation (e.g., acute, urgent or preventable care versus elective care) and on individual diagnoses, which can give insights about parental decisions to seek care for the child.

Figure 3: Effects of the Australian Baby Bonus on Health Care Utilization Within The First Year of Life



Note: This figure present graphical evidence that the Australian Baby Bonus reduced hospital care utilization within the first year of life for treated babies. The x-axis corresponds to birth dates aggregated in two-week windows around the threshold, and the y-axis corresponds to our health care utilization index. The red dashed line marks the implementation date of the Baby Bonus (July 1, 2004). This plot is produced using the `rdplot` Stata command (Calonico, Cattaneo and Titiunik, 2015; Calonico et al., 2017). We select the number of bins on each side of the threshold to be evenly spaced and variance-mimicking (ESMV), and we use the CER-optimal bandwidth estimated using the `rdrobust` Stata command (Calonico et al., 2017; Calonico, Cattaneo and Farrell, 2018, 2020). The black line represents the local linear fit using a triangular kernel. The gray shaded areas represent the 95% confidence interval around the mean of each bin. We exclude all births within seven days of the threshold.

5.1 Detailed Hospital Presentations: Acute and Severe Problems, Preventable Hospitalizations, and Elective Care

We show that the decline in health care utilization within the first year of life for treated babies is driven by an overall decline in hospital presentations at inpatient services and at emergency departments (although not statistically significant). At inpatient services and emergency departments, the Baby Bonus led to a significant decline in presentations for the most acute and severe problems, and presentations deemed preventable by doctors. Those findings suggest that the Baby Bonus may have led parents to take actions to prevent health problems from becoming acute or severe. Table 6 presents our findings by type of hospital presentations and Figure 4 the corresponding graphical evidence.

Panel A shows results on the probability of having a hospital presentation overall and split between emergency department and inpatient services, some of which are transfers from emergency departments in cases that require more examination. Treated babies are significantly less likely to have presentations at inpatient services by –3.4 percentage points (ppt) ($p = 0.040$, 11% decline compared to the sample mean) and at emergency departments –2.4 ppt (albeit not statistically significant, $p = 0.232$).[18]

[18]Our conclusions are robust to using a count data model to study the impact of the policy on the intensive margin

Panel B presents our findings on presentations deemed urgent or severe by hospital staff. 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 by –3.8 ppts ($p = 0.004$) and to inpatient services by –3.7 ppts ($p = 0.005$). Relative to the sample means, these effect imply a reduction of 29% and 22%, respectively. These findings suggest an improvement in baby health because hospital presentations for acute and severe problems are treated for free in Australia and are not likely treatable out of the hospital care sector (e.g., by GP services or physiotherapy), such that those outcomes are unlikely to suffer from biases arising from parental sorting in or out of the hospital care sector.

Panel C presents the effects of the implementation of the Baby Bonus on potentially preventable pediatric hospitalizations, as defined by medical staff upon presentation at the hospital. We also find that treated babies have significantly fewer hospital presentations deemed "preventable" by hospital staff at inpatient services by –2.8 ppts ($p = 0.005$), a 28% reduction relative to the sample mean. Those outcomes refer to health concerns that should have been dealt with earlier in the life-cycle of the disease or when the disease could have been avoided altogether with the availability of better financial resources. Those outcomes suggest that parents may have invested in preventive health care, thus preventing babies' conditions from becoming acute or severe.

Finally, Panel D shows that the Baby Bonus had no significant impact on planned hospital presentations within the first year of life. Planned hospital presentations could indicate an increase in parental investments in child health because these involve direct (money and time) costs. Due to those costs, those outcomes could be partly biased by parental sorting out of the hospital care sector. Our findings suggests that the policy had no impact on parental investments at this margin and no impact on sorting patterns for those types of presentations.

Overall, treated babies had fewer hospital presentations altogether, and in particular they have fewer of the more acute and costlier presentations, and fewer preventable hospitalizations. These findings suggest that the Baby Bonus may have had a positive impact on infant health, not just health care utilization Our findings regarding presentations deemed preventable by doctors suggest that some of those improvements in health may have occurred through parental behavioral changes.

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of presentations (Appendix Table B.3)—the estimates have the same sign, magnitude, and significance as our main results. Eligible babies have, on average, 0.12 fewer visits at the emergency department and 0.06 fewer presentations at inpatient services within their first year of life. These effects represent a decline of 20% and 12.5% compared to the sample means of 0.59 and 0.48 visits. Those are economically sizeable effects: following the Baby Bonus, five out of ten children have a presentation at the emergency department before their first birthday instead of seven out of ten before the policy, and three out of ten instead of four out of ten have a presentation at an inpatient service within their first year of life.

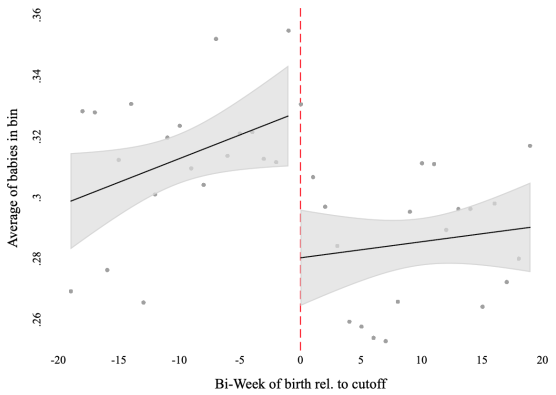
Table 6: Effects of the Baby Bonus on Hospital Presentations in the First Year of Life by Type of Presentation

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Health Care Utilization by Subitem							
Panel A. Presentations by hospital service:							
Any hospital service	−0.013	0.021	0.532	175	8,022	7,855	0.451
Emergency department	−0.024	0.020	0.230	152	6,906	6,764	0.310
Inpatient service	−0.034	0.017	**0.040**	215	9,832	9,611	0.313
Panel B. Presentations for urgent, acute or severe problems (by hospital service):							
Emergency department:							
Urgent, acute or severe problem	−0.017	0.018	0.369	172	7,833	7,667	0.207
Admission to ward	−0.038	0.013	**0.004**	173	7,891	7,772	0.130
Inpatient services:							
Urgent, acute or severe problem	−0.037	0.013	**0.005**	207	9,483	9,235	0.172
Admission to ward	0.002	0.007	0.761	206	9,426	9,202	0.027
Overnight admission	−0.022	0.012	*0.074*	299	13,994	13,856	0.204
Panel C. Potentially preventable pediatric presentations (by hospital service):							
Any PPP presentation	−0.033	0.019	*0.083*	153	6,906	6,764	0.218
Any PPP presentation, ED	−0.032	0.018	*0.071*	142	6,418	6,320	0.180
Any PPP presentation, IS	−0.028	0.010	**0.005**	215	9,832	9,611	0.105
Additional Items (not in Health Care Utilization Index)							
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):							
Emergency department:							
Planned visit	−0.010	0.005	*0.074*	147	6,619	6,497	0.017
Visit with med. referral	0.002	0.009	0.845	152	6,870	6,704	0.054
Inpatient services:							
Planned visit	−0.001	0.004	0.882	289	13,535	13,442	0.025
Visit with med. referral	−0.010	0.010	0.308	198	9,108	8,874	0.094
Booked elective procedure	0.003	0.008	0.657	230	10,579	10,481	0.056

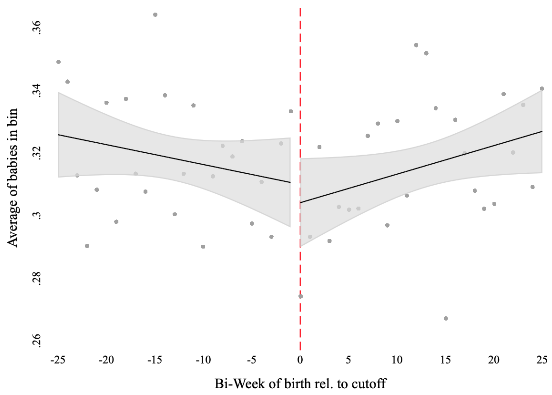
Note: This table presents the effects of the Australian Baby Bonus on hospital presentations by type of presentation. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. p-values in ***bold*** *indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

Figure 4: Effects of the Australian Baby Bonus on Hospital Presentations Within The First Year of Life

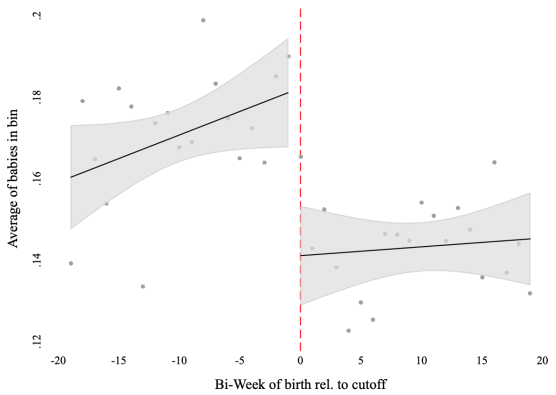
(a) Inpatient services visits



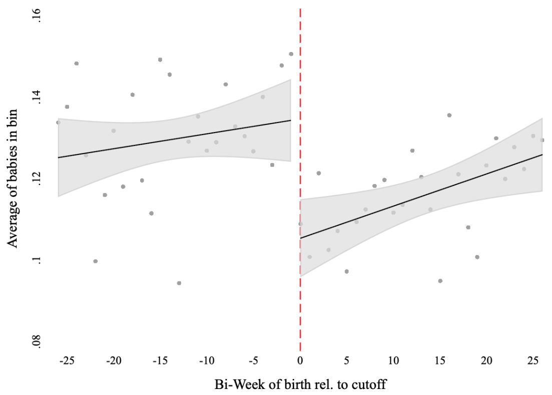
(b) Emergency department visits



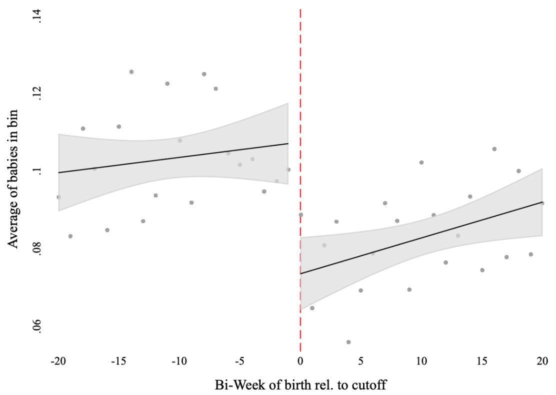
(c) Severe/acute problems, inpatient services



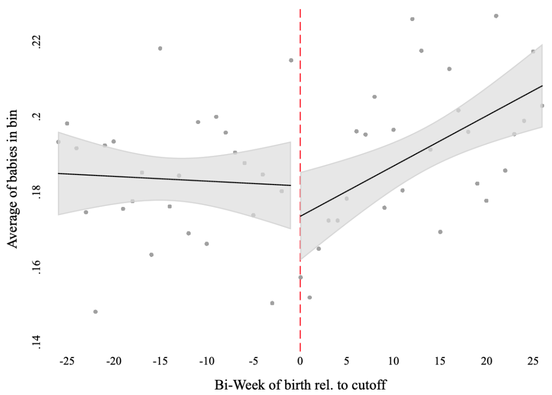
(d) Admission to emergency department ward



(e) PPP presentation, inpatient services



(f) PPP presentation, emergency department



Note: These RDPlots present graphical evidence that the introduction of the Australian Baby Bonus reduced hospital presentations within the first year of life for treated babies. On every figure, the x-axis corresponds to birth dates aggregated in two-week windows around the threshold, the y-axis to the outcome of interest, and the red dashed line marks the threshold (July 1, 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 threshold to be evenly spaced and variance-mimicking (ESMV), and we use for each outcome the CER-optimal bandwidth estimated using the* `rdrobust` *Stata command (Calonico et al., 2017; Calonico, Cattaneo and Farrell, 2018, 2020). The black line represents the local linear fit using a triangular kernel. The gray shaded areas represent the 95% confidence interval around the mean of each bin. We exclude births within seven days of the threshold.*

5.2 *Treatment Effects by Diagnosis*

Breaking down results by diagnosis, we show that the decline in hospital care utilization for treated babies is driven by a decline in presentations for respiratory problems, and in particular by potentially preventable bronchiolitis and gastroenteritis, the two most common types of potentially preventable pediatric presentations for babies in the first year of life in our data. Table 7 presents our estimation results for the five most common types of PPP presentations within the first year of life, and the ten most common problem/diagnosis groups organized in indices; Table B.4 presents results on each diagnosis.

Panel A presents results on the five most common types of PPP presentations for babies in the their first year of life. We find that the Baby Bonus led to a –2.5 ppts ($p = 0.012$) decline in emergency department presentations for potentially preventable bronchiolitis, the most common respiratory illness for infants, which is associated with asthma at later stages. This effect implies a reduction of 34% relative to the sample mean. In line with this finding, the Baby Bonus also led to a –12.7 ppts decline in presentations for respiratory problems. Figure 5 presents graphical evidence on the effect of the Baby Bonus on hospital presentations for respiratory problems and on emergency department presentations for potentially preventable bronchiolitis. The figure emphasizes the sharpness of the treatment effect at the birth-date eligibility threshold for respiratory health outcomes. In addition, we find evidence that the Baby Bonus decreased by –2.4 ppts presentations for preventable gastroenteritis (significant at the 10% level, $p = 0.061$), the third most common cause of hospital presentations for infants in the first year of life in our data.

Importantly, for presentations related to potentially negligent behaviour of parents, such as accidents, injury and trauma, we find no significant effect of the Baby Bonus with a treatment effect size of 2.9 ppt ($p = 0.481$, Table 7, Panel B). If anything, the Baby Bonus may have reduced presentations due to problems with external causes. Table 7 shows that the Baby Bonus reduced this probability at inpatient services by 1.2 ppt ($p = 0.047$, 23% relative to the sample mean).

