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 $3,000 unconditional cash transfer 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, with effects concentrated among disadvantaged families. National tax records reveal small maternal labor supply adjustments; survey data suggest increased food spending, reduced financial stress, and improved parental health and marital stability. Between 29% and 40% of the payout was recouped within the first year through reduced hospitalizations.

JEL: I38, I14, I12, J13

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 childhood poverty has large negative consequences for educational attainment, labor market outcomes, health, criminal behavior, and longevity in adulthood (Almond and Currie, 2011; Almond, Currie and Duque, 2018). More broadly, one of the most robust findings in the child development literature is the income gradient in child health and development (Currie and Almond, 2011; Milligan and Stabile, 2011). Poverty at birth is particularly harmful: the arrival of a newborn sharply increases financial pressure on parents at a moment of already heightened stress, with demonstrated consequences of income poverty on birth outcomes (Almond, Hoynes and Schanzenbach, 2011; Currie and Gruber, 1996; Doyle, Schurer and Silburn, 2022), acute illness and health care utilization in infancy (Case, Lubotsky and Paxson, 2002; Currie and Stabile, 2003; Doyle, Guthridge and Schurer, 2026), and developmental delays (Dahl and Lochner, 2012; Milligan and Stabile, 2011).

Unconditional cash transfers offer a promising remedy. Baby bonus policies in particular—one-off unconditional cash transfers to families at the birth of a child—are a longstanding tool for alleviating the financial pressures of early parenthood, offered by numerous jurisdictions including Australia, Québec, Singapore, Spain, France, Poland, and South Korea (Parr and Guest, 2011; McDonald, 2006*a*,*b*). Interest in such direct payments surged during the COVID-19 pandemic as governments sought rapid ways to support struggling households. Because they generate plausibly exogenous variation in household income at a well-defined moment, baby bonuses also offer a valuable natural experiment for researchers. Administered through the tax and welfare system, they are easy to scale, adjust, or cancel in times of fiscal austerity. By design, their one-off nature is not intended to shift permanent income or alter life-cycle consumption and saving; rather, they aim to ease the large, concentrated costs that follow a birth and that might otherwise crowd out other household expenditures. Whether they improve child outcomes depends critically on how parents spend them: expenditure on child-centered goods could benefit children (Dahl and Lochner, 2012), whereas spending on nonessential or risky goods might harm them (Currie and Gahvari, 2008).

In this paper, we estimate the effect of the Australian Baby Bonus (ABB), introduced in 2004 as

an unconditional cash transfer paid at birth, on children's health care utilization and health in the first years of life. We exploit a regression discontinuity design, comparing children born just before versus just after the ABB's introduction, using two complementary data sources: population-level linked administrative records from perinatal files, birth registrations, and all emergency department and inpatient presentations in public hospitals in South Australia; and nationally representative survey data and tax records on parental behaviors, which we use to examine mechanisms. The administrative records are exceptionally rich, covering the full spectrum of childhood health presentations from birth onward and allowing us to characterize each episode by diagnosis, urgency, severity, and whether it was planned or unplanned.

The Baby Bonus was an unconditional cash transfer initially set at A$3,000 (US$2,200 in 2004 and US$3,830 in 2026).[1] This was equivalent to 2.5 times the weekly median disposable household income in 2004, and 5.3 times that of households in the lowest income decile. It was introduced at a time when Australia was the only OECD country besides the United States without paid parental leave legislation, making it a meaningful income windfall for many families. Take-up was near-universal: national estimates suggest it exceeded 95% (see Drago et al., 2011, p. 383), and our own South Australian estimates place mother-level take-up at 86–90%, approaching 100% at the family level. Families received the payment within an average of 25 days of the birth. The Baby Bonus was discontinued in 2014, following the introduction of paid parental leave. For identification, we exploit the abrupt and largely unanticipated introduction of the bonus, with eligibility determined by a birth date threshold of July 1, 2004.

The Baby Bonus led to an overall decline of 0.096 standard deviations in health care utilization in the first year of life. This is an economically meaningful effect in a country with universal, free-at-point-of-use health care and a high standard of living. This decline is driven by presentations for urgent, acute, and severe conditions requiring hospital admission, which indicates genuine improvements in infant health rather than substitution away from out-of-hospital care. Crucially, these outcomes are free of charge for all Australian residents and not substitutable by private care, which makes them robust to parental sorting around the threshold.

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

Our findings hold across a comprehensive set of robustness checks: alternative donut widths around the threshold (5–15 days); data-driven optimal bandwidths (CER-, MSE-, asymmetric CER-, and half-CER-optimal); researcher-chosen bandwidths (60 and 90 days); and models controlling for predetermined child and parent characteristics. Permutation tests using placebo thresholds confirm that effects are driven by the eligibility threshold. Results are also robust to standard errors clustered at the date-of-birth level, heteroskedasticity-robust standard errors, and correction for multiple hypothesis testing (Romano and Wolf, 2005*a*,*b*, 2016; Holm, 1979).

We explore the channels through which the Baby Bonus improved infant health and find evidence of two channels identified in the literature: (1) the resources channel, whereby additional income enables families to purchase child-centered goods and services; and (2) the family-process channel, whereby the transfer enhances parental well-being and frees up time for more intensive caregiving (Mayer, 1997; Yeung, Linver and Brooks-Gunn, 2002; Milligan and Stabile, 2011). Our analysis of mechanisms yields five insights. First, using federal tax records from the Australian Tax Office, we find modest reductions in maternal labor supply: participation fell by 0.5–0.9 percentage points from a base of 43%, with no significant change in earnings. This suggests that low-earning mothers, who had limited access to employer-sponsored parental leave at the time (Broadway et al., 2020), used the transfer to extend time at home with their newborn (family-process channel), while for higher-income families the bonus functioned primarily as a modest income boost (resources channel).

Second, health gains are concentrated among disadvantaged households, which suggests the transfer alleviated short-run liquidity constraints and enabled greater parental investment in preventive care. Third, effects are larger for boys, consistent with the male-fragility hypothesis (Bertrand and Pan, 2013). Fourth, effects are almost entirely driven by non-firstborn children in disadvantaged families. This suggests that parents learn about the child human capital production function through their first child and use the transfer to shift resources toward higher-return inputs for subsequent children.

Finally, using the nationally representative HILDA survey, we find suggestive evidence of increased household food and grocery expenditure, reduced financial stress, and improvements

in marital stability and parental self-rated health. These findings point to broader spillovers for family well-being and reinforce the relevance of both channels. Together, the five insights paint a coherent picture: the Baby Bonus improved infant health by expanding material resources and by improving parental behavior and family dynamics.

Back-of-the-envelope calculations suggest the Baby Bonus generated a substantial return on investment through reduced hospital utilization: between 29% and 40% of the initial outlay was recouped in the first year of life alone. Our estimates are likely conservative because they are based solely on severe, acute, and urgent hospitalizations and exclude less severe but still costly presentations. More broadly, positive income shocks early in life may reduce the long-run economic burden to society through sustained savings in medical expenditure. Future research could extend these estimates along the full spectrum of health problems, drawing on Medicare expenditure or primary care data, and examine dynamic effects at later ages.

This study contributes to a longstanding literature on the effects of household resources during childhood on children's health and human capital (Currie and Almond, 2011; Almond, Currie and Duque, 2018; Heckman and Mosso, 2014; 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 child health is difficult because compelling large-scale experiments are rare; the principal exception is the Baby's First Years randomized controlled trial (see e.g., Noble et al., 2021).[2] The broader literature has addressed this by exploiting large income shocks: lottery winnings (Cesarini et al., 2016), welfare expansions (Duncan, Morris and Rodrigues, 2011; Hoynes, Miller and Simon, 2015; Dahl and Lochner, 2012), casino windfalls (Akee et al., 2010), universal basic income pilots (Bartik et al., 2024; Miller et al., 2024), and recurring transfers such as Supplemental Security Income (Hawkins et al., 2025).

[2]Recent findings from this trial, which provides US$1,000 monthly payments to low-income American households over multiple years, show that the transfers raised household spending on child-centered goods (Gennetian et al., 2024), produced small increases in medical spending (Miller et al., 2024), and modest gains in enriching child activities (Magnuson et al., 2025), with no change in overall earnings. Unlike baby bonus payments, these transfers alter permanent income rather than easing short-term liquidity constraints around the time of birth.

This study also contributes to a smaller literature on small, one-off unconditional cash transfers that buffer short-term financial stress without shifting permanent income, including baby bonus payments (Deutscher and Breunig, 2018; González, 2013; Borra et al., 2021; Cygan-Rehm and Karbownik, 2022) and earned-income tax credits and related transfers (Hoynes, Miller and Simon, 2015; Hoynes, Schanzenbach and Almond, 2016; Dahl and Lochner, 2012; Milligan and Stabile, 2011; Duncan, Morris and Rodrigues, 2011; Amarante et al., 2016; Barr, Eggleston and Smith, 2022). While there is growing consensus that such programs represent investments in children with large positive returns (Bailey et al., 2020; Hoynes and Schanzenbach, 2018; Aizer, Hoynes and Lleras-Muney, 2022), the mechanisms behind these returns remain poorly understood. Barr, Eggleston and Smith (2022), for instance, find that small cash transfers at birth increase adult earnings by up to 2%, but offer limited insight into why these long-run payoffs materialize. This study provides evidence on the mechanisms linking household resources to child health outcomes and the role of parental investment in generating positive returns to cash transfer policies.

We contribute to the literature on cash transfers and child health by providing empirical support for both the resources and family-process channels. Prior evidence is mixed across contexts (see Page, 2024, for a recent review). Mullins (2019) finds that welfare payments improve parental well-being, spousal stability, and enable mothers to extend time at home with their children. Bauer, Ruffini and Schanzenbach (2024) find that COVID-19 lump-sum food assistance reduced food insecurity and improved maternal mental health. Rittenhouse (2025) documents that tax credit income reduces child maltreatment. In contrast, Jacob et al. (2022) and Pilkauskas et al. (2022), studying disadvantaged households during the COVID-19 crisis in the United States, find only suggestive evidence of improvements in material hardship and mental health. Hawkins et al. (2025) find no long-run effects of large monthly Supplemental Security Income transfers for very low birthweight infants in California. Our findings suggest that even a small, one-off unconditional payment can meaningfully improve child health and parental outcomes.

Finally, our paper contributes to a narrower literature on baby bonus policies, which has focused largely on birth shifting as an unintended consequence of financial incentives. Surprisingly little is known about the effects of baby bonuses on child health outcomes or parental responses; Borra

et al. (2021) is a notable exception. The existing evidence, drawn primarily from the Spanish and Australian experiences, establishes that baby bonuses: (i) cause small increases in fertility through changes in conceptions and abortions (Sinclair, Boymal and De Silva, 2012; González, 2013; González and Trommlerová, 2021), and raise childbearing intentions particularly among lower-income women (Risse, 2010); (ii) allow mothers to extend time at home after birth (González, 2013); and (iii) have modest medium-term effects on children's human capital, concentrated among disadvantaged children (Deutscher and Breunig, 2018; Borra et al., 2021). We extend this literature by providing the most comprehensive evidence to date on the effects of a baby bonus on infant health, drawing on population-level hospital records, and by identifying the parental behavioral responses through which these effects operate.

2 Institutional Background

2.1 Background on the Baby Bonus Reform

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 and December 2004. A short period of less than seven weeks between announcement and implementation left no room for a short-run fertility response.[3]

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

[3]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.

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%.[4] Federal tax records show that 43% of mothers were in the labor force, with unconditional mean annual earnings of $8,000 and $18,000 conditional on participation; the Baby Bonus thus represented 38% and 16% of maternal annual earnings, respectively. In contrast, Barr, Eggleston and Smith (2022) study a USD$1,300 EITC-related transfer in the United States, representing approximately 10% of annual household income for low-income families, and the Spanish baby bonus studied by González (2013) and Borra et al. (2021) was 2,500 euros, approximately 9% of average annual disposable income.

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.

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).

[4]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.

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.

2.2 First-Stage Estimates

Using social security payments, we find that almost all eligible households took up the payment, confirming previous findings (see Drago et al., 2011, p. 383). The payment was immediately effective: the median household received the payment within 14 days of the birth of the child, while 90% received it within 49 days. 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.[5]

Figure 1 presents first-stage effects of the policy change on the take-up of maternity payments and on the dollar amount received by families.[6] The share of mothers who receive any maternity-related payment within one year post-birth sharply increases from roughly 86% to 90%, as a result of the expansion of maternity payments from means-tested to universal.[7] The dollar

[5] See their Table 1 for a detailed distributional breakdown by income decile.

[6] We use the same specification as our main results but we impose a fixed bandwidth of 182 days around the threshold to keep the size of the social security dataset manageable. For details on the Social Security records from the federal Department of Social Services (DSS) including data preparation, see Appendix B.

[7] We report take-up rates of mothers only because they are primary carers in the vast majority of cases; accounting also for fathers, the take-up rate at the family level is just below 99% after the reform. We focus on mothers because

amount received by eligible mothers sharply rose from roughly $854 to $3,000, representing an average $2,169 increase in pretax unearned income.[8]

While the take-up rate for lower socioeconomic status families saw an insignificant increase from 92-93% to 95% following the reform, the take-up rate for higher socioeconomic status families rose by 10-percentage points from 73-76% to 83-86%, reflecting the expansion of the Baby Bonus from means-tested to universal coverage (Table A.1).[9] The pre-existing Maternity Allowance of $843 per child tapered quickly after $85,000 annual household income. For families who would have received the pre-existing Maternity Allowance, the net gain from the reform was $2,200 pretax, and for wealthier families the gain was $3,000 pretax. Post-tax gains however remained larger for low-income families compared to higher-income families because of strong tax progressivity and lower maternal labor force participation in low-income households.

2.3 Background on the Australian Health Care System

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 among 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

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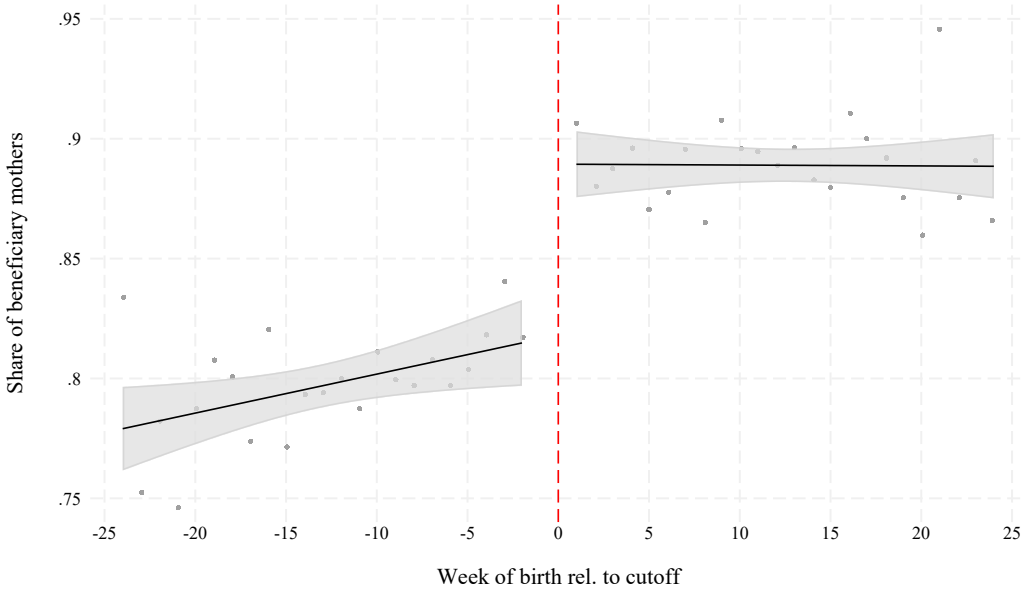
we are most confident about linking mothers to children.

[8]We find near identical estimates using Difference-in-Difference models estimated in federal Social Security records (Table B.3).

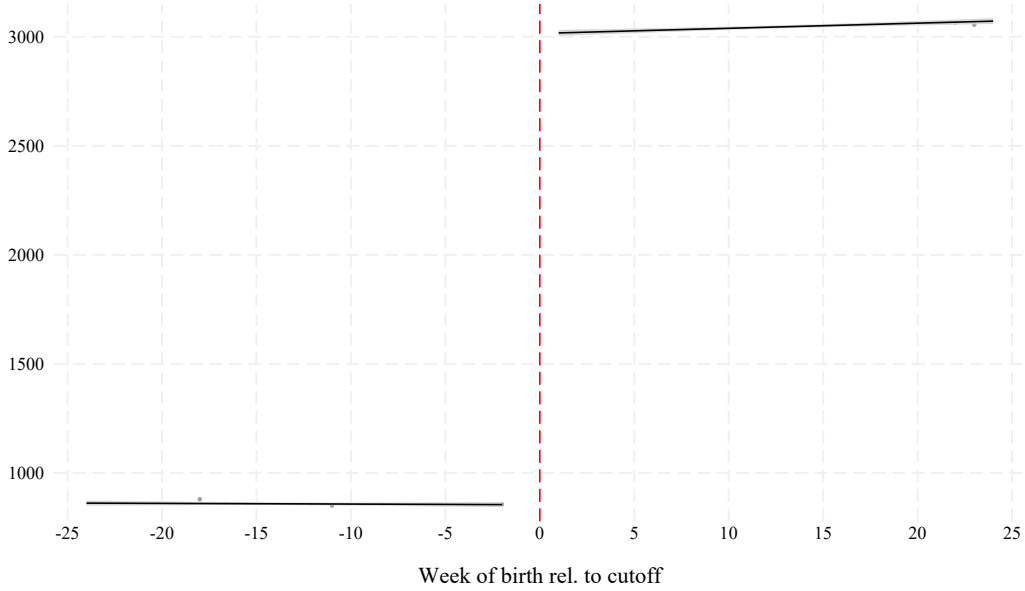
[9]Socioeconomic status is defined by fathers' occupation category from perinatal records. Unfortunately we do not have access to linked household income data to break down the socioeconomic spectrum along the pre-reform means-test. We define as lower socioeconomic status families where the father or both parents was a plant or machine operator, driver, laborer, student, pensioner, out of the labor force or unemployed – the 33% lowest skilled parents. We define as higher socioeconomic status families where the father or both parents was in a management, professional or administrator occupation at the time of birth – the 33% highest skilled parents.

Figure 1: First Stage Estimates: The Australian Baby Bonus and Cash Receipt

(a) Take-Up



(b) Amount Received



Note: These regression discontinuity plots present graphical evidence on the first-stage effects of the Australian Baby Bonus on the take-up of maternity payments (Figure 1a) and the amount received (Figure 1b). On both figures, 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 a 182-days bandwidth. 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.*

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).[10]

Perhaps most importantly, children are fully covered under Medicare as well and receive additional free services regarding dental care, immunizations, disability, autism, and vision impairment. Parents incur no out-of-pocket costs for visits to general practitioners, maternal and child health nurses, and emergency and inpatient hospital presentations. Table A.2 details the various pathways to hospital care, with related costs. Although parents could take children to private hospitals or general practitioners, it is well documented that 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 Data

3.1 The South Australian Early Childhood Data Project

We conduct our main analyses using linked administrative data from the South Australian Early Childhood Data Project (SA ECDP), one of the most comprehensive population-based administrative research databases on children and families in Australia, bringing 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; see also Appendix B for a discussion on the strengths and limitations of our data). Administrative records

[10] Other ancillary services are not covered. For more details, see https://www.servicesaustralia.gov.au/individuals/subjects/whos-covered-medicare/childrens-health-care. Australia 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).

are linked through probabilistic matching on full name, date of birth, address, and Medicare number, with linkage rates exceeding 95 percent for the birth-to-hospital linkage.

Birth and Perinatal Data. We use the Born Population dataset, a merge of the Births Registry and the South Australian Perinatal Statistics Collection covering the universe of children born in South Australia between 1991 and 2016. With this large sample of population-wide coverage, we expect to have sufficient statistical power to detect even small effects and high external validity for the Australian context. We observe the exact date of birth (day, month, year), 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 contain demographic characteristics of mothers, fathers, and children, and pregnancy histories of mothers (e.g., maternal gestational health, smoking behavior during pregnancy, and past pregnancies).

Hospital Records: Inpatient Services and Emergency Department Admissions. We derive health outcome measures from the Integrated South Australian Activity Collection (ISAAC) and the South Australian Emergency Department Data Collection (EDDC). In both datasets, a unit of observation is a presentation event. The ISAAC data cover the universe of presentations to inpatient services (IS) in public hospitals from July 2001 to 2014. The EDDC data cover the universe of presentations 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 record details of each presentation, including: the exact dates of presentation and discharge; mode of transport to hospital; whether the visit was planned or referral-based; whether it was a first admission or readmission; the severity of the patient's condition as assessed by a triage nurse; diagnosis codes and associated clinical indicators; length of stay; and the nature of the separation (discharge, transfer, or death). The data are collected by hospital staff and updated at the time of hospital separation. Our data thus provides objective health measures, which are coded consistently by medical professionals through the International Statistical Classification of Diseases and Related Health Problems, 10th edition, Australian Modification (ICD-10-AM).

3.2 Outcomes and Variables of Interest

Our primary outcomes are: (i) overall health care utilization, measured by a composite index; (ii) presentations for severe, acute, and urgent conditions; and (iii) presentations for conditions deemed preventable. As secondary outcomes, we examine two indicators of parental investment in child health: planned visits and those requiring co-payments, and detailed diagnosis and presenting-problem codes.

Primary Outcomes Measuring infant health is harder than measuring adult health because no unambiguous summary measure exists; the literature therefore relies on proxies for morbidity such as diagnoses and health care utilization (Corman, Dave and Reichman, 2018). Following this literature and the broader human capital tradition (Heckman, Stixrud and Urzua, 2006), we treat child health needs as a latent factor. Our primary measure is a composite index of health care utilization in the first year of life, excluding birth-related problems. All index components are based on presentations requiring no co-payment and no parental planning, so that the index captures health care needs rather than parental selection into care.

Alongside the index, we report nine outcomes linked to the same latent factor, in two groups:

- **Aggregate indicators (not in the index):** (i) any ED or inpatient presentation; (ii) any ED presentation; (iii) any inpatient presentation; (iv) any potentially preventable pediatric presentation.
- **Index components:** (i) any ED presentation for a severe, acute, or urgent problem; (ii) any inpatient presentation for a severe, acute, or urgent problem; (iii) any ED presentation leading to a ward admission; (iv) any ED potentially preventable pediatric presentation; (v) any inpatient potentially preventable pediatric presentation.

The health care utilization index is a summative scale of the five detailed items listed above, selected through a two-step exploratory and confirmatory factor analysis of twelve hospital care indicators (see Appendix B and Table B.1 for factor loadings). We exclude the four aggregate indicators from the index because they subsume the detailed items, which would mechanically inflate inter-item correlations. The index is standardized to mean 0 and standard deviation 1

in the reference population of babies born in South Australia between July 1, 2003 and July 1, 2004, so that coefficients are read in standard deviation units. Although the summative scale assigns equal weight to all components, Table B.2 shows that results are robust to using a factor-weighted index instead.

The index reduces noise across correlated outcomes and captures a latent factor reflecting the intensity and severity of child health problems that require care. These classifications originate at different points in the care pathway: severity is recorded by a triage nurse at the ED upon presentation or by a treating doctor at ED or inpatient services upon discharge; ward admissions from the ED are decided by hospital staff; and preventable status is flagged by a medical doctor at separation. Urgent presentations typically reflect acute shocks such as accidents or sudden illness. Preventable presentations are those that could have been avoided through timely primary care—for example, conditions that would not have reached the hospital with an earlier GP visit, prompt treatment, or vaccination.[11] Higher index values indicate greater health care needs and more severe health problems. We also report effects on each primary outcome separately to provide a more granular picture. Finally, we also construct intensive-margin measures counting the total number of ED and inpatient presentations in the first year of life.

Secondary Outcomes We consider two types of secondary outcomes: (i) visits requiring parental planning or co-payments, and (ii) detailed diagnoses and presenting problems.

Planned visits take three forms: follow-up appointments scheduled by hospital staff after an initial ED presentation (coded as "planned review" or "planned readmission"); presentations with a referral for specialist care; and presentations for elective interventions. These visits may involve out-of-pocket expenses and are distinct from unplanned presentations initiated by families.

We characterize in detail the presenting problems and diagnoses for which infants attend ED and inpatient services. Inpatient records contain ICD-10-AM diagnosis codes, from which we extract broad disease categories (chapters). ED records allow us to distinguish presentations by

[11] Presentations flagged as "potentially preventable pediatric" (PPP) are recorded in alignment with the Potentially Avoidable Hospitalization (PAH) tool developed in New Zealand, which classifies vaccine-preventable, acute, and chronic conditions that could have been avoided with timely primary care (Anderson et al., 2012).

presenting problem—as classified by the triage nurse upon arrival—and by diagnosis group, as recorded by the treating doctor upon inspection or discharge. Triage nurses classify presentations into broad problem categories consistent with ICD-10-AM chapters; doctors subsequently assign specific ICD-10-AM codes, which also determine hospital reimbursement. We draw on both sources in the analysis.

