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HOW FINANCIAL INCENTIVES SHAPE FERTILITY IN AUSTRALIA

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Summary

Australia, like many developed countries, is experiencing a significant decline in its total fertility rate, with the current rate being around 1.5 births per woman, well below the 2.1 replacement rate required for population stability. This demographic shift has important implications for economic growth, labour markets, and the sustainability of social welfare systems, particularly pensions and healthcare.

A declining fertility rate is not a new issue, and in the past, Australia has turned to pronatalist policies to counter it. These policies included the First Child Tax Refund in 2002 and the more expansive Baby Bonus introduced in 2004. Both policies aimed to reduce the financial cost of childbearing, with the Baby Bonus designed to encourage higher fertility. In this note, we examine the effects of these financial incentives on fertility outcomes using comprehensive administrative data. We focus primarily on the lump-sum Baby Bonus and its later expansions, while examining the earlier First Child Tax Refund for comparison. We assess not only whether these financial incentives increased birth rates, but also how their impacts varied across demographic groups, geographic regions, and birth orders. More importantly, we distinguish between whether these policies merely brought forward births that families were already planning or whether they genuinely increased the total number of children families ultimately have, a key question for the long-term effectiveness of demographic policy.

Our results show:

- The **Baby Bonus** increased monthly births by 6.5%, around 1,300 additional births per month.
- By maternal income: births increased by 10% among **mothers with no taxable income**, by 8% among **below-median income** mothers, while **high-income** mothers showed no response.
- By maternal education: **mothers with less than Year 12 education** had 7.7% more births, while **university-educated** mothers showed only a modest response.
- By birth order: first and second births increased by around 6–8%, with the strongest response at **third births** by around 9% and similarly large effects at **fourth and higher-order births**.
- By maternal age: births rose by 7–10% among **mothers aged 20–34**, by over 13% among **those aged 35–39**, and by around 6% among **those aged 40–44**.
- The **Baby Bonus did not affect birth spacing**, indicating births were not simply brought forward.
- Mothers who received the **Baby Bonus**, as a group, had 6.8% more children in total by 2022, showing it increased **completed fertility**.
- **Fiscal cost per additional birth:** approximately **$50,000** per additional birth in 2004 dollars (around **$86,000** in 2025 dollars). This cost reflects transfers to families, consistent with the policy's aim of both supporting parents and encouraging births.
- In comparison, the earlier **First Child Tax Refund** raised first births by 2.3% but did not affect overall fertility.

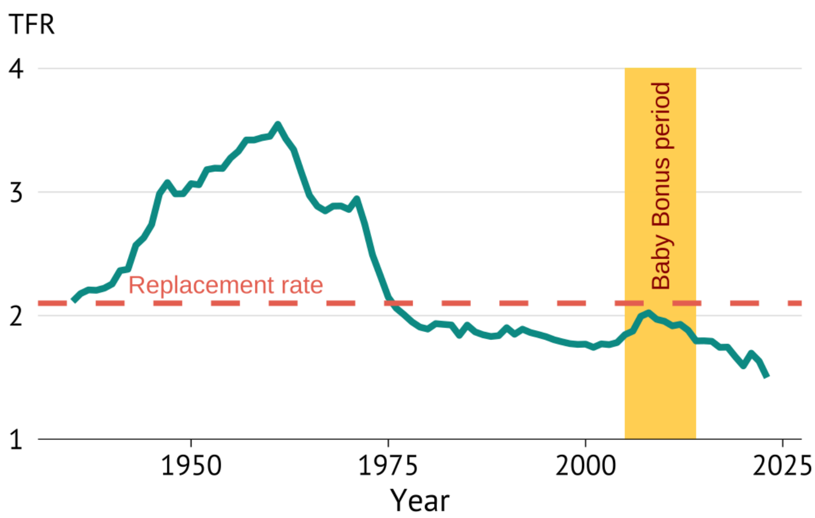
These findings demonstrate that well-designed financial incentives can achieve a genuine increase in birth rate. However, the effect depends on the design and targeting of the policy.

Australia's Demographic Challenge

Australia is facing a demographic challenge. In 2023, the total fertility rate fell to 1.5 births per woman, the lowest level ever recorded and well below the 2.1 replacement rate needed to maintain population levels without immigration (Australian Bureau of Statistics, 2023a) (Figure 1). This decline mirrors trends across developed nations, from South Korea's 0.72 to Italy's 1.20 fertility rate, creating what some demographers call a "global fertility collapse" (World Bank, 2025).

The consequences extend far beyond population statistics. Declining fertility rates can pose long-term threats to the sustainability of pension systems, reduce economic dynamism as the workforce eventually shrinks, and place increasing pressure on healthcare systems in an ageing society. In the near term, higher fertility can add to fiscal pressure by raising the youth dependency ratio, as more children require education, healthcare, and family support before entering the workforce. Looking ahead, if current trends continue, between 2022–23 and 2062–63, the old-age dependency ratio – the number of people

Figure 1: Australia's Total Fertility Trend



Source: ABS, e61

aged 65 and over for every 100 of working age (15 to 64) − is expected to increase from 26.6 per cent to 38.2 per cent, a shift that is expected to weigh heavily on GDP per capita growth (Treasury [Commonwealth of Australia], 2023).

These challenges raise an important question: can policy play a role in reversing or slowing fertility decline? Understanding the effectiveness of earlier policy responses is essential for shaping future strategies. In Australia, two major pronatalist policies stand out: the First Child Tax Refund (FCTR), introduced in 2002, and the Baby Bonus, which replaced it in 2004 and was later expanded. Both policies aimed to reduce the financial cost of childbearing, but they differed markedly in design and implementation.

First Child Tax Refund

The First Child Tax Refund (FCTR) was introduced by the federal government in the 2002–03 Budget and applied retroactively to first births after July 2001. It aimed to offset the costs of childbearing by refunding income taxes paid by mothers before giving birth. However, because payments were spread over several years and tied to both prior tax paid and post-birth earnings, and available only for first births, its immediate financial impact and visibility were limited.[1]

In July 2004, the Baby Bonus replaced the FCTR. Unlike the complex, tax-linked rebate for first births paid out over up to five years, the Baby Bonus provided a universal lump-sum cash payment at birth. Its simplicity, immediacy, and broader coverage marked a major shift in approach. Figure 2 summarises the key design differences.

Figure 2: Comparison of Two Policies

First Child Tax Refund 2002–2004
- Complex, tax-linked formula
- $0–2,500 *per year* (max 5 yrs)
- First birth only; must have paid tax
- Paid over multiple years

Baby Bonus (BBY) 2004–2014
- Simple lump sum ($3k ⇒ $5k)
- Paid immediately at birth
- All births eligible
- No income test; highly salient

The Baby Bonus Policy

The Baby Bonus, introduced in the May 2004 Budget, provided a universal $3,000 lump-sum payment for each birth, roughly equivalent to six weeks of median household income (see Table 1 for a detailed timeline). The payment was later increased

1 See Appendix A.3 for more details about FCTR.

to $4,000 in 2006 and $5,000 in 2008. Mean testing was introduced in 2009,[2] and the policy was eventually replaced by Paid Parental Leave in 2014.

Table 1: Key Dates and Changes in Australia's Pronatalist Policies

Announcement Date	Implementation Date	Policy and Details
May 14, 2002 (Budget)	July 2001 (retroactive)	Introduction of the *First Child Tax Refund*, providing an income tax rebate for families following the birth or adoption of their first child.
May 11, 2004 (Budget)	July 2004	Introduction of the *Baby Bonus* as a lump sum payment of **$3,000** to new parents.
May 9, 2006 (Budget)	July 2006	Increase in the Baby Bonus to **$4,000**.
May 8, 2007 (Budget)	July 2007	Increase to **$4,133** and indexing to inflation.
May 13, 2008 (Budget)	July 2008	Increase to **$5,000**.
May 13, 2008 (Budget)	January 2009	Introduction of a **means test** for the Baby Bonus.
October 2012 (MYEFO)	March 2013	Reduction to **$3,000** for second or subsequent births.
May 13, 2013 (Budget)	July 2014	Abolition of the Baby Bonus and replacement with the **Paid Parental Leave** scheme.

Its simplicity and universal design make the Baby Bonus a valuable case for evaluating how financial incentives influence fertility. The policy's introduction was unexpected, announced by Treasurer Peter Costello in the May 2004 budget with his famous exhortation to "have one for mum, one for dad, and one for the country." Its surprise introduction, combined with the natural lag of pregnancy, created a unique opportunity to observe the impact of financial incentives on fertility decisions.

Understanding Policy Impact

Measuring whether fertility policies actually work requires careful analysis because birth rates naturally fluctuate due to economic conditions, social trends, and employment opportunities. To examine the effect of FCTC and the Baby Bonus, we take advantage of the policies' unexpected announcement timing and the biological reality that pregnancies take approximately nine months to result in births.

Interrupted Time Series (ITS)

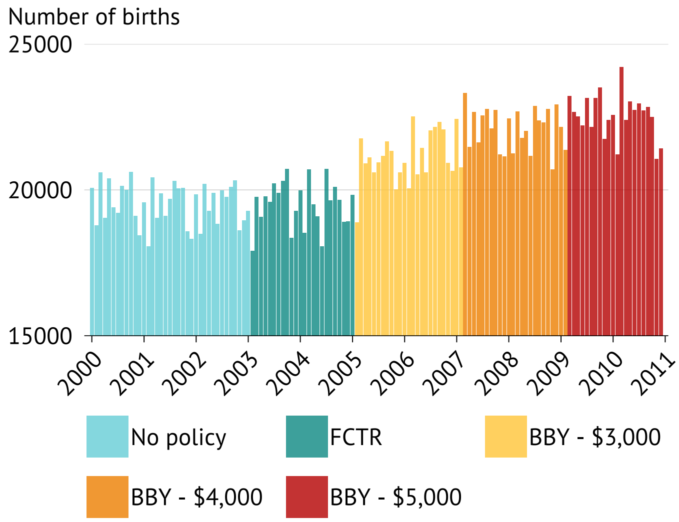
We examine the effect of FCTR and Baby Bonus using an interrupted time series approach, comparing birth rates before and after the policy while adjusting for seasonal patterns and longer-term trends (Figure 3). This allows us to examine policies' impact from broader demographic or economic shifts.

The ITS design exploits the announcement dates and a biologically grounded lag. We explicitly leverage anticipatory behaviour at conception: the effect is the change in births beginning $\sim$9 months after the announcement. Because every family could receive the payment, our estimates do not rely on comparing eligible versus ineligible groups. Instead, we investigate whether births increase sharply exactly nine months after the policy announcement, after allowing for normal month-of-year patterns and longer-run trends. We also check that similar jumps do not appear at other dates where no policy changed (placebo cut-offs) and that no overlapping policies or macro shocks can explain the result. Details of the methodology are presented in the Appendix A.4.1.

We investigate the effect of: (i) FCTR, (ii) Baby Bonus $3,000, (iii) Baby Bonus $4,000, and (iv) Baby Bonus $5,000 with the means test. Each regime starts nine months after its announcement so as to capture conception responses (e.g., BBY announced May 2004 $\rightarrow$ treatment starts February 2005). Our analysis accounts for several background factors that could influence birth patterns. We control for seasonal variations (some months naturally have more births), gradual changes over time, and differences between groups of mothers (by age, whether they were born in Australia, and whether they live in cities or regional areas). This approach helps us isolate the causal effect of policy changes from other underlying trends.