Together, those results lend additional support to our interpretation that the Baby Bonus had an overall positive impact of infant health, consistent with increased parental investments or behavioral responses with positive health returns.

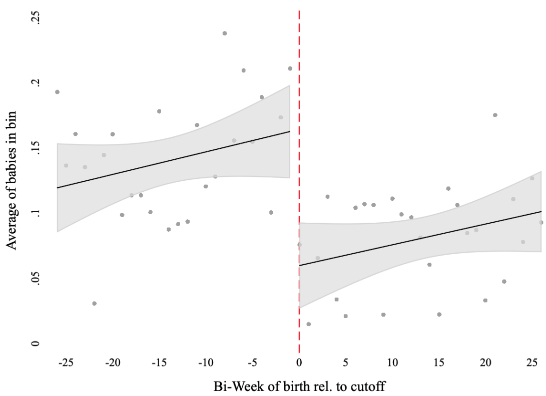
Table 7: Effects of the Australian Baby Bonus on Medical Diagnoses Within the First Year of Life

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Potentially preventable pediatric presentations:							
Emergency department:							
Bronchiolitis	−0.025	0.010	**0.012**	152	6,870	6,704	0.074
Gastroenteritis	−0.024	0.013	*0.061*	94	4,047	3,969	0.043
Laryngitis	−0.002	0.006	0.759	127	5,674	5,622	0.010
Otitis media	−0.002	0.005	0.706	136	6,056	5,978	0.009
Respiratory infection	−0.013	0.010	0.223	121	5,357	5,280	0.046
Inpatient services:							
Bronchiolitis	−0.013	0.008	0.105	160	7,272	7,126	0.057
Gastroenteritis	−0.007	0.005	0.141	208	9,507	9,292	0.020
Laryngitis	0.000	0.002	0.849	221	10,151	9,965	0.004
Otitis media	0.001	0.001	0.369	274	12,657	12,665	0.003
Respiratory infection	−0.003	0.004	0.453	271	12,540	12,537	0.013
Panel B. Presentations by ICD-10-AM diagnosis chapter and presenting problem:							
Respiratory	−0.127	0.032	**0.000**	309	14,530	14,437	0.132
Infection	−0.007	0.033	0.835	286	13,353	13,278	0.110
Digestive	−0.008	0.032	0.810	356	16,833	16,749	0.078
Unspecified	−0.021	0.029	0.476	367	17,335	17,211	0.040
Eyes and ears	0.035	0.028	0.209	417	19,809	19,620	0.033
Skin	0.016	0.025	0.527	392	18,613	18,479	0.024
Injury/Trauma/Poisoning	0.019	0.027	0.481	347	16,366	16,260	0.010
Nervous system	0.000	0.019	0.982	454	21,587	21,589	−0.017

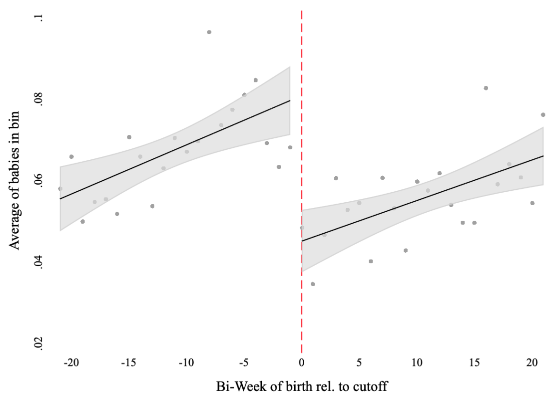
*Note: This table presents the results of regressions of the effect of the Australian Baby Bonus on presentations diagnoses for babies within their first year of life. Each line corresponds to a separate regression using our main specification, where outcomes are specific diagnoses. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. p-values in **bold** indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

Figure 5: Effects of the Baby Bonus on Presentations for Respiratory Problems in the First Year of Life

(a) Presentations for respiratory problems (index)



(b) PPP bronchiolitis, emergency department



Note: These RDPlots present graphical evidence that the Australian Baby Bonus 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 aggregated in two-week windows around the threshold, the red dashed line indicates the threshold date (July 1, 2004), and the y-axis to the outcome of interest. These plots are produced with the `rdplot` Stata command 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 threshold to be evenly spaced and variance-mimicking (ESMV), and we use for each outcome the CER-optimal bandwidth estimated using the `rdrobust` Stata command (Calonico et al., 2017; Calonico, Cattaneo and Farrell, 2018, 2020). The line represents the local linear fit using a triangular kernel. The gray shaded areas represent the 95% confidence interval around the mean of each bin. We exclude births within seven days of the threshold.

6 Longer-Term Impacts of the Baby Bonus

So far we have shown that the Australian Baby Bonus 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 five.

Table B.5 in Appendix shows 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. Although, the estimates are imprecise, the are relatively large in magnitude. For instance, in both age groups, treated babies are around –2 ppts less likely to present to an emergency department. Treated babies are –2.2 ppts less likely to present for preventable problems at emergency departments in the second year only. No further effects are detected at later ages.

Table B.6 indicates that the lower risk of presentations for preventable problems is driven by lower presentation risk for preventable otitis media (glue ear) (–1 ppt) and respiratory infections (–2.6 ppts) in the second year of life. Treated babies are also less likely to present at the emergency department for injuries, trauma and poisoning problems (–2.6 ppts), unspecified problems (–2.3 ppts) and eye- and ear-related problems (–1 ppt).

However, treated babies are significantly more likely to have planned visits (+1.7 ppts) or booked elective procedures (+1.6 ppts) 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 ppts) within their third year of life. It is these type of presentations that require a medical referral from a specialist and that incur co-payments.

We find precisely estimated null effects on any other subsequent diagnoses up until age five. That we do not find effects in the medium-run is consistent with a pattern emerging in the literature. Several studies find positive short-term and long-term impacts of social security policies and no impacts in the medium-term (see Aizer, Hoynes and Lleras-Muney (2022) for an excellent review). Our findings suggest that the decline in hospital presentations that we find in the first year of life are not associated with later additional hospital care utilization compensating for lack of early appropriate care or 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 that requires higher co-payments, which can be interpreted as a health investment.

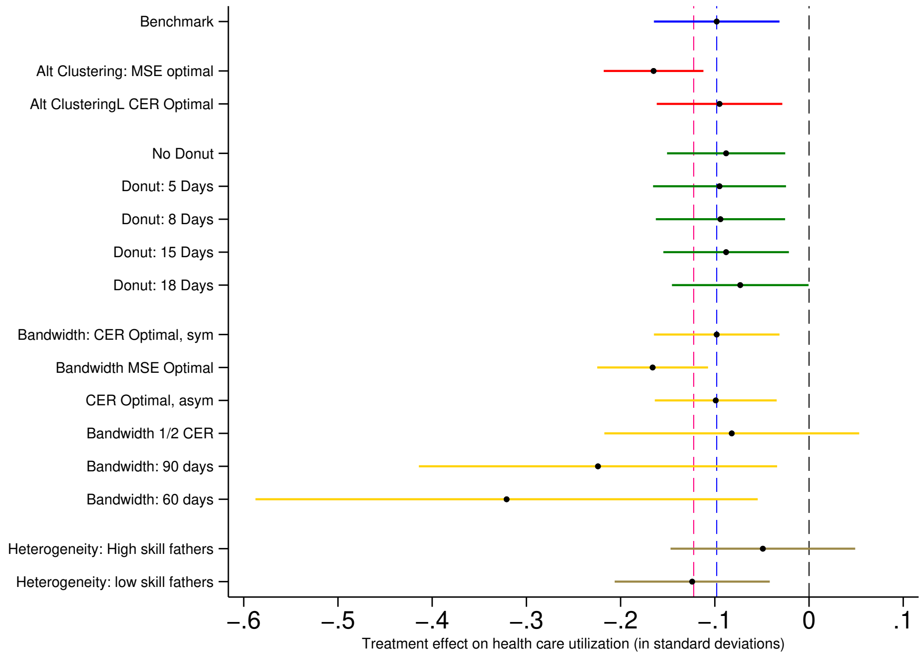
7 Robustness of Main Findings

In this section, we discuss the sensitivity of our findings to three types of concerns: i) robustness of our identification strategy; ii) limitations of our data and sensitivity to available data; and iii) robustness of our inference to multiple hypothesis testing corrections.

7.1 Robustness of Identification Strategy

We provide additional evidence on the validity of our research design, by showing that our findings are robust to alternative choices of bandwidth and are not driven by observations close to the threshold, by selective abortions, or by potential seasonalities (e.g., due to seasonal viruses) or nonlinearities around the eligibility threshold that could spuriously affect our estimates. Figure 6 presents a summary of our point estimates and 95% confidence intervals across our sensitivity analyses. As the figure shows, our point estimates and confidence intervals are robust to most sensitivity analyses, with a benchmark effect of -0.098 (SE 0.034) and a mean treatment effect of -0.112 across 16 different specifications, ranging from -0.049 (SE 0.05) for children born to high-skilled fathers to -0.321 (SE 0.136) for children born within 60 days around threshold. Our results suggest that the treatment effect is larger in magnitude and much less precisely estimated for babies born in winter months in the narrowly defined bandwidths of 60 days and 90 days around the threshold, this pattern is likely due to loss of statistical power. We discuss those analyses in details below.

Figure 6: Effects of the Australian Baby Bonus on Health Care Utilization Within The First Year of Life Across Specifications



Note: This figure presents our main coefficient estimate and 95% confidence interval for our main specification (blue), without clustering standard errors and using MSE- or CER-optimal symmetric bandwidths (red), using donuts of 0, 5, 8, 15 and 18 days around the threshold (green), using various data-driven (MSE-, CER-optimal symmetric and asymmetric bandwidths, 1/2 CER-optimal) and researcher-chosen bandwidths of 90 and 60 days (yellow), as well as for subsamples of children of high-skilled fathers and low-skilled fathers (khaki). The vertical dashed lines indicate 0 treatment effect (brown), our benchmark point estimate (blue) and the average effect across specifications (red).

7.1.1 Observations close to threshold

There could be a concern that our results are driven by a small number of high-leverage observations located near the threshold. Table B.8 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—donuts that exclude the same number of weekend days on each side of the eligibility threshold. In Appendix A we show that including the seven-day donut births closest to the birth date eligibility threshold of birth does not qualitatively change our conclusions either.

7.1.2 Selective fertility and abortions

The Australian Baby Bonus was announced only seven weeks prior to implementation, leaving little room for new conceptions. One could still be concerned in principle about selective abortions of women already expecting at the time of the policy announcement, who would be in the control group and would rather have another child later in the year in order to be in the treatment group. As discussed in Gans and Leigh (2009) and Deutscher and Breunig (2018), the short-notice announcement of the policy left little scope for selective abortions because pregnant women whose planned delivery date would be just before July 1, 2004, would be in their third trimester of their pregnancies, at which stage selective abortion is highly unlikely.[19] Table B.9 confirms that the introduction of the Baby Bonus did not induce changes in fertility (any past pregnancies, live births, miscarriages) or selective abortions around the eligibility threshold.

7.1.3 Choice of bandwidth

One could be concerned that our estimation results are sensitive to our choice of bandwidth. Table B.10 shows that our findings are robust to a variety of alternative data-driven bandwidth choices (CER-optimal, MSE-optimal, asymmetric CER-optimal). We find remarkably similar estimates in sign, magnitude, and significance across the different bandwidth specifications. The symmetric CER-optimal method yields our smallest bandwidths of roughly half a year on each side of the threshold (spread: 142–299 days, median: 182 days) and our largest bandwidths with the MSE-optimal bandwidth method, of around three-quarters of a year (spread: 217–460, median: 286 days). Although our point estimates are very similar across bandwidth choices, they tend to be slightly larger in magnitude and more precisely estimated when using MSE-optimal bandwidths. Both of these observations are explained by MSE-optimal bandwidths being generally larger than CER-optimal bandwidths: 1) with more observations, MSE-optimal bandwidths yield more precision than CER-optimal bandwidths, and 2) as they extend outward, MSE-optimal bandwidths include babies that are increasingly born under different seasons and that are less comparable at the outward bounds of the bandwidth compared to babies born close to the threshold. We therefore prefer the specification using the CER-optimal bandwidth to be more conservative with respect to both point estimation and inference.

7.1.4 Seasonality and timing of births

Another concern could be that our findings could be driven by spurious seasonalities in birth outcomes, for example, due to seasonal viruses especially because June to September are the coldest month of the year in South Australia. Based on Currie and Schwandt (2013), we could expect that babies born in the coldest months of the year are exposed to worse environmental conditions at birth compared to babies born further away from the colder months of the year on either side of the threshold. Table B.11 presents our main results using smaller bandwidths of one-half of the CER-optimal bandwidth, and 90 days and 60 days around the threshold.

[19] South Australia decriminalized abortions in 2022. However, abortions were feasible before that date. South Australia happened to be the only state that reported abortions and abortion rates. 98% of all abortions happened before 20 weeks of pregnancy. Abortion rates declined consistently over time in South Australia. in 2002 and 2003, there were 5,467 and 5,216 abortions, respectively or 23.6 and 23.0% of all live births respectively. In 2004 there was a further reduction in abortions, consistent with the declining trend in abortions over time. There were 4,931 abortions or 22.3% of all live births. See archives at http://www.johnstonsarchive.net/policy/abortion/australia/ab-aust-sa.html.

The magnitude of effects generally increases as the bandwidth narrows around the eligibility threshold down to two months around the threshold. We see two plausible reasons for this observation. On the one hand, with such narrow bandwidths, our estimation sample focuses on babies born in the coldest months of the year and who are most comparable around the threshold; for those babies, our treatment effect might legitimately be larger because families are more constrained in winter months (e.g., due to heating expenditures) and the Baby Bonus helped alleviate those constraints. On the other hand, by focusing on such narrow bandwidths we also loose a lot of statistical power, and low statistical power can lead to finding unreasonably large effects. To tease apart those two potential explanations, we explore to what extent this change in magnitude close to the threshold could be due to the presence of some seasonalities in infant health.