We focus on the six most common presenting problems in infancy: (i) respiratory; (ii) digestive; (iii) infections; (iv) skin; (v) injuries, trauma, and poisoning; and (vi) externally caused problems (accidents). For each, we construct an index summing three indicator variables corresponding to whether a report was made by an ED triage nurse, an ED doctor, or an inpatient doctor.

Table A.3 reports summary statistics for the index and primary outcomes for all 17,696 births in South Australia between July 1, 2003 and July 1, 2004. Forty-six percent of children have at least one hospital presentation: 32% at the ED, and 31% as inpatients. One in five has at least one ED presentation for a severe, urgent, or acute problem, and one in eight has a presentation leading to hospital admission. More than one in five children have a potentially preventable presentation, accounting for 18% of ED presentations and 10% of inpatient presentations. Figure A.2 presents full distributions of these outcomes, and Table A.4 presents summary statistics on the characteristics of parents and babies born within 90 days of the threshold.

4 Empirical Strategy

4.1 Regression Discontinuity Design

The introduction of the Baby Bonus on July 1, 2004 lends itself naturally to a sharp regression discontinuity design. No child born before that date received the bonus, while over 95% of those born on or after it did. We exploit this sharp discontinuity in eligibility to estimate the causal effect of the Baby Bonus on child health outcomes, comparing children born just before versus just after the threshold. Specifically, we estimate the following equation nonparametrically for child i with health outcome Y_i measured at age one. For longer-term effects, we replicate the analysis at ages 1–2 and 3–5:

$$Y_i = \alpha + \beta_0 D_i + \beta_1 f(R_i) + \beta_2 D_i \times g(R_i) + \varepsilon_i, \tag{1}$$

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, $f(.)$ and $g(.)$ are flexible functions of the child's date of birth. β_0 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).[12] We choose local linear estimation following Gelman and Imbens (2019) who warn against the use of global high-order polynomials that often give too much weight to observations away from the threshold and bias estimates at the threshold; in addition, we impose a triangular kernel to give more weight to observations closest to the threshold. We use robust bias-corrected inference to limit the risk of bias arising from nonlinear conditional expectation functions of outcomes near the threshold (Calonico, Cattaneo and Titiunik, 2014*b*). We cluster standard errors at the level of birth dates, although we later demonstrate that our results are robust to using heteroskedasticity-robust standard errors instead.[13]

Although Calonico, Cattaneo and Farrell (2020) show that the MSE-optimal bandwidth is optimal for point estimation and the CER-optimal bandwidth is optimal for inference, we prefer the CER-optimal bandwidth because MSE-optimal bandwidths are wider, potentially including babies born in different seasons which can have different health care needs in the first year of life (Buckles and Hungerman, 2013; Currie and Schwandt, 2013). We later demonstrate that seasonalities do not threaten the internal validity of our research design. Our findings are robust

[12] We use the `rdrobust` Stata command (Calonico, Cattaneo and Titiunik, 2014*a*; Calonico et al., 2017, January 2020 update). We also performed internal validity checks and main analyses under a parametric specification following Lee and Card (2008) and find similar results that are available upon request.

[13] Even though our running variable is discrete, we cluster standard errors at the experimental level following Bartalotti and Brummet (2017) and Abadie et al. (2020, 2017) who study settings like ours where treatment assignment is correlated within clusters.

to alternative researcher-chosen bandwidths as narrow as 60 and 90 days, alternative donut sizes, and falsification exercises such as using placebo thresholds.[14]

We exclude all births within seven days of the eligibility threshold date, implementing a "donut" regression discontinuity design, because of birth shifting induced by the policy announcement (Gans and Leigh, 2009; Deutscher and Breunig, 2018). The Australian government announced the introduction of the Baby Bonus on May 11, 2004, only seven weeks prior to July 1, 2004. The announcement period did allow some families to shift the birth of their baby. This "donut" excludes all potentially shifted births within seven days around July 1, 2004. In Appendix C we replicate in South Australia the results on birth shifting from Gans and Leigh (2009); Deutscher and Breunig (2018) and extend their analyses using the methodology from Borra, González and Sevilla (2019). In robustness checks we present results under alternative choices of donuts and without donut.[15]

4.2 Validity of the Regression Discontinuity Design

Our estimator yields a causal estimate of the effect of the Baby Bonus on infant health if the continuity assumption holds — that is, if potential outcomes vary smoothly through the eligibility threshold in the absence of treatment. In practice, we verify two testable implications of this assumption: (i) that there is no manipulation of the running variable around the threshold, and (ii) that there are no discontinuities in predetermined child and parental characteristics at the threshold.

Density of the Running Variable. Figure A.3a shows that the concerning window of births is concentrated within seven days of July 1, 2004, and Figure A.3b shows that after excluding the donut, daily births are smoothly distributed over 2002 to 2006.

Nonparametric density tests confirm that the running variable is smoothly distributed at the

[14] A feature of data-driven bandwidths is that the optimal bandwidth differs across outcomes, which means the estimation sample is not fixed throughout the analysis. An alternative is to use a fixed bandwidth such as 60 or 90 days around the threshold which maintains a consistent sample across all outcomes and validity checks.

[15] 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. In settings like ours, as Jacobson, Kogelnik and Royer (2021) note, the financial incentives tied to the birth date eligibility threshold create no incentives for parents to shift a birth earlier.

threshold (Cattaneo, Jansson and Ma (2020, 2018) in Table A.5), whether we use data-driven bandwidths or impose 60- and 90-days bandwidths around the threshold (Table A.6). This also holds with binomial density tests extending to 100 days from the seven-day donut around the threshold (Table A.7)[16].

Continuity of Predetermined Characteristics. The main endogeneity concern with birth shifting is that postponed births may be systematically healthier or sicker than unshifted births, biasing estimates in either direction. We find precisely estimated zero effects for 13 of 14 predetermined characteristics (Table A.8, Panel A), suggesting that selection bias from residual birth shifting is unlikely beyond the seven-day window. The single exception, a 1 percentage point higher likelihood of being born to a never-married mother among treated babies, is marginal (significant only at the 10% level) and unsurprising given the number of hypotheses tested and the precision of our tests: we can detect differences as small as 1.68 percentage points at the 1% level with 80% statistical power. This association disappears under 60- and 90-day bandwidths (Table A.9). [17]

Mothers of treated and control babies are also comparable in Social Security claims data for the main income support payments (representing 97% of all Centrelink transactions; see Table A.10). There was no confounding discontinuous change to eligibility, schedules or amounts to other payments coinciding with the threshold. Focusing on babies born within 90 days of the threshold, we see no difference in mothers' likelihood of claiming social security benefits within one year post birth, suggesting an absence of seasonal patterns in claims at the threshold. The marginally significant decline in unemployment insurance claims, which we discuss in Section 6, rather indicate maternal labor supply adjustments following the Baby Bonus.

Lastly, the Baby Bonus also did not induce changes in fertility or selective pregnancy terminations around the threshold (Table A.11). The policy was announced only seven weeks prior

[16]Binomial density tests are used in the local randomization regression discontinuity approach and for discrete running variables. We run 100 nested binomial density tests, comparing 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 smoothly distributed at the threshold in our seven-day donut design.

[17]We find a precisely estimated null association in the likelihood of being born to a teenage mother at the threshold.

to implementation, leaving no room for new conceptions at the threshold, and little room for selective terminations for pregnant women whose baby would be in the control group.[18]

Continuity of Birth Outcomes. Control and treated babies do not differ in 11 distinct birth outcomes, especially birth outcomes associated with birth shifting and predictive of health status in the first year of life: birth weight, gestational age in weeks, special care nursery, and Apgar scores at 1 and 5 minutes after birth (Table A.8, Panel B; see Figure 2 for regression discontinuity plots). We find a small, albeit statistically significant imbalance in Apgar scores one minute after birth ($p = 0.085$, a 2.5% decline from the pre-threshold mean) that dissipates at five minutes after birth, and is not consistently present when reducing the bandwidth to 90- and 60-days around the threshold (Table A.9).[19] Control and treated babies remain comparable when splitting the sample by socioeconomic status (Table A.12), sex (Table A.13), or birth order (Table A.14).

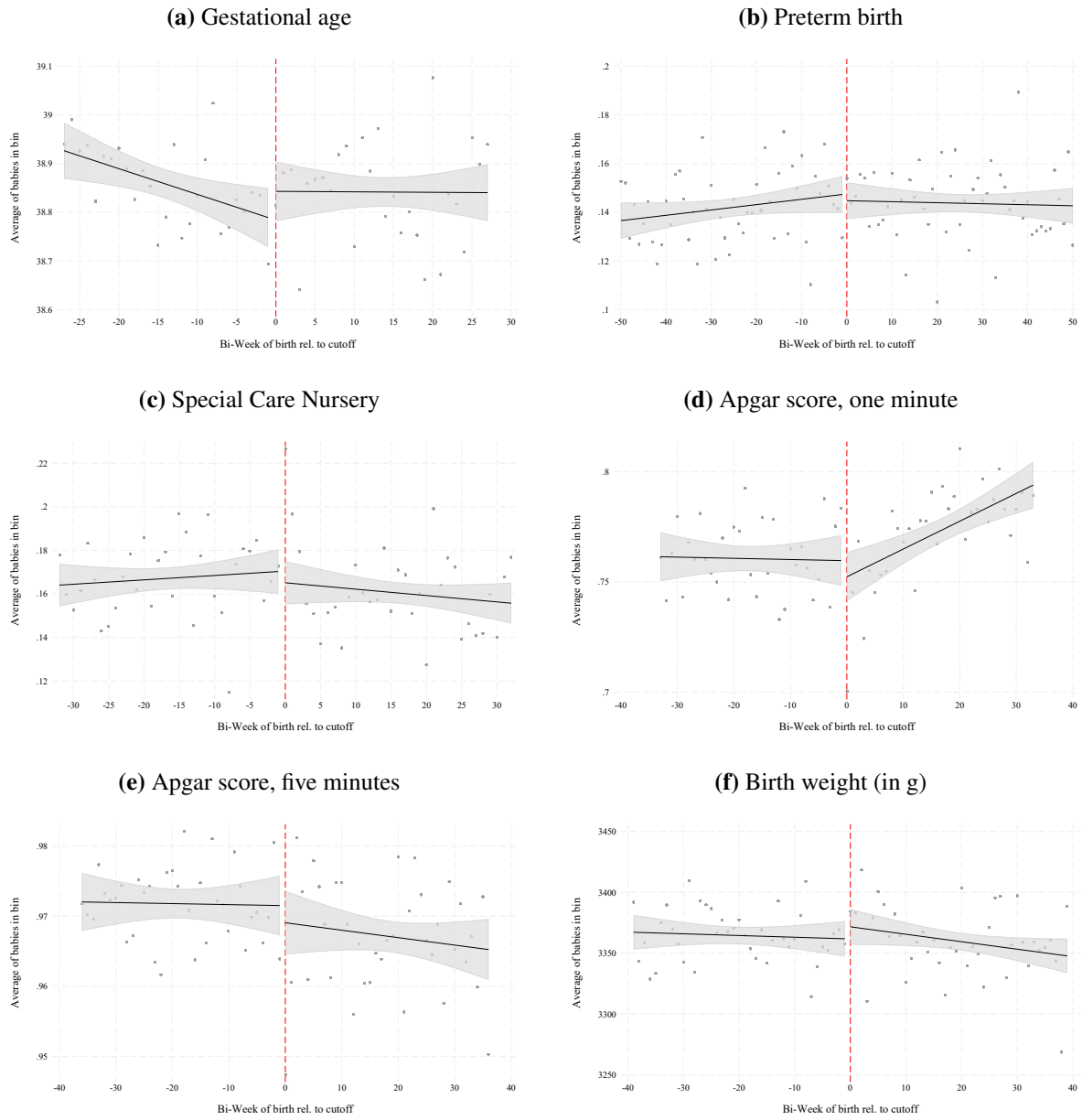
Placebo Outcomes in Prepolicy Years. We see no discontinuities in placebo outcomes in prepolicy years (Table A.15), further supporting the validity of our regression discontinuity design. We focus on outcomes from inpatient services for children born around July 1, 2002 and July 1, 2003 because outcomes measured in emergency department records only start in July 2003.

Seasonalities. We also see no evidence of suspicious seasonalities in (i) climate conditions affecting birth outcomes, (ii) seasonal selection into parenthood, and (iii) seasonalities in the health care system. Figure A.4 shows that babies born in South Australia between May and August 2004, the coldest months of the year, were exposed to overall worse environmental

[18] Gans and Leigh (2009) and Deutscher and Breunig (2018) also explain that the short-notice announcement of the policy left little scope for selective abortions because pregnant women with planned delivery date just before July 1, 2004, would be in the third trimester of their pregnancies, at which stage selective pregnancy termination is highly unlikely. South Australia decriminalized abortions only in 2022; still, abortions were feasible before that date and South Australia happens 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 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, with 4,931 abortions or 22.3% of all live births. See archives at http://www.johnstonsarchive.net/policy/abortion/australia/ab-aust-sa.html.

[19] Similarly, we see a small, marginally significant imbalance in the likelihood of any obstetric complication ($p = 0.095$, a 7% decline from the pre-threshold mean) but that is not consistent across bandwidths.

Figure 2: Continuity of Birth Outcomes at the Threshold



Note: These regression discontinuity plots 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.*

conditions and seasonal viruses compared to babies born further away on either side of the threshold, which likely affected their health conditions at birth (Currie and Schwandt, 2013).[20] Fortunately for our research design, however, weather conditions were roughly symmetrical around July 1, 2004; this pattern is consistent across 2003, 2004, and 2005, such that with a symmetric bandwidth seasonalities in weather conditions are unlikely to bias our findings at the threshold. This is consistent with treated and control babies being statistically comparable in predetermined characteristics or birth outcomes within 60- and 90-days bandwidths (Table A.9).

We also find no seasonal variation in selection into parenting and parental background characteristics, a point raised in Buckles and Hungerman (2013) (Table A.16). Lastly, we also find no meaningful discontinuities in the predetermined characteristics and birth outcomes of treated and control babies around July 1, 2002, and July 1, 2003 (Table A.17). These exercises suggest the absence of seasonalities in the health care system, such as seasonal congestion in health facilities created by end-of-financial year incentives or the natural rhythm of appointments that might affect birth outcomes or health care use within the first year, such as the regular schedule of checkups.

5 Results

Our main finding is that the cash transfer significantly decreased overall health care utilization for infants, driven by presentations for urgent, severe, acute and preventable presentations. There are no significant effects on other types of hospital presentations that require parental planning or out-of-pocket costs. However, we find a significant decrease of presentations for respiratory problems.

[20] Based on daily mean temperatures recorded at 86 weather stations across South Australia by the Bureau of Meteorology, for the years 2003–2005. Similarly, RSV—the leading cause of bronchiolitis in infants—follows a consistent winter peak around weeks 27–29 (late June to mid-July) in temperate Australia (Roche, Lambert and Spencer, 2003)

5.1 Cash and Infant Health Care Utilization

The Baby Bonus significantly decreased infant hospital care utilization by 0.096 standard deviations (standard error 0.040, $p < 0.018$) (Table 1 and Figure 3a for graphical evidence). This decline does not reflect a reduction in overall hospital contact, since we estimate no change in the probability of having any hospital presentation at either ED or IS (-0.012, $p = 0.579$). Instead, we find a significant decline in the likelihood of any inpatient presentation (-3.4 ppt, $p = 0.040$; a 10% decline from the pre-threshold mean; see Figure 3b) and any potentially preventable pediatric presentation (-3.2 ppt, $p = 0.088$; a 15% decline). The decline in any ED presentation (-2.6 ppt, $p = 0.200$) is in the same direction but not statistically significant. Note that many inpatient presentations are transfers from emergency departments for cases requiring further examination.

Table 1: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life: Primary Outcomes

	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. Extensive margin							
Health Care Utilization Index [std.]	-0.096^{**}	0.040	0.018	172	7,833	7,667	0.005
Aggregate hospital presentations, not in the index:							
Any presentation (ED or inpatient service)	-0.012	0.022	0.579	169	7,666	7,518	0.456
Any ED presentation	-0.026	0.020	0.200	149	6,781	6,629	0.316
Any inpatient presentation	-0.034^{**}	0.017	0.040	214	9,801	9,564	0.329
Any PPP presentation	-0.032^{*}	0.019	0.088	153	6,906	6,764	0.209
Index components:							
Any urgent, acute or severe problem, ED	-0.019	0.019	0.316	169	7,666	7,518	0.208
Any urgent, acute or severe problem, IS	-0.037^{***}	0.013	0.005	207	9,483	9,235	0.180
Any admission to ward, ED	-0.037^{***}	0.013	0.005	173	7,856	7,716	0.142
Any ED PPP presentation	-0.032^{*}	0.018	0.076	142	6,418	6,320	0.177
Any inpatient PPP presentation	-0.028^{***}	0.010	0.005	215	9,832	9,611	0.098
Panel B. Intensive margin							
Aggregate hospital presentations, not in the index:							
# presentations (ED or inpatient service)	-0.153^{*}	0.087	0.079	144	6,516	6,401	1.135
# ED presentations	-0.125^{*}	0.065	0.056	111	4,894	4,821	0.654
# inpatient presentations	-0.059^{*}	0.032	0.066	260	12,019	11,997	0.491
# PPP presentations	-0.111^{***}	0.041	0.007	143	6,418	6,320	0.396
Index components:							
# urgent, acute or severe problems, ED	-0.064^{**}	0.029	0.028	157	7,157	6,967	0.326
# urgent, acute or severe problems, IS	-0.059^{***}	0.022	0.008	215	9,832	9,611	0.250
# admissions to ward, ED	-0.059^{***}	0.021	0.006	159	7,194	7,016	0.197
# ED PPP presentations	-0.080^{**}	0.035	0.022	119	5,302	5,221	0.278
# inpatient PPP presentations	-0.047^{***}	0.014	0.001	236	10,855	10,779	0.135

*Note: This table presents RD treatment effects of the Baby Bonus on our primary outcomes. Panel A reports effects at the extensive margin (any presentation of the given type in the first year of life); the Health Care Utilization Index is standardized to mean 0 and standard deviation 1 for all babies born in South Australia between July 1, 2003, and July 1, 2004 (the reference population), so its coefficient is in standard deviation units. Panel B reports effects at the intensive margin (count of presentations of the given type in the first year of life). Column (1) reports coefficient estimates, Col. (2) their standard errors, (3) the p-value of the two-sided t-test, Col. (4) the bandwidth half-length, Col. (5) and (6) the total effective sample size on the left and right of the threshold, and Col. (7) the outcome mean right before the threshold. Each row 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.*

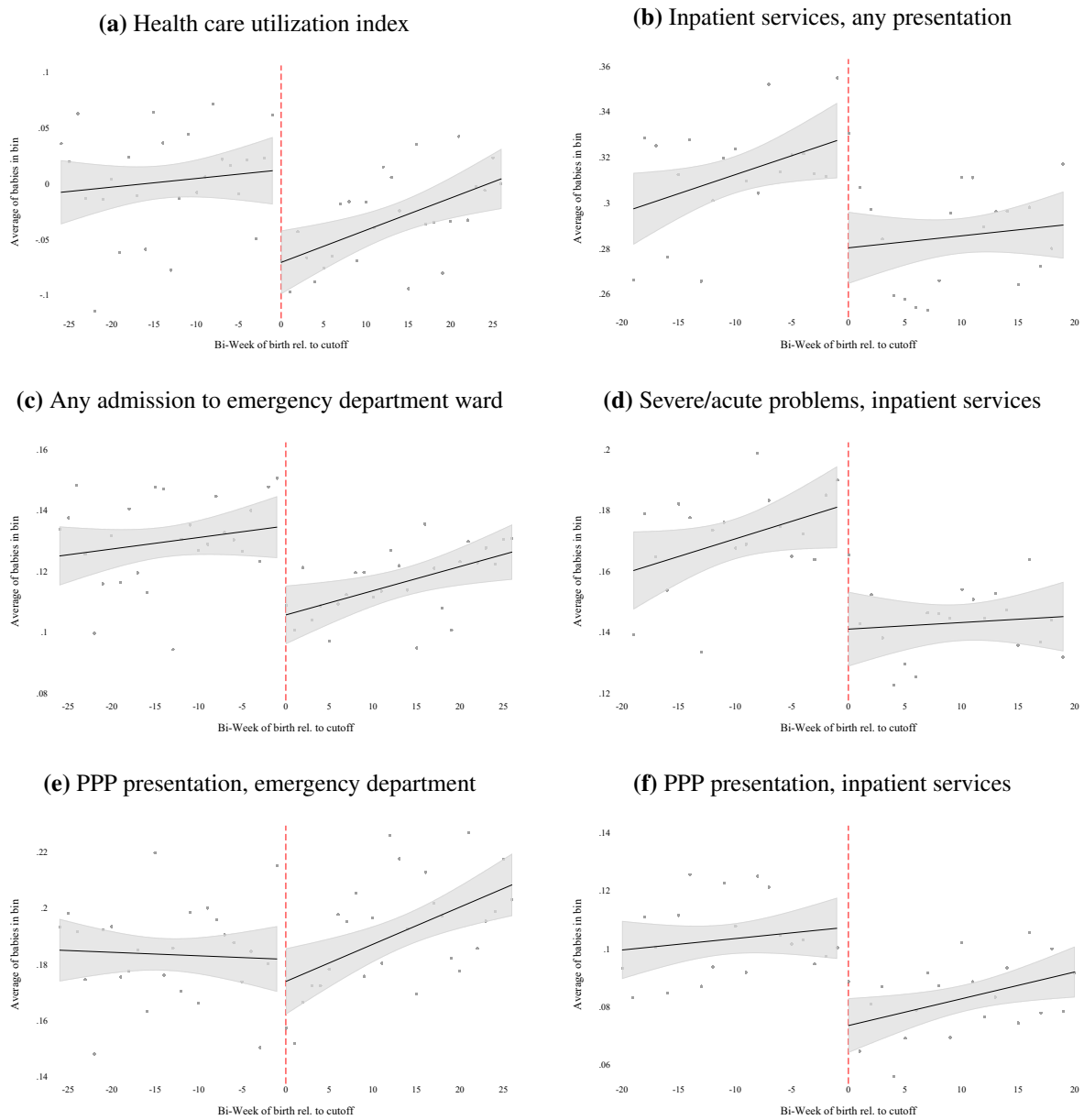
The index components reveal that the decline is concentrated in the most serious health episodes. Eligible babies are significantly less likely to be admitted to a ward from the ED (-3.7 ppt, $p = 0.005$; a 26% decline from the pre-threshold mean; Figure 3c) or to present with a severe, acute, or urgent problem at inpatient services (-3.7 ppt, $p = 0.005$; a 20% decline; Figure 3d). We also find significant declines in potentially preventable hospitalizations at inpatient services (-2.8 ppt, $p = 0.005$; a 29% decline; Figure 3f) and at the ED (-3.2 ppt, $p = 0.076$; an 18% decline; Figure 3e). As these presentations are free of charge and unlikely to be substitutable by out-of-hospital care (such as GP visits or physiotherapy), these estimates are unlikely to be biased by parental sorting into or out of hospital care.

The decline in potentially preventable hospitalizations is particularly informative. These 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.[21] These findings suggest that parents invested the cash transfer in preventive health care and took actions to prevent health problems from becoming acute or severe.

The Baby Bonus also significantly affected the intensive margin of health care utilization, where we count the total number of presentations instead of flagging any presentations in the first year of life (Table 1, Panel B). This suggests that it reduced the need for hospital care across the health status spectrum. Eligible babies have on average 0.12 fewer ED presentations and 0.06 fewer inpatient presentations in their first year of life—declines of 19% and 12% from pre-threshold means, respectively. These are economically meaningful effects: following the Baby Bonus, five rather than seven in ten infants present at the ED, and three rather than four in ten present at inpatient services. We also find reductions in presentations for urgent, acute, or severe problems at both the ED and inpatient services of approximately 0.06 (declines of 20% and 24% from their respective pre-threshold means), and in potentially preventable pediatric presentations: -0.08 at the ED (-28%) and -0.05 at inpatient services (-35%).

[21] One of the criteria used in the New Zealand PAH tool to identify preventable hospitalizations for children is whether hospitalization could have been avoided in the presence of "Government policies which ensured adequate socioeconomic resources were available to families with children (e.g. income support, childcare, assistance for solo parents returning to workforce)." (Anderson et al., 2012, p. 28). The Baby Bonus falls into this category.