2 Available only to families with annual incomes below $150,000.

Figure 3: Monthly Birth Counts by Policy Period

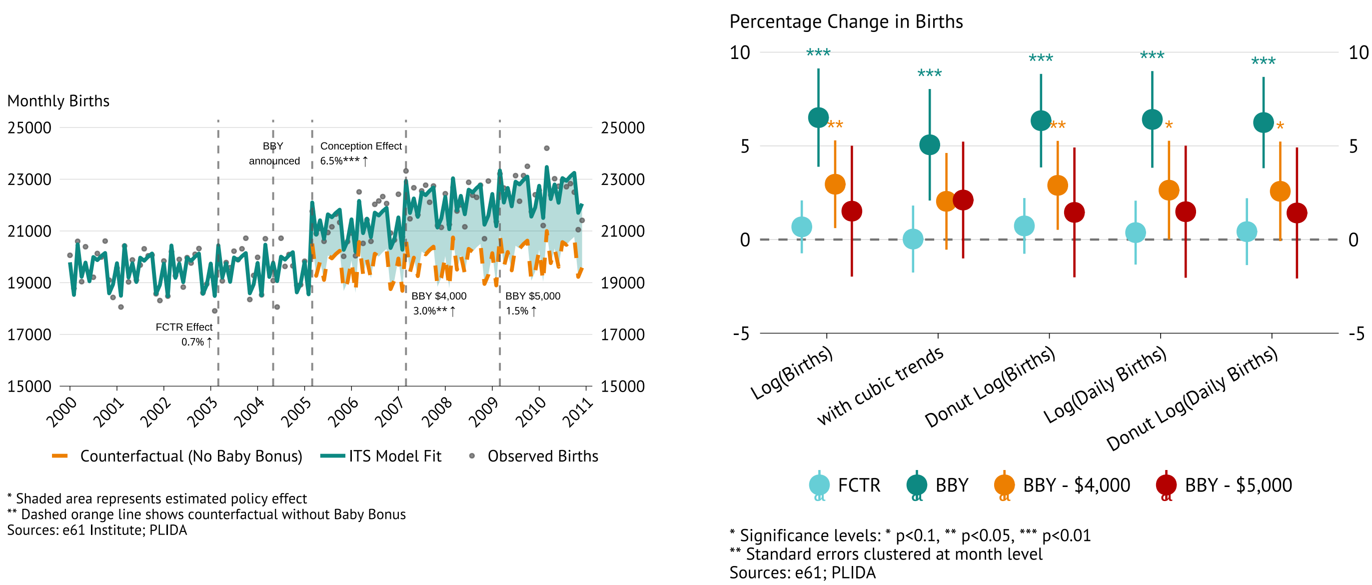


Sources: e61; PLIDA

We measure each policy change step-by-step. For example, when we look at the impact of increasing the payment from $3,000 to $4,000, we compare birth rates during the $4,000 period with those in the preceding $3,000 period, rather than with the original baseline before any payments existed. This approach shows us the additional effect of each policy boost.[3]

The Effect of Pronatalist Policies on Fertility

Figure 4: Main Results



A: Interrupted Time Series Visualisation

B: Effect Sizes Across Policy Periods

Our results, presented in Figure 4B, highlight three key patterns:

3 We confirm our main findings using alternative methods, including Regression Discontinuity in Time and comparisons with New Zealand (which had no similar policy). Full details are presented in Appendix A.4.2.

- The introduction of the $3,000 Baby Bonus led to a 6.5 % increase in births, or about 1,300 additional births per month.[4] However, the preceding First Child Tax Refund had no detectable effect on fertility, despite offering up to $12,500 for high earners.
- Increasing the payment from $3,000 to $4,000 generated an additional 3.0% rise in births. The subsequent increase to $5,000, coupled with means testing, produced only a marginal 1.4%, suggesting a much weaker impact. This pattern indicates that larger payments did not translate into proportionally larger increases in births.
- The Baby Bonus maintained its effect throughout the five-year program (see Figure 4A). This persistence suggests that the policy addressed a genuine constraint rather than simply bringing forward births that would have occurred anyway.

Taken together, these aggregate effects provide a clear picture of the policy's impact, but they may conceal important differences across groups. The 6.5% average increase could reflect modest responses from many families or stronger reactions concentrated in particular groups. It also remains an open question whether the policy led families to have more children overall (a quantum effect) or to adjust the timing of births (a tempo effect).

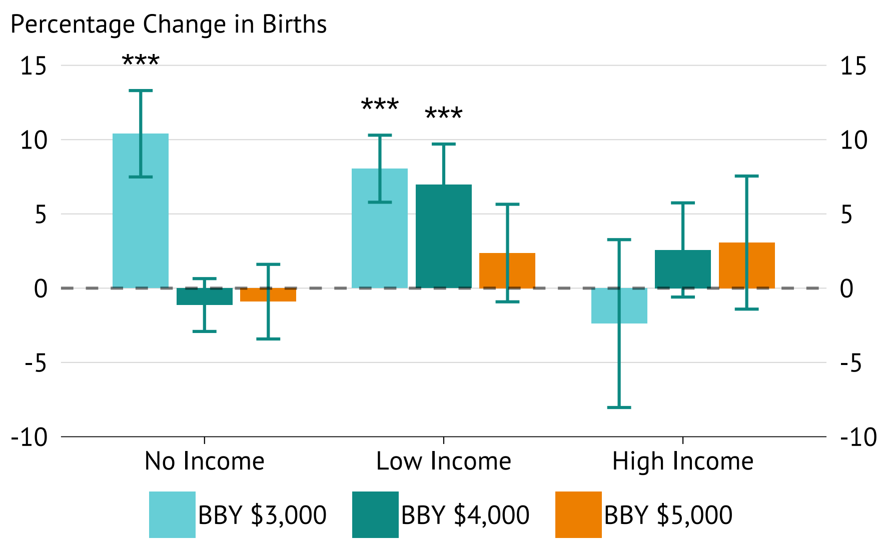
The following sections address these questions: Who responded to the Baby Bonus? And did they end up with larger families?

Differences in Responses Across Groups

Response by Maternal Income

The Baby Bonus prompted very different responses across the income distribution, highlighting a clear difference in fertility responsiveness.[5]

Figure 5: Policy Response by Maternal Income Level



* Significance levels: * p<0.10, ** p<0.05, *** p<0.01
** Income based on ATO taxable income records from the FY before birth.
Sources: e61; PLIDA

- Mothers with no taxable income show a 10.4% increase in births, about 60% larger than the overall effect (6.5%).[6]
- Those with incomes below the median income also respond strongly, by 8%.
- By contrast, high-income mothers show no response to the policy.

4 Alternative approaches, including Regression Discontinuity in Time and a comparison with New Zealand, point to the same 6–7% increase (Appendix A.6).
5 We cannot examine income effects for the First Child Tax Refund as our individual tax return data (ATO), which contains the income information, is only available from the 2002-03 financial year.
6 Household income information is not available in the administrative data.

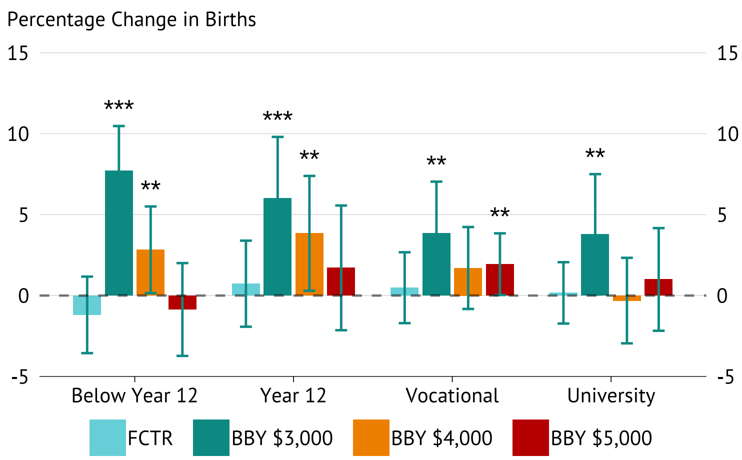
These results are consistent with economic intuition: a $3,000 payment provides a meaningful liquidity boost for low-income mothers but is marginal for more affluent mothers. We find no additional response among the no-income group when the payment rises to $4,000, while the low-income group shows a small further increase (Figure 5).

As mothers' income may not fully reflect household financial circumstances, we complement this analysis by examining education-based differences in the next section.

Policy Response by Maternal Education Level

The effect of the Baby Bonus may also differ by mothers' education. Figure 6 shows the effect of FCTR and Baby Bonus on fertility by maternal education level.

Figure 6: Policy Response by Maternal Education Level



* Significance levels: * p<0.10, ** p<0.05, *** p<0.01
** Education based on Census 2016 records.
Sources: Census 2016; e61; PLIDA

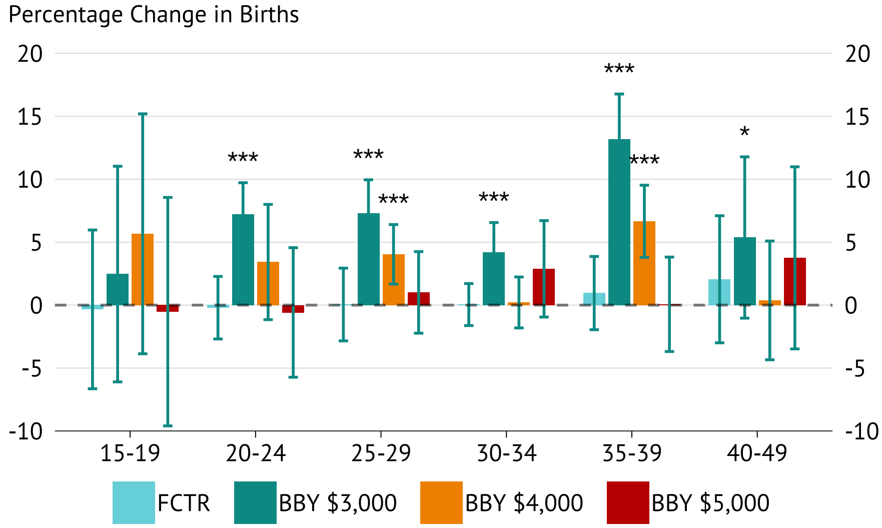
- Mothers with less than Year 12 education show the strongest response (7.7%) to $3,000 and remain responsive as amounts rise to $4000 and $5000.
- University-educated mothers respond modestly at $3,000 (3.8%) but not at higher amounts.
- Women with vocational training show a 3.8% increase in births during the $3,000 period and a 1.9% increase during the $5,000 period.

In sum, less-educated women were the most responsive to the Baby Bonus, whereas university-educated women showed only modest changes, consistent with higher opportunity costs limiting their fertility choices.

Policy Response by Maternal Age

The effect of the Baby Bonus may also differ by mothers' age. Figure 7 shows the effect of FCTR and Baby Bonus on fertility by maternal age group.

Figure 7: Policy Effects by Maternal Age



* Significance levels: * p<0.10, ** p<0.05, *** p<0.01
** All specifications include urban, country of birth, and month FE
Sources: e61; PLIDA

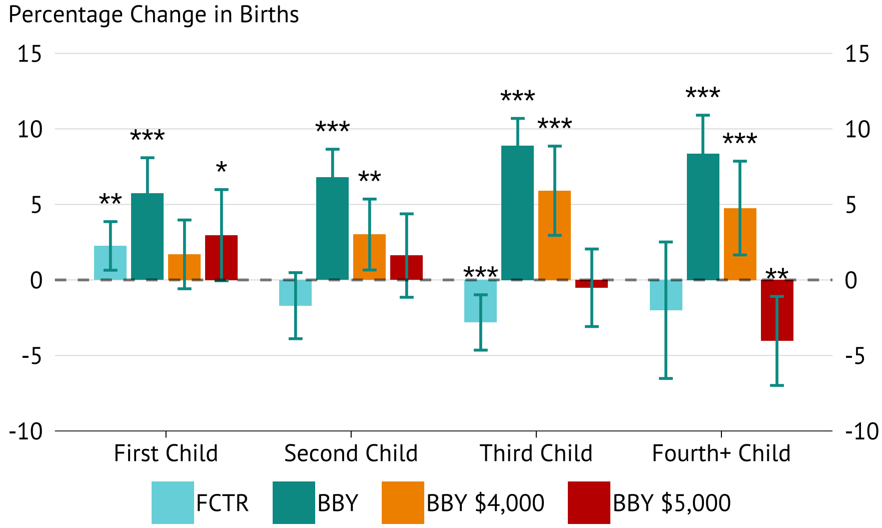
- The strongest and most consistent effects were observed among women in their 20s and early 30s. For mothers aged 20–24, 25–29, and 30–34, the introduction of the $3,000 Baby Bonus led to statistically significant increases in births of around 7–10%.
- The largest gains occurred for older mothers, where births increased by over 13%—the single strongest effect across all age groups.
- No significant effect was detected for 15–19 year-olds, although estimates are positive but imprecisely measured.
- Small positive effects are observed, particularly at ages 40–44 (∼6%), but these are less precisely estimated.