Falsification exercise using placebo thresholds To explore to what extent our findings could be partially driven by seasonalities at births around the threshold, we conduct a falsification exercise using placebo thresholds. The intuition is that if our results are driven by other factors that coincide roughly with the July 1, 2004, eligibility threshold (such as parental selection into timing of birth, as discussed in Buckles and Hungerman (2013)), then we should see statistically significant treatment effects also for randomly chosen threshold dates in the vicinity of July 1. This amounts to testing whether findings at the true threshold date are indeed stronger than effects at any other random date close to July 1, 2004. We conduct permutation tests for alternative thresholds(+/- 90 days before July 1, 2004) to assess whether the true threshold can be treated as an exogenously assigned threshold, and if the coefficient estimate at the true threshold is extreme in the distribution of all coefficient estimates at alternative thresholds (Ganong and Jäger, 2018). We then construct the percentile rank of our estimate at the true threshold and compare it to the distribution of all the estimates obtained and construct a randomization-based p-value. Table B.12 presents the results of these permutation tests for each outcome , and indicates that our main results can be rightfully attributed to the policy rather than to confounding factors such as seasonality surrounding the birth date eligibility threshold. In five out of seven significant treatment effects of the Baby Bonus, we find that the permutation test unambiguously states that the estimate at the true threshold is robust, with randomization-based p-values close to 0.1.[20].

From this exercise we conclude that 1) our findings are unlikely to be spuriously driven by seasonalities, and 2) point estimates are larger for narrow bandwidths likely because of loss of statistical power, not seasonalities. Overall our effects are robust to alternative choices of bandwidths and are unlikely to be driven by seasonalities. Our results remain qualitatively the same once we consider very small bandwidths, although point estimates become sensitive due

[20]There are two cases in which the permutation test yields a more ambiguous finding: inpatient services ($p = 0.696$) and PPP presentation 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.

to loss of statistical power.

7.2 Sample Selection

Using hospital records alone to assess the health of young children has both advantages and disadvantages. 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; see National Research Council (2011)). In this section we discuss to what extent our data from inpatient services and emergency department visits could provide a biased picture of the true impact of the Baby Bonus on child health and health care utilization.

7.2.1 Inpatient services and emergency department visits

By focusing on hospital emergency and inpatient data, we could 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, we focus on acute and potentially acute health conditions. In Australia parents are advised to take their babies or children to emergency departments if they become ill suddenly or if they had an incident such as an assault, fall or burn, poisoning, allergic reactions, broken bones, or breathing problems.[21] By using hospital inpatient records, our analysis also captures 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 concern could be that changes in the distribution of hospital presentations may reflect a substitution effect. Substitution effects may occur where additional financial resources could be used to purchase more appropriate but more costly care (e.g., primary care). In particular, free emergency care may be used as a substitute for costly primary care. This is not a relevant argument in the Australian health care setting, as primary care for children can be accessed

[21] See https://www.healthdirect.gov.au/hospital-emergency-departments for more details.

without co-payments. Therefore, we are not concerned about substitution effects regarding our main effects on the demand for hospital emergency care.

Another form of substitution effect however could affect our findings about elective inpatient care. Although we find no impact of the Baby Bonus on the demand for elective inpatient care, it is plausible that the Baby Bonus directly affected the demand for such procedures. This concern does not affect the demand for inpatient services that results from admissions through emergency care for acute conditions because those presentations come at no cost for patients, so we would not expect substitution effects to occur. The demand for elective procedures might however be affected in the case of planned admissions by medical referral for elective care, which do require co-payments. The pathway to elective hospital care goes through a general practitioner (which can be free of charge), who refers the patient to a specialist (usually not fully covered by Medicare), who then refers the patient to a hospital service. For specialists, patients pay on average 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 pediatrician outpatient services (Freed and Allen, 2018). The median initial consultation doctor fee for pediatric consultations is \$320, and in South Australia \$263 (based on 2014 data—the only data available). Thus, in South Australia families pay around \$130 out-of-pocket for each visit. Thus, it is possible that the Baby Bonus directly affected the demand for elective inpatient procedures. In this case, we would expect the Baby Bonus to have increased the demand for such services, which was not the case.

7.2.2 Records from public hospitals

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 were private. [22] Figure B.3 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 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 (private hospitals perform two-thirds of elective surgeries and 80% of rehabilitation care).

More importantly, young children are rarely treated in private hospitals (see Australian Institute

[22] For 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.

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 nonemergency care such as nonemergency medical care or surgery (around 2.5% of all separations in both cases), while between two (4.3%) to three times (7.2%) as many were treated in public hospitals for nonemergency medical care and surgery, respectively (see AIHW 2017, Figure 8.2 and Figure 7.2, respectively).

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

7.2.3 Migration in and out of South Australia

One could be concerned that the babies we observe in their first five years of life may be a nonrepresentative sample, as some families may be internal or international migrants. According to the Australian Bureau of Statistics, in 2004 only 2,060 children aged 0–4 departed from South Australia. Assuming 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%), which can be considered a low number. Out-of-state migration would only pose a problem for statistical inference if outward migration is systematically correlated to infant health, that is, if the unhealthiest babies leave 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 specialized Women and Children's public hospital.[23]

7.3 *Inference*

In this section we discuss the robustness of our findings to alternative inference procedures, in particular multiple-hypothesis corrected standard errors for statistical inference.

[23] See http://stat.data.abs.gov.au/Index.aspx?DataSetCode=ABS_DEM_QIM, accessed 17 April 2018.

In the main analysis we test hypotheses on multiple outcomes, which increases the chance of falsely rejecting a correct null hypothesis, simply by chance. To address this concern, we present our main results under alternative inference procedures that correct for multiple hypotheses testing. We use the Romano–Wolf step-down procedure (Romano and Wolf, 2005*a*,*b*, 2016), implemented using the `rwolf2` Stata command (Clarke, Romano and Wolf, 2020). This procedure, which is based on resampling methods, controls the familywise error rate—the probability of rejecting at least one true null hypothesis across a set of hypotheses tested—by ensuring that the familywise error rate does not exceed its predetermined significance. We treat our main results as being all part of the same family of tests. One key advantage of this method is its high power compared to previous methods that have been criticized for being too conservative (e.g., Bonferroni, 1935; Holm, 1979; Westfall and Young, 1993).

Table B.14 presents the original asymptotic p-values associated with our main results in column (1), and step-down Romano–Wolf p-values correcting our original p-values using a familywise error rate in column (2) (Romano and Wolf, 2005*a*,*b*, 2016). All the statistically significant coefficient estimates in our main results remain statistically significant when we using step-down p-values: admissions to ward for acute or urgent problems, urgent inpatient services, and PPP presentations at inpatient services. In addition, we also present p-values constructed using the more conservative procedure proposed by Holm (1979) in column (3). Our findings are robust for both multiple hypothesis testing adjustment procedures.[24]

8 Mechanisms

In this section, we focus on the role of parental behaviors on the impact of the Baby Bonus. Cash transfers can, in principle, impact child health through two channels (Mayer, 1997; Yeung, Linver and Brooks-Gunn, 2002; Milligan and Stabile, 2011): first, the "resources channel" that captures the direct impact of additional income that allows carers to purchase more goods and services, and second the "family process channel" that captures the indirect impact of income on the psychological well-being of the family, which allows parents to spend more time with children in productive activities. Children benefit directly through more income when parents use additional household resources to purchase child-centered goods (see Dahl and Lochner, 2012), such as high-quality health care, day care, food, shelter, and clothing (Milligan and Stabile, 2011). There is some evidence for the direct income channel from previous studies (e.g.,

[24] An alternative method is to use the sharpened false discovery rate q-values proposed in Anderson (2008). This method entails first constructing indices to aggregate multiple hypotheses and eventually test fewer hypotheses, and second constructing sharp-null p-values using resampling techniques from Westfall and Young (1993). This method, however, does not account for correlations between items, which is likely an issue in our context. For example, infants with more than one presentation at the hospital within their first year of life are likely to experience more-severe health outcomes and have preventable hospital presentations. For this reason, we prefer to adjust for multiple hypothesis testing using the Romano–Wolf step-down procedure (Romano and Wolf, 2005*a*,*b*, 2016).

Cesarini et al., 2016; Kuehnle, 2014; Milligan and Stabile, 2011; Currie and Almond, 2011; Currie, 2009; Case, Lee and Paxson, 2008; Currie, Shields and Price, 2007; Propper, Rigg and Burgess, 2007; Currie and Stabile, 2003; Case, Lubotsky and Paxson, 2002; Yeung, Linver and Brooks-Gunn, 2002; Mayer, 1997), yet the conclusions vary across context, perhaps due to important variation in access to health care and the extent to which families are able to meet their health care needs.[25] There is also evidence on the importance of the family-process channel in the literature (see e.g., McLoyd, 1990; Currie, Shields and Price, 2007; Propper, Rigg and Burgess, 2007; Khanam, Nghiem and Connelly, 2009; Mullins, 2019). In particular, Mullins (2019) shows that welfare payments significantly improve parental welfare and the stability of spousal relationships, as well as assist mothers in returning to work smoothly and spending more time with their children.

In the next subsections, we present evidence for both the "resources channel" and the "family process channel".

8.1 Heterogeneity by Socioeconomic Background

First, we study the heterogeneity of the impact of the Baby Bonus by socioeconomic status (SES) using our administrative hospital records. We hypothesize that poorer families will benefit more from the Baby Bonus, because they are generally more cash constrained and the Baby Bonus represents a greater income shock for these families relative to household income. We find that our results are larger for low-SES families. This is unsurprising, because 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 child health and health care utilization in the first years of life for poorer families. This is what we show in the Appendix Table B.15.[26]

In the absence of data on household income, we proxy family resources by the father's occupation

[25]Exploiting lottery wins and administrative data from Sweden, Cesarini et al. (2016) show that a substantial lottery win of 1 million Swedish kroner (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 hospitalizations 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 hospitalizations 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 United Kingdom. Kuehnle (2014) finds that doubling household income reduces the probability of respiratory illness by 46% relative to the base probability.

[26]See also balancing tests by SES in Appendix Table B.16. Note that we find two statistically significant associations with treatment status suggesting that babies born just after the eligibility threshold are 2.1 ppts more likely to be born to single mothers and 57 grams heavier on average. However, we find precisely estimated no associations for another 21 pre-treatment characteristics of parents and indicators of baby health at birth.

on birth records, which we classify into high skilled (Column 1) versus low skilled (Column 3). High-skilled occupations refer to these with managerial, professional, and administrative tasks. Low-skilled occupations refer to trade, service, plant and operational workers. 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 presentations at emergency departments and inpatient services (Panels A and B), and potentially preventable pediatric presentations (Panel C). For instance, children of low-skilled fathers born after the eligibility threshold are –4.2 ppts less likely to present to emergency departments compared to babies born before the threshold (versus –2 ppts for the high-skilled group) and are –5.1 ppts less likely to be admitted to a ward (versus –2.3 ppts for the high-skilled group). Children of low- versus high-skilled fathers are –4.8 ppts less likely versus +0.8 ppt more likely to visit the emergency department for potentially preventable pediatric problems. However, we find no significant differences for planned visits (Panel D). Overall, the Baby Bonus seems to have helped low-SES parents prevent adverse shocks to their children's health.

8.2 *Parental Behavioral Change: Evidence from the HILDA Survey*

To study in-depth parental behavioral responses to the Baby Bonus, we use auxiliary data from the Household, Income, and Labor Dynamics in Australia (HILDA) survey, the leading nationally representative household survey in Australia, which has followed 7,682 households and over 13,000 individuals yearly since 2001.[27] We cannot achieve the same level of statistical precision using HILDA data compared to population-level administrative records, therefore we treat our results using HILDA data as suggestive evidence on parental behavioral responses to the Baby Bonus that complement our findings on preventable presentations and diagnoses using population-level administrative hospital records (See Sections 5.1 and 5.2).

Data preparation We use the restricted version of HILDA to identify all babies born around the time of the introduction of the Baby Bonus in July 2004. We focus on Waves 1 to 5 covering the years 2001 until 2005. Most participants were interviewed between September and November each financial year (July to June in Australia). Interviewers collected information on household composition—the number of household members, the number of dependents between 0 and 24 years of age, and detailed information on new or leaving household members, their arrival date (month, year), and their age at arrival. Respondents then answered demographic

[27] 13,969 participants from 7,682 households were followed on an annual basis with all members of those initial households aged 15 years or older, or persons who joined the household later. Individuals gave oral informed consent for participation in the study. Additionally, consent was sought from parents or guardians before seeking the involvement of household members aged less than 18 years. In 2011, the sample was refreshed with an additional 2,153 households. Sample loss and attrition were low, with re-interview rates rising from 87% in wave 2 to over 95% by wave 8 and remaining above that level in subsequent waves (Watson and Wooden, 2021; Summerfield et al., 2021).

questions, including their marital status and history, employment status and history, child care use, and detailed information on their sources of earned and unearned income. Eventually they moved on to a self-completion questionnaire (SCQ), which includes information regarding financial hardship since January of the interview calendar year, and detailed information about self-assessed health issues. The nonresponse rate is low for the SCQ component (below 10%).

Empirical specification We estimate on this sample our preferred nonparametric regression discontinuity models with a seven-day donut. We cluster standard errors at the level of the parent, although findings are stable to clustering at the household level. We consider bandwidths of 90 and 120 days around the threshold.