Figure 3: Effects of the Australian Baby Bonus on Hospital Presentations Within The First Year of Life



Note: These regression discontinuity plots present graphical evidence that the Australian Baby Bonus reduced overall hospital care utilization and 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.*

Robustness of Main Findings. Our findings are robust to three types of concerns: (i) identification concerns related to birth-shifting; (ii) seasonality concerns; and (iii) inference concerns, in particular multiple hypothesis testing and clustering of standard errors.[22]

The first concern is that some shifted births might remain in our sample after excluding the seven-days donut from our RD design. Another concern could be that our results are driven by a small number of high-leverage observations close to the threshold. To address both concerns, we show that our coefficient estimates are highly consistent in sign, magnitude, and significance across various donuts (5 and 8 days in Table A.18, 12 and 15 days in Table A.19).[23] The estimate and conclusion are robust to including all births, ignoring birth shifting altogether (Table A.20). Reinforcing this conclusion, Table A.21 shows that our findings are unaffected by the inclusion of predetermined characteristics, with most coefficients remaining nearly identical in magnitude and statistical significance.

A second concern is that seasonalities might spuriously drive our conclusions. Seasonalities could affect our findings through our choice of bandwidth because wider bandwidth include babies born under increasingly different seasons and that are less comparable at the outward bounds of the bandwidth compared to babies born close to the threshold. Firstly, our findings are robust to alternative bandwidths (MSE-, CER-, 2-sided CER-optimal bandwidths in Table A.22, and 90- and 60-days bandwidths in Table A.23)[24]. We find systematically larger effects in magnitude as the bandwidth narrows around the eligibility threshold down to two months around the threshold. This is likely because we focus on the most comparable babies, born in the coldest months of the year[25].

[22] For a further detailed discussion of data limitations and sample selection concerns, see Appendix B.

[23] These alternative donuts ensure that we exclude the same number of weekend days on each side of the eligibility threshold to avoid introducing known weekly-seasonalities in birth scheduling that would spuriously affect our results.

[24] The Working Credit program, introduced on September 20, 2003 to encourage welfare recipients to participate in the labor force did not create an eligibility discontinuity at July 1, 2004 and therefore affected treated and control families symmetrically. Even our widest CER-optimal bandwidths include only births from late November 2003 onward, two months after the program took effect, such that all babies in our sample were born under the same Working Credit regime. Results at narrower 60- and 90-day bandwidths further confirm that the Working Credit program does not confound our estimates. See Wilkins and Leigh (2012) for more details on the policy.

[25] 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 as discussed in Section 6.

We also perform a falsification exercise using placebo thresholds following Ganong and Jäger (2018) and find that our main results are truly driven by the policy change and not at arbitrarily chosen alternative cutoffs (Table A.24). The intuition of this exercise is that if our results were driven by other factors that coincide roughly with the true eligibility threshold on July 1, 2004, (such as parental selection into timing of birth, as discussed in Buckles and Hungerman (2013)), then estimating our model with placebo thresholds should yield statistically significant treatment effects also for randomly chosen threshold dates in the vicinity of July 1, implying that the coefficient estimate at the *true* threshold would not be extreme in the distribution of all coefficient estimates at placebo thresholds.[26] Overall, the permutation tests indicate that our estimates at the true threshold are extreme in the distribution of all estimates across thresholds: using randomization-based *p*-values, three out of seven significant outcomes remain significant: the health care utilization index, urgent presentations at inpatient services, and admission to a ward at the emergency department. Another two outcomes are close to significant, with randomization-based *p*-values just above 0.1: any PPP and ED PPP presentations.[27] We further find no significant effects at placebo thresholds around July 1, 2002 and July 1, 2003 for outcomes from inpatient services records (Table A.25), which rules out seasonalities in outcomes around July 1 in both the year of the policy and prepolicy years.

Lastly, our main findings are robust to alternative inference methods, namely correcting standard errors for multiple-hypothesis testing using both the Romano-Wolf and the Holm method (Table A.26) and using heteroskedasticity robust standard errors instead of clustered standard errors (Table A.27, Panel B).

5.2 Effects on Secondary Outcomes: Elective Care and Diagnoses

Because our utilization index focuses on adverse outcomes, the negative effect suggests the Baby Bonus may be welfare-improving. However, the lump-sum transfer may induce parents to

[26] We re-estimate our model for each threshold from -/+90 days around July 1, 2004, then we construct the percentile rank of our estimate at the true threshold and compare it to the distribution of all the estimates obtained, and finally we construct a randomization-based *p*-value.

[27] We do note that presentations at inpatient services and PPP presentation at inpatient services become statistically insignificant in this exercise, however results for these two outcomes are consistent across all other sensitivity checks.

substitute away from hospital care for less acute conditions that still require eventual treatment (e.g., specialist visits). We next examine effects on presentations requiring parental planning or co-payments, and on detailed diagnoses and presenting problems, to shed light on the mechanisms driving parental care-seeking decisions.

Planned Visits, Electives and Referrals. We find overall no significant impacts of the cash transfer on the probability (Panel A) and number (Panel B) of planned hospital presentations within the first year of life (Table 2), except for a small, marginally significant decline in inpatient services presentations leading to an overnight stay and planned review visits at the ED. Planned hospital presentations could indicate an increase in parental investments in child health because these involve direct (money and time) costs.

Table 2: Effects of the Australian Baby Bonus on Planned and Elective Visits and Visits with Medical Referral: Secondary Outcomes

	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. Extensive margin							
Any admission to ward, IS	0.002	0.007	0.816	216	9,857	9,677	0.026
Any overnight stay, IS	−0.022*	0.013	0.083	291	13,593	13,479	0.221
Any planned visits or presentations with medical referral:							
Emergency department:							
Any planned review visit	−0.010*	0.005	0.071	148	6,688	6,548	0.022
Any visit with medical referral	0.001	0.009	0.952	156	7,072	6,898	0.050
Inpatient services:							
Any planned visit	−0.001	0.004	0.882	289	13,535	13,442	0.024
Any visit with medical referral	−0.010	0.010	0.313	198	9,054	8,837	0.094
Any visit for an elective intervention	0.003	0.008	0.657	230	10,528	10,421	0.051
Panel B. Intensive margin							
# admissions to ward, IS	0.003	0.008	0.704	210	9,645	9,463	0.029
# overnight stays, IS	−0.032*	0.019	0.094	309	14,530	14,437	0.277
Total planned visits or presentations with medical referral:							
Emergency department:							
# planned review visits	−0.014**	0.007	0.041	143	6,418	6,320	0.028
# visits with medical referral	−0.002	0.012	0.879	150	6,781	6,629	0.058
Inpatient services:							
# planned visits	0.000	0.007	0.997	265	12,245	12,269	0.029
# visits with medical referral	−0.023	0.021	0.276	191	8,704	8,505	0.128
# visits for elective interventions	0.001	0.013	0.955	217	9,973	9,790	0.066

*Note: This table presents RD treatment effects of the Baby Bonus on secondary outcomes related to health care utilization of infants. Panel A reports effects at the extensive margin (any presentation of the given type in the first year of life); Panel B reports effects at the intensive margin (count of presentations of the given type in the first year of life). Column (1) reports coefficient estimates, Col. (2) their standard errors, (3) the p-value of the two-sided t-test, Col. (4) the bandwidth half-length, Col. (5) and (6) the total effective sample size on the left and right of the threshold, and Col. (7) the outcome mean right before the threshold. Each row 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.*

Diagnoses and Presenting Problems. Breaking down results by diagnosis, we find that the decline in hospital care utilization for treated babies is driven by a decline in presentations for respiratory problems, and in particular for potentially preventable bronchiolitis and gastroenteritis, the most common types of potentially preventable pediatric presentations for babies in the first year of life in our data (Table 3, and graphical evidence in Figure 4).[28] The Baby Bonus led to a 2.5 ppt decline in emergency department presentations for potentially preventable bronchiolitis ($p = 0.012$; a 37% reduction from the pre-threshold mean)—the most common respiratory illness in infants and one associated with asthma at later developmental stages—and a 12.2 ppt decline in presentations for respiratory problems more broadly defined ($p = 0.001$). We find suggestive evidence of a decline in gastric problems: presentations for preventable gastroenteritis, the third most common cause of infant hospital presentations in the first year of life in our data, fall by 2.4 percentage points ($p = 0.061$), and digestive problems more broadly defined fall by 9.9 percentage points ($p = 0.085$), though both estimates are significant at the 10% level only.

[28] Panel A presents results for the five most common types of potentially preventable pediatric presentations within the first year of life, and Panel B for the ten most common problem/diagnosis groups organized in indices; Table A.28 presents results on detailed diagnoses disaggregated by source, and confirms that the decline in presentations is driven by a decline in respiratory problems across all sources.

Table 3: 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	0.067
Gastroenteritis	−0.024*	0.013	0.061	94	4,047	3,969	0.053
Laryngitis	−0.002	0.006	0.759	127	5,674	5,622	0.013
Otitis media	−0.002	0.005	0.706	136	6,056	5,978	0.008
Respiratory infection	−0.012	0.010	0.245	122	5,453	5,346	0.054
Inpatient services:							
Bronchiolitis	−0.013	0.008	0.105	160	7,272	7,126	0.045
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	0.003
Otitis media	0.001	0.001	0.369	274	12,657	12,665	0.002
Respiratory infection	−0.003	0.004	0.453	271	12,540	12,537	0.014
Panel B. Presentations by ICD-10-AM diagnosis chapter and presenting problem:							
Respiratory	−0.122***	0.037	0.001	177	8,131	7,924	0.039
Infection	−0.059	0.046	0.196	121	5,357	5,280	0.023
Digestive	−0.099*	0.057	0.085	109	4,780	4,678	0.035
Unspecified	−0.065	0.052	0.214	138	6,202	6,119	0.066
Eyes and ears	−0.047	0.062	0.452	119	5,239	5,168	0.016
Skin	0.036	0.038	0.346	149	6,781	6,629	0.051
Injury/Trauma/Poisoning	0.037	0.038	0.325	177	8,131	7,924	0.025
Nervous system	0.000	0.033	0.997	156	7,114	6,933	0.047

*Note: This table presents the results of regressions of the effect of the Australian Baby Bonus on presentation diagnoses for babies within their first year of life. Diagnoses are coded by medical staff upon presentation (triage nurse) and refined by doctors at discharge (emergency department and inpatient services doctors); we aggregate diagnosis dummies across medical staff into summative scales presented here. Column (1) reports coefficient estimates, Col. (2) their standard errors, (3) the p-value of the two-sided t-test, Col. (4) the bandwidth half-length, Col. (5) and (6) the total effective sample size on the left and right of the threshold, and Col. (7) the outcome mean right before the threshold. Each row corresponds to a separate regression using our main specification, where outcomes are 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. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Importantly, we find no significant effect of the Baby Bonus for presentations related to potentially negligent behavior of parents, such as injury, poisoning, and trauma ($p = 0.325$). Together, those results lend additional support to our interpretation that the Baby Bonus had an overall positive impact on infant health, consistent with increased parental investments or behavioral responses with positive health returns.

5.3 Longer-Term Impacts of the Baby Bonus

The Australian Baby Bonus led to a decline in hospital care utilization that is entirely driven by a decline in presentations for severe, acute and urgent presentations and preventable and acute respiratory problems in infants, and not by a decline in planned and elective visits or visits with medical referrals. To conclude whether this overall pattern is consistent with additional or more adequate parental investments in child health outside the hospital sector (e.g., GP visits, better home environment), we study effects until age five.[29] These later effects would indicate intertemporal substitution patterns, whereby the decline in presentations in the first year leads to a persistent decline in health care needs at later ages or rather to additional needs at later ages caused e.g., by a failure to detect health needs early on.

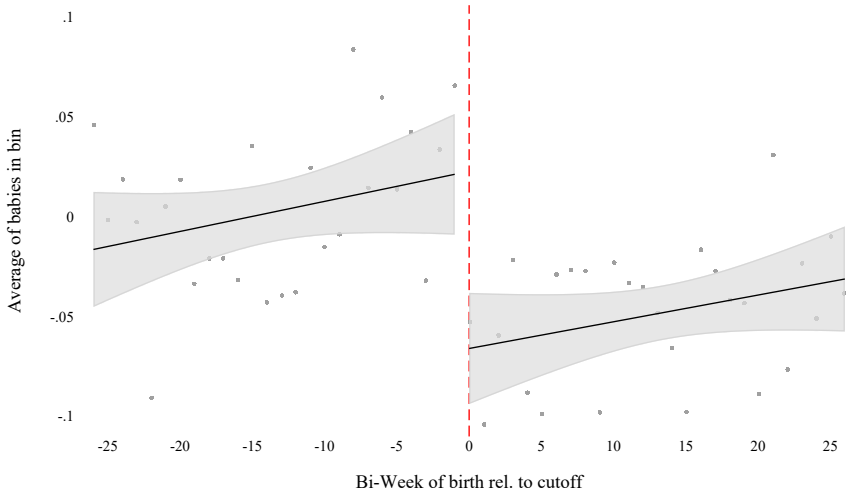
We find no significant impact of the Baby Bonus on health care utilization beyond the first year of life (Table A.29). However, the cash transfer had a positive and significant impact on the probability of a booked elective intervention in the second year of life, which may indicate an increase in parental investments in the child's health (Table A.30). This pattern may also be consistent with the observation that emergency department presentations for otitis media (escalated middle-ear infections) are significantly reduced at ages 1–2, which could reflect parental investment in elective interventions (Table A.31).

Our findings align with the literature documenting positive short- and long-term impacts of social insurance policies, with no compensating increases in medium-term utilization (see Aizer,

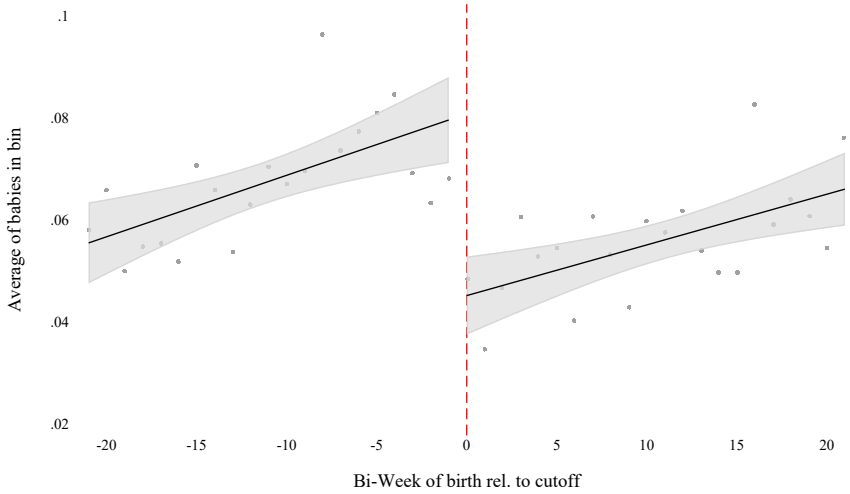
[29] We focus on effects until age five because the first 2000 days is a commonly used period of child development in the medical and public health literature. Within this period there is evidence that most heterogeneity in child health and health care utilization occurs in the first year of life and disappears after age one (see e.g., Yu et al., 2023, in the Australian health care system). In addition, children enter the school system at 5 years old. Because we want to tie changes in child hospital presentations to changes in parental investments in infant health, it is most relevant to focus on the preschool period when children spend most time with their primary carers.

Figure 4: 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 regression discontinuity plots 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.*

Hoynes and Lleras-Muney (2022) for a review). The decline in first-year hospital presentations does not appear to be offset by later utilization, which suggests that treated infants received more timely or appropriate care, whether through earlier treatment outside the hospital sector or greater use of elective, co-payment-bearing services. This is consistent with a health investment interpretation.

6 Mechanisms

We explore various margins through which the Baby Bonus cash transfer improved infant health. Firstly, we analyze maternal labor supply adjustments using additional federal-level data from the Australian Tax Authorities (ATO) and intended maternal labor supply using the Household, Income, and Labor Dynamics in Australia (HILDA) survey.[30] Next, we document heterogeneity in socioeconomic status, sex and birth order using South Australian administrative records, and we show that parental behavioral responses is the most likely explanation for these patterns. Lastly, we explore changes in household food and grocery expenditures, financial hardship and family well-being using the HILDA survey.

These analyses yield five important insights. (i) Mothers labor supply responses are overall small with labor force participation declining by 0.5 to 0.9 ppt (from 43%) and no significant changes in earnings, which suggests that low-earning mothers with little access to employer-sponsored paid parental leave may have used the cash transfer to delay returning to the labor force after birth. (ii) Our effects are driven by low-SES families, which suggests that the cash injection alleviated short-run liquidity constraints and enabled parents to increase investments in preventive health care. (iii) Our effects on child health are larger for boys than girls, and we can show that this pattern is consistent with higher parental investments in response to the "male fragility" hypothesis. (iv) Our effects are particularly concentrated in non-firstborn children in low-income families, which suggests that the experience of having a first child changes the

[30] Neither of those sources of data are linked to our South Australian Early Childhood Data Project. The Australian Tax Authorities data contain federal-level tax records. The Household, Income, and Labor Dynamics in Australia (HILDA) survey is the leading nationally representative household survey in Australia. HILDA has followed 7,682 households and over 13,000 individuals yearly since 2001. Appendix B provides details on the ATO data and the HILDA survey, data preparation and estimation, and presents evidence on the internal validity of our research design in those data.

elasticity of low-income families to investing early in child health. (v) We find suggestive evidence of increased expenditures in food and groceries, improved family well-being and marital stability, and reduced financial stress and hardship.

6.1 Maternal Labor Supply

To better understand maternal labor supply responses, we use a difference-in-differences design using tax records (ATO), and compare mothers with babies born in July-September 2004 versus April-June 2004, relative to the same comparison in years 2001-2003. We examine three outcomes: labor force participation (having any positive earnings in the financial year), total earnings (from wages, business, and partnerships), and earnings conditional on participation.

Table 4: National Effects of the Australian Baby Bonus on Labor Force Participation and Earnings Across Fiscal Years

	All years	Excl. 2003	May & Aug. births only	Excl. 2003, May & Aug. only
	(1)	(2)	(3)	(4)
Panel A. Labor Force Participation				
Baby Bonus	−0.0046	−0.0066**	−0.0072	−0.0091*
	(0.0030)	(0.0032)	(0.0052)	(0.0055)
Outcome Mean	0.43	0.42	0.43	0.42
Observations	592,022	442,469	198,713	148,707
Panel B. Earnings				
Baby Bonus	324.08	408.55*	414.98	561.78
	(224.46)	(235.02)	(445.18)	(462.25)
Outcome Mean	7,933.71	7,879.60	7,904.04	7,853.67
Observations	592,022	442,469	198,713	148,707
Panel C. Earnings Conditional on Participation				
Baby Bonus	821.82*	1,095.73**	1,114.45	1,533.44
	(491.79)	(520.50)	(983.62)	(1,029.66)
Outcome Mean	18,603.01	18,584.01	18,540.97	18,567.91
Observations	252,487	187,610	84,715	62,902

*Note: This table presents the effects of the introduction of the Australian Baby Bonus on overall earnings, labor force participation and earnings conditional on participation in the labor force, using Australian Tax records (ATO). We estimate difference-in-differences models comparing outcomes in financial year 2004–2005 for mothers giving birth in July–September 2004 v. April–June 2004, against mothers giving births in the same months in previous years (2001–2003). Each column and panel presents the regression results, reporting only the coefficient of interest. Panel A presents results for labor force participation, Panel B for overall earnings and Panel C for earnings conditional on participation. Col. (1) presents results including all years and months of birth, Col. (2) excludes 2003 among control years, Col. (3) focuses on August and May births, and Col. (4) excludes 2003 and focuses on August and May births. Heteroskedasticity-robust standard errors (HC3) are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.*

At the extensive margin (Table 4, Panel A), we find a small negative effect on labor force participation. Mothers who received the transfer are 0.5 to 0.9 ppts less likely to report any earnings in the financial year following birth, relative to a base of 43%. This effect at the extensive margin is mostly insignificant but becomes significant at the 5% level when we exclude 2003 from the comparison years. This small decline is consistent with the Baby Bonus allowing a modest fraction of mothers to delay returning to the labor force after birth.

At the intensive margin, the overall effect on maternal annual earnings is small and generally insignificant, with point estimates ranging from $324 to $562 in the most conservative specification (which excludes 2003 and focuses on May and August births, see (Table 4, Panel B). Relative to the unconditional mean of $7,900 in this sample, these estimates represent approximately a 4–7% increase in annual earnings, though we cannot reject the null hypothesis of no effect. In our most precise specification, we can rule out effects as large as $693.

Among mothers who remain employed (Table 4, Panel C), we find a small positive effect on conditional earnings ($822 to $1,533). The coefficient estimates are significant at the 5% level in some specifications but become insignificant when we focus on May and August births. Some low-earning mothers delay their return to the labor force, and others stay with their employer on unpaid leave rather than separating and claiming unemployment benefits, which is consistent with the –3.9 ppts decline in unemployment insurance claims that we estimate from Social Security records (Table A.10). Among mothers who do claim unemployment benefits, we find precisely estimated zero differences in benefit amounts or spell duration, confirming that the decline reflects mothers maintaining employer attachment rather than changes in job search behavior. Both channels remove lower earners from the pool of working mothers, which raises average conditional earnings even without any change in hours or wages. Thus the overall effect on aggregate maternal labor supply is minimal.

Importantly, the responses we observe are consistent with the context in which the Baby Bonus was introduced; one with no statutory paid parental leave and unequal access to employer-sponsored leave. Broadway et al. (2020) explains that "Prior to [the introduction of Paid Parental Leave in] 2011, each parent had a job-guaranteed right to unpaid leave for up to one year if

they had worked for their employer for at least twelve months prior to the birth" (p.35). Based on 2011 statistics from the HILDA, they show that access to paid parental leave however was highly concentrated among employees in permanent contracts and earning above-median wages (above 70% compared to less than 20% in casual contracts and less than 40% for below-median earners). Therefore, for the lowest earning mothers who would have likely not been offered employer-sponsored paid parental leave, the Baby Bonus could fund a few months of paid maternity leave while taking unpaid leave from employers instead of separating (hence the decline in unemployment insurance claims). For higher earning mothers, they would have likely been offered employer-sponsored paid parental leave, so the cash transfer would simply represent a pure income boost.[31]

Overall, our analyses suggest that the Baby Bonus had minimal impact on aggregate maternal labor supply, with modest reallocation at the extensive margin driven primarily by low-earning mothers delaying their return to the labor market. These findings on labor supply responses echo the findings in González (2013), however in a new setting without mandated paid maternity leave. This unique feature of our study brings nuance to maternal labor supply responses: our analyses suggest that low-earning mothers used the cash transfer to delay returning to the labor market while increasing labor market attachment, likely providing temporary relief to liquidity-constrained families and/or unpaid maternity leave. Because these responses are concentrated among lower-income mothers, we would expect the health effects of the Baby Bonus to be largest for disadvantaged families, a hypothesis we explore in the next section.

6.2 Heterogeneous Treatment Effects

Disadvantaged families. The effects of the Baby Bonus are concentrated among disadvantaged families, with larger responses across all outcomes and all markers of socioeconomic disadvantage (Table A.32). Infants of low-skilled fathers are 4.2 percentage points less likely to present to emergency departments and 5 percentage points less likely to be admitted to a ward, roughly twice the response for infants of high-skilled fathers. Results are qualitatively

[31] In this setting; indeed, HILDA did not include questions on parental leave-taking or time off work at the time.

similar using maternal postcode as a proxy: infants born in below-median Socio-Economic Indexes for Areas (SEIFA) postcodes show significant reductions in inpatient presentations, ward admissions, and potentially preventable pediatric presentations.

This heterogeneity likely reflects two reinforcing mechanisms. First, the transfer represented a larger relative income shock for poorer families. Second, the marginal return to cash may be higher among liquidity-constrained households, particularly if the windfall enabled early health investment and if baseline health problems are more prevalent among lower-income children.[32]

The small, short-term income boost may enable previously constrained families to pay for routine GP visits or prescription medicines, which could help detect early and treat health problems before they become acute, in other words a substitution effect of preventive care for acute care. Although we do not observe GP visits or the use of prescription medicines, this channel is consistent with our main findings on the decline in acute, severe and urgent presentations, and presentations for problems deemed preventable by doctors. These findings align with the "resource channel", in which additional household income translates into higher consumption of child-centered goods such as high quality health care, nutritious food, and safe housing (Dahl and Lochner, 2012; Milligan and Stabile, 2011).