By contrast, the FCTR shows no discernible effect across age groups. Later policy adjustments—raising the Baby Bonus from $3,000 to $4,000 and $5,000—also did not replicate the strong initial effects, with much smaller and statistically weaker increases.

Policy Response by Birth Order

The birth order matters in understanding the impact of financial incentives on fertility. First births reflect entry into parenthood, while higher-order births capture family expansion. Analysing responses across parity helps us understand whether the Baby Bonus primarily shifted timing or contributed to larger completed families. Evidence of stronger effects at higher-order births would point toward lasting increases in family size rather than short-term timing changes. Our results show that:

Figure 8: Policy Effects by Birth Order



* Significance levels: * p<0.10, ** p<0.05, *** p<0.01. All specifications include age group, and month FE
Sources: e61; PLIDA

- **First births:** Both the FCTR and the initial $3,000 Baby Bonus significantly increased first births. In contrast, the subsequent top-ups of BBY to $4,000 and $5,000 generated smaller effects that are estimated less precisely.
- **Second births:** Positive and statistically significant effects, with increases of around 6–8% under the $3,000 BBY.
- **Third births:** The strongest impact is observed here, with an increase of about 9% under the $3,000 BBY. This is the single largest effect across all birth orders.
- **Fourth and higher-order births:** Large and significant increases are also found, at around 8–9%.
- Of the 1,300 additional monthly births, approximately 460 came from first births, 430 from second births, 250 from third births, and 160 from fourth and higher-order births. This pattern suggests that the Baby Bonus both supported entry into parenthood and encouraged larger families.

The FCTR shows a small positive effect on first births, consistent with its design as a first-birth, tax-linked incentive. Beyond this, it has little impact on higher-parity births. Later policy adjustments—raising the Baby Bonus to $4,000 and $5,000—do not replicate the strong effects of the initial $3,000 payment, with coefficients smaller and often not statistically significant.[7]

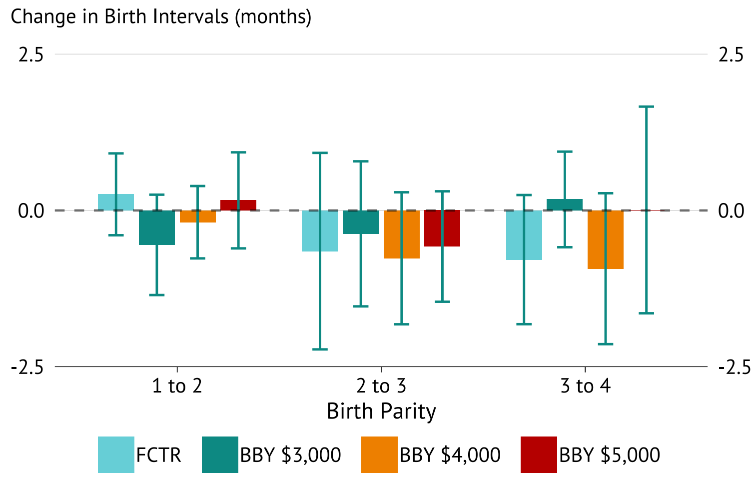
Timing Shift vs. Completed Fertility

The Baby Bonus policy increased the number of births. But the key question is whether it simply brought births forward or whether it led to families having more children overall. These two outcomes have very different implications: shifting the timing of planned births changes short-term population trends, while increasing completed family size has lasting economic and demographic effects. The fact that the policy raised fertility among women aged 35–39 and at third births provides strong evidence that it increased completed fertility, not just shifted timing.

To test this more directly, we draw on two pieces of evidence. First, we examine birth spacing to assess whether the Baby Bonus compressed spacing between children, which would show whether the policy simply brought births forward. Second, we turn to administrative data,[8] which links mothers to their birth histories and tracks fertility outcomes through 2022. This long horizon, 17 years after the policy's introduction, captures near-complete fertility for women who were in their prime childbearing years at the time the policy was introduced. For example, a woman aged 30 in 2004 would be 48 in 2022, by which point she would typically have completed her childbearing years. With this, we can directly assess whether the Baby Bonus raised the total number of children ever born.

7 See Appendix A.5 for results based on country of birth and location of mothers.
8 We use the Person Level Integrated Data Asset (PLIDA).

Figure 9: Policy Effects on Birth Spacing



* Significance levels: * p<0.10, ** p<0.05, *** p<0.01
Sources: e61; PLIDA

Results on Birth Spacing

The results on the effect of the FCTR and Baby Bonus on birth spacing show no meaningful change. On average, the interval between successive births remained about 42 months across all policy periods and birth orders. If these policies had simply brought births forward, we would expect shorter gaps between children, but the estimated changes are very small (within $\sim$ 0.8 months) and statistically imprecise.

Figure 9 illustrates this point: the largest estimated change (−0.93 months for Child 3→4 during the $4,000 period) is less than one month and not robust across birth orders or policy regimes. Confidence intervals overlap zero for all entries, and there is no systematic tendency toward shorter spacing in the baby bonus periods.

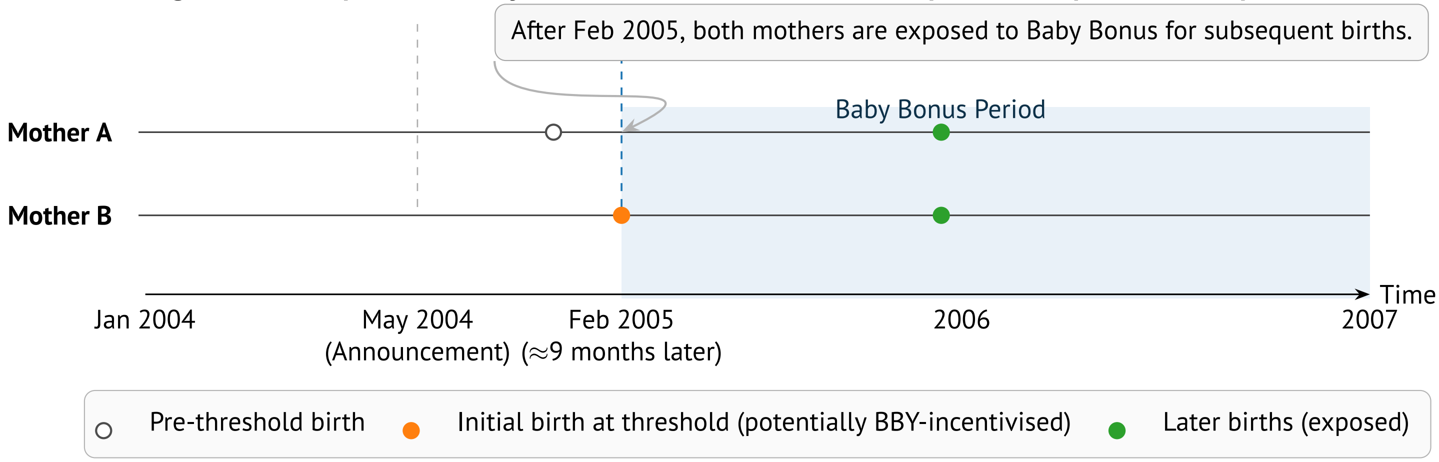
Taken together, the stability of spacing, combined with the concentration of effects among older mothers and higher-order births, points away from short-run timing shifts and toward genuine *quantum* responses. Having ruled out compressed spacing, we now turn to the completed family size in 2022.

Completed Fertility in 2022

Identifying the effect of the Baby Bonus on completed fertility is challenging because there are no clean comparison groups: one group that was always affected by the policy and another that was never affected. For example, women who gave birth before February 2005 (Mother A in Figure 10) remained exposed to the Baby Bonus for subsequent births, meaning their decisions to have additional children might be affected by the policy during our study period. The same applies to women who gave birth after February 2005 (Mother B in Figure 10). When we compare fertility outcomes, such as average children per mother, of these two groups of mothers, both groups had equal exposure to the Baby Bonus policy for all births after February 2005. This generates an identification problem: any policy effect we estimate is likely underestimated because our "comparison group" was also affected by the policy.

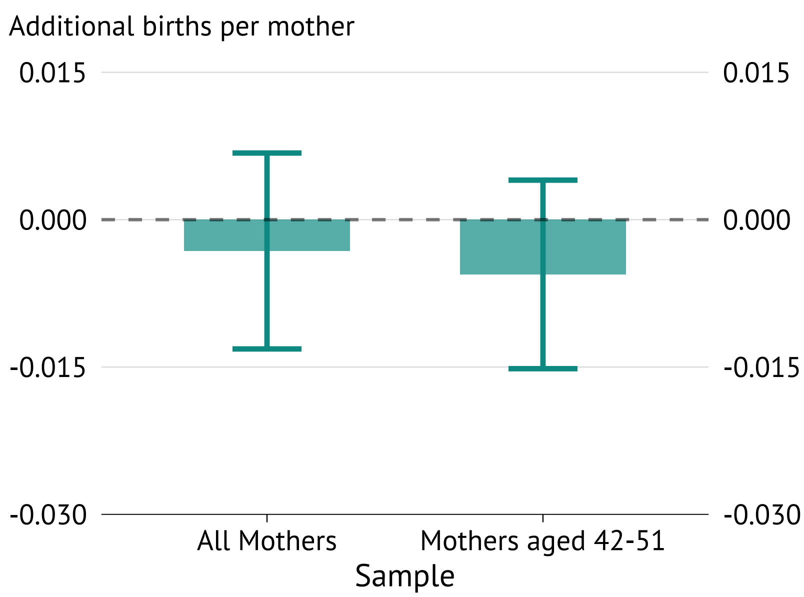
To understand whether the Baby Bonus had a lasting effect on completed fertility, we examine whether the short-run increase in births translated into more children per mother by 2022, rather than being offset by fewer births later on. The idea is straightforward. If the additional babies born in the short run were offset by fewer births later, then the initial effect of the Baby Bonus would simply fade over time. But if there was no reduction in births later on, it would mean that the average number of children per mother remained the same across cohorts — or even increased if some mothers went on to have additional children, so those extra babies would still be present in 2022. In that case, the Baby Bonus would represent a genuine increase in completed fertility, not just a short-term timing shift.

Figure 10: Completed Fertility in 2022: Absence of a Never-Exposed Comparison Group



Our analysis in Figure 11 shows no difference in subsequent childbearing between mothers on either side of the cutoff, February 2005. This means that the short-term increase in births persisted through 2022, providing evidence that the Baby Bonus raised completed fertility.[9]

Figure 11: The Effect of Baby Bonus on Additional Children per Mother by 2022

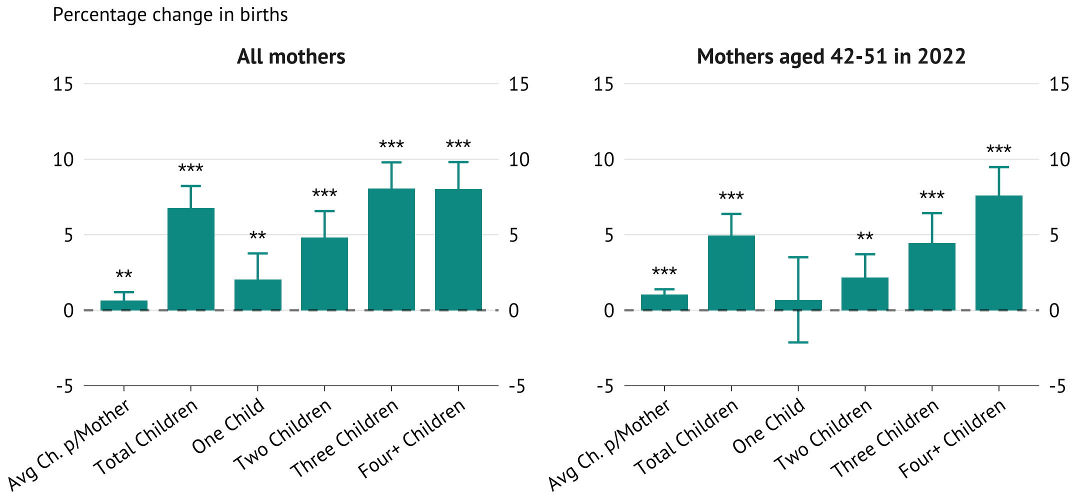


* Significance levels: * p<0.1, ** p<0.05, *** p<0.01
Sources: e61; PLIDA

Consistent with this, we find that mothers who gave birth about nine months after the announcement, in February 2005, had 6.8% more children in total by 2022 than comparable pre-policy mothers. We also observed a 1% increase in the average number of children per mother, although this probably understates the true effect because both the pre- and post-policy cohorts were exposed to Baby Bonus for subsequent births. As shown in the right panel of Figure A.4, women exposed to the Baby Bonus window were more likely to end up with three or more children in total.