Our specification applied to the HILDA data has limitations. We are forced to use relatively narrow bandwidths because of the timing of interview relative to the Baby Bonus eligibility threshold and the Australian fiscal year cutoff. With a broad bandwidth, we would be comparing families who answer about the financial year before 2004 (for control babies) and about the financial year after 2004 (for treatment babies). At the same time, narrow bandwidths severely restrict the number of families in our estimation sample. We observe respectively 58 and 82 unique babies born within 90 and 120 days of July 1, 2004. Figure C.1 presents the distribution of daily births within 120 days of July 1, 2004. Ultimately, we view our results using the HILDA survey as low-powered yet suggestive of mechanisms underlying our main effects.

Appendix C presents evidence on the internal validity of our research design in the HILDA data. Table C.1 shows that there is no evidence of manipulation in the running variable based on nonparametric density tests; Table C.2 indicates that there no evidence of a discontinuity at the threshold in parental age at birth, respondent sex, and the number of dependents in the household aged 0–4, 5–9, 10–14, 15–24 respectively. We do find that babies eligible for the Baby Bonus are slightly older at the time of interview, however controlling for this variable does not alter our conclusions.

Findings We find suggestive evidence that the Baby Bonus helped eligible families cope with the arrival of a newborn in the household. Table C.3 presents results focusing on babies born within 120 days of the threshold, and Table C.4 presents results focusing on babies born within 90 days of the threshold. Overall, effects in both tables are of the same sign, but effects in the narrower bandwidth are implausibly large due to low statistical power, while effects in the broader bandwidth are smaller but loose statistical significance. Therefore, we interpret our findings on mechanisms qualitatively without paying close attention to the magnitude of effects or significance.

Tables C.3 and C.4 indicate that the introduction of the Australian Baby Bonus helped eligible families increase their weekly expenditures on food and groceries, partially substituting away from meals and take-out—albeit we cannot know whether those additional expenditures are focused on child-centered goods. We also consistently find that the policy decreased financial stress and hardship for those families: eligible families were more likely to report being able to raise $2,000 in an emergency, and less likely to report having gone without meals, to have been unable to heat their home, to pay their rent or mortgage on time or to pay their utility bills on time. Eligible families were also less likely to ask for financial help from families, friends and community organizations, or to pawn or sell something. Those results suggest that the cash injection from the Baby Bonus alleviated short-term financial constraints for those families, which may contribute to the effects we found on infant health care utilization and health status. We note that our treatment took place in the coldest months of the year and in the coldest part of the country; the Baby Bonus may have helped households pay for better heating, which has been associated with lower risks of respiratory infections in children with asthma (Howden-Chapman et al., 2008). This could partly explain the decline in hospital presentations for preventable bronchiolitis and acute respiratory problems for treated babies.

In addition, Tables C.3 and C.4 suggest that the Baby Bonus also had positive consequences on the quality of parental relationships and parental self-assessed health, which may have an impact on child health care utilization and health status on their own. We see consistently that eligible families were more likely to remain married or in a de facto partnership one year after the birth of the child, which suggests that the policy may have helped reduce conflict in the home and thus improve marital stability. Parents of babies born after the threshold consistently report higher self-assessed health on most dimensions—physical functioning, bodily pain (reverse coding with positive indicating less bodily pain), emotional health, social functioning, general health and mental health. These findings relate to the evidence regarding the "family-process" channel through which household income can affect child health. McLoyd (1990) suggests that income poverty is associated with poor parental health and high levels of maternal depression and stress. 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 United Kingdom and Australia.[28] 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.

Last, Tables C.3 and C.4 suggest that the the Baby Bonus may have had some impact on intended maternal labor supply and intended and effective child care use, although the HILDA does not contain variables ideally suited to measure these effects. To measure of intended maternal labor supply, we use the hours that the respondent would like to work, and we find small but positive point estimates suggesting a potential positive impact of the policy on this dimension;

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

we interpret this with a grain of salt, however, because the question asks about labor contracts in general, not specifically about returning to work after the arrival of a child, and given our research design there is no reason to expect selection into the treatment or control group based on one's labor contract around the threshold. Across Tables C.3 and C.4, we see inconsistent effects on mothers' intended child care use around the threshold, but we do find consistent suggestive evidence that the policy may have helped mothers with older children temporarily increase their child care use after the introduction of the Baby Bonus. Using a few additional hours of child care after the arrival of a newborn may have helped mothers spend more time and attention on the newborn baby than in the absence of the cash injection. Delaying the use of child care may have on its own prevented babies from getting sick.

9 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 that are not associated with additional compensatory presentations for acute or severe problems in subsequent years, we can make back-of-the-envelope 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 Baby Bonus cost \$3,000 per child, but since it substituted for other maternity benefits, 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 July 1, 2004, and December 31, 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 Baby Bonus costs corresponded to an increased investment in children ranging between \$18,550,200 and \$25,800,000.

We focus on urgent/severe and PPP presentations in the first year only to avoid estimating a return on investment that could fail at discerning necessary from unnecessary hospital presentations—our back-of-the-envelope calculations should therefore be considered as conservative estimated of the true return on investment of the Baby Bonus because we ignore the majority of presentations for less-severe problems that remain necessary. For example, had we found evidence consistent with adverse health outcomes for treated babies, such as a decline in 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.

Table 5 shows that treated babies were –3.8 ppts less likely to be admitted to a ward for urgent care and were –2.2 ppts less likely to be admitted for an overnight stay within their first year of life. They were also –3.2 ppts less likely to present at an emergency department for a potentially preventable pediatric problem. We derive the average costs for potentially preventable presentations at emergency departments, admissions to wards and overnight stays (calculated at the average number of days admitted) from the National Hospital Cost Data Collection.[29] The average costs of admissions to each service are:

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

The total cost savings in the first year of the introduction of the policy amounts therefore to \$6,359,322. Given the payout costs of the Australian Baby Bonus (\$18,550,200 to \$25,800,000 in 2004), we calculate that between 24% and 34% of the immediate costs of the policy were recouped immediately through a reduction in acute and preventable hospitalizations in the first year of life of children eligible to the Baby Bonus.

The calculated cost savings underestimate true savings because 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 the second year of life of treated babies). Although these health investments are costly, international studies suggest that we can expect positive effects in the longer-run (Aizer, Hoynes and Lleras-Muney, 2022).

10 Conclusions

Early life health, family, and income shocks have a long-lasting impact on children's health and human capital development and their adult labor-market trajectories (Almond, Currie and Duque, 2017, 2018). Baby bonus policies can be a powerful lever to assist vulnerable children and offer them a better start in life. Yet, little is known about the effectiveness of those policies at improving children's health outcomes and the mechanisms through which they can influence child health.

We contribute to this literature by providing new evidence on children's hospital care utilization and health outcomes from birth until age five induced by the introduction of the Australian Baby Bonus, an unconditional and unanticipated cash transfer paid to families with a newborn

[29] See https://www.ihpa.gov.au/sites/g/files/net636/f/publications/nhcdcround18.pdf.

child. We estimate these effects by implementing a donut regression discontinuity design using high-quality linked administrative data from the state of South Australia.

We show that the Australian Baby Bonus reduced emergency department presentations and inpatient services utilization, with stronger effects for disadvantaged families. We argue that these effects are economically meaningful, especially in a country with universal access to health care and a high standard of living. Importantly, we demonstrate that this reduction in hospital care is driven by a reduction in the presentations for acute, urgent, and severe problems (in particular respiratory problems) but not by a reduction in the demand for elective care. Combined, these effects suggest that the Baby Bonus may have led to an improvement in the health status of treated infants.

We investigate the role of parental behavioral change in explaining the positive impact of the Baby Bonus on infant health. We document that the Baby Bonus led to a decline in presentations for potentially preventable pediatric hospitalizations especially acute bronchiolitis (as indicated in hospital records by medical staff upon separation), and did not lead to an increase in presentations for injuries, trauma or poisoning. These findings suggest that the cash transfer helped parents of treated babies increase their investments in infant health, whether they were direct investments in preventive or primary care or indirect investments in improving the home or spending more time with their children.

To document finer mechanisms on within-family responses to the cash injection, we use the Household Income and Labour Dynamics in Australia (HILDA) survey, Australia's leading household panel, which has run since 2000. Although our sample is small, we find evidence that the Baby Bonus allowed families to increase expenditures on food and groceries and decreased the incidence of financial stress and financial hardship. We also find that the transfer decreased the likelihood of parental separation, improved parental physical and emotional self-assessed health, and led to a small increase in childcare use for older children in the family, which may have helped mothers spend additional time with their newborn. Supplementary investments in food and groceries, reduced financial stress and hardship, improved parental health and additional time spent with newborns are all important channels through which a modest cash injection can improve early life health conditions.

Our findings indicate that the Australian Baby Bonus was a worthwhile public investment in children and families with positive returns. Through a simple accounting exercise, we show that although the Baby Bonus was not means-tested and effects were concentrated in disadvantaged families, our estimates translate into economically meaningful budgetary savings. At the intensive margin alone and looking only at the first year of life, the Baby Bonus reduced the share of potentially preventable pediatric hospitalizations from one in four to one in five infants in the first year of life. Similarly, the Baby Bonus 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%. Because the Baby Bonus reduced presentations for acute and severe problems, which are the costliest types of presentations in the hospital care system, we show that at least 34% of the initial expenditure of the Baby Bonus was recouped within the first year of life of babies eligible to the Baby Bonus. This is likely an underestimation of the true positive impact of the policy on the health of treated children as they age.

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 suggest that targeting such transfers toward disadvantaged families may be both more effective and more efficient. The Australian Baby Bonus was abolished in 2014; we suggest that this abolition was perhaps 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 May 16, 2004 of the Baby Bonus introduced on July 1, 2004. We first provide graphical evidence on the incidence of birth shifting around July 1, 2004. Next, we replicate the findings of Gans and Leigh (2009) in 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 seven days of the implementation of the Baby Bonus affects of 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 Australian Baby Bonus. 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)[30], ii) the vast majority of birth shifting occurred within the days contiguous to July 1, 2004, and iii) birth shifting mostly took place for women who could reschedule a planned Cesarean-section birth[31]

Figure 1 suggests that birth shifting did take place in South Australia in the days closest to the threshold of July 1, 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, June 28, Tuesday, June 29, and Wednesday, June 30). In the same way, there is a peak in the number of births in early July. On Thursday, July 1, and Friday, July 2, 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 three to four days around the threshold could be sufficient to exclude birth shifting. Yet, graphical inspection is not sufficient to decide the exact number of days to exclude because other factors may influence the

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[30] In 2004, in total 254,200 babies were born in Australia. On June 30, 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 July 1 and 2, 2004 (978 and 902 respectively). Source: Australian Bureau of Statistics (ABS).

[31] There is no systematic evidence on how women are able to shift birth dates to a later date. In the context of the Australian Baby Bonus, Dr Chris Tippett, 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% 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 Cesarean-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 Cesarean-sections." ABC November 8, 2007, Simon Santow "Mums 'delaying births' for maximum Baby Bonus".

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 were 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 were births that occurred within three days of the July 1, 2004, threshold.

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 versus before July 1, 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) that were also used in Borra, González and Sevilla (2019):

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

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

where the dependent variables are respectively the number of daily births in equation (A.1) and the log number of daily births in equation (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 birth date eligibility threshold for the Baby Bonus on July 1, 2004. Under the assumption that we can correctly account for pre-trends in births around July 1, 2004, β captures the causal impact of the introduction of the Baby Bonus on the timing of births around July 1, 2004. To accurately account for pretrends, we include four 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, $1\{\text{Day of Year}\}_i$, 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 May 16, 2004, also announced future increases of the Baby Bonus payment scheduled for July 1, 2006, and July 1, 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 seven days of July 1, yearly; the second column focuses on births within 14 days, the third column births within 21 days, and the fourth column focuses on births within 28 days of July 1, each year. Using the same method as Gans and Leigh (2009), we compute that within seven days of July 1, 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)–(4) show that birth shifting did not seem to extend much beyond seven days from the threshold because the number of potentially shifted births grows only slowly as we expand the window around the threshold, and the share of potentially shifted births declines to 6.7%.

Following Borra, González and Sevilla (2019), we also explore whether birth shifting was driven by private hospitals 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.

Last, Table A.3 presents the results of balancing tests on predetermined characteristics without excluding births within seven days away of the threshold date. We find that babies born just after the threshold date are born 32 grams heavier than babies born just before the threshold date, and are 0.11 weeks older in gestational age. These findings are consistent with shifting births from the end of June to the beginning of July. We also find that babies born just after the threshold have 2.6 percentage points fewer complications compared to babies born just before the threshold, which, in line with Gans and Leigh (2009), suggests that birth shifting mostly occurred for scheduled low-risk births.