Boys versus girls: the "Boys Fragility" hypothesis. Table A.33 (Panel A) shows larger effects for male than female infants, consistent with Barr, Eggleston and Smith (2022), with estimates for girls largely insignificant. Unlike that study, which focuses on adult earned income and cannot separately identify gendered earnings patterns from gendered parental investments (Bertrand and Pan, 2013; Autor et al., 2019; Laird, Nielsen and Nielsen, 2020), we study health in the first year of life and can attribute the differential directly to two sources: higher baseline health care needs among boys, and higher parental elasticity to the cash transfer for boys relative to girls. We find support for the boys fragility hypothesis: at birth, boys exhibit worse health outcomes (lower APGAR scores, higher rates of obstetric complications, Special Care Nursery

[32]Table A.12 shows that there is no hidden sorting patterns such as differential birth-shifting between SES subgroups. We see precisely estimated no associations for 21 pre-treatment characteristics of parents and indicators of baby health at birth. We only find two marginally statistically significant associations for birthweight and single mother status.

admissions, and preterm birth; Table A.13) and higher rates of subsequent hospital presentations. We can rule out sex selection at birth as a confound, leaving differential parental investment responses as the more plausible mechanism.

Firstborn versus non-firstborn infants. The effects of the Baby Bonus are concentrated among non-firstborn babies, particularly in lower-income families, which indicates that our results are not driven by families transitioning into parenthood (Table A.34, Panel A). This heterogeneity reflects differential elasticity of parental response to the transfer rather than alternative explanations, which we can rule out in turn. First, treatment dosage cannot explain the pattern because the transfer was fixed and not means-tested. Second, consistent with Barr, Eggleston and Smith (2022), parents of non-firstborn children are positively selected relative to parents of firstborn children, and non-firstborn babies have better health at birth (higher birthweight, higher APGAR scores, lower C-section rates), which rules out the possibility that non-firstborn babies have worse baseline health and higher healthcare needs (Table A.14). Yet non-firstborn babies have more hospital presentations at baseline, which suggests higher parental demand for care. That effects are larger and more precisely estimated for non-firstborn babies, especially in low-socioeconomic status families, suggests that the experience of raising a first child raises parents' expected returns to investing in child health. This pattern is consistent with parental learning about the child human capital production function: having a first child helps parents identify which inputs yield higher returns, and the Baby Bonus enabled otherwise income-constrained parents to act on this knowledge.[33]

6.3 Family-Process Channel: Consumption, Financial Hardship and Family Well-Being

We use the HILDA's detailed information on expenditures, financial hardship, marital stability and satisfaction, and parents' self-reported physical and mental health. We find suggestive

[33]This pattern also argues against the elimination of the First Child Tax Refund (FCTR) as a driver of our results. The FCTR applied only to first children born between July 2001 and June 2004 and created a disincentive for mothers to return to work before the child's fifth birthday. If the removal of this work disincentive were the main channel, we would expect effects concentrated among firstborn children, not non-firstborns.

evidence that the Baby Bonus helped eligible families cope with childbirth. Those findings are interpreted qualitatively, given small sample sizes and implausibly large effect sizes.

Consumption and Food Security. We observe an increase in weekly expenditures on food and groceries, partially substituting away from meals and take-out (Table A.35)—although we cannot know whether those additional expenditures are focused on child-centered goods. Eligible families are less likely to report having gone without meals, consistent with Bauer, Ruffini and Schanzenbach (2024) who find that the U.S. COVID-19 relief payment reduced food insecurity during the pandemic, and with Bartik et al. (2024) who document increased spending on housing, food, and transportation in low-income households participating in their income guarantee program.

Financial Hardship. We see a decrease in financial stress and financial hardship. Eligible families are more likely to report being able to raise $2,000 in an emergency, and less likely to report having 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 transfer alleviated short-term financial constraints for poorer families. Because the Baby Bonus was introduced in the coldest months of the year, we speculate that the extra cash may have helped households afford better heating. Improved indoor temperatures are linked to lower risks of respiratory infections in children with asthma (Howden-Chapman et al., 2008), which may partly explain the decline in hospital presentations for preventable bronchiolitis and acute respiratory problems for treated babies in South Australia, the country's coldest state.

Family Well-Being and Marital Stability. The Baby Bonus may have also improved parental relationship quality and self-reported health. Eligible families were more likely to remain married or in a de facto partnership one year after birth, which suggests the transfer may have reduced household conflict and improved marital stability. Parents of eligible infants consistently reported better health across most dimensions: physical functioning, bodily pain, emotional health, social functioning, general health, and mental health.

Overall, these findings support the family-process channel through which household income

affects child health, where evidence remains limited and mixed. McLoyd (1990) first established that income poverty is associated with poor parental health and elevated maternal depression and stress. Subsequent work shows that the income gradient in child health is mediated by maternal mental health in both the United Kingdom and Australia (Currie, Shields and Price, 2007; Propper, Rigg and Burgess, 2007; Khanam, Nghiem and Connelly, 2009).[34] Mullins (2019) finds that welfare payments improve parental well-being, spousal stability, and mothers' smooth return to work, while less-stressed mothers spend more time in productive activities with their children. Rittenhouse (2025) documents that tax credit income reduces child maltreatment, which further underscores the importance of household resources for family functioning. More recent evidence is mixed: Duncan et al. (2025), four years into the Baby's First Years trial, report null effects on maternal depression, anxiety, and BMI. This is consistent with Pilkauskas et al. (2022) and Jacob et al. (2022), who find no overall mental health effects of U.S. COVID-19 relief payments, but contrasts with Bauer, Ruffini and Schanzenbach (2024), who find improvements in maternal mental health from the U.S. COVID-19 food supplement.

7 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

[34] 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) finds 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.

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 1.

Treated babies were –3.7 ppts less likely to be admitted to a ward for urgent care and –3.4 ppts less likely to have any inpatient presentation 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.[35] The average costs of admissions to each service are:

1. Admission to a ward: $6,111; Hence the total cost is -0.037 × 17,200 × $6,111 = $3,889,040;
2. Inpatient presentation: $5,513; Hence the total cost is -0.034 × 17,200 × $5,513 = $3,224,002;
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 $7,392,095. Given the payout costs of the Australian Baby Bonus ($18,550,200 to $25,800,000

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

in 2004), we calculate that between 29% and 40% 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).

8 Conclusions

Early-life health, family, and income shocks have lasting effects on children's human capital and adult outcomes (Almond, Currie and Duque, 2018). Baby bonus policies offer a potentially powerful lever to support vulnerable children, yet little is known about their effectiveness in improving child health or the mechanisms through which they operate.

We contribute to this literature by examining the effects of the Australian Baby Bonus, an unconditional, unanticipated cash transfer paid at birth, on children's hospital care utilization and health outcomes from birth to age five, using a donut regression discontinuity design with linked administrative data from South Australia.

The Baby Bonus reduced emergency department and inpatient presentations, with effects concentrated among disadvantaged families. Crucially, the reduction is driven by presentations for acute, urgent, and severe problems (in particular respiratory conditions) rather than by a decline in elective care, which implies a genuine improvement in infant health status rather than reduced demand.

We identify two channels through which these effects operate: (1) the resources channel, in which additional income enables families to purchase child-centered goods and services; and (2) the family-process channel, in which the cash transfer enhances parental well-being and frees

up time for caregiving.

Five insights support this interpretation. First, maternal labor supply responses are small, with participation declining by 0.5 to 0.9 ppts from a base of 43% and no significant change in earnings, consistent with low-earning mothers using the transfer to extend time at home while higher-income families use it as an income supplement. Second, effects are concentrated among disadvantaged households, consistent with the transfer alleviating liquidity constraints. Third, effects are larger for boys, consistent with the male-fragility hypothesis. Fourth, effects are concentrated among non-firstborn children in disadvantaged families, consistent with parental learning about the child human capital production function through the first child. Fifth, HILDA data show increases in household food and grocery expenditures, reductions in financial stress, and improvements in marital stability and parental self-rated health. The Baby Bonus reduced potentially preventable pediatric hospitalizations from one in four to one in five infants, and emergency department presentations for respiratory problems from one in seven to one in ten, a reduction exceeding 50%. A back-of-the-envelope calculation suggests that between 29% and 40% of the initial cost was recouped within the first year of life alone, likely an underestimate of the policy's true long-run return.

Our findings provide a balanced account of the benefits of an unconditional cash transfer in one of the wealthiest OECD countries, in the absence of paid parental leave. They suggest that targeting such transfers toward disadvantaged families would be both more effective and more efficient. The Australian Baby Bonus was abolished in 2014; our evidence suggests this abolition was premature, particularly given its demonstrated effectiveness for disadvantaged families.

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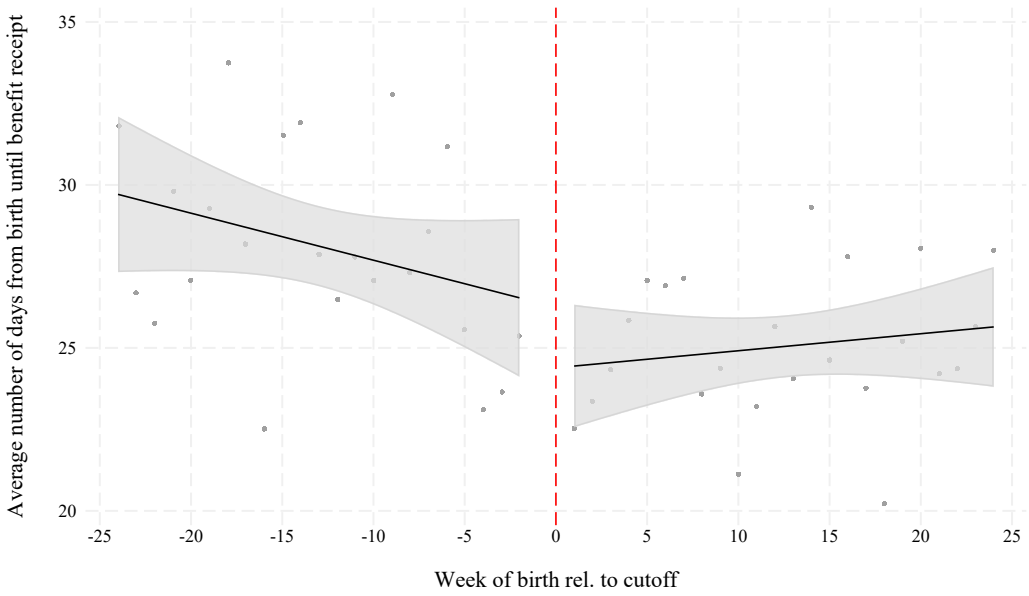
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Appendix A Additional Tables and Figures

Figure A.1: First Stage Estimates: The Australian Baby Bonus and Time to Cash Receipt

(a) Days until Cash Receipt



Note: This regression discontinuity plot presents graphical evidence on the first-stage effects of the Australian Baby Bonus on the number of days from birth until maternity payment receipt. On this 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 a 182-days bandwidth. 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 A.1: First Stage Effects of the Australian Baby Bonus

	Coef. Est.	Sd.err.	*p*-value	N.Obs.		Pre-threshold Mean
				Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)
All babies:						
Any maternity payment	0.044**	0.018	0.015	20,749	20,168	0.86
Amount received per child	2,169.513***	25.199	0.000	17,020	18,188	854.89
Days to receipt	2.642	2.803	0.346	17,020	18,188	20.75
By Socioeconomic Subgroup:						
High-skilled father:						
Any maternity payment	0.110***	0.036	0.002	6,206	6,153	0.76
Amount received per child	2,125.422***	13.962	0.000	4,245	5,270	849.66
Days to receipt	−0.289	3.560	0.935	4,245	5,270	25.17
Low-skilled father:						
Any maternity payment	0.032	0.027	0.244	7,112	6,611	0.92
Amount received per child	2,192.143***	42.525	0.000	6,477	6,179	860.85
Days to receipt	4.129	4.760	0.386	6,477	6,179	16.45
High-skilled Parents:						
Any maternity payment	0.100*	0.057	0.079	2,240	2,262	0.73
Amount received per child	2,141.838***	5.427	0.000	1,335	1,897	845.87
Days to receipt	−4.059	6.613	0.539	1,335	1,897	32.76
Low-skilled Parents:						
Any maternity payment	0.024	0.035	0.500	4,700	4,267	0.93
Amount received per child	2,186.679***	58.210	0.000	4,357	4,012	872.08
Days to receipt	4.467	6.454	0.489	4,357	4,012	14.11

*Note: This table presents the effect of giving birth after July 1, 2004, on receiving any maternity-related payment, the amount received and the number of days from birth until receipt. Column (1) reports the coefficient estimate, (2) its standard error, (3) the p-value of the two-sided t-test, (4) and (5) the sample sizes on each side of the threshold, and (6) the pre-threshold mean of the outcome. Each line corresponds to a separate regression, using local polynomial estimation with robust bias-corrected inference methods, with a fixed bandwidth of 182 days around July 1, 2004, and clustered 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 significant at the 10%, 5% and 1% respectively.*

Table A.2: Pathways into Hospital Care and Related Costs

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

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

Table A.3: Summary Statistics of Health Care Utilization at Ages 0–1

Born within:	**Panel A July 1, 2003 – July 1, 2004**		**Panel B April – September 2004**	
	Mean (1)	Sd (2)	Mean (3)	Sd (4)
Health Care Utilization Index [std.]	0.000	1.000	−0.032	0.976
Aggregate hospital presentations, not in the index:				
Any presentation (ED or inpatient service)	0.460	0.077	0.446	0.497
Any ED presentation	0.320	0.070	0.306	0.461
Any inpatient presentation	0.312	0.071	0.304	0.460
Any PPP presentation	0.220	0.063	0.206	0.404
Index subcomponents:				
Any urgent, acute or severe problem, ED	0.207	0.059	0.204	0.403
Any urgent, acute or severe problem, IS	0.168	0.055	0.157	0.364
Any admission to ward, ED	0.129	0.049	0.120	0.325
Any ED PPP presentation	0.183	0.059	0.175	0.380
Any inpatient PPP presentation	0.102	0.045	0.088	0.283
Additional items (not in the Health Care Utilization Index):				
Any inpatient presentation with admission to ward	0.026	0.025	0.028	0.164
Any inpatient presentation with overnight stay	0.202	0.064	0.206	0.404
Planned visits or presentations with medical referral:				
Emergency department:				
Any planned review visit	0.017	0.018	0.018	0.131
Any visit with medical referral	0.052	0.034	0.052	0.221
Inpatient services:				
Any planned visit	0.026	0.023	0.024	0.154
Any visit with medical referral	0.094	0.043	0.091	0.288
Any visit for an elective intervention	0.058	0.036	0.054	0.226

Note: This table presents descriptive statistics of the primary and secondary health care utilization outcomes for two samples: (A) the full sample of 17,696 babies born in South Australia between July 1, 2003, and July 1, 2004, and (B) the Born within 90-days sample of 7,708 babies born within 90 days of July 1, 2004 (April 2 – September 28, 2004). The health care utilization index is standardized to have mean 0 and standard deviation 1 in the full sample. Columns (1) and (2) present the sample mean and standard deviation in the full sample; columns (3) and (4) in the Born within 90-days sample.

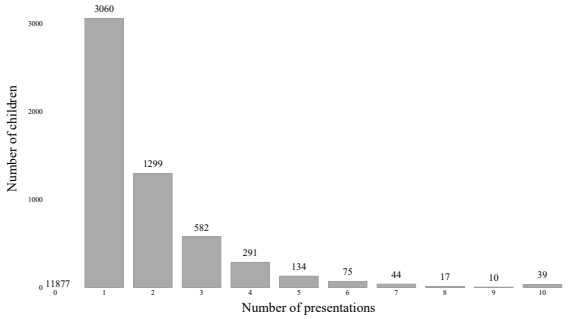
Table A.4: Child and Parental Characteristics of Babies Born in April to September 2004

	Mean (1)	Sd (2)	N (3)
Child characteristics:			
Female	0.489	0.500	7,708
Birth weight (grams)	3,364	600	7,703
Gestational age (weeks)	38.80	2.11	7,693
Preterm ($<$37 weeks)	0.149	0.356	7,693
Special nursery admission	0.171	0.376	7,693
NICU admission	0.028	0.165	7,693
Apgar score $\geq$7 at 1 min	0.753	0.431	7,683
Apgar score $\geq$7 at 5 min	0.969	0.173	7,685
Mother and birth characteristics:			
Mother age $>$35	0.183	0.387	7,708
Never married	0.116	0.320	7,704
Married	0.873	0.334	7,704
Obstetric complications	0.323	0.468	7,693
C-section	0.323	0.468	7,693
Antenatal visits	10.74	2.87	7,124
Mother smoked during pregnancy	0.192	0.394	7,601
Disadvantaged postcode	0.437	0.496	7,684
Born in private hospital	0.339	0.474	7,693

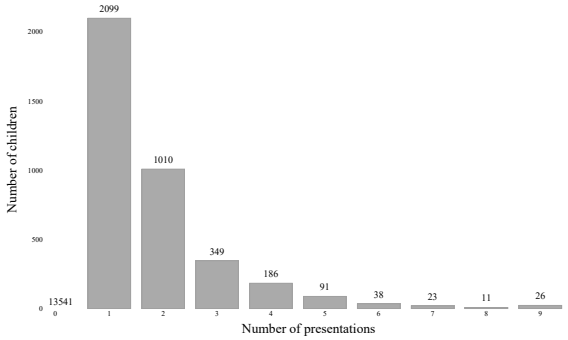
Note: This table presents descriptive statistics for the Born within 90-days sample: babies born in South Australia within 90 days of July 1, 2004 (April 2 – September 28, 2004). Column (1) presents the sample mean, column (2) the standard deviation, and column (3) the number of observations for each characteristic. Sample sizes vary across rows because of missingness; the maximum N is 7,708.

Figure A.2: Distribution of Hospitalizations at Ages 0–1

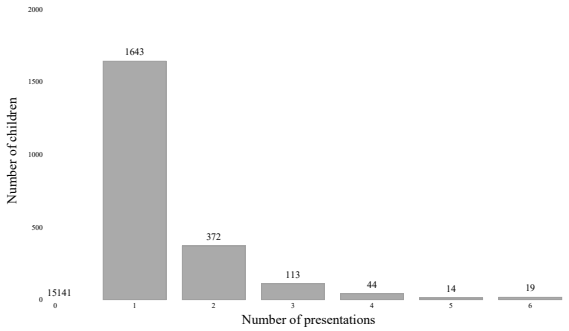
(a) Emergency department presentations



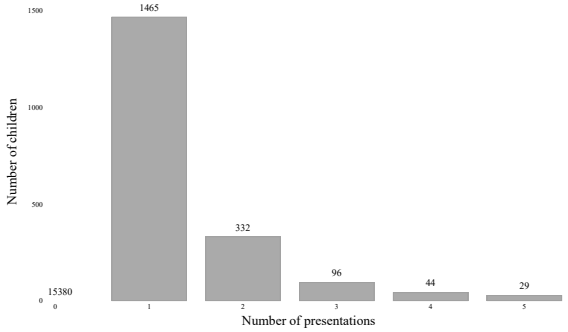
(b) Potentially preventable pediatric hospitalizations



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



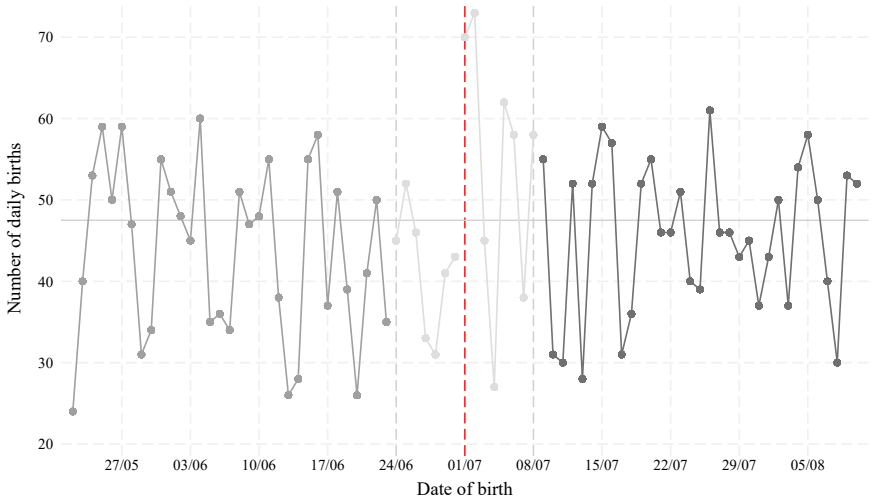
(d) Respiratory problems recorded at presentation by triage nurse



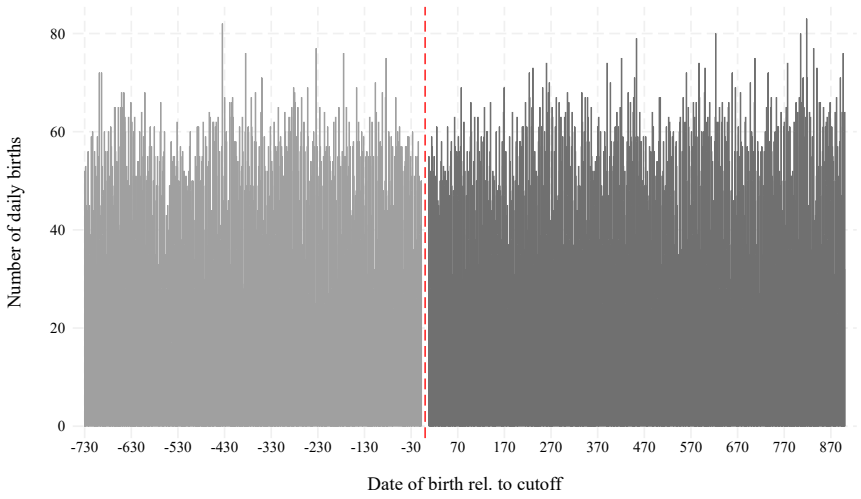
Source: Potentially preventable pediatric hospitalizations: Integrated South Australian Activity Collection (ISAAC); Emergency department presentations and emergency department presentations for respiratory problems: South Australian Emergency Department Data Collection (EDDC). The sample is comprised of babies born in South Australia between July 1, 2003, and July 1, 2004, excluding babies born overseas.