9 We calculate "additional births per mother" as the number of children mothers had after a given birth month, up to June 2022, divided by the number of mothers. For example, for mothers who gave birth in April 2005, we count all children they had between April 2005 and February 2022 and divide that total by the number of mothers in the group.

Figure 12: The Effect of Baby Bonus on Completed Fertility by 2022



* Significance levels: * p<0.1, ** p<0.05, *** p<0.01
** Completed fertility observed as of 2022
Sources: e61; PLIDA

These lasting effects did not come from the reduced spacing between births, which remained unchanged. Instead, they reflect two channels: entry, as more women began childbearing during the eligibility window, and persistence, as some of these women ultimately had more children.

Overall, the Baby Bonus led to a lasting increase in completed fertility, with the additional births observed in the short run still present in 2022.

Understanding the Magnitude: Why $3,000 Generated Large Effects

It might be argued that the 6.5% fertility increase is a large effect given that $3,000 represents only a fraction of the lifetime costs of raising a child. Several factors may help explain this magnitude.

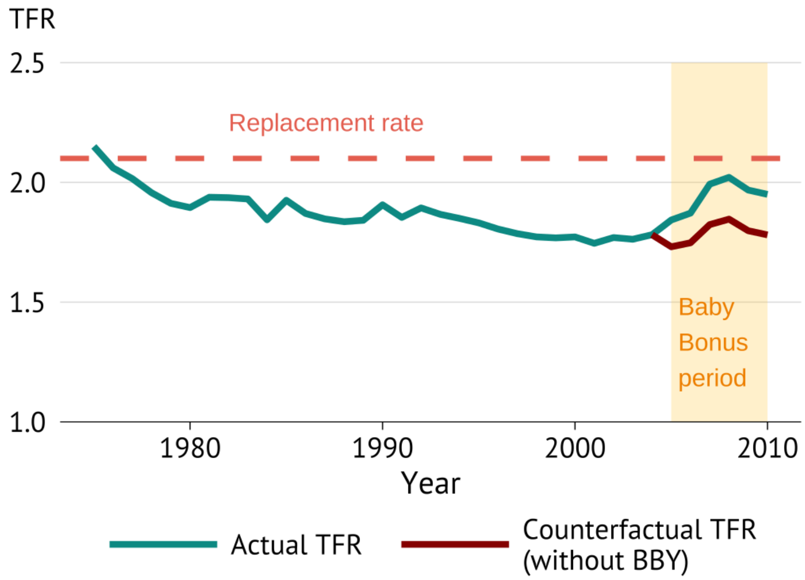
First, policy design may have mattered more than generosity. The First Child Tax Refund offered comparable amounts but did not generate a fertility response, perhaps because of its complexity and delayed payments. By contrast, the Baby Bonus's immediate and universal structure created clarity and salience that could have amplified its effect beyond the monetary value.

Second, $3,000 may have acted as a short-term liquidity relief. For families already considering children, it could have eased immediate financial pressures at the decision point, particularly during the period of peak financial stress when one parent reduces work. This interpretation aligns with the larger responses observed among mothers with zero or low taxable income, for whom $3,000 represented several months of household income.

Third, the Australian policy context may have made families more responsive compared to other countries. Cross-country evidence shows that fertility elasticities vary with existing support systems. Spain's €2,500 baby bonus[10] in 2007 generated an elasticity around 0.24 (González & Trommlerová, 2023), but Spanish families already had 16 weeks of paid maternity leave and an established child care infrastructure. In 2004, Australia offered very limited family support, which may have increased marginal responsiveness to this first major intervention. The estimated elasticity of 0.88 could reflect this context.[11]

10 Spain's €2,500 baby bonus in 2007 translates to roughly AUD 3,500–3,850 in 2004 dollars.
11 The fertility elasticity with respect to the baby bonus is calculated as follows: The $3,000 AUD baby bonus represented 7.4% of average annual income ($40,648 AUD, based on weekly earnings of $781.70), and generated a 6.5% increase in monthly birth counts. The elasticity is therefore 6.5% ÷ 7.4% = 0.88.

Figure 13: Total Fertility Rate with and without Baby Bonus



Source: ABS, e61

Figure 13 illustrates this responsiveness by comparing actual fertility rates with a counterfactual scenario absent the Baby Bonus. The counterfactual scenario for 2005-2010 total fertility rates is calculated using aggregate ABS birth statistics, reducing birth counts for each age group using the coefficients in Figure 7, and then recalculating the counterfactual TFR using yearly female population data. The gap between these lines during 2005-2010 reveals the policy's substantial impact, where the TFR is lifted from what would have been approximately 1.75 to nearly 2.0 at its peak.

The magnitude of this response likely reflects both Australia's limited baseline support and effective policy communication. Treasurer Costello's popular "one for mum, one for dad, one for the country" message may have provided social reinforcement that amplified the financial incentive's impact, creating a cultural moment around fertility that extended beyond the monetary transfer itself.

Conclusion and Policy Implications

Our analysis suggests that Australia's Baby Bonus increased births by around 6–7% and contributed to a higher completed fertility by 2022. Although broader economic or demographic trends are often thought to explain the increase in fertility at the time, our approach uses sharp month-to-month cut-offs nine months after each policy announcement, which helps us to isolate the policy's impact from those wider factors.

The effects were larger among older mothers and third-or-higher births, groups where timing shifts are less likely, and we find no evidence of compressed birth intervals. Taken together, this pattern is consistent with liquidity relief driving quantum rather than tempo effects. The estimated fiscal cost was approximately $50,000 per additional birth in 2004 dollars (around $86,000 in 2025 dollars).[12] These results highlight the value of simple, well-targeted financial incentives that address immediate financial barriers. However, they also emphasise the need for complementary policies, such as affordable child care, paid parental leave, and flexible work arrangements, which can help reduce the opportunity costs of childbearing and support long-term fertility decisions.

12 Given approximately 250,000 births per year before the policy, the 6.5% increase translates to about 16,250 additional births annually. At $3,000 per birth, this totals around $799 million per year, or $50,000 per additional birth (in 2004 dollars).

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A.1. Literature Review

A.1.1 Australian Baby Bonus Literature

The Australian Baby Bonus has generated a substantial body of research examining its effects on fertility outcomes and child welfare. This literature provides important context for our analysis and demonstrates both the policy's measurable impacts and the methodological challenges in evaluating fertility interventions.

Fertility Effects: Consistent Evidence of Positive Response

Multiple studies have documented significant increases in birth rates following the introduction of Baby Bonus. Lain et al. (2009) analysed New South Wales birth records using Poisson regression and found that crude annual birth rates reversed their downward trend after 2004, with particularly strong effects among teenagers and for third or higher-order births. Similarly, Sinclair et al. (2012) employed structural time series models using national ABS birth registry data and estimated approximately 108,000 additional births attributable to the policy over the observation period, at a cost of approximately $43,000 per additional child.

Using HILDA survey data, Drago et al. (2011) applied instrumental variable probit models to examine both fertility intentions and actual births. Their analysis revealed that fertility intentions rose following the Baby Bonus announcement, with the birth rate estimated to have increased modestly as a result. However, they calculated a substantially higher marginal cost to the government of at least $126,000 per additional birth. This discrepancy with Sinclair et al. (2012) highlights the sensitivity of cost-effectiveness calculations to methodological choices and the data.

More recent synthetic control analysis by Reich (2024) confirmed these findings, estimating a 6.82% increase in total fertility between 2005 and 2012. This methodology, which constructs counterfactual scenarios using other countries' fertility patterns, provides additional validation that the observed increases represent genuine policy effects rather than coincidental demographic shifts.

Unlike other studies, Parr and Guest (2011) used multilevel logistic regression models with HILDA data and found that while the effects of the Baby Bonus were modest, the effects of education, income, occupation, marital status, age, and parity are significant.

Heterogeneous Responses Across Demographics

A key finding across multiple studies is substantial heterogeneity in policy responses. Rawlings et al. (2016) used multivariate analysis of national birth data from 2001-2013 and found that birth rates in the 15-19 age group rose by 8.1% during key Baby Bonus years, with the lowest two socioeconomic quintiles experiencing increases of 10% and 12% respectively. This pattern of stronger responses among younger and lower-income mothers is consistent with economic theory predicting larger effects where liquidity constraints bind most tightly.

Risse (2010) examined childbearing intentions using longitudinal ordered probit models with HILDA data and similarly found the strongest increases among women from lower-income households. The author noted that these women were "relatively likely to be reliant on welfare assistance to raise their children over the long term," suggesting the policy may have enabled births among families facing the greatest financial constraints.

Interestingly, Langridge et al. (2012) found contrasting results regarding socioeconomic gradients. Using Western Australia birth registry data with generalised linear models, they observed the greatest absolute increase in births among women in the highest socioeconomic areas, despite these areas having the lowest baseline fertility rates. This geographic variation highlights the complexity of fertility responses and suggests that policy effectiveness may depend critically on local economic conditions and demographic composition.

Finally, Bonner and Sarkar (2020) employed a triple difference-in-differences strategy with HILDA data to evaluate the policy's quasi-experimental setting using propensity score matching. Their analysis highlighted the crucial role of immigrant women in driving policy success, with the highest impacts found among immigrant women with low human capital levels.

Beyond Fertility: Child Health Outcomes

De Gendre et al. (2021) examined the policy's effects on child health using administrative data from South Australia and a regression discontinuity design. They found that treated babies had fewer preventable, acute, and urgent hospital presentations in the first two years of life, while later increasing demand for elective care. Notably, effects were strongest for disadvantaged families, and the authors estimated that up to 34% of the payout was recouped within the first year through reduced healthcare costs.

This health study represents important evidence that the Baby Bonus had effects beyond simple birth timing, providing additional support for quantum rather than tempo effects. The concentration of health benefits among disadvantaged families also aligns with the fertility literature's finding of stronger responses in lower-income groups.

Birth Timing and Strategic Behaviour

Several studies (Gans and Leigh, 2009) have documented strategic manipulation of birth timing around the policy's implementation date, 1 July 2024. This short-term scheduling represents a distinct effect from the longer-term fertility responses examined in most studies, but highlights the behavioural salience of the policy. The existence of such manipulation provides additional evidence that the families were highly aware of and responsive to the financial incentive.

Limitations and Gaps

Despite this substantial literature, several limitations remain. Most studies focus on short-to-medium-term fertility responses and cannot definitively establish whether the policy increased completed family size versus accelerated childbearing timing. Additionally, while studies consistently find heterogeneous responses across demographic groups, the mechanisms driving these differences remain incompletely understood. Finally, cost-effectiveness estimates vary widely across studies, partly reflecting different methodological approaches and time horizons but also highlighting uncertainty about the policy's true fiscal efficiency. For a broader overview of the literature, see Gray et al. (2022).

Our analysis contributes to this literature by leveraging linked administrative data to track individual mothers through 2022, enabling examination of completed fertility, which is a crucial gap in existing research. Furthermore, our multiple identification strategies and extended observation period provide new insights into both the magnitude and persistence of the Baby Bonus effects.

A.1.2 International Literature on Pro-natalist Policies

This work contributes to a large empirical literature on whether financial and family policies shape fertility decisions. In classic models of household fertility choice, children are a (durable) consumption/investment good produced with time and market inputs; prices and incomes therefore shift the marginal utility and timing of births (Becker, 1960). Cross-country work documents substantial heterogeneity in the bundles of family policies—cash, tax, leave, child care—and suggests they can modestly raise completed fertility when designed as coherent packages rather than one-off transfers (Gauthier & Hatzius, 1997; Thévenon, 2011).