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

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

Window (W)	+/– 7 days	+/– 14 days	+/– 21 days	+/– 28 days
	(1)	(2)	(3)	(4)
Panel A. Number of daily births				
Baby Bonus	-14.22^{***}	-8.45^{***}	-7.18^{***}	-6.45^{***}
	(3.85)	(3.19)	(2.36)	(2.01)
R^2	0.76	0.68	0.63	0.61
N. shifted births	49.8	59.1	75.4	90.3
Panel B. Log number of daily births				
Baby Bonus	-0.26^{***}	-0.15^{**}	-0.14^{***}	-0.13^{***}
	(0.08)	(0.07)	(0.05)	(0.04)
R^2	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 versus 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: $\beta \times W/2$; 2) Share of shifted births in window W: $exp(\beta/2) - 1$. See Gans and Leigh (2009) for details.*

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)
Panel A. Number of births, by type of hospital								
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)
R^2	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
Panel B. Number of births, by mother patient status								
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)
R^2	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 versus July within the relevant window, by hospital type and mother's 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 $\times$ day of week. We cluster standard errors at the level of the date of birth. Windows relative to July 1, 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: $\beta \times W/2$; 2) Share of shifted births in window W: $exp(\beta/2) - 1$. See Gans and Leigh (2009) for details.*

Table A.3: Balancing Tests on Predetermined Characteristics and Birth Outcomes Without Donut

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Child and Parental Pretreatment Characteristics:							
Child is female	0.017	0.010	*0.081*	509	24,429	24,626	0.483
Birth in private hospital	0.010	0.012	0.379	399	19,149	19,144	0.341
No. of antenatal visits	−0.050	0.082	0.544	353	15,492	15,463	10.672
Mother smokes	0.003	0.008	0.665	618	28,878	29,581	0.205
Mother's age:							
35+	−0.003	0.008	0.731	592	28,164	28,673	0.180
40+	−0.002	0.004	0.668	503	24,129	24,355	0.031
Father's occupation:							
High skilled	−0.002	0.010	0.805	672	30,479	30,909	0.330
Low skilled	0.015	0.010	0.145	670	30,428	30,851	0.555
Mother's marital status:							
Never Married	0.008	0.007	0.234	531	25,422	25,670	0.116
Married	−0.004	0.008	0.618	451	21,684	21,775	0.872
Single	−0.003	0.003	0.204	447	21,514	21,611	0.013
Mother's race:							
Caucasian	0.000	0.006	0.962	512	24,562	24,788	0.908
Asian	0.003	0.004	0.469	565	26,879	27,285	0.047
Aboriginal or TSI	−0.004	0.004	0.413	470	22,546	22,747	0.045
Child birth outcomes:							
Baby weight	32.066	15.364	**0.037**	383	18,422	18,385	3348.9
Special Nursery	−0.005	0.010	0.599	412	19,797	19,828	0.168
NICU	−0.002	0.003	0.654	700	33,410	34,054	0.028
PICU	0.000	0.001	0.762	468	22,436	22,639	0.002
Neonatal death	−0.001	0.002	0.479	302	14,413	14,437	0.008
Apgar 1 min > 7	−0.007	0.010	0.477	515	24,610	24,788	0.759
Apgar 5 min > 7	−0.000	0.004	0.913	471	22,564	22,748	0.971
Gestational age	0.114	0.067	*0.088*	293	14,016	14,048	38.758
Preterm birth	−0.006	0.007	0.370	721	34,378	35,063	0.148
Obstetric complication	−0.026	0.015	*0.095*	274	12,969	13,115	0.318
C-section birth	0.007	0.012	0.547	708	33,741	34,438	0.308

Note: This table presents the results of balancing tests on pretreatment characteristics of children and their parents as well as birth outcomes based on birth and perinatal records, when we do not exclude births within seven days of July 1, 2004. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths 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* italics *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 threshold. 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 threshold. We perform a series of nonparametric density continuity test, where we progressively exclude days closest to the threshold date until we cannot reject the null that the control and treated observations are similarly distributed.

Table A.4 reports the results of these tests when we exclude zero days (Panel A), one day (Panel B) and five days (Panel C). Each panel reports the result of the density test according to three estimation methods: First, in line 1 we report the results of the test under unrestricted inference with two distinct estimated bandwidths ("U, 2-h"). Second, in line 2 we present the results of the test under unrestricted inference with one common estimated bandwidth ("U, 1-h"). Last, in line 3 we report the results of the test under restricted inference with one common estimated bandwidth ("R, 1-h"). In columns (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 threshold 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 threshold. We can reject the null at the 10% level that units before and after the threshold are similarly distributed in two out of three methods. Panel B shows that excluding one day on each side of the threshold substantially reduces this manipulation as the *p*-values sharply increase in all three methods. Yet, we can still reject the null at the 5% for one method out of three. Panel C shows that once we exclude five days on each side of the threshold, control and treated units are not statistically differently distributed[32]. Thus, we conclude that the minimum donut required for our estimations is five days around the threshold.

[32]Binomial tests confirm the findings of Table A.4.

Table A.4: Nonparametric Density Test for Alternative Donuts

	Estimated Bandwidth		Observations		Density Test
Estimation Method	Left	Right	Left	Right	p-val.
(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Excluding zero days					
Models with symmetric bandwidth:					
Restricted, linear	195	195	9,237	9078	0.726
Restricted, 2nd-order polynomial	556	556	17,626	17540	0.102
Unrestricted, linear	156	156	7,363	7259	0.579
Unrestricted, 2nd-order polynomial	88	88	4,082	4086	**0.041**
Models with asymmetric bandwidth:					
Unrestricted, linear	225	176	10,573	8216	0.646
Unrestricted, 2nd-order polynomial	66	105	2,983	4859	*0.078*
Panel B. Excluding one day					
Models with symmetric bandwidth:					
Restricted, linear	193	193	9,095	8897	0.731
Restricted, 2nd-order polynomial	490	490	17,583	17467	*0.096*
Unrestricted, linear	143	143	6,666	6608	0.488
Unrestricted, 2nd-order polynomial	90	90	4,129	4115	*0.068*
Models with asymmetric bandwidth:					
Unrestricted, linear	190	171	8,952	7955	0.994
Unrestricted, 2nd-order polynomial	67	106	2,977	4838	0.120
Panel C. Excluding five days					
Models with symmetric bandwidth:					
Restricted, linear	186	186	8,654	8393	0.799
Restricted, 2nd-order polynomial	412	412	17,432	17275	*0.077*
Unrestricted, linear	117	117	5,239	5159	0.315
Unrestricted, 2nd-order polynomial	95	95	4,190	4093	0.142
Models with asymmetric bandwidth:					
Unrestricted, linear	134	165	6,055	7444	0.480
Unrestricted, 2nd order polynomial	70	112	2,986	4917	0.168

Note: This table presents the result of three nonparametric density tests of the running variable, date of birth, for three alternative donuts (Panel A, zero days; Panel B, one day; Panel C, five days) around July 1, 2004. We conduct Cattaneo, Jansson and Ma (2020)'s test using the Stata command `rddensity` *(Cattaneo, Jansson and Ma, 2018). Column (1) indicates the local polynomial fit method and the bandwidth estimation method. Columns (2) and (3) indicate the estimated bandwidth on either side of the threshold (if applicable), and columns (4) and (5) indicate the number of observations used in the test on either side of the threshold. Column (6) presents the p-value of each density test comparing the distribution of births on each side of the threshold to a Gaussian approximation. Large p-values indicate that the distribution of births on either side of the threshold are not statistically different from one another. The sample used is the universe of children born in South Australia between July 1, 2003, and July 1, 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* italics *indicate effects that are statistically significant at the 10% level.*

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 observable and unobservable 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 worse health later in life or additional health problems that required hospital attention. Gans and Leigh (2009) show that shifted babies were more likely to be postponed vaginal births eventually delivered by Cesarean-section, and there is some evidence that babies delivered by C-section experience worse health outcomes[33]. 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 seven days of the threshold (Table 5), and slightly smaller for PPP presentations; these findings indicate that birth-shifting events induced a small negative bias on the true treatment effect of the Australian Baby Bonus on hospital presentations in general and a small positive bias on potentially preventable pediatric 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 parental sorting into birth-shifting and demand for hospital care. The positive bias we find for PPP presentations suggests rather the latter: babies born closest to the threshold have more PPP presentations than babies born slightly later. Overall, we would have overestimated the impact of the Baby Bonus if we failed to exclude births closest to the threshold date.

[33] For 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.5: The Effects of the Australian Baby Bonus on Hospital Presentations Within the First Year of Life Without Donut

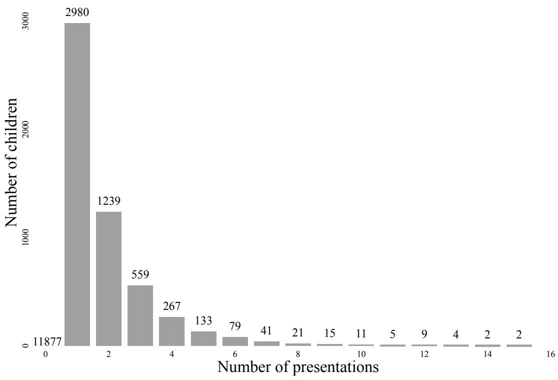
	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Health Care Utilization Index [std.]	−0.088	0.032	**0.007**	285	13,543	13,616	0.193
Health Care Utilization by Subitem							
Panel A. Presentations by hospital service:							
Any hospital service	−0.031	0.019	0.106	187	8,848	8,728	0.451
Emergency department	−0.024	0.016	0.123	163	7,667	7,652	0.311
Inpatient services	−0.039	0.016	**0.014**	224	10,524	10,518	0.311
Panel B. Presentations for urgent, acute or severe problems (by hospital service):							
Emergency department:							
Urgent, acute or severe problem	−0.021	0.015	0.156	184	8,691	8,602	0.205
Admission to ward	−0.028	0.011	**0.013**	196	9,237	9,148	0.129
Inpatient services:							
Urgent, acute or severe problem	−0.033	0.012	**0.005**	236	11,121	11,158	0.170
Admission to ward	0.004	0.007	0.497	176	8,313	8,286	0.027
Overnight admission	−0.026	0.013	**0.038**	279	13,191	13,314	0.205
Panel C. Potentially preventable pediatric presentations (by hospital service):							
Any PPP presentation	−0.023	0.015	0.125	174	8,182	8,203	0.218
Any PPP presentation, ED	−0.029	0.014	**0.037**	152	7,161	7,135	0.181
Any PPP presentation, IS	−0.015	0.010	0.109	193	9,138	9,040	0.104
Additional Items (not in Health Care Utilization Index)							
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):							
Emergency department:							
Planned visit	−0.005	0.005	0.270	160	7,519	7,508	0.017
Visit with med. referral	−0.002	0.007	0.835	172	8,147	8,147	0.054
Inpatient services:							
Planned visit	−0.002	0.007	0.835	172	8,147	8,147	0.026
Visit with med. referral	−0.012	0.008	0.173	231	10,918	10,985	0.095
Booked elective procedure	0.002	0.006	0.737	242	11,433	11,419	0.057

Note: This table presents the effects of the Australian Baby Bonus on hospital presentation of infants, when we do not exclude births within seven days of July 1, 2004. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths 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* italics *indicate effects that are statistically significant at the 10% level.*

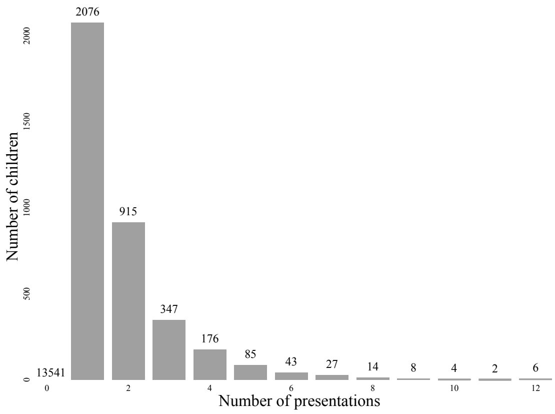
Appendix B Additional Tables and Figures

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

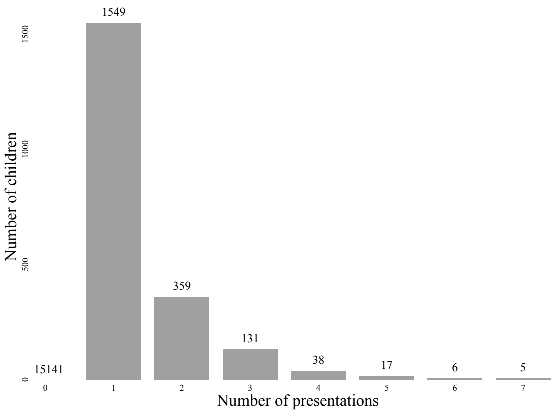
(a) Emergency department presentations



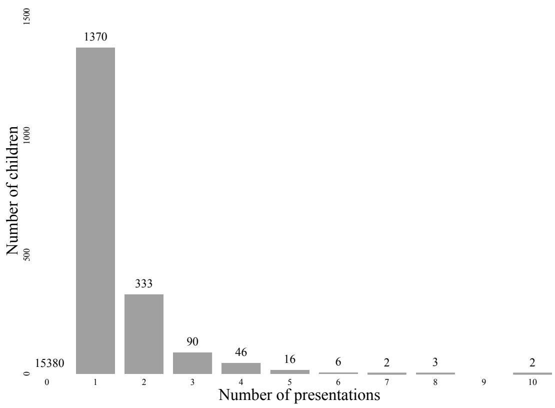
(b) Potentially preventable pediatric hospitalizations



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



(d) Respiratory problems recorded at presentation by triage nurse



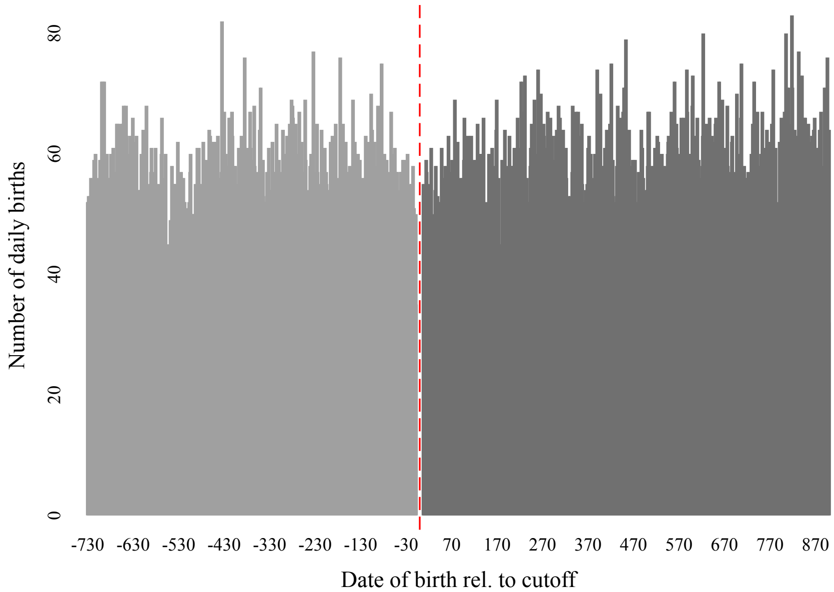
Source: Potentially preventable pediatric hospitalizations: Integrated South Australian Activity Collection (ISAAC); Emergency department presentations and emergency department presentations for respiratory problems: South Australian Emergency Department Data Collection (EDDC). Data are presented for the 2004 birth cohort, excluding 37 babies born overseas.