Figure A.3: Daily Number of Births in South Australia

(a) Daily Births Within 30 Days of Thursday, July 1, 2004



(b) Daily Births in 2002–2006



Note: Figure A.3a plots 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. Figure A.3b is 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 A.5: Results of Local Polynomial Density Test

Estimation Method	Est. Bandwidth		Observations		Density Test
	Left	Right	Left	Right	*p*-val.
(1)	(2)	(3)	(4)	(5)	(6)
Models with symmetric bandwidth:					
Restricted, linear	154	154	6,957	6,811	0.555
Restricted, 2nd-order polynomial	107	107	4,748	4,651	0.204
Unrestricted, linear	186	174	8,557	7,772	0.839
Unrestricted, 2nd-order polynomial	96	96	4,135	4,030	0.157
Models with asymmetric bandwidth:					
Unrestricted, linear	186	174	8,557	7,772	0.839
Unrestricted, 2nd-order polynomial	72	119	2,990	5,221	0.145

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 A.6: Non-parametric density tests using 60- and 90-days bandwidths

Estimation Method	Est. Bandwidth		Observations		Density Test
	Left	Right	Left	Right	*p*-val.
(1)	(2)	(3)	(4)	(5)	(6)
90-days bandwidth:					
Restricted, linear	37	37	1,270	1,330	0.229
Unrestricted, linear	29	29	943	1,001	0.997
Unrestricted, linear, asym. bandwidth	29	90	943	3,827	0.211
60-days bandwidth:					
Restricted, linear	29	29	943	1,001	0.655
Unrestricted, linear	29	29	943	1,001	0.997
Unrestricted, linear, asym. bandwidth	29	29	943	1,001	0.997

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 within 90 days of July 1, 2004, excluding 93 children born abroad during this time, and all children born within seven days of July 1, 2004.*

Table A.7: 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	0.045
...	...	..	...	...
100	107	4,748	4,651	0.322
		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 A.8: Continuity of Predetermined Characteristics and 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)
Panel A. Predetermined child and parental characteristics							
Child is female	0.015	0.011	0.175	477	22,681	22,733	0.483
Birth in private hospital	0.012	0.012	0.345	405	19,174	19,061	0.333
No. of antenatal visits	−0.057	0.091	0.529	321	13,822	13,611	10.791
Mother smokes	0.002	0.008	0.780	593	27,413	27,870	0.188
Mother's age:							
35+	−0.005	0.008	0.509	565	26,620	26,846	0.184
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	0.560
Mother's marital status:							
Never Married	0.011*	0.006	0.077	620	29,180	29,639	0.107
Married	−0.006	0.008	0.464	503	23,821	23,910	0.878
Single	−0.004	0.003	0.115	425	20,117	20,039	0.014
Mother's race:							
Caucasian	0.001	0.006	0.889	500	23,699	23,755	0.910
Asian	0.003	0.004	0.533	553	26,089	26,246	0.047
Aboriginal	−0.004	0.005	0.352	474	22,465	22,544	0.044
Panel B. Child birth outcomes							
Birth weight:							
Grams	22.067	14.375	0.125	483	22,955	23,004	3,355.824
Low (<2,500g)	0.000	0.007	0.950	469	22,300	22,348	0.065
Very Low (<1,500g)	−0.005	0.004	0.133	308	14,415	14,305	0.014
Special Nursery	0.001	0.010	0.923	381	18,007	17,856	0.168
NICU	0.001	0.004	0.787	673	31,773	32,255	0.028
PICU	0.000	0.001	0.823	476	22,574	22,645	0.002
Neonatal Death	−0.001	0.001	0.730	326	15,314	15,159	0.003
Apgar 1 min >7	−0.019*	0.011	0.085	419	19,791	19,647	0.759
Apgar 5 min >7	−0.002	0.005	0.613	467	22,108	22,164	0.971
Gestational Age	0.058	0.062	0.348	339	15,923	15,779	38.793
Preterm Birth (<37 weeks)	−0.004	0.007	0.611	666	31,498	31,953	0.148
Obstetric Complication	−0.024*	0.015	0.095	290	13,565	13,454	0.338
C-section Birth	0.008	0.013	0.564	599	28,101	28,480	0.315

*Note: This table presents balancing tests at the July 1, 2004 eligibility threshold. Panel A reports tests on predetermined child and parental characteristics from birth and perinatal records; Panel B reports tests on child birth outcomes from the same records. Column (1) reports the coefficient estimate, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the bandwidth half-length, (5) and (6) the sample sizes on each side of the threshold, and (7) the pre-threshold mean of the outcome. 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. ***, ** and * denote statistical significance at the 1%, 5% and 10% level.*

Table A.9: The Effects of the Australian Baby Bonus on Predetermined Characteristics and Birth Outcomes (60- and 90-days bandwidths)

	Panel A. 60-days bandwidth				Panel B. 90-days bandwidth			
	Coef. Est. (1)	Sd.err. (2)	*p*-value (3)	N.Obs. (4)	Coef. Est. (1)	Sd.err. (2)	*p*-value (3)	N.Obs. (4)
Child and Parental Predetermined Characteristics:								
Child is Female	0.048	0.085	0.567	4,751	0.047	0.055	0.394	7,604
Birth in Private hospital	-0.019	0.079	0.813	4,739	0.026	0.052	0.609	7,589
No. antenatal visits	0.196	0.525	0.709	4,383	0.185	0.347	0.593	7,030
Mother smokes	-0.052	0.063	0.414	4,691	-0.024	0.042	0.566	7,497
Mother's age:								
35+	0.027	0.060	0.658	4,751	0.026	0.040	0.525	7,604
40+	-0.005	0.024	0.835	4,751	0.001	0.016	0.929	7,604
Father's occupation:								
High skilled	0.085	0.081	0.293	4,498	-0.037	0.053	0.484	7,203
Low skilled	-0.094	0.085	0.268	4,498	0.040	0.056	0.478	7,203
Mother's marital status:								
Never Married	-0.035	0.054	0.516	4,747	-0.024	0.036	0.493	7,600
Married	0.085	0.058	0.144	4,747	0.064*	0.038	0.096	7,600
Single	-0.050*	0.026	0.057	4,747	-0.039**	0.016	0.014	7,600
Mother's race:								
Caucasian	-0.007	0.047	0.889	4,739	0.019	0.031	0.541	7,589
Asian	0.027	0.036	0.461	4,739	0.003	0.024	0.903	7,589
Aboriginal or TSI	-0.020	0.032	0.530	4,739	-0.022	0.022	0.311	7,589
Child birth outcomes:								
Birthweight:								
In grams	18.943	109.930	0.863	4,746	67.361	69.591	0.333	7,599
Low (<2,500g)	0.043	0.043	0.327	4,746	0.023	0.029	0.422	7,599
Very Low (<1,500g)	0.000	0.020	0.989	4,746	-0.022*	0.012	0.078	7,599
Special Nursery	0.064	0.067	0.339	4,739	0.071*	0.043	0.099	7,589
NICU	0.050	0.032	0.117	4,739	0.020	0.020	0.315	7,589
PICU	0.005	0.005	0.340	4,739	0.004	0.004	0.225	7,589
Neonatal death	0.007	0.013	0.573	4,739	0.003	0.007	0.692	7,589
Apgar 1 min > 7	-0.171**	0.074	0.021	4,730	-0.078	0.048	0.106	7,579
Apgar 5 min > 7	-0.062*	0.035	0.075	4,732	-0.047**	0.022	0.036	7,581
Gestational age (weeks)	0.206	0.424	0.626	4,739	0.342	0.259	0.187	7,589
Preterm birth	0.028	0.059	0.633	4,739	0.013	0.038	0.735	7,589
Obstetric complications	-0.025	0.080	0.759	4,739	-0.065	0.053	0.214	7,589
C-section	-0.102	0.078	0.192	4,739	0.021	0.051	0.679	7,589

*Note: This table presents RD treatment effects of the Baby Bonus on predetermined characteristics of children and their parents as well as birth outcomes imposing a 60-days bandwidth (Panel A) and a 90-days bandwidth (Panel B). In each panel, Column (1) reports coefficient estimates, Col. (2) their standard errors, Col.(3) p-value of two-sided t-test, and Col.(4) the total effective sample size. Each line by panel corresponds to a separate regression using our main specification. We use local polynomial estimation with robust bias-corrected inference methods, with standard errors clustered at the level of birth dates. We exclude 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 A.10: Effects of the Australian Baby Bonus on Other Social Security Benefits Spells

	Coef. Est.	Sd.err.	Bandwidth 1/2 length	N.Obs.		Pre-threshold Mean
				Left	Right	
	(1)	(2)	(3)	(4)	(5)	(6)
Family Payment:						
Any receipt	−0.089	0.055	90	9,510	9,428	0.352
Total amount received	−801.503	952.065	90	3,367	3,000	8,350.757
Days until next spell	6.745	11.401	90	3,367	3,000	55.282
Disability Insurance or Support:						
Any receipt	0.001	0.030	90	9,510	9,428	0.067
Total amount received	−1,644.225	2,969.251	90	637	654	5,390.485
Days until next spell	16.851	58.072	90	641	654	89.122
Unemployment Insurance:						
Any receipt	−0.039*	0.023	90	9,510	9,428	0.059
Total amount received	410.478	1,588.163	90	559	565	1,290.346
Days until next spell	47.203	35.348	90	559	565	37.239

*Note: This table presents the effects of the Baby Bonus on the likelihood that mothers receive any social security benefits within 365 days of giving birth to a live baby. We estimate those models using mothers' Social Security records linked to Birth Registry records. Column (1) report the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, (4) and (5) the effective sample sizes on each side of the threshold, and (6) the pre-threshold mean of the outcome. Each line corresponds to a separate regression using local polynomial estimation with robust bias-corrected inference methods, a fixed 90-days bandwidth and 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 statistically significant at the 10%, 5% and 1% levels.*

Table A.11: 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	0.333	314	14,743	14,639	0.697
Number of live births	−0.016	0.024	0.516	528	25,059	25,140	0.908
Any miscarriage	−0.009	0.010	0.368	500	23,705	23,766	0.235
Pregnancy Terminations:							
Any abortion	−0.007	0.008	0.384	546	25,890	25,976	0.143
Number of abortions	−0.007	0.011	0.493	603	28,332	28,794	0.185
Days since last abortion	−1.287	31.759	0.968	392	8,892	8,511	1,072.662

*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. ***, ** and * denote statistical significance at the 1%, 5% and 10% level.*

Table A.12: The Effects of the Australian Baby Bonus on Pretreatment Characteristics and Birth Outcomes by Socioeconomic Status

	Father Occupation														
	Panel A. High-Skilled					Panel B. Low-Skilled					Panel C. Disadvantaged Postcode				
Variable	Coef. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N (5)	Coef. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N (5)	Coef. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N (5)
Child and Parental Predetermined Characteristics:															
Child is Female	0.004	0.023	0.845	470	14,046	0.020	0.013	0.119	576	29,169	0.010	0.016	0.515	513	21,488
Birth in Private hospital	-0.017	0.020	0.392	469	14,014	0.017	0.015	0.264	460	23,308	-0.001	0.013	0.970	474	19,870
No. antenatal visits	-0.167*	0.097	0.085	534	14,272	0.032	0.100	0.747	467	21,953	-0.074	0.143	0.604	342	12,945
Mother smokes	-0.019	0.012	0.100	433	12,724	0.008	0.012	0.507	539	27,077	0.006	0.013	0.649	643	26,664
Mother's age:															
35+	-0.035*	0.018	0.051	493	14,702	0.012	0.009	0.150	728	37,022	0.007	0.009	0.454	717	30,309
40+	-0.007	0.008	0.433	456	13,597	-0.007	0.005	0.172	431	21,817	-0.008	0.005	0.103	421	17,649
Father's occupation:															
High skilled											-0.002	0.013	0.863	642	25,336
Low skilled											0.025*	0.014	0.082	711	28,220
Mother's marital status:															
Never Married	0.003	0.007	0.678	576	17,115	0.021***	0.008	0.007	675	34,298	0.018*	0.011	0.097	632	26,520
Married	-0.001	0.008	0.887	511	15,250	-0.018**	0.008	0.038	622	31,547	-0.016	0.012	0.170	613	25,660
Single	-0.001	0.003	0.686	569	16,894	-0.005	0.003	0.124	493	25,041	-0.002	0.004	0.561	691	29,109
Mother's race:															
Caucasian	-0.011	0.010	0.289	416	12,359	-0.003	0.008	0.710	561	28,337	0.013	0.011	0.253	444	18,557
Asian	0.010	0.008	0.226	474	14,134	0.002	0.006	0.790	576	29,117	-0.010	0.007	0.138	675	28,424
Aboriginal or TSI	0.002	0.006	0.758	424	12,590	0.001	0.004	0.881	699	35,471	-0.002	0.008	0.755	430	17,945
Child birth outcomes:															
Birthweight:															
Grams	18.693	20.532	0.363	619	18,447	57.507**	22.749	0.011	375	18,887	39.588*	23.738	0.095	406	16,917
Low (<2,500g)	-0.007	0.012	0.574	376	11,173	-0.001	0.009	0.913	489	24,795	-0.002	0.011	0.891	380	15,822
Very Low (<1,500g)	-0.002	0.004	0.663	303	8,941	-0.007	0.005	0.176	342	17,211	-0.009	0.005	0.117	336	13,804
Special Nursery	-0.007	0.016	0.681	450	13,375	0.007	0.013	0.590	410	20,649	-0.014	0.013	0.280	559	23,318
NICU	-0.004	0.008	0.634	339	9,975	0.003	0.005	0.568	616	31,214	-0.007	0.007	0.323	397	16,469
PICU	0.002	0.002	0.161	673	20,095	0.000	0.001	0.928	441	22,301	-0.002	0.001	0.177	479	20,052
Neonatal death	0.001	0.002	0.734	539	16,015	-0.001	0.002	0.547	342	17,125	0.000	0.002	0.782	517	21,608
Apgar 1 min > 7	0.000	0.015	0.994	657	19,564	-0.007	0.015	0.654	532	26,957	0.003	0.013	0.783	683	28,664
Apgar 5 min > 7	0.000	0.007	0.969	548	16,235	-0.004	0.006	0.506	471	23,826	0.008	0.006	0.176	440	18,341
Gestational age	0.064	0.083	0.437	467	13,906	0.144	0.095	0.132	311	15,572	0.108	0.100	0.280	333	13,639
Preterm birth	-0.009	0.012	0.467	722	21,638	-0.009	0.011	0.418	548	27,750	-0.001	0.013	0.924	491	20,603
Obstetric complication	-0.004	0.019	0.831	525	15,565	-0.023	0.018	0.210	330	16,490	-0.002	0.020	0.913	334	13,675
C-section	0.011	0.019	0.566	692	20,703	0.004	0.015	0.787	670	33,973	0.012	0.015	0.437	694	29,248

*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. In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the bandwidth half-length, (5) the total effective sample size. Each line by panel reports the results of a separate regression analysis using local linear estimation and robust bias-corrected inference, and CER-optimal bandwidths with standard errors clustered at the level of birth dates. 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 A.13: The Effects of the Australian Baby Bonus on Predetermined Characteristics and Birth Outcomes By Sex

	Panel A. Boys						Panel B. Girls					
	Coef. Est. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N.Obs. (5)	Mean (6)	Coef. Est. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N.Obs. (5)	Mean (6)
Child and Parental Predetermined Characteristics:												
Birth in Private hospital	0.031*	0.018	0.079	356	17,113	0.315	-0.011	0.017	0.520	478	22,130	0.351
No. antenatal visits	0.129	0.099	0.192	489	21,703	10.589	-0.278**	0.127	0.029	254	10,514	11.042
Mother smokes	-0.013	0.013	0.325	483	23,220	0.201	0.013	0.012	0.287	494	22,603	0.177
Mother's age:												
35+	0.003	0.011	0.791	638	31,146	0.177	-0.014	0.011	0.221	598	27,578	0.191
40+	0.002	0.005	0.678	490	23,925	0.030	-0.009	0.007	0.187	304	13,819	0.029
Father's occupation:												
High skilled	0.012	0.019	0.535	372	17,102	0.328	-0.004	0.015	0.781	578	25,181	0.340
Low skilled	0.006	0.017	0.726	491	22,738	0.562	0.018	0.014	0.198	754	33,223	0.555
Mother's marital status:												
Never Married	0.005	0.010	0.652	514	25,077	0.110	0.015*	0.008	0.080	678	31,504	0.108
Married	0.001	0.012	0.909	445	21,620	0.871	-0.014	0.009	0.134	595	27,359	0.881
Single	-0.005	0.004	0.126	455	22,123	0.016	-0.002	0.003	0.622	489	22,682	0.012
Mother's race:												
Caucasian	0.007	0.008	0.401	505	24,622	0.910	-0.006	0.009	0.513	554	25,447	0.911
Asian	-0.001	0.006	0.936	559	27,112	0.048	0.006	0.006	0.336	577	26,511	0.045
Aboriginal or TSI	-0.005	0.006	0.341	525	25,512	0.043	-0.001	0.006	0.925	534	24,631	0.044
Child birth outcomes:												
Birthweight:												
Grams	24.571	16.502	0.137	605	29,446	3418.764	17.958	21.877	0.412	457	21,178	3288.593
Low (<2,500g)	0.000	0.007	0.973	640	31,229	0.061	0.002	0.009	0.867	508	23,471	0.070
Very Low (<1,500g)	0.002	0.003	0.460	716	35,045	0.010	-0.009*	0.005	0.063	323	14,755	0.018
Special Nursery	0.014	0.014	0.323	352	16,980	0.179	-0.011	0.013	0.376	435	20,077	0.156
NICU	0.004	0.005	0.414	531	25,870	0.030	-0.002	0.005	0.650	637	29,482	0.025
PICU	0.001	0.001	0.681	426	20,657	0.002	0.000	0.001	0.992	761	35,334	0.002
Neonatal death	-0.002	0.002	0.343	324	15,559	0.003	0.001	0.002	0.553	470	21,774	0.002
Apgar 1 min > 7	-0.026	0.017	0.115	413	19,891	0.748	-0.007	0.014	0.591	516	23,727	0.770
Apgar 5 min > 7	0.000	0.006	0.979	509	24,749	0.962	-0.005	0.006	0.358	476	21,941	0.981
Gestational age	0.056	0.064	0.387	598	29,062	38.773	0.058	0.082	0.480	373	17,109	38.795
Preterm birth	0.000	0.011	0.975	520	25,324	0.151	-0.007	0.011	0.528	614	28,273	0.144
Obstetric complication	-0.018	0.017	0.291	421	20,346	0.334	-0.019	0.019	0.312	368	16,830	0.331
C-section	0.007	0.016	0.679	589	28,620	0.322	0.009	0.015	0.564	699	32,426	0.307

*Note: This table presents RD treatment effects of the Baby Bonus on predetermined characteristics of children and their parents as well as birth outcomes for boys (Panel A) and girls (Panel B). In each panel, Column (1) reports coefficient estimates, Col. (2) their standard errors, Col.(3) p-value of two-sided t-test, Col.(4) bandwidth half-length, and Col. (5) total effective sample size. Each line by 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 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 A.14: The Effects of the Australian Baby Bonus on Predetermined Characteristics and Birth Outcomes By Firstborn v. Nonfirstborn Status

	Panel A. Non-Firstborn Children						Panel B. Firstborn Children					
	Coef. Est. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N.Obs. (5)	Mean (6)	Coef. Est. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N.Obs. (5)	Mean (6)
Child and Parental Predetermined Characteristics:												
Child is Female	0.012	0.015	0.436	479	23,514	0.481	0.018	0.013	0.166	664	30,479	0.481
Birth in Private hospital	-0.018	0.015	0.219	523	25,622	0.333	0.042**	0.017	0.015	352	16,028	0.333
No. antenatal visits	-0.105	0.099	0.290	470	20,995	10.567	0.054	0.118	0.648	373	15,548	11.016
Mother smokes	0.000	0.011	0.993	622	30,028	0.204	0.004	0.011	0.705	567	25,540	0.172
Mother's age:												
35+	-0.001	0.011	0.928	613	30,072	0.221	-0.017	0.011	0.115	470	21,621	0.147
40+	-0.005	0.006	0.442	381	18,545	0.034	0.001	0.004	0.862	673	30,902	0.026
Father's occupation:												
High skilled	-0.003	0.013	0.843	667	31,361	0.333	0.014	0.018	0.429	392	16,900	0.330
Low skilled	0.003	0.015	0.861	530	24,830	0.562	0.012	0.019	0.540	424	18,290	0.560
Mother's marital status:												
Never Married	0.008	0.008	0.317	550	26,918	0.080	0.012	0.011	0.259	620	28,378	0.137
Married	0.003	0.010	0.770	421	20,614	0.899	-0.014	0.012	0.229	590	26,954	0.855
Single	-0.010**	0.004	0.016	356	17,252	0.020	0.002	0.003	0.523	563	25,786	0.008
Mother's race:												
Caucasian	0.000	0.008	0.984	499	24,493	0.921	0.001	0.009	0.943	561	25,601	0.899
Asian	0.002	0.005	0.664	691	34,142	0.040	0.003	0.006	0.652	555	25,312	0.054
Aboriginal or TSI	-0.004	0.006	0.534	477	23,423	0.041	-0.004	0.006	0.550	642	29,430	0.047
Child birth outcomes:												
Birthweight:												
Grams	34.934	22.397	0.119	434	21,212	3385.457	0.554	15.529	0.972	775	35,535	3331.360
Low (<2,500g)	0.000	0.009	0.995	436	21,353	0.064	0.001	0.007	0.867	743	34,152	0.066
Very Low (<1,500g)	-0.005	0.004	0.204	373	18,138	0.014	-0.005	0.004	0.308	347	15,807	0.013
Special Nursery	-0.004	0.011	0.682	580	28,335	0.157	0.005	0.016	0.775	340	15,441	0.179
NICU	0.003	0.006	0.548	506	24,823	0.026	-0.002	0.006	0.722	506	23,207	0.029
PICU	0.000	0.001	0.718	560	27,389	0.002	-0.001	0.002	0.658	438	19,994	0.002
Neonatal death	0.002	0.002	0.361	465	22,812	0.002	-0.002	0.002	0.278	325	14,801	0.004
Apgar 1 min > 7	-0.006	0.012	0.603	691	34,008	0.784	-0.023	0.018	0.196	351	15,957	0.731
Apgar 5 min > 7	0.000	0.004	0.921	609	29,748	0.975	-0.006	0.007	0.439	483	22,129	0.967
Gestational age (weeks)	0.039	0.072	0.591	435	21,234	38.678	0.090	0.081	0.269	347	15,737	38.902
Preterm birth	-0.007	0.010	0.457	779	38,572	0.155	-0.003	0.010	0.772	603	27,537	0.143
Obstetric complications	-0.005	0.016	0.751	434	21,177	0.318	-0.037**	0.019	0.046	345	15,699	0.349
C-section	-0.006	0.016	0.719	662	32,640	0.308	0.022	0.016	0.158	543	24,847	0.322

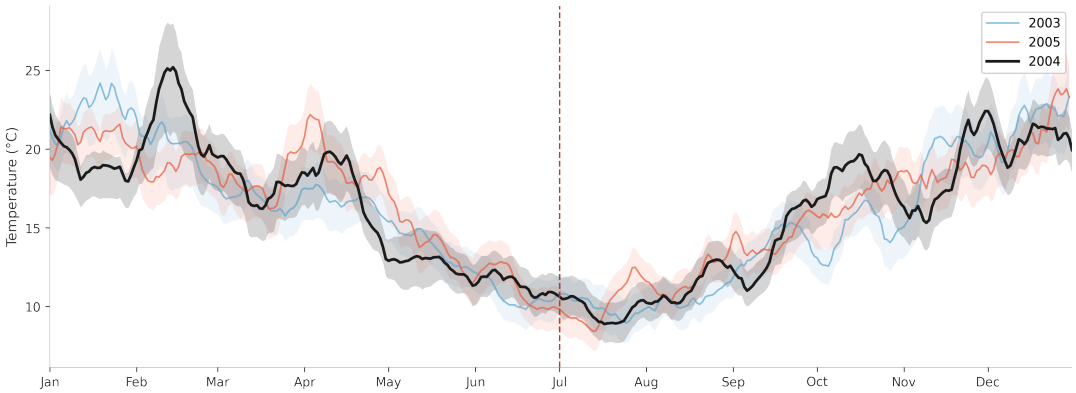
*Note: This table presents RD treatment effects of the Baby Bonus on predetermined characteristics of children and their parents as well as birth outcomes for firstborn children (Panel A) and non-firstborn children (Panel B). In each panel, Column (1) reports coefficient estimates, Col. (2) their standard errors, Col.(3) p-value of two-sided t-test, Col.(4) bandwidth half-length, and Col. (5) total effective sample size. Each line by 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 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 A.15: Placebo Effects of the Australian Baby Bonus on Hospital Presentations in Prepolicy Years 2002 and 2003

	Panel A. Placebo Year 2002				**Panel B. Placebo Year 2003**			
	Coef. Est. (1)	Sd.err. (2)	1/2 Band. (3)	N.Obs. (4)	Coef. Est. (1)	Sd.err. (2)	1/2 Band. (3)	N.Obs. (4)
Aggregate inpatient presentations, not in the index:								
Any inpatient presentation	0.029	0.019	162	14,969	0.002	0.019	191	17,567
Index subcomponents (inpatient services only):								
Any urgent, acute or severe problem, IS	0.012	0.014	175	16,198	0.013	0.024	95	8,536
Any inpatient PPP presentation	-0.018	0.011	192	17,677	0.035**	0.017	74	6,398
Other items (not in Health Care Utilization Index):								
Any Inpatient admission to ward	-0.004	0.009	175	16,198	-0.001	0.007	204	18,827
Any Inpatient overnight admission	0.010	0.017	162	14,885	-0.015	0.021	147	13,482
Any planned visit	0.002	0.006	198	18,228	0.006	0.008	136	12,542
Any visit with med. referral	0.011	0.014	154	14,125	-0.010	0.018	97	8,736
Any booked elective procedure	0.018	0.012	163	15,061	-0.003	0.012	131	12,029

*Note: This table presents RD treatment effects of the Baby Bonus on hospital presentations in prepolicy years 2002 (Panel A) and 2003 (Panel B). In each panel, Column (1) reports coefficient estimates, Col. (2) their standard errors, Col.(3) bandwidth half-length, and Col. (4) total effective sample size. Each line by placebo year 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 statistical significance at the 10%, 5% and 1% respectively.*

Figure A.4: Average Daily Temperatures in South Australia, 2003–2005



Note: This figure presents smoothed daily mean temperatures in South Australia for 2003 (blue), 2004 (black), and 2005 (red), computed as the cross-station average across 86 Bureau of Meteorology weather stations. Shaded bands indicate the interquartile range (25th–75th percentile) of station-level temperatures. All series are smoothed using a 14-day centered rolling window. The vertical dashed red line indicates July 1.