Within-country quasi-experimental evidence generally finds that direct financial incentives raise fertility on the margin. Studying Quebec's birth grant, Milligan (2005) shows sizeable increases in higher-order births, with elasticities consistent with economically meaningful price effects. Using administrative micro data from Israel, Cohen et al. (2013) identify a positive response to child subsidy changes and a small income effect, aligning with Beckerian predictions. For Spain, González and Trommlerová (2023) exploit both the introduction and cancellation of a universal lump-sum maternity allowance, finding increases at introduction and a symmetric decline at cancellation, alongside evidence separating timing (tempo) from level (quantum) effects. In Germany, Riphahn and Wiynck (2017) exploit heterogeneous reforms to child benefits and show that responses vary across parity and income groups.

A complementary strand studies leave and work–family policies. Using Austrian reforms, Lalive and Zweimüller (2009) show that extending paid, job-protected parental leave increases higher-order fertility in the short run while delaying mothers' return to work—highlighting interactions between labour market institutions and childbearing decisions. More broadly, survey and review evidence argues that cash alone rarely sustains large, permanent fertility increases; combinations that reduce the shadow price of childrearing (affordable child care, job protection, predictable leave) tend to be more effective (Brainerd, 2014; Gauthier & Hatzius, 1997; Thévenon, 2011).

Heterogeneity is central. Responses differ by socioeconomic status, parity, and local conditions: larger effects for certain income or education groups, and policy cancellations may bite hardest in lower-income regions (González & Trommlerová, 2023; Riphahn & Wiynck, 2017). Evidence from pronatalist bonuses in Asia likewise shows uneven take-up across age and education (Malak et al., 2019). Our study adds to this literature by leveraging the Australian Baby Bonus setting and linked administrative outcomes to (i) quantify both timing and quantum responses, (ii) map heterogeneity across parity and socioeconomic strata.

A.2. Data Construction and Validation

This study draws on multiple data sources to evaluate the effects of Australia's Baby Bonus policy on fertility outcomes. The primary data come from the Person Level Integrated Data Asset (PLIDA), a secure resource maintained by the Australian Bureau of Statistics that combines individual-level information from six government agencies.[13] For analyses extending beyond the coverage period of PLIDA, we supplement our work with publicly available monthly birth counts from the ABS, as well as comparable data from New Zealand.

A.2.1 PLIDA: Comprehensive Administrative Data

Our main analytical dataset is constructed from PLIDA, which offers whole-of-life insights into health, education, government payments, income, employment, and population demographics in Australia (Australian Bureau of Statistics, 2018).[14] We identify all children born in Australia from January 2000 to December 2010, spanning periods before and during the Baby Bonus policy.

Figure A.1: ABS Data linkage workflow: PLIDA, DOMINO, ATO, and Census

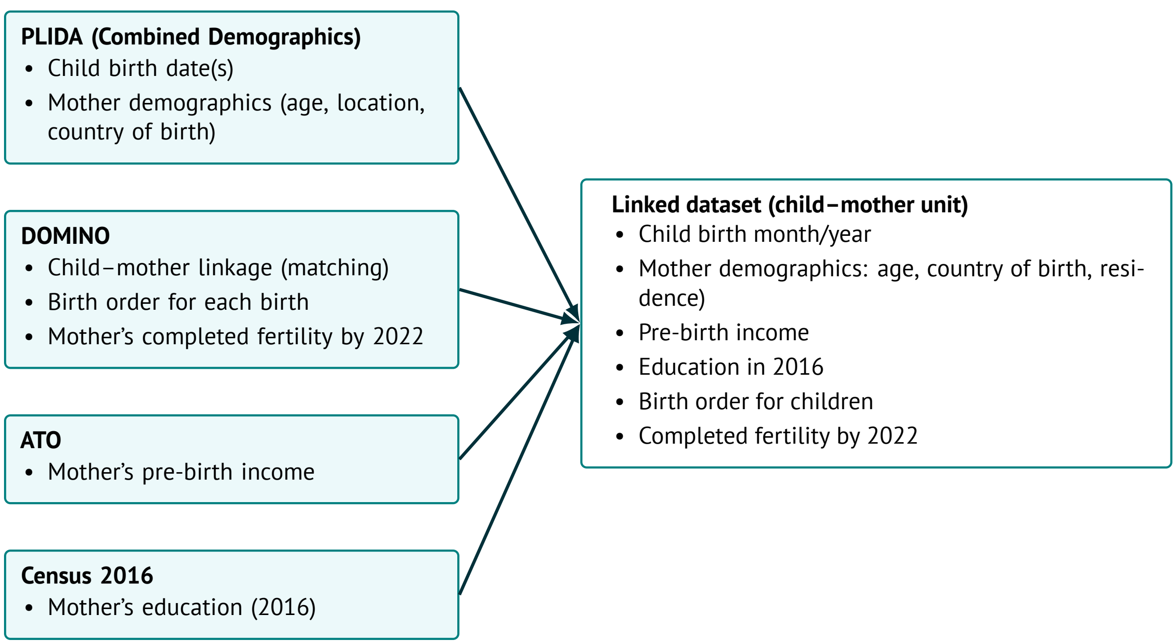


Figure A.1 illustrates our data construction process. We link birth records to their mothers using the Department of Social Services' Data Over Multiple Individual Occurrences (DOMINO) database, which tracks individuals' interactions with Australia's social security system. This linkage enables us to retrieve detailed maternal characteristics including age, residential location,

13 Australian Bureau of Statistics, Australian Taxation Office, Department of Education, Department of Health, Department of Human Services, and Department of Social Services.
14 Further details on the development of PLIDA can be found in Biddle et al. (2019).

and country of birth, while also tracking birth order and completed fertility through 2022. We supplement these core records with pre-birth income information from Australian Taxation Office data and 2016 education data from the Census.

This comprehensive linkage creates a child-mother dataset that captures both immediate policy responses and long-term fertility outcomes—addressing a key limitation in existing fertility policy research. The resulting dataset allows us to investigate not only overall policy impacts but also demographic heterogeneity across various subgroups, while distinguishing between timing effects and genuine changes in completed family size.

Coverage validation shows we consistently capture approximately 90 percent of births each month throughout our study period (see Appendix Section A.6.1). This rate is naturally lower than comprehensive ABS birth statistics, as our dataset specifically focuses on births to Australian residents and citizens eligible for Baby Bonus payments, which DOMINO precisely identifies through its comprehensive tracking of social security interactions.

Key Variables

The principal outcome variable is the *logarithm of monthly birth counts*. This specification offers several analytical advantages: it facilitates interpretation of coefficients as percentage changes, reduces the influence of outliers, and stabilises variance across different time periods and demographic groups. The log transformation is particularly appropriate for count data exhibiting seasonal patterns and varying baseline levels across subgroups, as is characteristic of birth data. By stratifying data along maternal age bands (15–19, 20–24, 25–29, 30–34, 35–39, and 40–49), residence at birth (capital city vs. regional), and mothers' countries of birth (Australian-born vs. immigrant), we gain additional insight into which populations respond most strongly to financial incentives.

As a robustness check, we also examine average daily birth rates (births per 1,000 women aged 15–49) using publicly available ABS Data Explorer birth counts combined with count of women. This analysis confirms that our main findings are not sensitive to the choice of outcome specification or data source.

A.2.2 ABS Data Explorer: Public Births Data

To validate our PLIDA-based findings and extend our temporal coverage, we incorporate publicly available monthly birth registry data from the ABS Data Explorer, specifically the "Births, year and month of occurrence by state or territory of usual residence, 1975 onwards" series (Australian Bureau of Statistics, 2023b). This complementary data source serves two essential functions: providing a robustness check against aggregate national statistics and enabling analysis through the policy's abolition in 2014—beyond PLIDA's reliable coverage period. As Figure A.2 demonstrates, PLIDA-derived birth counts align closely with ABS public data during overlapping periods, validating both our data linkage procedures and the representativeness of our PLIDA sample. While these publicly available data lack the individual-level demographic detail essential for our heterogeneity analysis, they provide comprehensive national coverage throughout our extended study period.

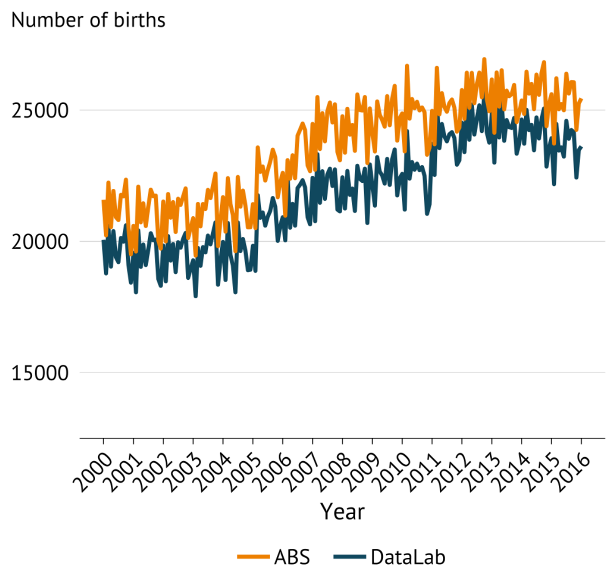
A key methodological consideration shapes our analytical approach. DataLab's incorporation of Census linkages beginning in 2011 causes PLIDA coverage to improve substantially—jumping from 90% to 95%—creating an artificial discontinuity. To ensure that we attribute changes in birth patterns to policy effects rather than data improvements, we restrict our main PLIDA-based analysis to the pre-2011 period when coverage rates remained stable. For both datasets, we maintain methodological consistency by employing identical analytical procedures: dividing monthly birth counts by days per month and standardising by the female population of reproductive age. This unified approach enables us to track policy effects comprehensively—from the Baby Bonus introduction through its various modifications and eventual abolition—while controlling for underlying demographic shifts in the population of women of childbearing age.

A.2.3 New Zealand Data

New Zealand provides an ideal comparison country due to its unique relationship with Australia, sharing historical, cultural, and economic ties that produce highly comparable demographic trends. Both countries experienced similar economic and social developments during our study period, including comparable healthcare systems and cultural attitudes toward family formation. Crucially, New Zealand implemented no baby bonus or equivalent pronatalist policies during 2000-2010, providing a clean counterfactual for isolating Australia's policy effects from broader regional demographic shifts.

As a robustness check, we implement our differences-in-discontinuities design using monthly birth counts from Statistics New Zealand's Infoshare database (Statistics New Zealand, 2023). We follow identical procedures to our Australian analysis,

Figure A.2: Monthly Birth Counts: PLIDA vs ABS Birth Registry



Source: e61, ABS Data Explorer, PLIDA

standardising birth counts by month length and female population to ensure direct comparability. This approach allows us to control for region-wide demographic trends, economic cycles, or cultural shifts that might affect fertility patterns across both countries, thereby isolating the specific impact of Australia's Baby Bonus policy from temporal confounders.

A.3. First Child Tax Refund Policy Details

This appendix provides detailed documentation of the FCTR policy as established under the Taxation Laws Amendment (Baby Bonus) Bill 2002. The FCTR represented Australia's initial foray into direct financial support for families with newborns, operating from July 1, 2001, until its replacement by the Baby Bonus in 2004.

A.3.1 Policy Overview and Structure

The FCTR was structured as a tax offset (refundable tax credit) rather than a direct cash payment, designed to reimburse new mothers for taxes paid in the year prior to childbirth. The policy was explicitly limited to first children only, with eligibility contingent on the child being born on or after July 1, 2001. Unlike the subsequent Baby Bonus, the FCTR required recipients to have paid income tax in a previous year to receive the maximum benefit.

The program operated under Subdivision 61-I of the Income Tax Assessment Act 1997, establishing a complex framework that tied benefit amounts to prior tax liability and comparative income levels between the base year and claim year. This design inherently favored higher-income mothers who had paid more tax, while providing minimal benefits to those with little or no prior tax liability.

A.3.2 Eligibility Criteria

To qualify for the FCTR, applicants had to satisfy several stringent conditions simultaneously:

1. **Child Event Requirement**: The applicant must have become legally responsible for a child on or after July 1, 2001, where neither the applicant nor any other person was legally responsible for the child before this date.
2. **First Child Limitation**: The benefit was available only for the first child. Applicants who had previously had a child event for another child were ineligible (Section 61-365).
3. **Ongoing Care Requirements**: At the time of claiming, the child must be under 5 years old, in the applicant's care, and the applicant must be legally responsible for the child.