Table B.1: Summary Statistics of Hospitalizations at Ages 0–1

	Mean	Sd
Share of children with records before age 1:		
Health Care Utilization Index [std.]	0.183	0.160
Health Care Utilization: Subitems		
Hospital presentations:		
Any presentation, either ED or inpatient services	0.455	0.073
Any ED presentation	0.317	0.067
Any inpatient record	0.301	0.070
Presentation for urgent/acute problems:		
Emergency department:		
Any presentation for an urgent, acute or severe problem	0.205	0.058
Any presentation with admission to ward	0.123	0.047
Inpatient services:		
Any presentation for an urgent, acute or severe problem	0.157	0.053
Any presentation with admission to ward	0.025	0.025
Any presentation with overnight admission	0.195	0.062
Potentially preventable pediatric presentations:		
Any PPP presentation	0.221	0.059
Any PPP presentation, ED	0.187	0.056
Any PPP presentation, inpatient services	0.094	0.044
Additional Items (not in Health Care Utilization Index)		
Planned visits or presentations with medical referral:		
Emergency department:		
Any planned visit	0.017	0.019
Any visit with medical referral	0.051	0.033
Inpatient services:		
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

Note: This table presents descriptive statistics of the main outcome variables. The health care utilization index sums up hospital presentations for babies from birth until age 1 excluding birth-related problems. This index is standardised to mean 0 and standard deviation 1 for all babies born in South Australia between 1991 to 2016. The sample used in this table includes all babies born in South Australia between July 1, 2003 and July 1, 2005. For all variables, the sample contains 35,236 observations.

Figure B.2: Daily Number of Births in South Australia (2002–2006)



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

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

Window 1/2 Length	Furthest Day Away from Threshold	Observations		Density Test p-value
		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. Column (1) indicates the half-length of the sample evaluated, column (2) the distance from the cutoff date of the furthest days in the sample evaluated. Columns (3) and (4) display the number of observations on each side of the threshold in the sample evaluated, and column (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 July 1, 2003, and July 1, 2005, excluding 93 children born abroad during this time, and all children born within seven days of July 1, 2004.

Table B.3: The Effects of the Australian Baby Bonus on Number of Hospital Presentations Within the First Year of Life

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs. Left	N.Obs. Right	Pre-threshold Mean
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Health Care Utilization by Subitem							
Panel A. Presentations by hospital service:							
Any hospital service	−0.150	0.086	*0.082*	146	6,561	6,453	1.07
Emergency department	−0.121	0.064	*0.060*	112	4,944	4,879	0.59
Inpatient services	−0.060	0.032	*0.061*	267	12,362	12,371	0.48
Panel B. Presentation for urgent, acute or severe problems (by hospital service):							
Emergency department:							
Urgent, acute or severe problem	−0.059	0.029	**0.038**	161	7,272	7,126	0.32
Admission to ward	−0.060	0.021	**0.005**	159	7,194	7,016	0.18
Inpatient services:							
Urgent, acute or severe problem	−0.059	0.022	**0.008**	215	9,857	9,677	0.24
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	0.26
Panel C. Potentially preventable pediatric presentations (by hospital service):							
Any PPP presentation	−0.110	0.041	**0.007**	143	6,480	6,355	0.41
Any PPP presentation, ED	−0.079	0.035	**0.024**	120	5,302	5,221	0.28
Any PPP presentation, IS	−0.047	0.014	**0.001**	236	10,855	10,779	0.13
Additional Items (not in Health Care Utilization Index)							
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):							
Emergency department:							
Planned visit	−0.013	0.007	*0.059*	144	6,480	6,355	0.02
Visit with med. referral	0.000	0.012	0.971	146	6,619	6,497	0.06
Inpatient services:							
Planned visit	0.000	0.007	0.997	265	12,245	12,269	0.03
Visit with med. referral	−0.023	0.021	0.274	191	8,704	8,505	0.12
Booked elective procedure	0.001	0.013	0.961	217	9,921	9,731	0.07

Note: : This table presents results on the effects of the Australian Baby Bonus on the number of hospital presentations within the first year of life. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. p-values in ***bold*** *indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

Table B.4: The Effects of the Australian Baby Bonus on Detailed Medical Diagnostics Within the First Year of Life

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs. Left	N.Obs. Right	Pre-threshold Mean
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Presentations by detailed ICD-10-AM diagnostic chapter and presenting problem:							
Emergency Department ICD-10-AM Chapter:							
Respiratory problems	−0.032	0.012	**0.010**	168	7,666	7,518	0.128
Injuries, trauma and poisoning	0.004	0.007	0.595	182	8,316	8,069	0.034
Infections	−0.012	0.013	0.365	134	5,958	5,882	0.087
Digestive problems	−0.011	0.007	0.123	156	7,072	6,898	0.025
Skin problems	0.001	0.006	0.854	164	7,491	7,290	0.017
Externally caused problems	0.000	0.000	0.573	159	7,194	7,016	0.000
Unspecified problems	−0.019	0.012	*0.098*	160	7,228	7,077	0.070
Nervous system problems	0.001	0.001	0.205	170	7,731	7,587	0.001
Eye- and ear-related problems	−0.001	0.006	0.916	147	6,619	6,497	0.014
Emergency Department Triage Nurse:							
Respiratory problems	−0.038	0.012	**0.001**	164	7,438	7,252	0.113
Injuries, trauma and poisoning	0.011	0.006	*0.059*	163	7,376	7,221	0.022
Infections	−0.008	0.010	0.456	157	7,114	6,933	0.076
Digestive problems	−0.026	0.016	0.106	88	3,791	3,725	0.070
Skin problems	0.008	0.008	0.346	141	6,360	6,285	0.034
Unspecified problems	−0.011	0.015	0.462	151	6,828	6,671	0.074
Nervous system problems	−0.001	0.003	0.822	153	6,957	6,811	0.006
Eye- and ear-related problems	−0.009	0.008	0.286	130	5,768	5,662	0.019
Inpatient Services: ICD-10-AM Chapter							
Respiratory problems	−0.037	0.008	**0.000**	267	12,362	12,371	0.077
Injuries, trauma and poisoning	−0.003	0.003	0.345	298	13,960	13,821	0.009
Infections	−0.006	0.006	0.319	206	9,483	9,235	0.030
Digestive problems	−0.005	0.005	0.290	218	9,973	9,790	0.015
Skin problems	0.004	0.002	*0.070*	155	7,072	6,898	0.004
Externally caused problems	−0.012	0.006	**0.047**	302	14,151	14,032	0.052
Unspecified problems	−0.001	0.006	0.925	205	9,426	9,202	0.027
Nervous system problems	−0.001	0.001	0.549	264	12,146	12,133	0.002
Eye- and ear-related problems	0.003	0.003	0.400	169	7,731	7,587	0.007

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 polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. p-values in ***bold*** *indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

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

Child age:	1–2		2–3		3–4		4–5	
	Coef. Est. (1)	Sd.err. (2)	Coef. Est. (3)	Sd.err. (4)	Coef. Est. (5)	Sd.err. (6)	Coef. Est. (7)	Sd.err. (8)
Health Care Utilization Index [std.]	−0.028	0.044	0.009	0.031	−0.029	0.033	0.004	0.028
Health Care Utilization by Subitem								
Panel A. Presentations 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. Presentation for urgent, acute or severe problems (by hospital service):								
Emergency department:								
Urgent, acute or severe problem	−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
Inpatient services:								
Urgent, acute or severe problem	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 D. Potentially preventable pediatric presentations (by hospital service):								
Any PPP presentation	−0.017	0.022	0.014	0.016	−0.002	0.010	−0.002	0.008
Any PPP presentation, ED	−0.026	0.022	0.013	0.014	0.009	0.010	−0.003	0.008
Any PPP presentation, IS	−0.002	0.012	−0.005	0.007	−0.009**	0.005	−0.001	0.004
Additional Items (not in Health Care Utilization Index)								
Panel C. Any planned visits or presentations with referral from medical staff (by hospital service):								
Emergency department:								
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
Inpatient services:								
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

*Note: This table presents RD treatment effects of the Baby Bonus on hospital presentations at ages 1–5. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. Panel A presents results in the second year of life, Panel B presents results in the third year, Panel C presents results in the fourth year and Panel D presents results in the fifth year of life. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Table B.6: The Effects of the Australian Baby Bonus on Diagnoses at Ages 1–5

Child age:	1–2		2–3		3–4		4–5	
	Coef. Est. (1)	Sd.err. (2)	Coef. Est. (3)	Sd.err. (4)	Coef. Est. (5)	Sd.err. (6)	Coef. Est. (7)	Sd.err. (8)
Panel A. Potentially preventable pediatric presentations (combined ED/IS):								
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:								
Emergency department:								
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
Eye- and ear-related problems	−0.011∗	0.006	0.002	0.004	0.003	0.003	−0.001	0.003
Inpatient services:								
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
Eye- and ear-related 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 diagnoses for presentations at ages 2–5. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. Panel A presents results in the second year of life, Panel B presents results in the third year, Panel C presents results in the fourth year and Panel D presents results in the fifth year of life. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Table B.7: Sensitivity of Main Results to Clustering Choice

Bandwidth Method:	MSE-optimal		CER-optimal	
	Coef.	Sd.err.	Coef.	Sd.err.
	(1)	(2)	(3)	(4)
Health Care Utilization Index [std.]	-0.165^{***}	(0.027)	-0.095^{***}	(0.034)
Health Care Utilization by Subitem				
Panel A. Presentations by hospital service:				
Any hospital service	-0.020	(0.020)	0.000	(0.024)
Emergency department	-0.024	(0.020)	-0.025	(0.024)
Inpatient service	-0.044^{***}	(0.015)	-0.018	(0.019)
Panel B. Presentations for urgent, acute or severe problems (by hospital service):				
Emergency department:				
Urgent, acute or severe problem	-0.020	(0.016)	-0.020	(0.019)
Admission to ward	-0.038^{***}	(0.013)	-0.038^{**}	(0.016)
Inpatient services:				
Urgent, acute or severe problem	-0.042^{***}	(0.012)	-0.031^{**}	(0.015)
Admission to ward	0.002	(0.005)	0.007	(0.007)
Overnight admission	-0.027^{**}	(0.011)	-0.018	(0.013)
Panel C. Potentially preventable pediatric presentations (by hospital service):				
Any PPP	-0.034^{**}	(0.017)	-0.033	(0.021)
Any PPP, ED	-0.031^{*}	(0.016)	-0.037^{**}	(0.021)
Any PPP, inpatient services	-0.034^{***}	(0.009)	-0.025^{**}	(0.011)
Additional Items (not in Health Care Utilization Index)				
Panel D. Planned visits or with referral (by hospital service):				
Emergency department:				
Planned visit	-0.010^{*}	(0.005)	-0.009	(0.007)
Visit with med. referral	0.000	(0.009)	0.002	(0.011)
Inpatient services:				
Planned visit	-0.005	(0.004)	0.001	(0.005)
Visit with med. referral	-0.016^{*}	(0.010)	-0.002	(0.012)
Booked elective procedure	-0.001	(0.007)	0.004	(0.009)

*Note: This table presents the effects of the Australian Baby Bonnus on hospital presentation of infants without clustering standard errors, using MSE- and CER-optimal bandwidths. Each row reports the results of a separate regression analysis using local linear estimation, robust bias-corrected inference and optimal bandwidth selection. We exclude children born overseas, and all births within seven days of July 1, 2004. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Table B.8: Sensitivity of Main Results to Observations Near the Threshold

Exclude births within:	5 days		8 days		12 days		15 days	
	Coef. Est. (1)	Sd.err. (2)	Coef. Est. (3)	Sd.err. (4)	Coef. Est. (5)	Sd.err. (6)	Coef. Est. (7)	Sd.err. (8)
Health Care Utilization Index [std.]	−0.095***	0.036	−0.094***	0.035	−0.088**	0.034	−0.073**	0.037
Health Care Utilization by Subitem								
Panel A. Presentations 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. Presentations for urgent, acute or severe problems (by hospital service):								
Emergency department:								
Urgent, acute or severe problem	−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
Inpatient services:								
Urgent, acute or severe problem	−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. Potentially preventable pediatric presentations (by hospital service):								
Any PPP presentation	−0.029*	0.017	−0.027	0.019	−0.023	0.020	−0.005	0.020
Any PPP presentation, ED	−0.030*	0.016	−0.027	0.018	−0.021	0.018	−0.005	0.018
Any PPP presentation, IS	−0.026***	0.010	−0.029***	0.010	−0.030***	0.011	−0.028**	0.012
Additional Items (not in Health Care Utilization Index)								
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):								
Emergency department:								
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
Inpatient services:								
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

*Note: This table presents the sensitivity of our main results to observations close to the threshold, that is, sensitivity to our choice of donut. In Panel A, results also exclude all births within five days of July 1, 2004; Panel B excludes all births within 8 days of the threshold; Panel C excludes all births within 12 days of the threshold; and Panel D excludes all births within 15 days of the threshold. Each donut has the same number of weekend days on each side of the threshold. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Table B.9: The Effects of the Australian Baby Bonus on on Selective Abortions