Table A.16: Seasonalities in Parental Characteristics by Month of Birth

	Teen mom	Mother Marital Status:			Mother race:			Father:	
	(1)	Never married (2)	Married (3)	Single (4)	Caucasian (5)	Asian (6)	Aboriginal (7)	High-skilled (8)	Low-skilled (9)
February	-0.003	-0.003	0.004	-0.001	-0.002	-0.002	0.004	-0.011	0.009
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.008)
March	-0.002	-0.002	0.004	-0.002	-0.000	-0.002	0.003	-0.002	0.003
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
April	-0.003	0.001	0.000	-0.002	0.005	-0.006*	0.001	0.003	-0.007
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.008)
May	-0.003	-0.001	0.004	-0.003*	-0.002	-0.000	0.003	-0.002	-0.005
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.008)
June	-0.004	0.002	-0.003	0.001	0.002	-0.003	0.001	0.004	-0.004
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.008)
July	-0.003	0.003	-0.001	-0.002	-0.002	-0.002	0.004	0.010	-0.013*
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
August	-0.007***	-0.008*	0.009*	-0.001	0.005	-0.004	-0.000	0.004	-0.006
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
September	-0.004	-0.004	0.007	-0.003	0.000	-0.001	0.001	0.004	-0.005
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
October	-0.004	-0.004	0.008	-0.003*	-0.003	0.002	0.001	0.011	-0.004
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
November	-0.003	-0.006	0.006	0.000	-0.001	0.004	-0.002	-0.004	0.004
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
December	-0.003	-0.002	0.001	0.001	-0.006	-0.001	0.007**	0.003	-0.005
	(0.003)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.007)
Mean of y	0.032	0.119	0.865	0.016	0.917	0.044	0.039	0.307	0.568

Note: This table presents the results of linear regressions of maternal characteristics on dummies for babies' month of birth. The sample is all births in South Australia recorded in Birth Records between January 1, 1997 and December 31, 2003. We defined teenage motherhood as having a child before the age of 18. Heteroskedasticity-robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.17: The Effects of the Australian Baby Bonus on Predetermined Characteristics and Birth Outcomes in Prepolicy Years 2002 and 2003

	Panel A. Placebo Year 2002				Panel B. Placebo Year 2003			
	Coef. Est. (1)	Sd.err. (2)	1/2 Band. (3)	N.Obs. (4)	Coef. Est. (1)	Sd.err. (2)	1/2 Band. (3)	N.Obs. (4)
Child and Parental Predetermined Characteristics:								
Child is Female	0.005	0.014	304	28,234	-0.005	0.013	373	34,930
Birth in Private hospital	0.033	0.024	151	13,829	0.001	0.014	315	29,533
No. antenatal visits	-0.085	0.110	206	16,741	0.177*	0.094	287	24,067
Mother smokes	0.001	0.010	397	36,601	-0.014	0.010	382	35,178
Mother's age:								
35+	0.001	0.009	461	43,603	-0.003	0.008	519	48,836
40+	-0.008**	0.004	426	40,116	0.000	0.004	464	43,417
Father's occupation:								
High skilled	0.010	0.012	454	40,839	-0.002	0.012	386	34,286
Low skilled	-0.007	0.011	495	44,636	-0.004	0.013	363	32,214
Mother's marital status:								
Never Married	-0.001	0.009	419	39,341	-0.008	0.007	461	43,069
Married	0.007	0.009	405	37,943	0.009	0.007	560	52,650
Single	-0.006	0.004	315	29,340	0.001	0.003	363	33,863
Mother's race:								
Caucasian	0.002	0.008	313	29,018	-0.002	0.006	532	49,944
Asian	-0.002	0.005	407	38,060	0.002	0.004	575	53,890
Aboriginal or TSI	0.001	0.006	321	29,802	-0.001	0.004	581	54,566
Child birth outcomes:								
Baby weight (in grams)	10.754	16.199	343	31,973	12.134	14.284	504	47,380
Special Nursery	0.016	0.011	321	29,802	-0.014	0.011	374	34,863
NICU	-0.002	0.005	309	28,670	-0.001	0.004	473	44,125
PICU	0.003**	0.001	319	29,610	-0.001	0.001	524	49,099
Neonatal death	0.001	0.001	333	31,066	0.000	0.001	559	52,362
Apgar 1 min >7	-0.022*	0.012	345	32,027	0.004	0.011	356	33,133
Apgar 5 min >7	0.002	0.004	391	36,535	0.000	0.004	420	38,947
Gestational age	-0.038	0.062	325	30,173	-0.017	0.053	480	44,836
Preterm birth	0.000	0.010	340	31,768	0.001	0.009	461	43,010
Obstetric complication	0.009	0.015	321	29,802	-0.043***	0.013	350	32,686
C-section	-0.004	0.018	317	29,498	-0.014	0.014	396	36,854

*Note: This table presents RD treatment effects of the Baby Bonus on predetermined characteristics of children and their parents as well as birth outcomes in prepolicy years 2002 (Panel A) and 2003 (Panel B), sourced from birth and perinatal records. In each panel, Column (1) reports coefficient estimates, Col. (2) their standard errors, Col.(3) bandwidth half-length, and Col. (4) total effective sample size. Each line by placebo year 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 significant at the 10%, 5% and 1% respectively.*

Table A.18: Sensitivity of Main Results to Observations Near the Threshold (Table 1/2)

Exclude births within:	**Panel A. 5 days**				**Panel B. 8 days**			
	Coef. (1)	Sd.err. (2)	Band. (3)	N (4)	Coef. (1)	Sd.err. (2)	Band. (3)	N (4)
Health Care Utilization Index [std.]	−0.086**	0.038	181	16,466	−0.087**	0.041	171	15,410
Aggregate hospital presentations, not in the index:								
Any presentation (ED or inpatient service)	−0.017	0.020	187	17,047	−0.007	0.022	166	14,869
Any ED presentation	−0.024	0.018	155	14,067	−0.019	0.020	158	14,034
Any inpatient presentation	−0.032*	0.017	222	20,387	−0.034**	0.017	215	19,444
Any PPP presentation	−0.029*	0.017	160	14,498	−0.027	0.019	158	14,034
Index subcomponents:								
Any urgent, acute or severe problem, ED	−0.015	0.017	178	16,321	−0.012	0.018	175	15,787
Any urgent, acute or severe problem, IS	−0.032**	0.013	217	19,845	−0.037***	0.014	209	18,907
Any admission to ward, ED	−0.035***	0.012	187	17,166	−0.037***	0.013	174	15,668
Any ED PPP presentation	−0.030*	0.016	148	13,429	−0.027	0.018	148	13,146
Any inpatient PPP presentation	−0.026***	0.010	213	19,558	−0.029***	0.010	214	19,275

*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. Each donut has the same number of weekend days on each side of the threshold. In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, and (4) the total effective sample size. Each line by 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 *** denote effects significant at the 10%, 5% and 1% respectively.*

Table A.19: Sensitivity of Main Results to Observations Near the Threshold (Table 2/2)

Exclude births within:	**Panel C. 12 days**				**Panel D. 15 days**			
	Coef. (1)	Sd.err. (2)	Band. (3)	N (4)	Coef. (1)	Sd.err. (2)	Band. (3)	N (4)
Health Care Utilization Index [std.]	-0.083^{*}	0.045	172	15,185	-0.048	0.053	163	13,896
Aggregate hospital presentations, not in the index:								
Any presentation (ED or inpatient service)	-0.010	0.022	184	16,285	0.005	0.024	172	14,871
Any ED presentation	-0.015	0.022	161	14,116	0.002	0.022	174	14,962
Any inpatient presentation	-0.038^{**}	0.017	224	19,933	-0.041^{**}	0.018	223	19,493
Any PPP presentation	-0.023	0.020	161	14,011	-0.005	0.020	165	14,169
Index subcomponents:								
Any urgent, acute or severe problem, ED	-0.008	0.020	174	15,371	0.007	0.021	163	13,989
Any urgent, acute or severe problem, IS	-0.039^{***}	0.014	206	18,241	-0.036^{**}	0.015	209	18,186
Any admission to ward, ED	-0.036^{**}	0.014	172	15,185	-0.027^{*}	0.015	177	15,276
Any ED PPP presentation	-0.021	0.020	155	13,583	-0.005	0.020	159	13,604
Any inpatient PPP presentation	-0.030^{***}	0.011	213	18,978	-0.028^{**}	0.012	207	18,017

*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 C, results exclude all births within twelve days of July 1, 2004; 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. In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, and (4) the total effective sample size. Each line by 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 *** denote effects significant at the 10%, 5% and 1% respectively.*

Table A.20: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life: No 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.]	−0.082**	0.034	0.018	189	8,933	8,841	0.005
Aggregate hospital presentations, not in the index:							
Any presentation (ED or inpatient service)	−0.031	0.019	0.104	184	8,746	8,648	0.451
Any ED presentation	−0.025	0.016	0.107	162	7,620	7,605	0.312
Any inpatient presentation	−0.039**	0.016	0.014	222	10,470	10,446	0.312
Any PPP presentation	−0.022	0.015	0.133	174	8,237	8,243	0.218
Index subcomponents:							
Any urgent, acute or severe problem, ED	−0.023	0.015	0.123	180	8,554	8,441	0.207
Any urgent, acute or severe problem, IS	−0.033***	0.012	0.005	236	11,121	11,158	0.170
Any admission to ward, ED	−0.028**	0.011	0.015	195	9,189	9,090	0.130
Any ED PPP presentation	−0.028**	0.014	0.040	152	7,161	7,135	0.181
Any inpatient PPP presentation	−0.015	0.010	0.109	193	9,138	9,040	0.104

*Note: This table presents the effects of the Baby Bonus on health care utilization and its subcomponents (our primary outcomes) when including all births (no donut). Column (1) reports coefficient estimates, Col. (2) their standard errors, (3) the p-value of the two-sided t-test, Col. (4) the bandwidth half-length, Col. (5) and (6) the total effective sample size on the left and right of the threshold, and Col (7) the outcome mean right before the threshold. Each row corresponds to a separate regression using local polynomial estimation with robust bias-corrected inference methods, a 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 A.21: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life: Controlling for Predetermined Characteristics

	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.]	−0.092**	0.041	0.018	173	7,469	7,304	−0.049
Aggregate hospital presentations, not in the index:							
Any presentation (ED or inpatient service)	−0.012	0.022	0.577	166	7,156	6,952	0.413
Any ED presentation	−0.021	0.020	0.283	157	6,771	6,541	0.312
Any inpatient presentation	−0.036**	0.017	0.038	212	9,170	8,944	0.275
Any PPP presentation	−0.027	0.019	0.161	160	6,840	6,647	0.188
Index subcomponents:							
Any urgent, acute or severe problem, ED	−0.019	0.019	0.304	173	7,469	7,304	0.201
Any urgent, acute or severe problem, IS	−0.039***	0.014	0.005	213	9,225	8,980	0.153
Any admission to ward, ED	−0.038***	0.013	0.004	178	7,698	7,446	0.131
Any ED PPP presentation	−0.026	0.018	0.157	147	6,338	6,147	0.165
Any inpatient PPP presentation	−0.029***	0.010	0.005	217	9,395	9,156	0.080

*Note: This table presents our main results on our primary outcomes: the standardized index of health care utilization and its subcomponents, now including predetermined characteristics as control variables. Column (1) reports coefficient estimates, Col. (2) their standard errors, (3) the p-value of the two-sided t-test, Col. (4) the bandwidth half-length, Col. (5) and (6) the total effective sample size on the left and right of the threshold, and Col (7) the outcome mean right before the threshold. Each row 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 include the following predetermined characteristics as control variables (based on Birth records): a dummy for disadvantaged postcode at birth (mother's postcode is below median SEIFA), born to a low-skilled father, born to a single mother or to a mother who was never married. 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 A.22: Sensitivity of Main Results to Choice of Bandwidth (Table 1/2)

Bandwidth:	**Panel A. MSE-optimal Bandwidth**				**Panel B. Two-sided CER-optimal Bandwidth**				
	Coef. (1)	Sd.err. (2)	Band. (3)	N (4)	Coef. (1)	Sd.err. (2)	Band. (3)		N (5)
Health Care Utilization Index [std.]	−0.103***	0.037	262	24,178	−0.090***	0.035	221	300	24,085
Aggregate hospital presentations, not in the index:									
Any presentation (ED or IS)	−0.021	0.020	258	23,775	−0.025	0.018	227	314	25,070
Any ED presentation	−0.024	0.018	229	20,837	−0.012	0.016	221	336	25,763
Any inpatient presentation	−0.045***	0.015	330	30,892	−0.046***	0.016	176	361	24,962
Any PPP presentation	−0.033*	0.017	234	21,373	−0.024	0.016	207	260	21,504
Index subcomponents:									
Any urgent, acute or severe problem, ED	−0.022	0.016	258	23,775	−0.014	0.015	216	351	26,422
Any urgent, acute or severe problem, IS	−0.042***	0.012	318	29,842	−0.041***	0.012	172	609	37,001
Any admission to ward, ED	−0.037***	0.012	264	24,379	−0.036***	0.011	220	316	24,847
Any ED PPP presentation	−0.029*	0.016	218	19,763	−0.018	0.015	215	269	22,265
Any inpatient PPP presentation	−0.034***	0.009	331	30,992	−0.027***	0.009	198	317	23,830

*Note: This table presents the sensitivity of our main results to our alternative choices of data-driven optimal bandwidth (MSE-, or two-sided CER-optimal bandwidth). Panel A presents results under the MSE-optimal bandwidth and Panel B results under the two-sided CER-optimal bandwidth. In each Panel, Col. (1) reports the coefficient estimate, Col. (2) its standard error, Col. (3) the bandwidth half-length, and Col. (4) the total effective sample size. Each line by panel corresponds to a separate regression using local polynomial estimation with robust bias-corrected inference methods, standard errors clustered at the level of birth dates and the indicated choice of bandwidth. The sample always excludes 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 A.23: Sensitivity of Main Results to Choice of Bandwidth (Table 2/2)

Bandwidth:	**Panel C. 90 days**			**Panel D. 60 days**		
	Coef. (1)	Sd.err. (2)	N (3)	Coef. (1)	Sd.err. (2)	N (3)
Health Care Utilization Index [std.]	−0.212**	0.090	7,604	−0.290**	0.126	4,751
Aggregate hospital presentations, not in the index:						
Any presentation (ED or inpatient service)	−0.076	0.050	7,604	−0.133*	0.072	4,751
Any ED presentation	−0.096**	0.040	7,604	−0.143**	0.057	4,751
Any inpatient presentation	−0.007	0.041	7,604	−0.035	0.059	4,751
Any PPP presentation	−0.100**	0.041	7,604	−0.119**	0.056	4,751
Index subcomponents:						
Any urgent, acute or severe problem, ED	−0.077*	0.046	7,604	−0.135**	0.066	4,751
Any urgent, acute or severe problem, IS	−0.042	0.035	7,604	−0.050	0.053	4,751
Any admission to ward, ED	−0.071**	0.031	7,604	−0.100**	0.047	4,751
Any ED PPP presentation	−0.094**	0.039	7,604	−0.123**	0.053	4,751
Any inpatient PPP presentation	−0.022	0.026	7,604	−0.009	0.037	4,751

*Note: This table presents the sensitivity of our main results to fixed bandwidths of 90 and 60 days. Panel C presents results using a 90-day bandwidth and Panel D results using a 60-day bandwidth. In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, and (3) the total effective sample size. Each line by panel corresponds to a separate regression using local polynomial estimation with robust bias-corrected inference methods, standard errors clustered at the level of birth dates and the indicated choice of bandwidth. The sample always excludes 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 A.24: 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.]	-0.095	0.040	172	3	0.018	0.033
Aggregate hospital presentations, not in the index:						
Any presentation (ED or inpatient service)	−0.012	0.022	169	94	0.579	0.972
Any ED presentation	−0.026	0.020	149	3	0.200	0.033
Any inpatient presentation	−0.034	0.017	214	62	0.040	0.685
Any PPP presentation	−0.032	0.019	153	10	0.088	0.110
Index subcomponents:						
Any urgent, acute or severe problem, ED	−0.019	0.019	169	22	0.316	0.243
Any urgent, acute or severe problem, IS	−0.037	0.013	207	9	0.005	0.099
Any admission to ward, ED	−0.037	0.013	173	4	0.005	0.044
Any ED PPP presentation	−0.032	0.018	142	11	0.076	0.122
Any inpatient PPP presentation	−0.028	0.010	215	24	0.005	0.265

Note: This table presents the results of permutation tests following Ganong and Jäger (2018), performed on our primary outcomes. Each line corresponds to the result of a separate regression using our main specification at the true threshold (July 1, 2004). Column (1) reports coefficient estimates from our main specification, Col. (2) their standard errors, Col. (3) the bandwidth half-length, Col. (4) the rank of the estimated coefficient at the true threshold in the distribution of all estimates at 180 alternative thresholds, ranging from -90 to +90 around the true threshold, Col. (5) the asymptotic p-value at the true threshold and (6) the randomization-based p-value at the true threshold. Each row 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.

Table A.25: Permutation Tests at Placebo Thresholds in Prepolicy Years 2002 and 2003

	Panel A. Placebo Year 2002				Panel B. Placebo Year 2003			
	Coef. Est.	Sd.err.	Est. rank	Rand. *p*-val	Coef. Est.	Sd.err.	Est. rank	Rand. *p*-val
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Aggregate inpatient presentations, not in the index:								
Any inpatient presentation	0.029	0.019	175	0.077	0.002	0.019	104	0.862
Index subcomponents (inpatient services only):								
Any urgent, acute or severe problem, IS	0.012	0.014	179	0.033	0.013	0.024	134	0.530
Any inpatient PPP presentation	−0.018	0.011	29	0.320	0.035	0.017	178	0.044
Other items (not in Health Care Utilization Index):								
Any Inpatient admission to ward	−0.004	0.009	59	0.652	−0.001	0.007	69	0.762
Any Inpatient overnight admission	0.010	0.017	134	0.530	−0.015	0.021	77	0.851
Any planned visit	0.002	0.006	104	0.862	0.006	0.008	143	0.431
Any visit with med. referral	0.011	0.014	124	0.641	−0.010	0.018	67	0.740
Any booked elective procedure	0.018	0.012	155	0.298	−0.003	0.012	69	0.762

Note: This table presents the results of permutation tests following Ganong and Jäger (2018), performed on outcomes from inpatient services records in prepolicy years 2002 (Panel A) and 2003 (Panel B). In each panel, Column (1) reports the coefficient estimate at the true threshold (July 1), Col. (2) its standard error, Col. (3) the rank of the estimated coefficient in the distribution of all estimates at 180 alternative thresholds ranging from −90 to +90 days around the true threshold, and Col. (4) the randomization-based p-value at the true threshold. Each line by placebo year 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).

Table A.26: Robustness of Main Results to Multiple Hypothesis Testing

	p-Values		
	Original	Romano–Wolf	Holm
Health Care Utilization Subitems	(1)	(2)	(3)
Aggregate hospital presentations, not in the index:			
Any presentation (ED or inpatient service)	0.579	0.543	0.543
Any ED presentation	0.200	0.210	0.204
Any inpatient presentation	**0.040**	**0.036**	**0.036**
Any PPP presentation	*0.088*	*0.087*	0.104
Index subcomponents:			
Any urgent, acute or severe problem, ED	0.316	0.350	0.332
Any urgent, acute or severe problem, IS	**0.005**	**0.002**	**0.007**
Any admission to ward, ED	**0.005**	**0.002**	**0.009**
Any ED PPP presentation	*0.076*	*0.076*	0.115
Any inpatient PPP presentation	**0.005**	**0.002**	**0.008**

*Note: This table presents p-values of RD treatment effects of the Australian Baby Bonus on health care utilization (index subcomponents) of infants, under three 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), treating all subitems as belonging to the same family of treatment effects. 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 A.27: Comparison of Clustered and Heteroskedasticity-Robust Standard Errors

Standard Errors:	Panel A. Clustered					Panel B. Heteroskedasticity-robust				
	Coef. (1)	SE (2)	*p*-value (3)	BW (4)	N (5)	Coef. (6)	SE (7)	*p*-value (8)	BW (9)	N (10)
CER-optimal Bandwidth										
Health Care Utilization Index [std.]	−0.096**	0.040	0.018	172	15,500	−0.093*	0.048	0.055	133	11,725
Aggregate hospital presentations, not in the index:										
Any presentation (ED or inpatient service)	−0.012	0.022	0.579	169	15,184	0.000	0.025	0.997	132	11,725
Any ED presentation	−0.026	0.020	0.200	149	13,410	−0.028	0.025	0.256	114	10,034
Any inpatient presentation	−0.034**	0.017	0.040	214	19,365	−0.017	0.019	0.358	170	15,406
Any PPP presentation	−0.032*	0.019	0.088	153	13,670	−0.033	0.021	0.122	117	10,205
Index subcomponents:										
Any urgent, acute or severe problem, ED	−0.019	0.019	0.316	169	15,184	−0.023	0.019	0.232	134	11,932
Any urgent, acute or severe problem, IS	−0.037***	0.013	0.005	207	18,718	−0.031**	0.015	0.044	167	15,063
Any admission to ward, ED	−0.037***	0.013	0.005	173	15,572	−0.038**	0.016	0.018	137	12,137
Any ED PPP presentation	−0.032*	0.018	0.076	142	12,738	−0.037*	0.021	0.074	109	9,458
Any inpatient PPP presentation	−0.028***	0.010	0.005	215	19,443	−0.025**	0.011	0.029	174	15,663
MSE-optimal Bandwidth										
Health Care Utilization Index [std.]	−0.103***	0.037	0.005	262	24,178	−0.103***	0.040	0.010	247	22,725
Aggregate hospital presentations, not in the index:										
Any presentation (ED or inpatient service)	−0.021	0.020	0.299	258	23,775	−0.022	0.020	0.288	246	22,644
Any ED presentation	−0.024	0.018	0.193	229	20,837	−0.026	0.020	0.190	214	19,365
Any inpatient presentation	−0.045***	0.015	0.003	330	30,892	−0.044***	0.015	0.003	320	29,917
Any PPP presentation	−0.033*	0.017	0.053	234	21,373	−0.033*	0.017	0.053	218	19,879
Index subcomponents:										
Any urgent, acute or severe problem, ED	−0.022	0.016	0.186	258	23,775	−0.022	0.016	0.172	250	22,978
Any urgent, acute or severe problem, IS	−0.042***	0.012	0.000	318	29,842	−0.042***	0.012	0.001	314	29,440
Any admission to ward, ED	−0.037***	0.012	0.002	264	24,379	−0.037***	0.013	0.005	255	23,407
Any ED PPP presentation	−0.029*	0.016	0.060	218	19,763	−0.030*	0.016	0.060	203	18,316
Any inpatient PPP presentation	−0.034***	0.009	0.000	331	30,992	−0.034***	0.009	0.000	326	30,614
90-days Bandwidth										
Health Care Utilization Index [std.]	−0.212**	0.090	0.018	90	7,604	−0.212*	0.113	0.059	90	7,604
Aggregate hospital presentations, not in the index:										
Any presentation (ED or inpatient service)	−0.076	0.050	0.129	90	7,604	−0.076	0.055	0.166	90	7,604
Any ED presentation	−0.096**	0.040	0.016	90	7,604	−0.096*	0.051	0.060	90	7,604
Any inpatient presentation	−0.007	0.041	0.862	90	7,604	−0.007	0.052	0.891	90	7,604
Any PPP presentation	−0.100**	0.041	0.014	90	7,604	−0.100**	0.046	0.028	90	7,604
Index subcomponents:										
Any urgent, acute or severe problem, ED	−0.077*	0.046	0.092	90	7,604	−0.077*	0.045	0.085	90	7,604
Any urgent, acute or severe problem, IS	−0.042	0.035	0.230	90	7,604	−0.042	0.042	0.318	90	7,604
Any admission to ward, ED	−0.071**	0.031	0.020	90	7,604	−0.071*	0.037	0.057	90	7,604
Any ED PPP presentation	−0.094**	0.039	0.016	90	7,604	−0.094**	0.043	0.029	90	7,604
Any inpatient PPP presentation	−0.022	0.026	0.387	90	7,604	−0.022	0.032	0.490	90	7,604