4. **Residency Requirements**: The applicant must be an Australian resident both at the time of the child event and when claiming the offset.
5. **Priority Rules**: When multiple persons met the eligibility criteria for the same child, priority was given in the following order: natural mother, adoptive mother (if only one), any woman (if only one), natural father, adoptive father (if only one), or Commissioner determination based on agreements and other relevant factors.

The policy included provisions allowing spouses to transfer entitlements, provided the transferee was the spouse during all periods of primary entitlement and did not have their own primary entitlement for any child during the income year.

A.3.3 Benefit Calculation Formula

The FCTR amount was calculated using a complex two-stage formula that adjusted payments based on income changes between the base year and claim year:

Annual Offset Amount:

$$\text{Annual Offset} = \text{Entitlement Amount} \times \frac{\text{Total Entitlement Days}}{365} \tag{1}$$

Entitlement Amount Calculation:

$$\text{Entitlement Amount} = \text{Base Amount} \times \left(1 - \frac{\text{Current Year Taxable Income}}{\text{Base Year Taxable Income}}\right) \tag{2}$$

Where the **Base Amount** was defined as:

$$\text{Base Amount} = \min\left(\frac{\text{Base Year Tax Liability}}{5}, \$2{,}500\right) \tag{3}$$

Key Components:

- **Base Year**: Generally the income year immediately before the child event year, though applicants could elect the event year itself as the base year.
- **Base Year Tax Liability**: The basic income tax liability from Step 2 of the tax calculation in the base year.
- **Income Adjustment Factor**: The formula $(1 - \frac{\text{Current Income}}{\text{Base Income}})$ meant that benefits decreased proportionally as current income approached base year levels.

Minimum Payment Provision: For low-income recipients, a minimum entitlement of $500 applied when:

- The current income year was not the base year; AND
- Current year taxable income was $25,000 or less; AND
- The calculated entitlement amount was less than $500

A.3.4 Payment Structure and Limitations

The FCTR was distributed annually over up to five years, with the total benefit capped at the recipient's total tax paid in the base year. This structure created several limitations:

1. **Tax Liability Ceiling**: No recipient could receive more than they had paid in tax during their base year, regardless of the calculated entitlement amount.
2. **Extended Payment Period**: Unlike lump-sum payments, benefits were spread across multiple years, reducing the immediate financial impact during the period when families typically face the highest child care-related expenses.
3. **Income-Dependent Reductions**: The formula's income adjustment mechanism meant that mothers returning to work at similar income levels received substantially reduced benefits.
4. **Complex Claiming Process**: Benefits had to be claimed through annual tax returns or approved forms, with claims being irrevocable once made.

A.3.5 Administrative Features

The legislation included several administrative provisions designed to prevent double-dipping and ensure appropriate targeting:

- **Irrevocable Elections**: All choices regarding base year selection, benefit transfers, and claims were permanent once made.
- **Death Provisions**: Special rules extended entitlements through the end of the income year if a child died before age 5, and allowed subsequent first child claims.
- **Refundable Status**: The FCTR was classified as a refundable tax offset, meaning recipients could receive refunds even if they owed no tax.
- **Mutual Exclusivity**: Recipients could not simultaneously claim FCTR benefits for multiple children or transfer already-claimed entitlements.

This complex structure, while attempting to provide targeted support for new mothers who had reduced their workforce participation, created significant barriers to uptake and understanding, ultimately contributing to the policy's replacement by the simpler, universal Baby Bonus in 2004.

A.4. Methodology

Identifying the causal effect of pro-natalist policies on fertility presents significant challenges due to concurrent economic shocks, evolving social norms, and potential confounding from other family support programs. We exploit the discrete timing of Australia's Baby Bonus announcement in May 2004—a universal cash transfer of AUD 3,000 per birth—to overcome these identification challenges.

Our primary empirical strategy employs an Interrupted Time Series (ITS) design that leverages this sharp temporal discontinuity in policy exposure. The ITS approach is particularly compelling in this context because the policy's universal eligibility eliminates selection concerns, while the unexpected announcement and rapid implementation limit anticipatory behavioural responses. This design allows us to disentangle the policy's impact from smooth fertility trends by comparing birth patterns immediately before and after implementation, controlling for seasonal patterns and pre-existing trends.

A potential concern with our identification strategy is the lack of a true control group, since the Baby Bonus applied universally to all eligible Australians from a fixed implementation date. However, several factors support the validity of our approach during the study period. The Australian economy remained relatively stable throughout the policy implementation period, with GDP growth and employment rates showing steady patterns without major structural breaks that could confound our fertility estimates (Australian Bureau of Statistics, 2023c). Women's labour force participation remained stable, rising modestly from 56.3% to 57.6% between 2004 and 2006—a gradual change that our flexible time trends can accommodate. Similarly, while Australia has experienced long-term declines in marriage rates, no significant discontinuous changes in marriage patterns occurred during our study window that might spuriously correlate with the policy timing (Australian Bureau of Statistics, 2023c). Most importantly for our identification, comprehensive policy reviews confirm that no other major family support programs were introduced or substantially modified during the Baby Bonus implementation period, ensuring that our estimates isolate the specific effects of this cash transfer policy rather than broader changes in the family policy landscape.

To address potential concerns about specification sensitivity and external validity, we complement our main analysis with two additional approaches: a Regression Discontinuity in Time (RDiT) design that focuses on births in a narrow window around the policy introduction to minimise confounding from time-varying factors, and a Differences-in-Discontinuities framework that exploits New Zealand as a control to further isolate causal effects. Together, these methods provide a comprehensive assessment of how financial incentives shape fertility decisions, with each approach offering distinct advantages in addressing specific threats to identification.

A.4.1 Interrupted Time Series Analysis

Our ITS design exploits the discrete timing of policy announcements to identify causal effects on fertility. Following biological constraints, we expect policy-induced conception changes to manifest in birth data approximately nine months post-announcement. We therefore construct treatment indicators that account for this gestational lag.

Treatment Period Construction

We define five distinct policy regimes based on announcement dates listed in Table 1:

0. **Baseline** (January 2000–January 2003): Pre-policy period

1. **First Child Tax Refund** (February 2003–January 2005): maximum $2,500 refundable tax credit over 5 years
2. **Baby Bonus $3,000** (February 2005–January 2007): Universal cash transfer
3. **Baby Bonus $4,000** (February 2007–January 2009): Increased transfer amount
4. **Baby Bonus $5,000 + Means Test** (February 2009–December 2010): Higher payment with income test

Each period begins nine months after the policy announcement, capturing conception responses. We construct stepwise indicators where $D_j = 1$ for all observations in period j or later, then set $D_j = 0$ if the observation falls in any subsequent period. This specification identifies the incremental effect of each policy transition.

Empirical Specification

We estimate policy effects using monthly birth data disaggregated by maternal age group (a), nativity (n), and urban status (r):

$$\ln(Births_{anrt}) = \alpha + \sum_{j=1}^{4} \beta_j D_{jt} + f(t) + \delta_m + \mu_{anr} + \epsilon_{anrt} \tag{4}$$

where D_{jt} are the stepwise policy indicators, $f(t)$ represents flexible time trends (up to cubic polynomials), δ_m are month-of-year fixed effects controlling for seasonality, and μ_{anr} are the separate demographic group fixed effects. The coefficients β_j identify the average treatment effect of each policy transition on log daily births.

Our identification relies on the assumption that, conditional on smooth time trends and fixed effects, no other factors caused discontinuous changes in fertility precisely at policy transition dates. This assumption is plausible given that: (i) policy announcements were unexpected, limiting anticipation effects; (ii) no other major family policies changed at these exact dates; and (iii) macroeconomic conditions evolved smoothly during our sample period.

We assess robustness through several tests: varying the polynomial order of $f(t)$, implementing placebo tests at non-policy dates, and examining pre-trends in narrow windows before each transition. Standard errors are clustered at the calendar month level to account for serial correlation arising from persistent seasonal patterns and unobserved monthly shocks that affect fertility across years.

A.4.2 Robustness: Alternative Identification Strategies

We complement our ITS analysis with two additional approaches that address specific threats to identification.

Regression Discontinuity in Time

While ITS identifies policy effects through deviations from smooth trends, concerns remain about specification sensitivity and the influence of distant pre-policy observations. We therefore implement a Regression Discontinuity in Time (RDiT) design focusing on births in a narrow window around February 2005 (nine months post-announcement). Following Hausman and Rapson (2018), we estimate:

$$\ln(Births_t) = \alpha + \tau \cdot \mathbf{1}(t \geq t^*) + f(t - t^*) + g(t - t^*) \cdot \mathbf{1}(t \geq t^*) + \epsilon_t \tag{5}$$

where t^* denotes February 2005, and $f(\cdot)$ and $g(\cdot)$ are separate polynomial functions of time on either side of the threshold. The parameter τ identifies the local average treatment effect at the discontinuity.

We implement both parametric specifications with polynomials of varying order and non-parametric local linear regression using triangular kernels. This approach isolates the immediate policy impact while minimising contamination from long-term trends or distant observations.

Differences-in-Discontinuities Design

A remaining concern is that the observed discontinuity might reflect broader fertility shocks affecting similar countries. We address this using New Zealand as a control, implementing a differences-in-discontinuities design that nets out common trends.

New Zealand provides an ideal comparison: it shares similar demographic characteristics, fertility patterns, and economic cycles with Australia, but did not implement comparable pronatalist policies during our sample period. Both countries experienced similar pre-2004 fertility trends (correlation = 0.61 with p=value < 0.001) and comparable economic conditions.

We estimate:

$$\ln(Births_{ct}) = \alpha + \beta_1 AUS_c + \beta_2 POST_t + \beta_3 (AUS_c \times POST_t) + f_c(t) + \epsilon_{ct} \tag{6}$$

where c indexes country, $POST_t = 1(t \geq Feb2005)$, and $f_c(t)$ allows for country-specific polynomial trends. The coefficient β_3 identifies Australia's differential fertility response, purged of regional shocks.

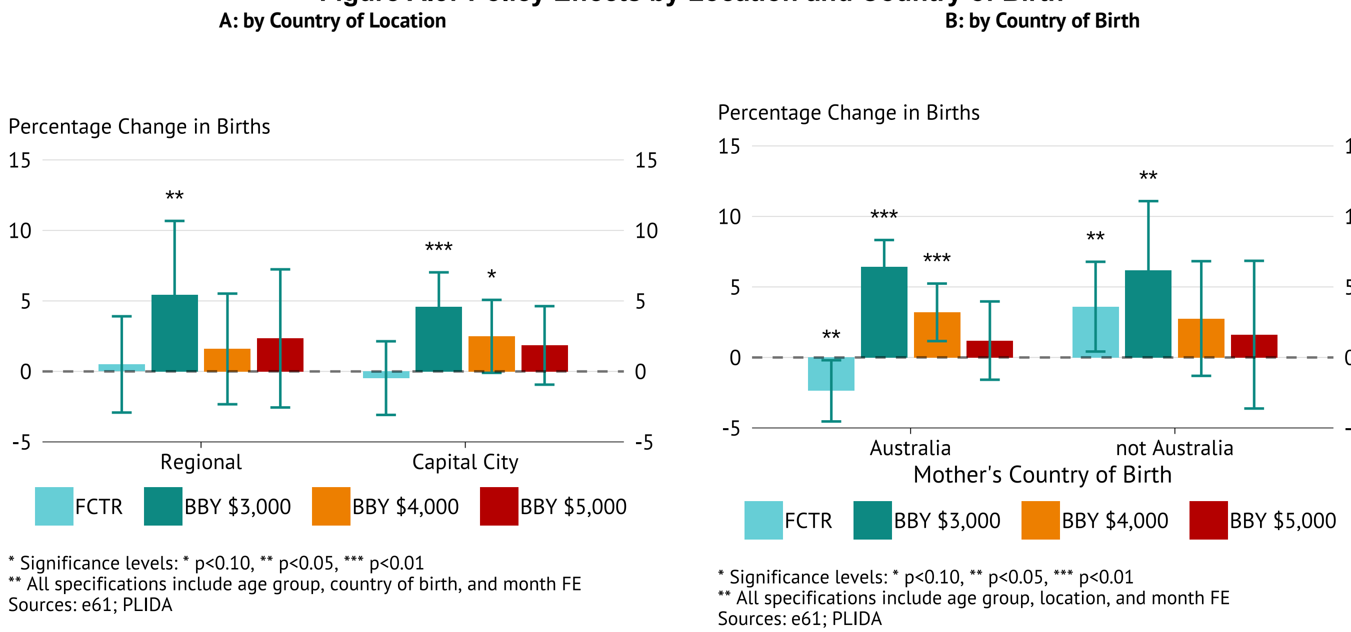
The identifying assumption is that absent the Baby Bonus, the fertility discontinuity in February 2005 would have been identical across both countries. We validate this parallel trends assumption by testing for differential pre-policy discontinuities at placebo dates. Standard errors are clustered by country-month to account for serial correlation.