	Coef. Est.	Sd.err.	*p*-value	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
					Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Past Pregnancies:							
Any past pregnancy	−0.015	0.016	0.337	313	14,639	14,529	0.689
Number of live births	−0.016	0.027	0.567	419	19,937	19,783	0.916
Any miscarriage	−0.011	0.011	0.311	439	20,859	20,742	0.226
Abortions:							
Any abortion	−0.006	0.007	0.340	671	31,792	32,261	0.136
Number of abortions	−0.007	0.010	0.508	664	31,378	31,841	0.177
Days since last abortion	−1.276	31.730	0.968	392	8,892	8,511	1092.650

Note: This table presents additional balancing tests related to selective abortions and fertility decisions induced by the announcement of the Baby Bonus on May 12, 2004. Because the announcement was so close to the implementation of the policy on July 1, 2004, and because abortions are restricted in South Australia, we should not expect to find any significant differences between babies before and after the threshold 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 with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. p-values in ***bold*** *indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

Table B.10: Sensitivity of Main Results to Optimal 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)
Health Care Utilization Index [std.]	−0.098***	0.034	−0.166***	0.03	−0.099***	0.033
Health Care Utilization by Subitem						
Panel A. Presentations 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. Presentation for urgent, acute or severe problems (by hospital service):						
Emergency department:						
Urgent, acute or severe problem	−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
Inpatient services:						
Urgent, acute or severe problem	−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. Potentially preventable pediatric presentations (by hospital service):						
Any PPP presentation	−0.033*	0.019	−0.034**	0.017	−0.025	0.016
Any PPP presentation, ED	−0.032*	0.018	−0.030*	0.016	−0.019	0.015
Any PPP presentation, IS	−0.028***	0.010	−0.034***	0.009	−0.027***	0.009
Additional Items (not in Health Care Utilization Index)						
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):						
Emergency department:						
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
Inpatient services:						
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

*Note: This table presents the effects of the Australian Baby Bonus on hospital presentation of infants using three different optimal bandwidth selection methods (CER, MSE and Two-sided CER). Each line represents a separate regression analysis, using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. *, **, and *** denote effects significance at the 10%, 5% and 1% respectively.*

Table B.11: Sensitivity of Main Results to Narrower Bandwidth

Bandwidth Method:	CER-optimal		1/2 CER-optimal		90 days		60 days	
	Coef.	Sd.err.	Coef.	Sd.err.	Coef.	Sd.err.	Coef.	Sd.err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Health Care Utilization Index [std.]	-0.098^{***}	0.034	-0.082	0.069	-0.224^{**}	0.097	-0.321^{**}	0.136
Health Care Utilization by Subitem								
Panel A. Presentations by hospital service:								
Any hospital service	-0.013	0.021	-0.078	0.052	-0.072	0.051	-0.125^{*}	0.073
Emergency department	-0.024	0.020	-0.105^{**}	0.046	-0.092^{**}	0.040	-0.135^{**}	0.058
Inpatient service	-0.034^{**}	0.017	-0.007	0.035	-0.007	0.041	-0.035	0.059
Panel B. Presentations for urgent, acute or severe problems (by hospital service):								
Emergency department:								
Urgent, acute or severe problem	-0.017	0.018	-0.075	0.047	-0.069	0.045	-0.123^{*}	0.064
Admission to ward	-0.038^{***}	0.013	-0.073^{**}	0.032	-0.070^{**}	0.031	-0.101^{**}	0.047
Inpatient services:								
Urgent, acute or severe problem	-0.037^{***}	0.013	-0.042	0.031	-0.042	0.035	-0.050	0.053
Admission to ward	0.002	0.007	0.047^{**}	0.021	0.055^{**}	0.024	0.091^{**}	0.038
Overnight admission	-0.022^{*}	0.012	0.031	0.025	0.029	0.039	0.050	0.055
Panel C. Potentially preventable pediatric presentations (by hospital service):								
Any PPP presentation	-0.033^{*}	0.019	-0.108^{**}	0.045	-0.100^{**}	0.041	-0.117^{**}	0.036
Any PPP presentation, ED	-0.032^{*}	0.018	-0.110^{**}	0.046	-0.094^{**}	0.039	-0.121^{**}	0.053
Any PPP presentation, IS	-0.028^{***}	0.010	-0.019	0.022	-0.022	0.026	-0.009	0.037
Additional Items (not in Health Care Utilization Index)								
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):								
Emergency department:								
Planned visit	-0.010^{*}	0.005	-0.012	0.017	-0.016	0.014	-0.003	0.021
Visit with med. referral	0.002	0.009	0.019	0.021	0.016	0.018	0.009	0.025
Inpatient services:								
Planned visit	-0.001	0.004	0.006	0.010	-0.024^{*}	0.014	-0.014	0.021
Visit with med. referral	-0.010	0.010	0.020	0.023	0.025	0.025	0.085^{**}	0.035
Booked elective procedure	0.003	0.008	0.017	0.017	0.018	0.020	0.062^{**}	0.029

*Note: This table presents the effects of the Australian Baby Bonus on hospital presentation of infants using the CER-optimal bandwidth (our preferred specification), half of the CER-optimal bandwidth, and two fixed bandwidths at 90 days and 60 days around the threshold. Each line represents a separate regression analysis using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, clustering standard errors at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. *, **, and *** denote effects significance at the 10%, 5% and 1% respectively.*

Table B.12: Main Results and Placebo Eligibility Thresholds

	Coef. Est.	Sd.err.	Bandwidth 1/2 length	Est. rank at true cutoff	P-value	
					Asymp.	Rand.-based
	(1)	(2)	(3)	(4)	(5)	(6)
Health Care Utilization Index [std.]	−0.098	0.034	306.2	2	0.004	0.022
Health Care Utilization by Subitem						
Panel A. Presentations by hospital service:						
Any hospital service	−0.013	0.021	175.3	91	0.532	1.006
Emergency department	−0.024	0.020	152.1	5	0.230	0.055
Inpatient service	−0.034	0.017	214.6	63	0.040	0.696
Panel B. Presentations for urgent, acute or severe problems (by hospital service):						
Emergency department:						
Urgent, acute or severe problem	−0.017	0.018	171.9	37	0.369	0.409
Admission to ward	−0.038	0.013	173.3	4	0.004	0.044
Inpatient services:						
Urgent, acute or severe problem	−0.037	0.013	206.7	9	0.005	0.099
Admission to ward	0.002	0.007	206.0	158	0.761	0.265
Overnight admission	−0.022	0.012	298.8	83	0.074	0.917
Panel C. Potentially preventable pediatric presentations (by hospital service):						
Any PPP presentation	−0.033	0.019	152.6	10	0.083	0.110
Any PPP presentation, ED	−0.032	0.018	142.1	11	0.071	0.122
Any PPP presentation, IS	−0.028	0.010	214.8	24	0.005	0.265
Additional Items (not in Health Care Utilization Index)						
Panel D. Any planned visits or presentations with referral from medical staff (by hospital service):						
Emergency department:						
Planned visit	−0.010	0.005	146.5	4	0.074	0.044
Visit with med. referral	0.002	0.009	151.9	108	0.845	0.818
Inpatient services:						
Planned visit	−0.001	0.004	289.4	123	0.882	0.652
Visit with med. referral	−0.010	0.010	198.3	94	0.308	0.972
Booked elective procedure	0.003	0.008	230.2	162	0.657	0.221

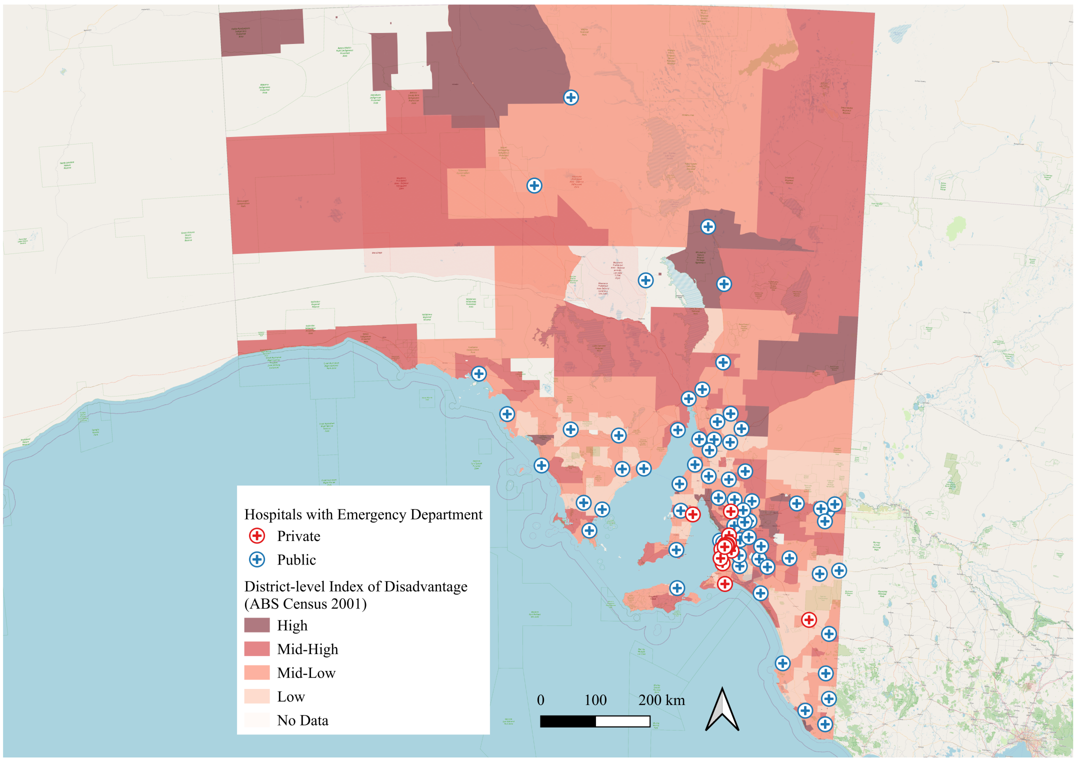
Note: This table presents the results of permutation tests following Ganong and Jäger (2018). Each line corresponds to the result of a separate regression using our main specification at the true threshold (July 1, 2004). We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. For each outcome variable, column (4) presents the rank of the coefficient estimate at the true threshold compared to coefficient estimates at 180 alternative thresholds, ranging from -90 to +90 around the true threshold. Column (5) presents asymptotic p-values, and column (6) randomization-based p-values.

Table B.13: The Effects of the Australian Baby Bonus on Predetermined Characteristics and Birth Outcomes in Prepolicy Years

	Placebo Prepolicy Years Thresholds			
	July 1, 2002		July 1, 2003	
	Coef. Est.	Sd.err.	Coef. Est.	Sd.err.
	(1)	(2)	(3)	(4)
Child and Parental Predetermined Characteristics:				
Mother's age:				
35+	0.001	0.009	−0.003	0.008
40+	−0.008**	0.004	0.000	0.004
Father's occupation:				
High skilled	0.010	0.012	−0.002	0.012
Low skilled	−0.007	0.011	−0.004	0.013
Mother's marital status:				
Never Married	−0.001	0.009	−0.008	0.007
Married	0.007	0.009	0.009	0.007
Single	−0.006	0.004	0.001	0.003
Mother's race:				
Caucasian	0.002	0.008	−0.001	0.006
Asian	−0.002	0.005	0.001	0.004
Aboriginal or TSI	0.001	0.006	−0.001	0.004
Child birth outcomes:				
Female	0.005	0.014	−0.005	0.013
Baby weight	10.754	16.199	12.134	14.284
Special Nursery	0.017	0.011	−0.013	0.011
NICU	−0.002	0.005	−0.001	0.004
PICU	0.003**	0.001	−0.001	0.001
Neonatal death	0.001	0.001	0.000	0.001
Apgar 1 min > 7	−0.022*	0.012	0.004	0.011
Apgar 5 min > 7	0.002	0.004	0.000	0.004
Gestational age	−0.039	0.062	−0.016	0.053
Preterm birth	0.000	0.010	0.001	0.009
Obstetric complication	0.009	0.015	−0.043***	0.013
C-section	−0.004	0.018	−0.014	0.014
Private hospital	0.033	0.024	0.001	0.014
No. antenatal visits	−0.085	0.110	0.178**	0.094
Mother smokes	0.001	0.010	−0.014	0.010

*Note: This table presents RD treatment effects of the Baby Bonus on predetermined characteristics of children and their parents as well as birth outcomes based on birth and perinatal records in prepolicy years (2002 and 2003). Each line x panel corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Figure B.3: 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.