*Note: This table presents the effects of the Australian Baby Bonus on hospital presentation of infants, comparing results using clustered standard errors at the level of date of birth (main specification, Panel A.) against heteroskedasticity-robust standard errors (Panel B). We compare results across CER- and MSE-optimal bandwidths and a 90 days bandwidth. In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the bandwidth half-length, (5) the total effective sample size. Each line by panel reports the results of a separate regression analysis using local linear estimation within the chosen bandwidth, with robust bias-corrected inference and corresponding standard errors. 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 A.28: 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.		Pre-threshold Mean
					Left	Right	
	(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	0.009	170	7,731	7,587	0.132
Injuries, trauma and poisoning	0.004	0.007	0.593	181	8,316	8,069	0.034
Infections	−0.012	0.013	0.360	133	5,903	5,822	0.084
Digestive problems	−0.011	0.007	0.123	156	7,072	6,898	0.034
Skin problems	0.001	0.006	0.854	164	7,491	7,290	0.016
Unspecified problems	−0.019*	0.012	0.099	160	7,228	7,077	0.087
Nervous system problems	0.001	0.001	0.205	170	7,731	7,587	0.000
Eye- and ear-related problems	−0.003	0.007	0.672	132	5,903	5,822	0.014
Emergency Department Triage Nurse:							
Respiratory problems	−0.040***	0.012	0.001	165	7,522	7,348	0.129
Injuries, trauma and poisoning	0.011*	0.006	0.057	163	7,438	7,252	0.015
Infections	−0.008	0.010	0.461	156	7,072	6,898	0.071
Digestive problems	−0.026	0.016	0.106	88	3,747	3,678	0.086
Skin problems	0.007	0.008	0.358	142	6,418	6,320	0.031
Unspecified problems	−0.011	0.015	0.462	151	6,828	6,671	0.086
Nervous system problems	−0.001	0.003	0.837	152	6,906	6,764	0.005
Eye- and ear-related problems	−0.009	0.009	0.294	131	5,844	5,762	0.030
Inpatient Services: ICD-10-AM Chapter							
Respiratory problems	−0.037***	0.008	0.000	267	12,362	12,371	0.090
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	0.028
Digestive problems	−0.005	0.005	0.290	218	9,973	9,790	0.017
Skin problems	0.004*	0.002	0.070	155	7,072	6,898	−0.001
Unspecified problems	−0.001	0.006	0.925	205	9,426	9,202	0.029
Nervous system problems	−0.001	0.001	0.549	264	12,146	12,133	0.001
Eye- and ear-related problems	0.003	0.003	0.400	169	7,731	7,587	0.003

*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. Column (1) reports coefficient estimates, Col. (2) their standard errors, (3) the p-value of the two-sided t-test, Col. (4) the bandwidth half-length, Col. (5) and (6) the total effective sample size on the left and right of the threshold, and Col (7) the outcome mean right before the threshold. Each row 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. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Table A.29: The Effects of the Australian Baby Bonus on Hospital Presentations at Ages 1–5: Primary Outcomes

Age group:	**Panel A. Ages 1-2**				**Panel B. Ages 3-5**			
	Coef. (1)	Std. Err. (2)	BW (3)	N (4)	Coef. (1)	Std. Err. (2)	BW (3)	N (4)
Health Care Utilization Index [std.]	−0.066	0.061	165	14,870	0.040	0.089	176	15,877
Aggregate hospital presentations, not in the index:								
Any presentation (ED or inpatient service)	−0.007	0.031	116	10,109	0.005	0.019	185	16,779
Any ED presentation	−0.020	0.033	106	9,180	−0.001	0.021	159	14,210
Any inpatient presentation	0.007	0.011	285	26,520	0.001	0.008	397	37,556
Any PPP presentation	−0.017	0.022	128	11,296	0.014	0.016	173	15,572
Index subcomponents:								
Any urgent, acute or severe problem, ED	−0.031	0.022	121	10,725	−0.001	0.015	156	14,047
Any urgent, acute or severe problem, IS	0.002	0.011	206	18,628	0.000	0.008	338	31,682
Any admission to ward, ED	−0.008	0.013	149	13,334	0.004	0.009	237	21,737
Any ED PPP presentation	−0.027	0.022	121	10,637	0.013	0.014	162	14,597
Any inpatient PPP presentation	−0.002	0.012	168	15,063	−0.005	0.007	320	30,016

*Note: This table presents RD treatment effects of the Baby Bonus on health care utilization (index and subcomponents) at ages 1–2 (Panel A) and 3–5 (Panel B). In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, and (4) the total effective sample size. Each line by 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 *** denote effects significant at the 10%, 5% and 1% respectively.*

Table A.30: The Effects of the Australian Baby Bonus on Planned and Elective Visits and Visits with Medical Referral at Ages 1–5: Secondary Outcomes

Age group:	**Panel A. Ages 1-2**				**Panel B. Ages 3-5**			
	Coef. (1)	Std. Err. (2)	BW (3)	N (4)	Coef. (1)	Std. Err. (2)	BW (3)	N (4)
Any admission to ward, IS	−0.001	0.001	395	37,282	0.001	0.001	434	41,126
Any overnight admission, IS	−0.001	0.005	282	26,269	−0.001	0.004	385	36,425
Any planned visits or presentations with medical referral (by hospital service):								
Emergency department:								
Any planned review visit	−0.006	0.005	121	10,725	0.007*	0.004	164	14,781
Any visit with medical referral	−0.004	0.008	140	12,546	0.003	0.007	192	17,372
Inpatient services:								
Any planned visit	0.017***	0.007	218	19,879	−0.001	0.004	415	39,229
Any visit with medical referral	0.013	0.008	250	22,978	0.003	0.005	393	37,192
Any booked elective intervention	0.016**	0.007	222	20,194	0.002	0.004	482	45,780

*Note: This table presents RD treatment effects of the Baby Bonus on planned and elective visits and visits with medical referrals: our secondary outcomes related to health care utilization of infants at ages 1–2 (Panel A) and 2–5 (Panel B). In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, and (4) the total effective sample size. Each line by 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 *** denote effects significant at the 10%, 5% and 1% respectively.*

Table A.31: The Effects of the Australian Baby Bonus on Diagnoses at Ages 1–5

Age group:	**Panel A. Ages 1-2**				**Panel B. Ages 3-5**			
	Coef. (1)	Std. Err. (2)	BW (3)	N (4)	Coef. (1)	Std. Err. (2)	BW (3)	N (4)
Potentially preventable pediatric presentations:								
Bronchiolitis	−0.024	0.050	140	12,546	0.020	0.052	145	12,917
Gastroenteritis	0.061	0.051	117	10,298	0.066	0.049	126	11,176
Laryngitis	0.013	0.049	159	14,210	0.059	0.041	155	13,970
Otitis media	−0.072**	0.037	155	13,874	−0.027	0.048	166	14,959
Respiratory infection	−0.111*	0.061	114	9,939	0.030	0.045	185	16,672
Presentations by ICD-10-AM diagnosis chapter and presenting problem:								
Respiratory	−0.044	0.044	175	15,877	0.035	0.045	173	15,572
Infection	−0.072	0.053	141	12,546	0.057	0.051	134	11,840
Digestive	0.061	0.045	161	14,398	−0.043	0.047	168	15,063
Unspecified	−0.008	0.041	181	16,385	0.082	0.059	128	11,363
Eyes and ears	−0.033	0.043	147	13,116	0.000	0.044	157	14,124
Skin	−0.017	0.045	172	15,500	−0.053	0.051	145	12,917
Injury/Trauma/Poisoning	−0.091*	0.052	134	11,840	0.004	0.036	172	15,572
Nervous system	−0.038	0.044	183	16,571	−0.037	0.063	176	15,977

*Note: This table presents RD treatment effects of the Baby Bonus on diagnoses for presentations at ages 1–2 (Panel A) and 2–5 (Panel B). In each Panel, Column (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, and (4) the total effective sample size. 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 A.32: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life by Socioeconomic Status

Variable	Father Occupation: High-Skilled Coef. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N (5)	Father Occupation: Low-Skilled Coef. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N (5)	Disadvantaged Postcode Coef. (1)	Sd.err. (2)	*p*-value (3)	1/2 Band. (4)	N (5)
Panel A. Primary outcomes															
Health Care Utilization Index [std.]	-0.046	0.066	0.488	164	4,691	-0.132**	0.052	0.012	174	8,352	-0.101*	0.057	0.078	169	6,732
Aggregate hospital presentations, not in the index:															
Any presentation (ED or inpatient service)	0.034	0.038	0.366	161	4,579	-0.054*	0.028	0.055	178	8,567	-0.033	0.028	0.243	178	7,097
Any ED presentation	0.022	0.041	0.599	141	4,042	-0.042*	0.025	0.092	174	8,352	-0.033	0.029	0.254	168	6,614
Any inpatient presentation	-0.012	0.024	0.626	264	7,725	-0.063**	0.025	0.011	188	9,045	-0.046**	0.023	0.046	263	10,585
Any PPP presentation	0.011	0.033	0.729	157	4,496	-0.049*	0.027	0.064	163	7,772	-0.048*	0.025	0.053	165	6,476
Index subcomponents:															
Any urgent, acute or severe problem, ED	-0.028	0.029	0.332	153	4,369	-0.022	0.022	0.334	186	8,979	-0.020	0.026	0.438	173	6,851
Any urgent, acute or severe problem, IS	-0.025	0.020	0.213	225	6,503	-0.054***	0.018	0.003	172	8,266	-0.051***	0.015	0.001	296	12,120
Any admission to ward, ED	-0.023	0.021	0.269	161	4,579	-0.050***	0.018	0.006	167	7,979	-0.038**	0.019	0.049	166	6,564
Any ED PPP presentation	0.010	0.033	0.757	150	4,287	-0.048**	0.023	0.042	152	7,257	-0.033	0.025	0.201	171	6,821
Any inpatient PPP presentation	-0.024	0.016	0.117	230	6,620	-0.028**	0.012	0.019	283	14,023	-0.035**	0.015	0.015	211	8,375
Panel B. Secondary outcomes															
Any admission to ward, IS	0.003	0.008	0.720	196	5,653	0.003	0.009	0.747	270	13,338	0.012	0.012	0.309	164	6,439
Any overnight stay, IS	0.015	0.026	0.569	189	5,449	-0.038*	0.021	0.071	209	10,068	-0.027	0.019	0.167	307	12,574
Any planned visits or presentations with medical referral:															
Emergency department:															
Any planned review visit	0.003	0.010	0.746	169	4,819	-0.010	0.007	0.142	152	7,257	-0.021***	0.008	0.009	130	5,009
Any visit with medical referral	0.008	0.015	0.604	170	4,869	-0.003	0.013	0.805	177	8,528	0.003	0.016	0.859	141	5,542
Inpatient services:															
Any planned visit	0.007	0.008	0.354	253	7,367	-0.005	0.007	0.468	207	9,977	-0.002	0.008	0.764	257	10,325
Any visit with medical referral	-0.009	0.014	0.487	285	8,418	-0.012	0.015	0.436	179	8,643	-0.017	0.015	0.256	234	9,428
Any visit for an elective intervention	0.003	0.011	0.754	268	7,827	0.004	0.012	0.739	205	9,888	0.012	0.014	0.388	207	8,238

*Note: This table presents RD treatment effects of the Baby Bonus by socioeconomic status at birth. Panel A reports effects on primary outcomes (the standardized health care utilization index and its subcomponents); Panel B reports effects on secondary outcomes (planned and elective visits and visits with medical referrals). Columns are grouped into three subgroups based on father's occupational skill (high- vs low-skilled) and maternal postcode (disadvantaged vs not). Within each subgroup, Col. (1) reports the coefficient estimate, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the bandwidth half-length, (5) the total effective sample size. Each line within each panel reports a separate regression using our main specification: local linear estimation with robust bias-corrected inference and CER-optimal bandwidths, with standard errors clustered at the level of birth dates. 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% levels respectively.*

Table A.33: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life by Sex

	Baby Sex							
	Boys				Girls			
Variable	Coef. (1)	Sd.err. (2)	1/2 Band. (3)	N (4)	Coef. (5)	Sd.err. (6)	1/2 Band. (7)	N (8)
Panel A. Primary outcomes								
Health Care Utilization Index [std.]	-0.118*	0.065	164	7,498	-0.062	0.054	176	7,779
Aggregate hospital presentations, not in the index:								
Any presentation (ED or inpatient service)	-0.018	0.030	172	7,942	-0.006	0.027	176	7,735
Any ED presentation	-0.023	0.032	144	6,592	-0.025	0.024	183	8,067
Any inpatient presentation	-0.052**	0.024	212	9,831	-0.018	0.022	244	10,918
Any PPP presentation	-0.032	0.026	156	7,163	-0.029	0.026	169	7,415
Index subcomponents:								
Any urgent, acute or severe problem, ED	-0.033	0.029	150	6,840	0.000	0.024	183	8,021
Any urgent, acute or severe problem, IS	-0.044**	0.019	213	9,882	-0.030	0.019	212	9,368
Any admission to ward, ED	-0.048**	0.021	167	7,639	-0.024	0.019	180	7,884
Any ED PPP presentation	-0.037	0.025	144	6,592	-0.023	0.024	169	7,476
Any inpatient PPP presentation	-0.032**	0.016	207	9,635	-0.026**	0.011	290	13,214
Panel B. Secondary outcomes								
Any admission to ward, IS	-0.005	0.008	278	13,125	0.016	0.014	149	6,570
Any overnight stay, IS	-0.040**	0.016	282	13,439	0.009	0.022	218	9,695
Any planned visits or presentations with medical referral:								
Emergency department:								
Any planned review visit	-0.014*	0.008	153	7,018	-0.007	0.007	171	7,558
Any visit with medical referral	-0.010	0.014	172	7,978	0.011	0.012	154	6,798
Inpatient services:								
Any planned visit	0.002	0.009	270	12,726	-0.001	0.004	304	13,819
Any visit with medical referral	-0.017	0.015	194	8,964	-0.009	0.013	254	11,417
Any visit for an elective intervention	-0.009	0.014	203	9,380	0.012	0.009	281	12,783

*Note: This table presents RD treatment effects of the Baby Bonus by sex at birth. Panel A reports effects on primary outcomes (the standardized health care utilization index and its subcomponents); Panel B reports effects on secondary outcomes (planned and elective visits and visits with medical referrals). Within each subgroup, Col. (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, (4) the total effective sample size. Each line within each panel reports a separate regression using our main specification: local linear estimation with robust bias-corrected inference and CER-optimal bandwidths, with standard errors clustered at the level of birth dates. 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% levels respectively.*

Table A.34: Effects of the Australian Baby Bonus on Health Care Utilization Within the First Year of Life by Birth Order

	Birth Order							
	Non-Firstborn				Firstborn			
Variable	Coef. (1)	Sd.err. (2)	1/2 Band. (3)	N (4)	Coef. (5)	Sd.err. (6)	1/2 Band. (7)	N (8)
Panel A. Primary outcomes								
Health Care Utilization Index [std.]	-0.136**	0.057	166	7,641	-0.043	0.054	163	7,090
Aggregate hospital presentations, not in the index:								
Any presentation (ED or inpatient service)	-0.036	0.027	167	7,732	0.008	0.033	178	7,867
Any ED presentation	-0.056**	0.027	148	6,846	0.008	0.026	177	7,835
Any inpatient presentation	-0.052***	0.018	325	15,657	-0.025	0.027	202	8,947
Any PPP presentation	-0.064**	0.025	160	7,369	0.000	0.024	149	6,528
Index subcomponents:								
Any urgent, acute or severe problem, ED	-0.039*	0.023	163	7,554	0.001	0.026	182	7,990
Any urgent, acute or severe problem, IS	-0.063***	0.014	316	15,188	-0.021	0.019	179	7,867
Any admission to ward, ED	-0.042**	0.020	167	7,686	-0.029*	0.017	170	7,451
Any ED PPP presentation	-0.069***	0.024	153	7,011	0.008	0.024	145	6,284
Any inpatient PPP presentation	-0.023	0.015	188	8,724	-0.030**	0.013	222	9,845
Panel B. Secondary outcomes								
Any admission to ward, IS	-0.006	0.009	195	8,960	0.005	0.008	314	14,327
Any overnight stay, IS	-0.026	0.017	275	13,093	-0.008	0.022	236	10,500
Any planned visits or presentations with medical referral:								
Emergency department:								
Any planned review visit	-0.019**	0.009	133	6,088	0.001	0.009	148	6,488
Any visit with medical referral	-0.001	0.015	158	7,261	0.002	0.013	179	7,867
Inpatient services:								
Any planned visit	0.000	0.008	199	9,242	0.015	0.010	145	6,284
Any visit with medical referral	-0.022*	0.013	217	10,060	-0.002	0.014	207	9,190
Any visit for an elective intervention	0.004	0.009	280	13,294	0.001	0.012	214	9,486

*Note: This table presents RD treatment effects of the Baby Bonus by birth order (firstborn vs. non-firstborn). Panel A reports effects on primary outcomes (the standardized health care utilization index and its subcomponents); Panel B reports effects on secondary outcomes (planned and elective visits and visits with medical referrals). Within each subgroup, Col. (1) reports the coefficient estimate, (2) its standard error, (3) the bandwidth half-length, (4) the total effective sample size. Each line within each panel reports a separate regression using our main specification: local linear estimation with robust bias-corrected inference and CER-optimal bandwidths, with standard errors clustered at the level of birth dates. 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% levels respectively.*

Table A.35: The Effects of the Australian Baby Bonus on Families (HILDA, 90-days bandwidth)

	Coef. Est.	Sd.err.	*p*-value	Pre-threshold Mean
Current Weekly Household Expenditures (in $)				
All groceries	74.438*	43.558	0.087	173.715
Food	98.651**	40.357	0.015	127.363
Meals/Take-out	−35.075	32.452	0.280	52.123
Financial Stress				
Currently able to raise an emergency $2,000	0.390**	0.177	0.028	0.842
Since January, went without meals	−0.152*	0.078	0.050	0.035
Since January, unable to heat home	−0.104*	0.057	0.067	0.012
Since January, could not pay elec./gas/telephone bills	−0.394*	0.204	0.054	0.211
Since January, could not pay mortgage/rent on time	−0.145	0.145	0.316	0.130
Since January, had to pawn or sell something	−0.160	0.140	0.251	0.123
Since January, asked for financial help from family/friends	−0.163	0.219	0.457	0.199
Since January, asked for financial help from welfare/community organizations	−0.275**	0.113	0.015	0.023
Marital stability	0.237**	0.111	0.034	0.955
Parental Self-Assessed Health Status				
Physical functioning	19.891**	9.025	0.028	91.082
Bodily pain	20.881*	12.406	0.092	80.741
Ability to perform job	17.507	14.247	0.219	87.130
General health	14.306	13.406	0.286	76.427
Vitality	1.083	14.572	0.941	58.363
Emotional health	26.450*	13.898	0.057	87.843
Social functioning	20.329*	11.697	0.082	85.892
Mental health	15.775	10.759	0.143	75.661
Maternal Labor Supply				
Currently unemployed	−0.034	0.028	0.223	0.010
Currently out of the labor force	−0.096	0.112	0.387	0.098
Current weekly wages	−126.182	416.837	0.762	533.372
Hours would like to work	1.477	11.448	0.897	36.144
Child Care Use				
Intended use*	−0.121	0.299	0.689	1.458
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	17.960	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. Column (1) report the coefficient estimate of the association between the predetermined characteristic and treatment assignment, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the pre-threshold mean of the outcome. Each line corresponds to a separate regression, using 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. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively. 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.*

Appendix B Data

B.1 Record Linkage

The South Australian Early Childhood Data Project links administrative records from more than 30 government data sources through probabilistic matching. The linkage procedure matches individuals across datasets using combinations of full name, date of birth, residential address, and Medicare number. For the birth-to-hospital linkage central to our analysis, linkage rates exceed 95 percent. Further details on the construction of the SA ECDP and the linkage methodology are provided in Nuske et al. (2016).

B.2 Health Care Utilization Index: Index Construction

We build an index of health care utilization from multiple variables to reduce noise in related presentation outcomes that capture a latent factor such as health care need.

We conducted an exploratory factor analysis on twelve leaf-level binary indicators of primary and secondary outcomes, all dummy variables with similar standard deviations. We deliberately excluded aggregate items (any hospital presentation, any ED presentation, any inpatient presentation, and any PPP presentation) because they already include the detailed items, which would mechanically inflate correlations and distort the latent factor structure. The twelve leaf items cover severe, acute and urgent presentations and ward admissions at the emergency department; severe, acute and urgent presentations, admissions and overnight stays at inpatient services; potentially preventable pediatric presentations at both services; planned and referred visits at both services; and elective visits at inpatient services.

This first exploratory analysis resulted in a strong first factor with eigenvalue 3.310 and a Cronbach alpha of 0.78, and a second factor with an eigenvalue of 1.70. The first factor alone explained only 65% of the variation while the two factors combined explained close to 99% of the variation. We proceeded to refine the item set in two steps. First, we found that three inpatient services items (planned visits, visits with medical referral, and elective visits) loaded clearly on the second factor rather than the first, with second-factor loadings around 0.6 to 0.7 compared to

first-factor loadings around 0.3 to 0.5. We excluded these three items. They appeared to capture a distinct latent factor associated with selection into additional investments in health for out-of-pocket services. We interpret these as "good hospital presentations" rather than presentations driven by severe and acute health problems. Second, we re-estimated the exploratory factor analysis on the remaining nine items. In this second iteration, a further four items had clearly lower loadings on the first factor (0.13 to 0.37): planned review visits and visits with referral at the emergency department, along with admissions to ward and overnight stays at inpatient services. We excluded these four items as well. The five remaining items loaded consistently and strongly on the first factor. These are: any ED presentation for a severe, acute or urgent problem; any ED presentation with admission to a ward; any inpatient services presentation for a severe, acute or urgent problem; any potentially preventable pediatric presentation at the ED; and any potentially preventable pediatric presentation at inpatient services. Conceptually, these five items capture hospital visits for severe problems immediately flagged as emergency by the hospital system, either by triage nurses or by medical staff through transfers from ED for further observation and treatment, along with potentially preventable pediatric hospitalizations.

After removing these items, we conducted a confirmatory factor analysis and found one strong factor with consistent factor loadings on that factor across the remaining items. The first factor had an eigenvalue of 2.701 and the next largest eigenvalue was 0.353; the first factor alone explained 100% of the variation, and the screeplot visually showed one clear factor. All factor loadings on the first factor have the same sign and range between 0.56 and 0.86. We then checked that the item-rest correlation between those items were consistent and high (around 0.52 to 0.78), and we obtained a Cronbach alpha of 0.85, which indicates a high-quality index.

Eventually, we constructed the index as a simple summative scale that sums the five retained items. All items are binary with similar means and standard deviations, so a summative scale is well-defined and interpretable as a count of severe, acute and preventable presentations in the first year of life. It requires no estimated weights, which keeps the index fully transparent and reproducible and avoids introducing additional researcher degrees of freedom. Table B.2 further shows that our main conclusions are robust to using a factor-weighted version of the index, with

Table B.1: Health Care Utilization Index: Exploratory and Confirmatory Factor Analysis, Inter-Item Correlations and Cronbach Alpha

	Exploratory Analysis:		Confirmatory Analysis:		
	First Factor Loadings (1)	Inter-item Corr. (2)	First Factor Loadings (3)	Inter-item Corr. (4)	Weights (5)
Severe, Acute or Urgent presentations:					
Emergency Department:					
Any severe, acute or urgent	0.624	0.514	0.677	0.629	0.179
Any with admission to ward	0.795	0.624	0.860	0.783	0.353
Inpatient Services:					
Any severe, acute or urgent	0.821	0.654	0.826	0.725	0.306
Any with admission to ward	0.284	0.270			
Any overnight stay	0.414	0.384			
Potentially Preventable Pediatric presentations:					
Any PPP presentation, ED	0.511	0.420	0.563	0.520	0.138
Any PPP presentation, inpatient services	0.665	0.520	0.711	0.636	0.162
Planned, elective presentations or with referral:					
Emergency Department:					
Any planned visit	0.133	0.127			
Any visit with referral	0.333	0.303			
Inpatient Services:					
Any planned visit	0.293	0.282			
Any visit with referral	0.516	0.492			
Any elective visit	0.424	0.412			
First Factor Eigenvalue	3.310		2.701		
Cronbach Alpha		0.776		0.848	

Note: This table presents factor loadings, inter-item correlations and Cronbach Alphas from exploratory and confirmatory factor analyses underlying our Health Care Utilization Index. The sample used in this table includes all babies born in South Australia in 1997 to 2003. The exploratory factor analysis is estimated on 12 binary indicators covering severe/acute presentations, potentially preventable presentations, and planned/elective/referred visits to both emergency departments and inpatient services. Based on the exploratory results, we retain the five items with the strongest factor loadings and inter-item correlations for the confirmatory analysis, which defines our composite index. Column (1) presents factor loadings from the exploratory analysis, Column (2) the Pearson inter-item correlations used in the exploratory analysis and Cronbach Alpha, Column (3) presents factor loadings from the confirmatory analysis, Column (4) the Pearson inter-item correlations used in the confirmatory analysis and Cronbach Alpha, and Column (5) presents the regression-based factor weight used to construct the composite index.

point estimates and significance levels close to the summative benchmark.