A.5. Other Results

Effects by Location and Country of Birth

The Baby Bonus showed consistent effects across geographic areas, with no significant difference between capital cities and regional areas. This indicates the policy achieved a broad geographic reach rather than concentrating benefits in specific regions.

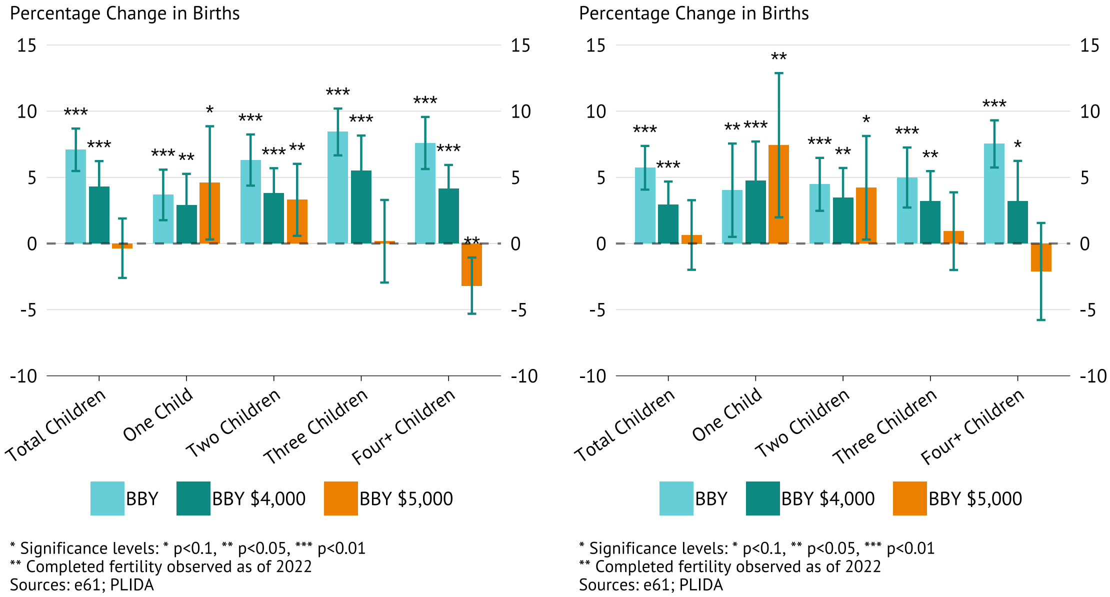
Figure A.3: Policy Effects by Location and Country of Birth



The First Child Tax Refund revealed a striking divergence: foreign-born mothers responded positively while Australian-born mothers showed negative effects. This counterintuitive pattern likely reflects the FCTR's design as a tax benefit requiring workforce participation. Foreign-born mothers may have had stronger labour force attachment or were more motivated to maintain employment to access the benefit, while Australian-born mothers may have reduced work hours after childbirth, making them less likely to benefit from tax-based incentives. The $4,000 policy increase showed modest demographic variation, with Australian-born mothers and those in capital cities being slightly more responsive. However, the $5,000 increase with means testing produced uniform effects across all groups. The lack of differential responses to means testing is particularly noteworthy, as it suggests income-based targeting did not enhance policy effectiveness among lower-income families as intended.

These heterogeneity patterns indicate the Baby Bonus did not have sizeable differences for nativity or location, suggesting the policy achieved relatively equitable effects across these demographic groups.

Figure A.4: Completed Fertility by 2022



A: All mothers

B: Mothers aged 42-51 in 2022

Effects of All Policy Periods on Completed Fertility

Figure A.4 reports the effects of each policy period on the number of children observed in 2022. Panel A (all mothers) shows that mothers who gave birth during the initial Baby Bonus exposure window had about 7.1% more children by 2022 than comparable pre-policy cohorts, with the gains concentrated at third-and-higher-order births.

Panel B restricts to mothers aged 42–51 in 2022—women largely at the end of their childbearing years—and the pattern persists. Cohorts exposed to the initial Baby Bonus and the subsequent $4,000 increase display higher total children than adjacent pre-policy cohorts, consistent with a genuine lift in completed fertility rather than a short-term timing shift.

Effects on the Share of Mothers by Completed Parity

The earlier results completed fertility allow us to count additional births. Those totals are mechanically sensitive to changes in the share of women who became mothers. To separate this composition effect from changes in family size, we examine the parity distribution among mothers, focusing on the shares who end with 1, 2, 3, or 4+ children.

Two features imply the estimates in Table A.1 are conservative. First, eligibility extended to women who gave birth before February 2005, who then faced incentives to progress to higher parity, which blurs a clean treated versus untreated comparison. Second, the policy regimes were phased in over time rather than introduced at a single sharp cutoff. Both features tend to attenuate the estimated effects toward zero.

Table A.1 shows that the Baby Bonus and the $4,000 increase raised the share of mothers who completed childbearing with three children, with offsetting declines concentrated among one- and two-child outcomes. The estimated increase is about 1.4 percentage points relative to the baseline. This pattern is consistent with the completed-fertility results in Section and provides additional evidence that the policy shifted the parity distribution upward for treated cohorts.

A.6. Robustness Checks

A.6.1 ITS - Birth Registry Data Analysis

Our main analysis employs individual-level data from the PLIDA survey, which provides unique advantages for examining fertility responses by tracking the same individuals over time and capturing detailed demographic characteristics essential for analysing both aggregate effects and heterogeneity across population subgroups.

Table A.1: ITS estimates of Baby Bonus effects on the share of mothers by completed parity

	(1) Share of One Child	(2) Share of Two Children	(3) Share of Three Children	(4) Share of Four+ Children
Tax Refund	0.001	-0.000	-0.000	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)
BBY	-0.003	-0.003	0.004**	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
BBY $4,000	-0.003*	-0.000	0.004*	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)
BBY $5,000	0.004*	0.008***	-0.002	-0.010***
+ means test	(0.002)	(0.002)	(0.003)	(0.002)
Observations	660	660	660	660
R-squared	0.98	0.96	0.94	0.95
Trend	Quadratic	Quadratic	Quadratic	Quadratic
Avg pre-BBY	0.09	0.36	0.28	0.27

Notes: OLS regressions using aggregated fertility data by parity. Dependent variables are the shares of births at each completed number of children level. Policy indicator variables capture conception effects of the First Child Tax Refund and the Baby Bonus policies. All regressions include quadratic time trends and seasonality controls. Standard errors clustered at the month level.

To assess the robustness of our findings and extend the analysis beyond our primary sample period, we re-estimate our models using aggregate birth counts from the Australian Data Explorer's Birth Registry. This administrative data source offers several complementary advantages: first, state-level disaggregation enables the inclusion of more flexible geographic controls and identification strategies; second, extended temporal coverage (2000-2021) allows us to examine additional policy periods including the transition to paid parental leave; third, linked population estimates permit construction of standardised birth rates per 100,000 women aged 15-49; and fourth, availability of state-level economic indicators enables more precise control for local labour market conditions.

Table A.2 presents results using this alternative data source. The estimates strongly corroborate our main findings while providing new insights about subsequent policy changes.

The initial Baby Bonus effect remains large and precisely estimated across all specifications. Using state-specific trends in the third column of Table A.2 (our preferred specification), we find an 8.5% increase in log births following the $3,000 Baby Bonus introduction—slightly larger than our main estimate of 6.3%. When expressed as daily birth rates per 100,000 women, representing an increase of 1.43 births per 100,000 women (an 8.4% increase from the average 17.12), highlighting the policy's substantial demographic impact.

The extended time series reveals important patterns in subsequent policy periods. The increase to $4,000 maintains a significant positive effect (4.5%, $p < 0.01$), while the introduction of means testing with the $5,000 payment shows no significant impact, consistent with eligibility restrictions offsetting the higher payment amount. Notably, the 2011-2014 period, when PPL operated alongside the Baby Bonus, showed renewed positive effects (2.2%, $p < 0.10$), suggesting complementarity between cash transfers and parental leave provisions. However, once the Baby Bonus was eliminated and only PPL remained (2014 onwards), fertility effects disappeared entirely.

The inclusion of state-level economic controls reveals that unemployment rates positively correlate with birth rates when using state-specific trends, possibly reflecting reduced opportunity costs of childbearing during economic downturns. Importantly, these controls do not attenuate our policy effect estimates, indicating that the Baby Bonus impacts operate independently of business cycle fluctuations.

To further validate our identification strategy, we implement a novel placebo test by artificially shifting all policy dates backward by exactly nine years. This maintains the seasonal timing and inter-policy spacing while testing whether our results might spuriously capture cyclical fertility patterns or specification artefacts. Table A.3 presents these placebo estimates for the 1992-2003 period. The results provide strong validation of our main findings: none of the placebo policy indicators show effects comparable to our actual estimates. The placebo "Baby Bonus at $3,000" shows a small negative coefficient (-1.5%) that is one-sixth the magnitude of the true effect and statistically insignificant in the preferred specification. The only marginally significant placebo effect appears for the FCTR (1.7%, $p < 0.10$), but this is an order of magnitude smaller than the 8.5% actual Baby Bonus effect.

Table A.2: Estimated Effects of Baby Bonus Policies using Birth Registry Data

	Aggregate Trends		State-Specific Trends	
	Log Births	Daily Birth Rate	Log Births	Daily Birth Rate
Tax Refund	-0.000	-0.058	0.003	0.011
	(0.008)	(0.126)	(0.005)	(0.084)
BBY - \$3,000	0.081***	1.350***	0.085***	1.428***
	(0.012)	(0.203)	(0.008)	(0.144)
BBY - \$4,000	0.047***	0.757***	0.045***	0.720***
	(0.008)	(0.132)	(0.008)	(0.138)
BBY - \$5,000 + means test	0.001	-0.000	-0.002	-0.065
	(0.009)	(0.160)	(0.013)	(0.214)
PPL + BBY	0.020**	0.293**	0.022*	0.322
	(0.007)	(0.120)	(0.010)	(0.170)
PPL Only	0.002	-0.003	0.002	-0.004
	(0.008)	(0.135)	(0.009)	(0.156)
Employment Ratio (lagged)	-0.004	-0.067	0.003	0.052
	(0.004)	(0.066)	(0.003)	(0.050)
Unemployment Rate (lagged)	-0.003	-0.066	0.005*	0.082*
	(0.003)	(0.051)	(0.002)	(0.043)
Observations	2112	2112	2112	2112
R^2	0.998	0.717	0.998	0.776
Outcome avg pre-BBY	21,133	17.12	21,133	17.12
Trend Specification	Quadratic	Quadratic	State Quadratic	State Quadratic

Notes: OLS regressions using monthly Birth Registry data from 01/2000 to 12/2021. The dependent variable in Columns (1) and (3) is the logarithm of the number of births, while Columns (2) and (4) use the daily birth rate. Columns (1)–(2) include aggregate quadratic time trends, while Columns (3)–(4) include state-specific quadratic time trends as indicated. The policy indicator variables capture the birth effects of sequential Australian family policy reforms: the First Child Tax Refund (2003-2004), the Baby Bonus at \$3,000 (2005-2007), \$4,000 (2007-2009), and \$5,000 with means testing (2009-2010), followed by the combined Paid Parental Leave and Baby Bonus period (2011-2014), and finally the PPL-only period (2014 onwards). The initial reference period is pre-First Child Tax Refund (01/2000-10/2002). All regressions include month and state fixed effects, plus controls for 9 months lagged employment ratio, unemployment rate, and log women population aged 15-49. Standard errors are clustered at the state level.