Table B.14: Robustness of Main Results to Multiple Hypothesis Testing

		P-Values	
	Original	Romano–Wolf	Holm
	(1)	(2)	(3)
Health Care Utilization Index [std.]	**0.004**	**0.014**	**0.018**
Health Care Utilization by Subitem			
Panel A. Presentations by hospital service:			
Any hospital service	0.532	0.935	1.000
Emergency department	0.230	0.617	0.839
Inpatient service	**0.040**	0.123	*0.052*
Panel B. Presentations for urgent, acute or severe problems:			
Emergency department:			
Urgent, acute or severe problem	0.369	0.818	1.000
Admission to ward	**0.004**	**0.014**	**0.017**
Inpatient services:			
Urgent, acute or severe problem	**0.005**	**0.016**	**0.015**
Admission to ward	0.761	0.980	1.000
Overnight admission	*0.074*	0.246	0.538
Panel C. Potentially preventable pediatric presentations:			
Any PPP presentation	*0.083*	0.246	0.180
Any PPP presentation, ED	*0.071*	0.242	0.288
Any PPP presentation, IS	**0.005**	**0.016**	**0.016**
Additional Items (not in Health Care Utilization Index)			
Panel D. Planned visits/presentations with medical referral:			
Emergency department:			
Planned visit	*0.074*	0.246	0.230
Visit with med. referral	0.845	0.980	1.000
Inpatient services:			
Planned visit	0.882	0.980	0.875
Visit with med. referral	0.308	0.744	0.930
Booked elective procedure	0.657	0.965	1.000

*Note: This table presents p-values of RD treatment effects of the Australian Baby Bonus on hospital presentations of infants, under three alternative inference methods. Our main specification uses local polynomial estimation and robust bias-corrected inference methods, with CER-optimal bandwidths and clustering standard errors at the level of birth dates. Column (1) indicates p-values obtained from our main estimation method. Column (2) presents Romano-Wolf p-values corrected for familywise error rate (see Romano and Wolf, 2005*a*, *2016; Clarke, Romano and Wolf, 2020). Column (3) presents Holm corrected p-values (Holm, 1979). We exclude children born overseas, and all births within seven days of July 1, 2004. p-values in* ***bold*** *indicate effects that are statistically significant at least at the 5% level; p-values in* italics *indicate effects that are statistically significant at the 10% level.*

Table B.15: The Effects of the Australian Baby Bonus on Hospital Presentations by Socioeconomic Status

Sample split:	Father's occupation:			
	High skilled		Low skilled	
	Coef. Est.	Sd.err.	Coef. Est.	Sd.err.
	(1)	(2)	(3)	(4)
Health Care Utilization Index [std.]	−0.049	0.050	−0.124***	0.042
Health Care Utilization by Subitem				
Panel A. Presentations 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. Presentations for urgent, acute or severe problems (by hospital service):				
Emergency department:				
Urgent, acute or severe problem	−0.028	0.029	−0.020	0.022
Admission to ward	−0.023	0.021	−0.051***	0.018
Inpatient services:				
Urgent, acute or severe problem	−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. Potentially preventable pediatric presentations:				
Any PPP presentation	0.009	0.033	−0.049*	0.027
Any PPP presentation, ED	0.008	0.033	−0.048**	0.023
Any PPP presentation, IS	−0.024	0.016	−0.028**	0.012
Additional Items (not in Health Care Utilization Index)				
Panel D. Any planned visits or presentations with medical referral:				
Emergency department:				
Planned visit	0.003	0.010	−0.010	0.007
Visit with med. referral	0.008	0.015	−0.001	0.013
Inpatient services:				
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

*Note: This table presents RD treatment effects of the Baby Bonus on hospital presentations across SES at birth. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas in 2004 and all births within seven days of July 1, 2004. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

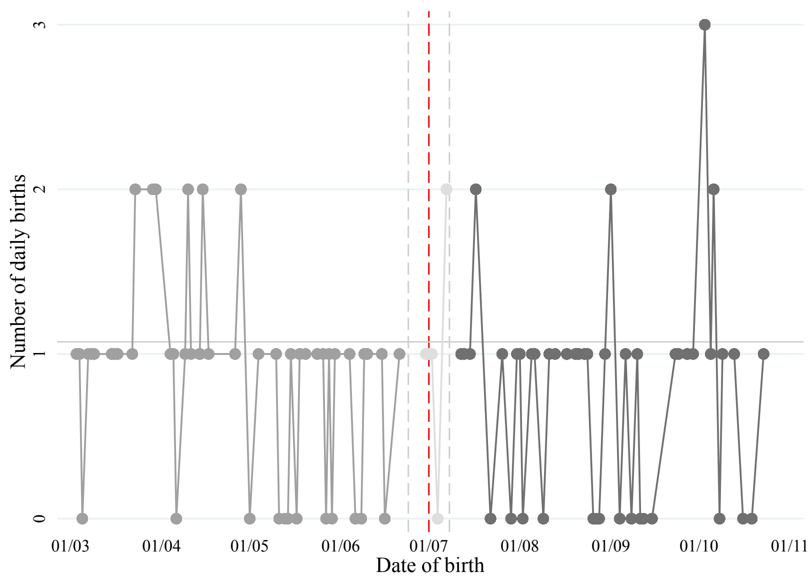
Table B.16: The Effects of the Australian Baby Bonus on Pretreatment Characteristics and Birth Outcomes by Socioeconomic Status

Sample split:	Father's occupation:			
	High skilled		Low skilled	
	Coef. Est.	Sd.err.	Coef. Est.	Sd.err.
	(1)	(2)	(3)	(4)
Child and Parental Predetermined Characteristics:				
Child is female	0.004	0.023	0.02	0.013
Private hospital	−0.017	0.02	0.017	0.015
No. antenatal visits	−0.169*	0.098	0.034	0.099
Mother smokes	−0.019	0.012	0.008	0.012
Mother's age:				
35+	−0.035*	0.018	0.012	0.009
40+	−0.007	0.008	−0.007	0.005
Mother's marital status:				
Never Married	0.003	0.007	0.021***	0.008
Married	−0.001	0.008	−0.018**	0.008
Single	−0.001	0.003	−0.005	0.003
Mother's race:				
Caucasian	−0.011	0.01	−0.003	0.008
Asian	0.010	0.008	0.002	0.006
Aboriginal or TSI	0.002	0.006	0.001	0.004
Child birth outcomes:				
Baby weight	18.693	20.532	57.507**	22.749
Special Nursery	−0.007	0.016	0.007	0.013
NICU	−0.004	0.008	0.003	0.005
PICU	0.002	0.002	0.000	0.001
Neonatal death	0.001	0.002	−0.001	0.002
Apgar 1 min > 7	0.000	0.015	−0.006	0.015
Apgar 5 min > 7	0.000	0.007	−0.004	0.006
Gestational age	0.063	0.083	0.143	0.096
Preterm birth	−0.009	0.012	−0.009	0.011
Obstetric complication	−0.004	0.019	−0.022	0.018
C-section	0.010	0.019	0.004	0.015

*Note: This table presents RD treatment effects of the Baby Bonus on pretreatment characteristics of children and their parents based on birth and perinatal records across SES at birth. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. We exclude 38 children born overseas and all births within seven days of July 1, 2004. *, **, and *** denote effects significance at the 10%, 5% and 1% respectively.*

Appendix C Supplementary Evidence from the HILDA Household Survey

Figure C.1: Daily Number of Births to HILDA Households in Australia within 90 Days of July 1, 2004



Note: This figure shows the number of daily births to HILDA respondents in Australia between March 1, 2004, and November 1, 2004 (90 days within July 1, 2004). The red vertical line indicates the threshold date, July 1, 2004 (Thursday). The gray area represents births within seven days of the threshold, which are excluded from our estimation sample.

Table C.1: HILDA - Results of Local Polynomial Density Test

Estimation Method (1)	Est. Bandwidth Left (2)	Est. Bandwidth Right (3)	N. Unique Babies Left (4)	N. Unique Babies Right (5)	Density Test *p*-val. (6)
Excluding 7-day donut:					
Restricted, linear	1,236	1,236	670	156	0.472
Unrestricted, linear	167	167	67	52	0.317
Including all births:					
Restricted, linear	1,038	1,038	516	156	0.508
Unrestricted, linear	146	146	55	48	0.199

Note: This table presents the results of nonparametric density tests of the running variable around July 1, 2004 using HILDA data. We conduct Cattaneo, Jansson and Ma (2020)'s test using the Stata command `rddensity` (Cattaneo, Jansson and Ma, 2018). Column (1) indicates the local polynomial fit method and the bandwidth estimation method. Columns (2) and (3) indicate the estimated bandwidth on either side of the threshold (if applicable), and columns (4) and (5) indicate the number of observations used in the test on either side of the threshold. Column (6) presents the p-value of each density test comparing the distribution of births on each side of the threshold to a Gaussian approximation. Large p-values indicate that the distribution of births on either side of the threshold are not statistically different from one another. The sample used is HILDA respondents with children born in Australia between July 1, 2003, and July 1, 2005, excluding all children born within seven days of July 1, 2004.

Table C.2: Balancing Tests on Predetermined Characteristics (HILDA)

	Coef. Est.	Sd.err.	*p*-value	Pre-threshold Mean
Respondent age at interview (in days)	−606.099	1137.713	0.594	12128.156
Respondent sex	−0.065	0.318	0.839	0.528
# children aged 0-4 in the household	−0.378	0.308	0.221	1.459
# children aged 5-9 in the household	−0.214	0.366	0.559	0.307
# children aged 10-14 in the household	0.074	0.179	0.681	0.104
# children aged 15-24 in the household	−0.068	0.046	0.137	0.026
Baby age at interview (in days)	66.21	21.363	*0.002*	326.234

Note: This table presents the results of balancing tests on pretreatment characteristics of children and their parents using data from the Household, Income, and Labor Dynamics in Australia (HILDA) survey. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods with bandwidths of 120 days around the threshold. We cluster standard errors at the level of respondents. We exclude all children born within seven days of July 1, 2004. The sample contains 82 unique babies and 149 parents. p-values in italics indicate effects statistically significant at least at the 10% level.

Table C.3: The Effects of the Australian Baby Bonus on Parental Behaviors (HILDA)

	Coef. Est.	Sd.err.	*p*-value	Pre-threshold Mean
Current Weekly Household Expenditures (in $)				
All groceries	23.440	35.514	0.509	167.468
Food	25.930	34.124	0.447	123.316
Meals/Take-out	−33.670	25.027	0.179	49.710
Financial Stress Since January				
Able to raise emergency $2,000	0.243	0.152	0.108	0.826
Went without meals	−0.065	0.087	0.454	0.032
Unable to heat home	0.011	0.052	0.837	0.014
Could not pay elec./gas/telephone bills	−0.262	0.180	0.146	0.248
Could not pay the mortgage or rent on time	−0.166	0.128	0.196	0.171
Had to pawn or sell something	−0.129	0.128	0.315	0.115
Asked for financial help from friends/family	−0.018	0.197	0.926	0.248
Asked for help from welfare/community organizations	−0.075	0.104	0.473	0.046
Marital stability	0.193	0.093	0.038	0.952
Parental Self-Assessed Health Status				
Physical functioning	11.119	8.072	0.168	90.617
Bodily pain	5.944	10.515	0.572	80.014
Ability to perform job	−4.260	12.279	0.729	88.079
General health	7.231	10.258	0.481	74.807
Vitality	−1.868	11.390	0.870	57.867
Emotional health	14.345	11.852	0.226	88.479
Social functioning	10.752	10.170	0.290	86.124
Mental health	5.691	8.861	0.521	75.702
Maternal Labor Supply				
Hours would like to work	8.662	8.663	0.317	35.698
Child Care Use				
Intended use*	0.192	0.243	0.431	1.455
Total weekly hours of child care				
All school-aged children, during term	−22.795	7.002	0.001	7.167
All school-aged children, during holidays	16.441	26.778	0.539	24.714
All not-yet-at-school children	10.812	9.938	0.277	23.010

*Note: This table presents our findings on the effects of the Australia Baby Bonus on parental behaviors based on data from the Household, Income, and Labor Dynamics in Australia (HILDA) survey. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods with a 120-day bandwidth around the threshold and cluster standard errors at the level of respondents. We exclude all children born within seven days of July 1, 2004. Each line is based on a sample of 82 unique babies born to 149 parents within 120 days of July 1, 2004, who are HILDA respondents. p-values in italics indicate effects statistically significant at least at the 10% level. Intended Use * : "In last 12 months, have you used/thought about using child care so you can work?". Self-assessed health status are summative scales transformed into 0–100 scale by the HILDA team.*

Table C.4: The Effects of the Australian Baby Bonus on Parental Behaviors (HILDA)

	Coef. Est.	Sd.err.	*p*-value	Pre-threshold Mean
Current Weekly Household Expenditures (in $)				
All groceries	74.17	43.70	*0.090*	173.76
Food	98.24	40.48	*0.015*	126.78
Meals/Take-out	−35.41	32.59	0.277	52.23
Financial Stress Since January				
Able to raise an emergency $2,000	0.391	0.178	*0.028*	0.84
Went without meals	−0.153	0.078	*0.050*	0.036
Unable to heat home	−0.104	0.057	*0.067*	0.012
Could not pay elec./gas/telephone bills	−0.396	0.205	*0.053*	0.213
Could not pay mortgage/rent on time	−0.146	0.145	0.315	0.132
Had to pawn or sell something	−0.160	0.140	0.254	0.124
Asked for financial help from family/friends	−0.165	0.220	0.454	0.201
Asked for financial help from welfare/community organizations	−0.276	0.113	*0.015*	0.024
Marital stability	0.238	0.112	*0.033*	0.954
Parental Self-Assessed Health Status				
Physical functioning	19.842	9.048	*0.028*	91.095
Bodily pain	21.157	12.432	*0.089*	80.607
Ability to perform job	17.855	14.273	0.211	86.976
General health	14.551	13.449	0.279	76.272
Vitality	1.155	14.606	0.937	58.314
Emotional health	26.643	13.930	*0.056*	87.698
Social functioning	20.436	11.725	*0.081*	85.799
Mental health	15.622	10.772	0.147	75.751
Maternal Labor Supply				
Hours would like to work	1.102	11.463	0.923	36.285
Child Care Use				
Intended use*	−0.121	0.300	0.686	1.460
Total weekly hours of child care				
All school-aged children, during term	−8.143	6.529	0.212	5.000
All school-aged children, during holidays	110.835	25.669	*0.000*	24.200
All not-yet-at-school children	17.96	11.778	0.127	21.616

*Note: This table presents our findings on the effects of the Australia Baby Bonus on parental behaviors based on data from the Household, Income, and Labor Dynamics in Australia (HILDA) survey. Each line corresponds to a separate regression using our main specification. We use local polynomial estimation and robust bias-corrected inference methods with a 90-day bandwidth around the threshold and cluster standard errors at the level of respondents. We exclude all children born within seven days of July 1, 2004. Each line is based on a sample of 58 babies born within 90 days of July 1, 2004, born to 106 parents who are HILDA respondents. p-values in italics indicate effects statistically significant at least at the 10% level. Intended Use * : "In last 12 months, have you used/thought about using child care so you can work?". Self-assessed health status are summative scales transformed into 0–100 scale by the HILDA team.*