Table B.2: Effects on Health Care Utilization - Sensitivity to Index Construction Decisons

	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 (benchmark)	-0.212*	0.113	0.059	90	3,833	3,771	0.105
Health Care Utilization Factor (alternative)	-0.192*	0.110	0.081	90	3,833	3,771	0.107

*Note: This table presents RD treatment effects of the Baby Bonus on health care utilization in the first year of life, using two different measures of health care utilization. The first row corresponds to a summative scale of hospital presentations, presentations for severe, acute and urgent problems and potentially preventable pediatric presentations (our benchmark index). The second row corresponds to an alternative index based on a factor analysis, where each component is given a weight based on factor loadings (exact weights presented in Table B.1). Column (1) report coefficient estimates, (2) standard errors, (3) p-value of two-sided t-test, (4) bandwidth half-length, (5) and (6) sample sizes on each side of the threshold, and (7) the below-threshold mean of each outcome. Each row corresponds to a separate regression, using local polynomial estimation, 90-days bandwidths, robust bias-corrected inference methods and standard errors clustered at the level of birth dates. We exclude 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.*

B.3 Hospital Records: Emergency Departments and Inpatient Services

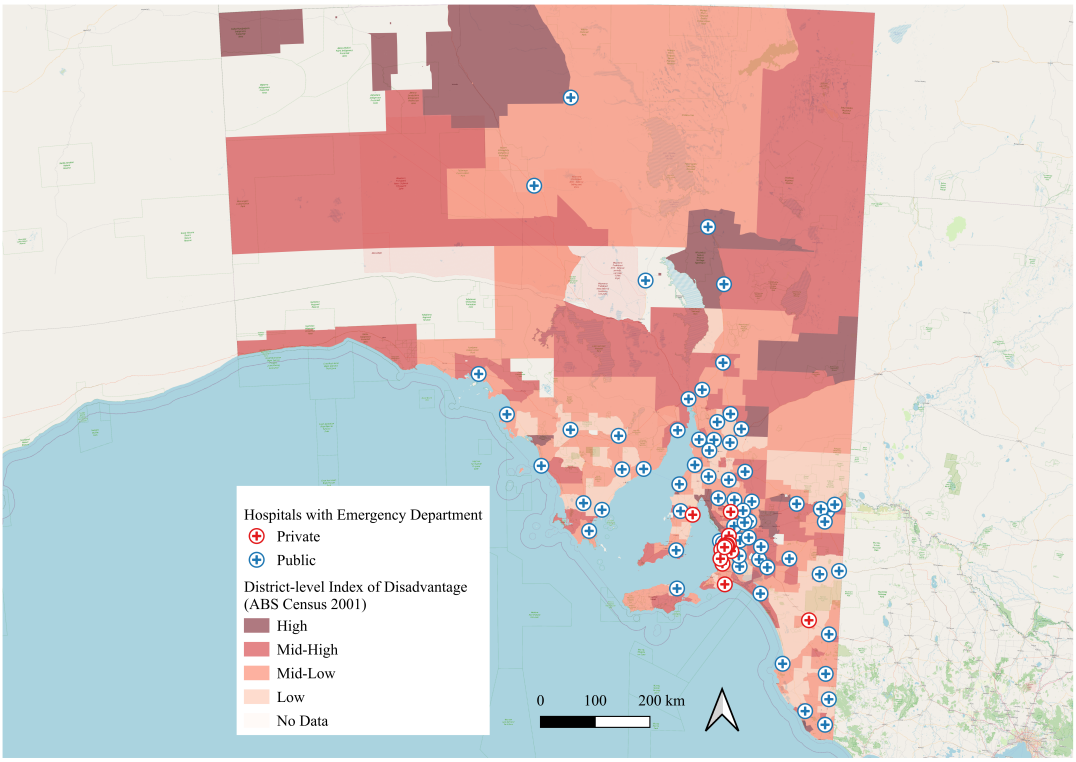
Data Quality and Coverage Using hospital records to assess the health and health care utilization of young children has both advantages and disadvantages. An ideal set of health measures would combine health status measures recorded by different health care providers (pediatricians, nurses, primary care, specialty care) and in different health settings (hospitals, ambulatory care; see National Research Council (2011)).

A first limitation 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 exploit the strength of our data which is to allow us to 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.

A second limitation 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.[36] Figure B.1 shows that all private hospitals are located in the Greater Adelaide region, mainly in economically advantaged areas (councils of Calvary Wakefield, Ashford, and St. Andrews) and in the vicinity of a public hospital. More 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).

[36]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.

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



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

B.4 Social Security (Centrelink) Records

We obtain Social Security records from the federal Department of Social Services (DSS), linked to the South Australian Early Childhood Data Project. Social security payments and services are delivered to Australian citizens and permanent residents by Centrelink.

Data preparation. To identify mothers of babies born around the Baby Bonus reform, we start from the "Family File" available in the South Australia Early Childhood Project. These files indicate parent-child relationships based on birth records. From this file, we only keep mothers and we reshape the file to have children in wide by increasing birth order. We flag all babies born within 2 years around the time of the baby bonus reform.

Next we focus on the complete Centrelink records of mothers. In those records, we observe one row per mother per Centrelink spell. For payment that are paid repeatedly over spells (e.g.

weekly, bi-weekly basis) the transaction is presented as a daily rate, dividing the total paid by days in the spell. For lump sum payments like the Baby Bonus, the spell is one day and the daily rate represents the lump-sum transaction. The dataset is extremely large because we have many observations per person. We reduce its size by focusing on the main payments that affect families and workers: maternity related (to check first stage effects), parenting payments (Single for single parents and Partnered for two-parent households), unemployment insurance (NewStart Allowance, Youth Allowance), and disability-related payments (Disability Support Pension, Carer Allowance and Carer Payment).

For each mother and type of payment (maternity-related, parenting-related, unemployment insurance, and disability-related), we construct three variables relative to the birth date of their baby born around the timing of the baby bonus reform: (i) whether she received any amount within one year post birth, (ii) how long (in days) after the baby's birth until the next payment of this type occurred within a one year limit, and (iii) the amount received within one year post birth.

Estimation. Because those records are linked to the Early Childhood Data Project, we observe exact babies dates of births. This allows us to estimate RD models following our main specification. For first-stage regressions, we analyze differences in outcomes between mothers with a baby born within -/+182 days around July 1, 2004, where outcomes are receiving any maternity-related payment, the number of days after birth until they receive the payment and the amount they received in dollars. For regressions on all other Centrelink payments, we compare differences between mothers with a baby born within -/+90 days of July 1, 2004, in the number of days post birth until they receive the payment and the amount received.

B.5 Australian Tax Authorities (ATO) Records

We use national tax records linked to national Medicare records through PLIDA[37]. PLIDA is a data asset of The Australian Bureau of Statistics that combines information from multiple federal

[37]We thank Deborah Cobb-Clark, Sarah Dahman, Hailey Fisher and Nicolás Salamanca for generously helping us produce those results with Daniel Fisher's outstanding research assistance

agencies (health, education, social security payments, taxation), and population demographics from Census. Importantly, however, PLIDA only provides dates of birth up to the month and year of birth.

Data Preparation. To identify our sample of interest, we start from the Medicare "relationship file", a file that reports parent-child, guardian-child and spousal relationships as per Medicare and Social Security records. We identify women in a parent-child relationship, and then merge in those children's month and year of birth and gender from the Medicare demographics files (exact date of birth are not available in those data for confidentiality reasons). To ensure that we focus on birthing mothers from those parent-child relationships, we then use the "special benefits files" from the social security registry which contain all mothers who ever received any maternity-related payment between 2000 and 2005, and we identify maternity payment transaction dates and amounts. We keep all mother-child pairs where a child is born between April and September each birth year between 2000 and 2004.

For our analysis, we are interested in birthing events, irrespective of the number of children born. To do that, we reshape the data to have children in wide, and we flag the child born closest prior to a maternity payment. When required by multiple birth events, we aggregate all maternity payment transactions related to a specific birth event into a single payment row per focal child (setting the start and end dates to be the date of the first transaction). We obtain a dataset of repeated cross-sections of birth events with roughly 20,000 births per month in this period.

Finally, we create a dataset of earned income for those mothers from their tax records in the year of their birthing events within the financial years 2000-2001 to 2004-2005. We create an earned income variable summing all earned income from wages and businesses, and a binary labor force participation variable, taking value 1 if there was any earned income in the financial year and 0 otherwise. We merge then merge earned income and labor supply variables to repeated cross-sectional dataset of birth events.

Estimation. Our data do not contain exact dates of birth, rendering the estimation of RD models impossible in those data. Instead, we estimate Difference-in-Differences models in which we compare the differences in earnings in FY2004-2005 for mothers with a baby born in

July to September in FY2004-2005 v. April to June in FY2003-2004 (birth year 2004), against mothers with a baby born in July to September v. April to June in previous years. We also estimate alternative models: 1) excluding the birth year 2003 due to concern that the Working Credit Program introduced on September 20, 2003 (see Wilkins and Leigh, 2012) may confound effects in 2003; 2) restricting the sample to births in May and August due to birth shifting concerns in the seven days around the threshold; and 3) combining both restrictions. Results are highly stable across specifications. We expect our Difference-in-Differences estimates to be very close to estimates from RD models given that we focus on babies born within at most 3 months on either side of the threshold after excluding June-July births. For a mother i with outcome Y_it in FYt-$t+1$, we estimate the following equation:

$$Y_it = \alpha + \beta_0 D_i + \beta_1 \mathbb{I}\{R_i > 0\} + \beta_2 D_i \times \mathbb{I}\{R_i > 0\} + \varepsilon_i, \tag{B.1}$$

where Y_it can be earned income, labor supply or maternity-related payments in financial year starting in t; D_i is a dummy variable taking value 1 if t is 2004 (financial year 2004-2005) and 0 if t is before 2004, i.e. a post-reform dummy; $\nVdash\{R_i > 0\}$ is a dummy taking value 1 if the mother had a child born after 1 July in any year. β_2 is the parameter of interest capturing the interacted effect of having a baby eligible to the Baby Bonus (born after 1 July) and in the post-reform period. We use heteroskedasticity robust standard errors.

Table B.3 provides reassuring first stage estimates from Difference-in-Differences models using national data. The table shows that maternity-related payments increase in FY2004-2005 by roughly AUD$2,200, matching closely our first stage estimates estimated using regression discontinuity models in Centrelink records linked to the South Australian Early Childhood Data Project (see Section 4).

Table B.3: National Effects of the Australian Baby Bonus on Maternity Payments

	All	Excl. 2003	May & Aug. births only	Excl. 2003, May & Aug. only
	(1)	(2)	(3)	(4)
Maternity payment (in AUD$)	2,189.34***	2,189.50***	2,191.33***	2,191.88***
	(2.28)	(2.33)	(3.77)	(3.85)
R^2	0.95	0.95	0.95	0.95
Outcome Mean	1,155.98	1,314.97	1,177.73	1,346.45
Observations	243,113	162,036	86,644	57,930

*Note: This table presents the effects of the introduction of the Australian Baby Bonus on the dollar amount received by mothers in maternity payments, using Australia-wide Centrelink records (PLIDA). We estimate difference-in-differences models comparing maternity payments in Financial year 2004-2005 for mothers giving birth in July-September 2004 v. April to June 2004, against mothers giving births in the same months in previous years (2001 to 2003). Each column presents the regression results, reporting only the coefficient of interest. Col. (1) presents results including all years and months of birth, Col. (2) excludes 2003 among control years, Col. (3) focuses on August and May births, and Col. (4) excludes 2003 and focuses on August and May births. Heteroskedasticity-robust standard errors (HC3) are shown in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.*

B.6 The HILDA Household Survey

The Household, Income, and Labor Dynamics in Australia (HILDA) survey is the leading nationally representative household survey in Australia, akin to the Panel Study of Income Dynamics (PSID) in the United States. HILDA has followed 7,682 households and over 13,000 individuals yearly since 2001.[38]

Data preparation We use the restricted version of HILDA (release 23) 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

[38] 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). We thank Tanya Gupta for outstanding research assistance.

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 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%).

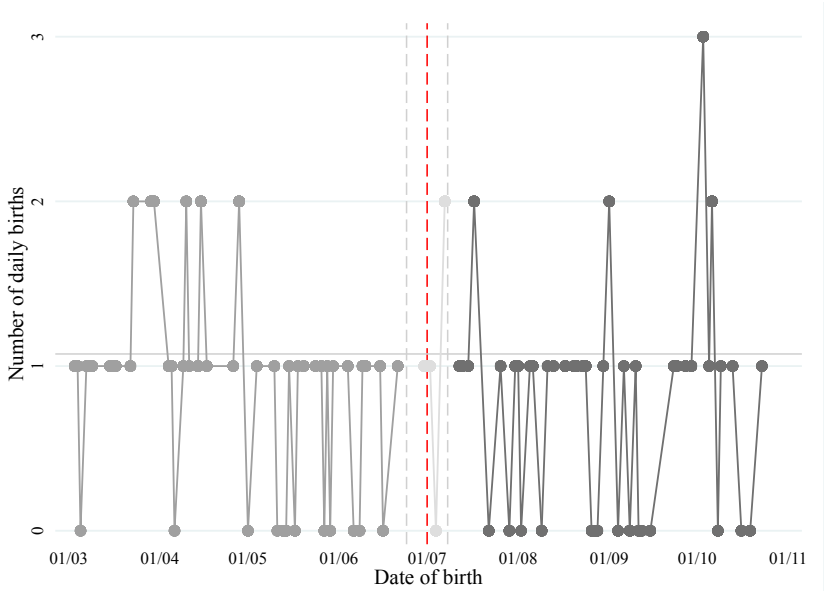
Estimation We estimate on this sample our preferred nonparametric regression discontinuity models with a seven-day donut. We cluster standard errors at the level of the parent, although findings are stable to clustering at the household level. We consider a bandwidth of 90 days around the threshold.

Table B.4 describes all HILDA outcome variables used in our mechanisms analysis and reports their sample means and standard deviations.

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 58 unique babies born within 90 days of July 1, 2004. Figure B.2 presents the distribution of daily births within 90 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.

Internal Validity Checks Table B.5 shows that there is no evidence of manipulation in the running variable based on nonparametric density tests; Table B.6 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.

Figure B.2: 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 B.4: HILDA Outcome Variables: Definitions and Descriptive Statistics

Variable	Description / HILDA Source	Mean	SD
Current Weekly Household Expenditures ($)			
All groceries	Current household weekly expenditure on all groceries (imputed)	180.2	90.9
Food	Current household weekly expenditure on food (imputed)	134.1	79.5
Meals/Take-out	Current household weekly expenditure on meals eaten out (imputed)	44.4	49.7
Financial Stress Since January (binary)			
Able to raise emergency $2,000	SCQ C3a: Could you raise $2,000 in a week in an emergency? Recoded: 1 = Yes (could raise), 0 = No	0.816	0.388
Went without meals	SCQ C2d: Since January, went without meals	0.041	0.199
Unable to heat home	SCQ C2e: Since January, was unable to heat home	0.027	0.163
Could not pay bills	SCQ C2a: Since January, could not pay electricity/gas/telephone bills on time	0.215	0.412
Could not pay mortgage/rent	SCQ C2b: Since January, could not pay the mortgage or rent on time	0.107	0.309
Had to pawn or sell something	SCQ C2c: Since January, pawned or sold something	0.113	0.317
Asked friends/family for help	SCQ C2f: Since January, asked for financial help from friends or family	0.215	0.412
Asked welfare org. for help	SCQ C2g: Since January, asked for help from welfare/community organizations	0.065	0.247
Marital stability	Recoded from current marital status: 1 = Married or de facto, 0 = Single/separated/divorced/widowed	0.949	0.220
Parental Self-Assessed Health Status (SF-36, 0–100)			
Physical functioning	SF-36 subscale: physical functioning (transformed, 0–100)	89.1	19.4
Role-physical	SF-36 subscale: role limitations due to physical health (0–100)	82.3	34.1
Bodily pain	SF-36 subscale: bodily pain (0–100)	77.6	23.4
General health	SF-36 subscale: general health perceptions (0–100)	74.0	20.1
Vitality	SF-36 subscale: vitality/energy (0–100)	57.3	20.6
Role-emotional	SF-36 subscale: role limitations due to emotional problems (0–100)	86.1	31.4
Social functioning	SF-36 subscale: social functioning (0–100)	83.6	23.4
Mental health	SF-36 subscale: mental health (0–100)	75.2	17.2
Maternal Labor Supply			
Currently unemployed	Derived from employment status (ESDTL): 1 = Unemployed (looking for FT or PT work), 0 = otherwise	0.009	0.095
Currently out of the labor force	Derived from ESDTL: 1 = Not in the labor force (marginally attached or not), 0 = otherwise	0.103	0.304
Current weekly wages ($)	Current weekly gross wages and salary, all jobs (imputed, weighted topcode)	537.1	714.0
Hours would like to work	Preferred total weekly hours of work	34.2	14.1
Child Care Use			
Intended use	In last 12 months, have you used/thought about using child care so you can work? (1 = Yes, 2 = No)	1.512	0.501
Weekly hours, school-aged (term)	Total hours per week of child care during term, all school-aged children; aggregated across all care types	9.4	10.9
Weekly hours, school-aged (holidays)	Total hours per week of child care during holidays, all school-aged children	28.2	23.4
Weekly hours, not-yet-at-school	Total hours per week of child care, all not-yet-at-school children; aggregated across all care types	21.4	14.7

Note: This table describes the outcome variables from the Household, Income, and Labor Dynamics in Australia (HILDA) survey used in our mechanisms analysis. "Mean" and "SD" report the sample mean and standard deviation from the 90-day bandwidth estimation sample (respondent parents of babies born within 90 days of July 1, 2004, excluding births within seven days of the threshold). Financial stress indicators and marital stability are binary. Self-assessed health status variables are SF-36 subscales transformed into 0–100 scales by the HILDA team, where higher values indicate better health. The Bodily Pain subscale is reverse-coded so that higher values indicate less bodily pain. Expenditure and wage variables are in current Australian dollars. "Intended use" is coded 1 (Yes) or 2 (No). Child care hours are weekly totals aggregated across all care types.

Table B.5: HILDA - Results of Local Polynomial Density Test

	Est. Bandwidth		N. Unique Babies		Density Test
Estimation Method	Left	Right	Left	Right	*p*-val.
(1)	(2)	(3)	(4)	(5)	(6)
Excluding 7-day donut:					
Restricted, linear	1,210	1,210	655	156	0.439
Unrestricted, linear	167	167	67	52	0.317
Including all births:					
Restricted, linear	1,055	1,055	527	156	0.471
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 B.6: Balancing Tests on Predetermined Characteristics (HILDA)

	Coef. Est.	Sd.err.	*p*-value	Pre-threshold Mean
Respondent age at interview (in days)	−601.621	1137.000	0.597	12135.927
Respondent sex	−0.062	0.318	0.844	0.532
# children aged 0-4 in the household	−0.380	0.308	0.218	1.455
# children aged 5-9 in the household	−0.215	0.366	0.556	0.305
# children aged 10-14 in the household	0.073	0.179	0.683	0.103
# children aged 15-24 in the household	−0.068	0.046	0.135	0.026
Baby age at interview (in days)	66.211***	21.359	0.002	326.233

*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. Column (1) report the coefficient estimate of the association between the predetermined characteristic and treatment assignment, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the pre-threshold mean of the outcome. Each line corresponds to a separate regression, using 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. *, **, and *** denote effects significant at the 10%, 5% and 1% respectively.*

Appendix C The Baby Bonus and Birth Shifting

The Australian government announced the introduction of the Baby Bonus on May 12, 2004, only seven weeks prior to July 1, 2004, which allowed a small number of families to postpone the birth of their baby.

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, which 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.

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.

C.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 Australian 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 (0.004% of the 254,200 children born in Australia in 2004)[39], 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.[40]

[39] 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).

[40] 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

Figure A.3a 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.

Figure A.3a 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 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.

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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".

C.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 for births between 1991 and 2005 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):

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

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

where the dependent variables are respectively the number of daily births in equation (C.1) and the log number of daily births in equation (C.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 C.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 C.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%.

Our replication exercise in our sample confirms 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. 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).

Following Borra, González and Sevilla (2019), we also explore whether birth shifting was

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

Window (W)	+/– 7 days (1)	+/– 14 days (2)	+/– 21 days (3)	+/– 28 days (4)
Panel A. Number of daily births				
Baby Bonus	-14.10^{***}	-8.63^{***}	-7.27^{***}	-6.44^{***}
	(3.86)	(3.14)	(2.34)	(2.00)
R^2	0.76	0.67	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^{***}	-0.16^{**}	-0.14^{***}	-0.13^{***}
	(0.08)	(0.07)	(0.05)	(0.04)
R^2	0.77	0.68	0.64	0.63
Share shifted births	13.9	7.8	7.2	6.7
Clusters	225	435	645	855
N Obs.	11,467	21,790	32,227	42,784

*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.*

driven by private hospitals and mothers with private health insurance. Our findings in Table C.2 indicate that birth shifting was only marginally more prevalent in private hospitals and for mothers treated as privately insured patient.

Last, Table C.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

Table C.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	-14.04^{***}	-13.99^{***}	-8.37^{***}	-9.34^{***}	-7.04^{***}	-8.02^{***}	-6.23^{***}	-7.09^{***}
	(3.92)	(3.84)	(3.17)	(3.13)	(2.34)	(2.37)	(2.00)	(2.03)
R^2	0.76	0.77	0.67	0.68	0.63	0.63	0.61	0.62
N. Obs.	8,451	2,783	16,142	5,194	23,873	7,700	31,674	10,243
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	-14.17^{***}	-14.24^{***}	-8.54^{***}	-8.99^{***}	-7.25^{***}	-7.45^{***}	-6.41^{***}	-6.61^{***}
	(3.97)	(3.72)	(3.19)	(3.14)	(2.35)	(2.38)	(2.00)	(2.07)
R^2	0.76	0.76	0.67	0.68	0.63	0.63	0.61	0.61
N. Obs.	7,487	3,937	14,298	7,397	21,223	10,872	28,139	14,470
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 × 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.*

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.

Table C.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	0.480
Birth in private hospital	0.010	0.012	0.395	418	20,084	20,061	0.333
No. of antenatal visits	−0.051	0.083	0.538	348	15,235	15,209	10.747
Mother smokes	0.003	0.008	0.663	620	28,971	29,660	0.188
Mother's age:							
35+	−0.003	0.008	0.731	592	28,164	28,673	0.183
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	0.336
Low skilled	0.015	0.010	0.145	670	30,428	30,851	0.557
Mother's marital status:							
Never Married	0.008	0.007	0.234	531	25,422	25,670	0.110
Married	−0.004	0.008	0.618	451	21,684	21,775	0.874
Single	−0.003	0.003	0.204	447	21,514	21,611	0.014
Mother's race:							
Caucasian	0.000	0.006	0.983	504	24,181	24,432	0.910
Asian	0.003	0.004	0.471	571	27,186	27,548	0.047
Aboriginal or TSI	−0.004	0.004	0.418	473	22,730	22,935	0.044
Child birth outcomes:							
Baby weight	32.066	15.364	**0.037**	383	18,422	18,385	3,350.940
Special Nursery	−0.005	0.010	0.599	413	19,797	19,828	0.174
NICU	−0.001	0.003	0.712	746	35,545	36,348	0.029
PICU	0.000	0.001	0.761	469	22,489	22,698	0.002
Neonatal death	−0.001	0.002	0.480	304	14,558	14,557	0.004
Apgar 1 min >7	−0.007	0.010	0.462	510	24,370	24,567	0.755
Apgar 5 min >7	0.000	0.004	0.910	468	22,399	22,594	0.969
Gestational age	0.114	0.066	*0.087*	296	14,110	14,155	38.737
Preterm birth	−0.006	0.007	0.375	730	34,746	35,447	0.151
Obstetric complication	−0.026	0.015	*0.094*	272	12,805	12,943	0.345
C-section birth	0.007	0.012	0.552	684	32,592	33,206	0.318

Note: This table presents the results of balancing tests on pretreatment characteristics of children and their parents and child birth outcomes based on birth and perinatal records, when we do not exclude births within seven days of July 1, 2004. Column (1) report the coefficient estimate of the association between the predetermined characteristic and treatment assignment, (2) its standard error, (3) the p-value of the two-sided t-test, (4) the bandwidth half-length, (5) and (6) the sample sizes on each side of the threshold, and (7) the pre-threshold mean of the outcome. 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.*

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

C.3 Birth Shifting and 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 approach 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. Our results are robust to excluding all births within 5, 8, 12, and 15 days from the threshold (Tables A.18 and A.19).

Data-Driven Approach to Choice of Donut. 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 C.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[41]. Thus, we conclude that the minimum donut required for our estimations is five days around the threshold.

[41] Binomial tests confirm the findings of Table C.4.

Table C.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.*

C.4 The Impact of Birth Shifting for our Findings: Results without Donut

Compared to our preferred estimated treatment effects (Table 1), ignoring the donut in Table A.20 causes only a very small bias in estimated coefficients, that would not have altered our conclusions overall.

This bias arises because of selection into treatment on observable and unobservable characteristics that are correlated with health outcomes. Ex-ante, 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[42]. 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.

Ignoring the donut leads to slightly larger estimated effects in magnitude, and slightly smaller for PPP presentations, indicating 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. The sign of this bias suggests 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.

[42] 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).