Table A.3: Placebo Effects of Baby Bonus Policies using Birth Registry Data

	Aggregate Trends		State-Specific Trends	
	Log Births	Daily Birth Rate	Log Births	Daily Birth Rate
Tax Refund	0.017*	0.353*	0.016	0.330*
	(0.009)	(0.166)	(0.009)	(0.165)
BBY - \$3,000	-0.019*	-0.266	-0.015	-0.193
	(0.010)	(0.156)	(0.010)	(0.157)
BBY - \$4,000	0.009	0.160	0.006	0.092
	(0.007)	(0.127)	(0.007)	(0.123)
Employment Ratio (lagged)	0.003	0.046	0.001	0.016
	(0.002)	(0.050)	(0.002)	(0.051)
Unemployment Rate (lagged)	0.003*	0.061	0.003	0.042
	(0.002)	(0.038)	(0.002)	(0.043)
Observations	1000	1000	1000	1000
R^2	0.999	0.851	0.999	0.858
Trend Specification	Quadratic	Quadratic	State Quadratic	State Quadratic

Notes: OLS regressions using monthly data from 01/1992 to 01/2003. The dependent variable in Columns (1) and (3) is the logarithm of the number of births, while Columns (2) and (4) use the daily birth rate. Columns (1)–(2) include aggregate quadratic time trends, while Columns (3)–(4) include state-specific quadratic time trends as indicated. The policy indicator variables capture the placebo birth effects of sequential Australian family policy reforms: the First Child Tax Refund (1994-1995), the Baby Bonus at \$3,000 (1996-1998), and \$4,000 (1999-2001) The reference period is pre-First Child Tax Refund (1992-1994). All regressions include month and state fixed effects, plus controls for 9 months lagged employment ratio, unemployment rate, and log women population aged 15-49. Standard errors are clustered at the month level.

These placebo results strengthen our causal interpretation in two ways. First, they demonstrate that our large Baby Bonus effects are not artefacts of the functional form or seasonal patterns in fertility. Second, they show that major fertility increases do not randomly appear at arbitrary nine-year intervals, confirming that the 2004-2005 discontinuity represents a unique response to policy intervention.

Together, the alternative data source analysis and placebo tests provide robust validation of our main findings. The Baby Bonus generated substantial, lasting increases in Australian fertility that cannot be attributed to data peculiarities, specification choices, or cyclical patterns in childbearing.

A.6.2 Regression Discontinuity in Time

While our interrupted time series analysis provides robust evidence of the Baby Bonus's fertility effects, we complement this approach with a Regression Discontinuity in Time (RDiT) design. This methodology, following Hausman and Rapson (2018), focuses on births in a narrow window around the policy discontinuity, thereby minimising concerns about confounding from long-term trends or distant observations.

The RDiT approach exploits the sharp temporal discontinuity created by the Baby Bonus announcement in May 2004. Given the biological constraint of human gestation, we expect policy-induced conceptions to manifest in birth data approximately nine months later, in February 2005. This precise timing allows us to implement a quasi-experimental design that compares births immediately before and after this threshold.

Table A.4 presents our RDiT estimates (following equation 5) using various specifications and bandwidths. Our preferred specification (Column 2, also see Figure A.5) employs a linear polynomial with an 18-month bandwidth, yielding a highly significant effect of 6.1%. This estimate is remarkably consistent with our main ITS finding of 6.3%, strengthening confidence in the causal interpretation.

Table A.4: RDiT Estimates of Baby Bonus Effect on Birth Rates

	Dependent Variable: Log Total Births				
	(1) Linear	(2) Linear	(3) Quadratic	(4) Triangular	(5) Linear
Treatment	0.039	0.061***	0.055**	11.376***	0.060***
	(0.026)	(0.015)	(0.023)	(2.822)	(0.013)
Observations	25	37	37	35	35
Bandwidth (months)	12	18	18	18	18
Polynomial order	1	1	2	1	1
Kernel	Uniform	Uniform	Uniform	Triangular	Uniform
Donut hole	No	No	No	No	Yes

Notes: The dependent variable is log total births in columns (1)-(3) and (5), and year-over-year percentage change in births in column (4). Column (1) uses a linear specification with bandwidth = 12 months. Columns (2), (3) and (5) use bandwidth = 18 months with linear and quadratic specifications. Column (4) uses a local linear specification with triangular kernel weights and bandwidth = 18 months and Column (5) drops the observations around the policy implementation months. All models include month-of-year fixed effects and control for the running variable interacted with treatment status. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Several features of these results merit emphasis. First, the stability of estimates across different bandwidths demonstrates that our findings are not sensitive to the choice of local window. The effect ranges from 3.9% with a 12-month bandwidth to 6.1% with an 18-month bandwidth, with all estimates statistically significant at conventional levels. This pattern suggests that the discontinuity is genuine rather than an artefact of specification choices.

Second, the quadratic specification (Column 3) yields nearly identical results to the linear model, indicating that higher-order polynomials do not substantially alter our conclusions. The slight reduction in the point estimate (5.5% vs. 6.1%) likely reflects the additional flexibility allowing the model to capture pre-existing nonlinear trends, but the effect remains economically and statistically significant.

Third, our triangular kernel-weighted estimates (Column 4), which give greater weight to observations closer to the threshold, produce a substantially larger effect when measured in year-over-year percentage changes (11.4%). This specification addresses potential concerns about observations far from the discontinuity unduly influencing our estimates.

A unique advantage of our administrative data is the ability to examine RDiT effects by birth parity, providing insights into which families responded most strongly to the policy. Table A.5 reveals striking heterogeneity across birth orders. The RDiT estimates by parity show that all birth orders experienced significant increases, but with important variation in magnitude. First births increased by 5.0%, second births by 7.1%, third births by 5.9%, and fourth or higher-order births by 8.2%. The particularly strong response for higher-order births provides compelling evidence of quantum effects—families having additional children they would not have had otherwise—rather than mere timing shifts.

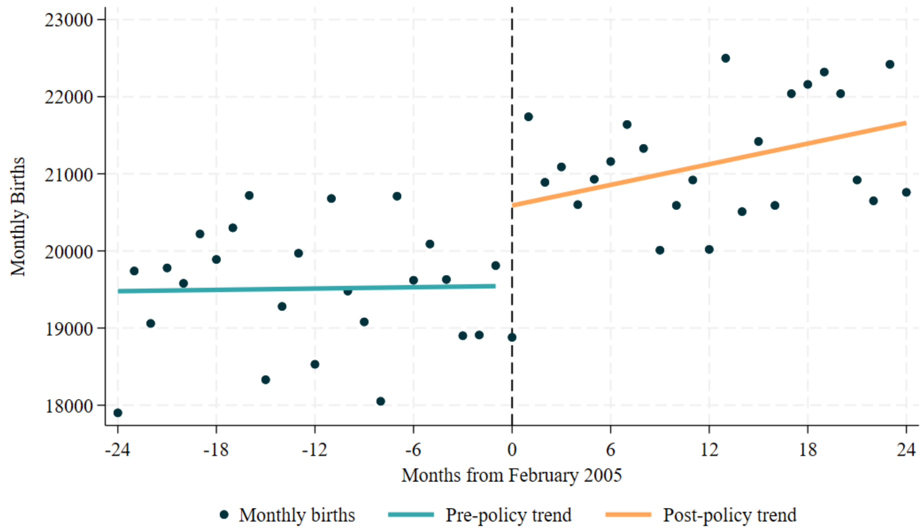


Figure A.5: RDiT: The effect of BBY on births

Table A.5: RDIT: The effect of Baby Bonus on Birth Parities

	(1) All Births	(2) First	(3) Second	(4) Third	(5) Fourth+
BBY	0.061***	0.050***	0.071***	0.059***	0.082***
	(0.015)	(0.014)	(0.019)	(0.020)	(0.019)
N	37	37	37	37	37
Birth Order	All	1	2	3	4+

Notes: The dependent variable is log births. All regressions use a linear specification with bandwidth = 18 months and are restricted to births within the specified parity group. Birth parity groups are defined as first birth, second birth, third birth, and fourth or higher birth. All models include month-of-year fixed effects and control for the running variable interacted with treatment status. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

To validate our RDiT estimates, we conduct several robustness checks. First, we implement a "donut RD" specification that excludes observations immediately adjacent to the threshold (June 2004 and July 2004). This approach, recommended by Barreca et al. (2011), addresses concerns about strategic manipulation of birth timing documented by Gans and Leigh (2009). Column (5) in Table A.4 shows that the donut specification yields virtually identical results, confirming that short-term birth shifting does not contaminate our conception-based estimates.

Second, we test sensitivity to bandwidth selection by estimating effects across a range of bandwidths from 12 to 30 months. Figure A.6 plots these estimates with 95% confidence intervals, showing remarkable stability across different windows. The effect remains statistically significant and economically meaningful regardless of bandwidth choice, though precision naturally decreases with narrower windows.

To further validate our design, we implement placebo RDiT analyses at false discontinuity dates. Specifically, we test for discontinuities 18 months before the actual policy (August 2003) when no policy change occurred. The placebo analysis yields a precisely estimated null effect, confirming that our main results capture a unique policy-induced discontinuity rather than a general feature of the data. Figure A.7 shows estimates from several placebo dates for the policy.

The RDiT results provide strong causal evidence that the Baby Bonus increased fertility by approximately 6%, with effects concentrated among higher-parity births. The consistency of this estimate across multiple specifications, bandwidths, and

Figure A.6: RDiT estimates based on different bandwidth windows

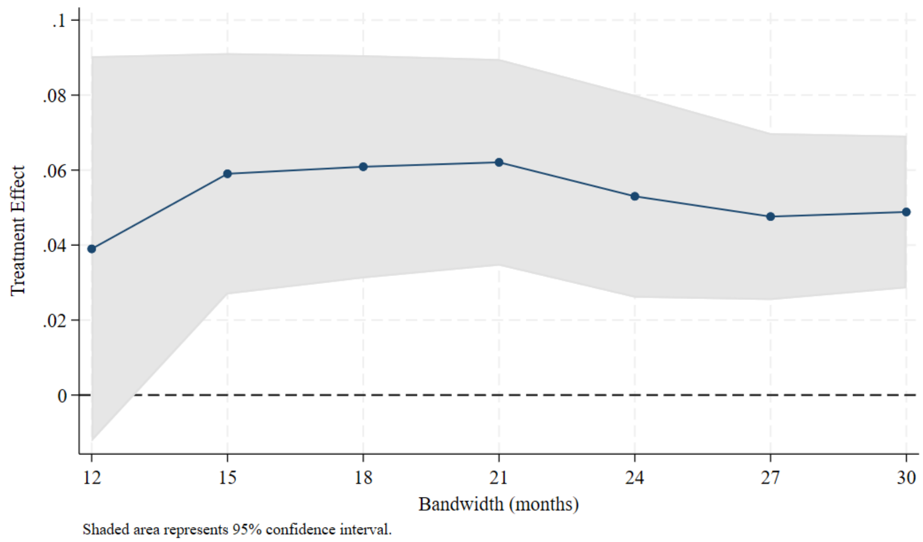
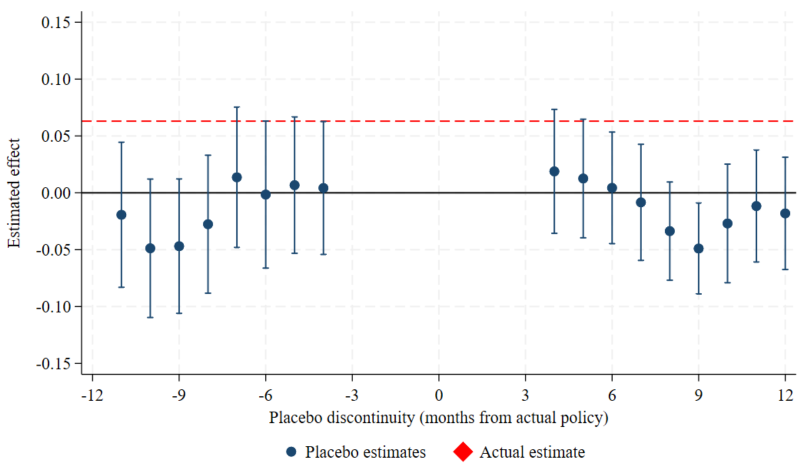


Figure A.7: RDiT: placebo BBY dates on births



identification strategies reinforces our conclusion that financial incentives can meaningfully influence both the timing and quantum of fertility decisions.

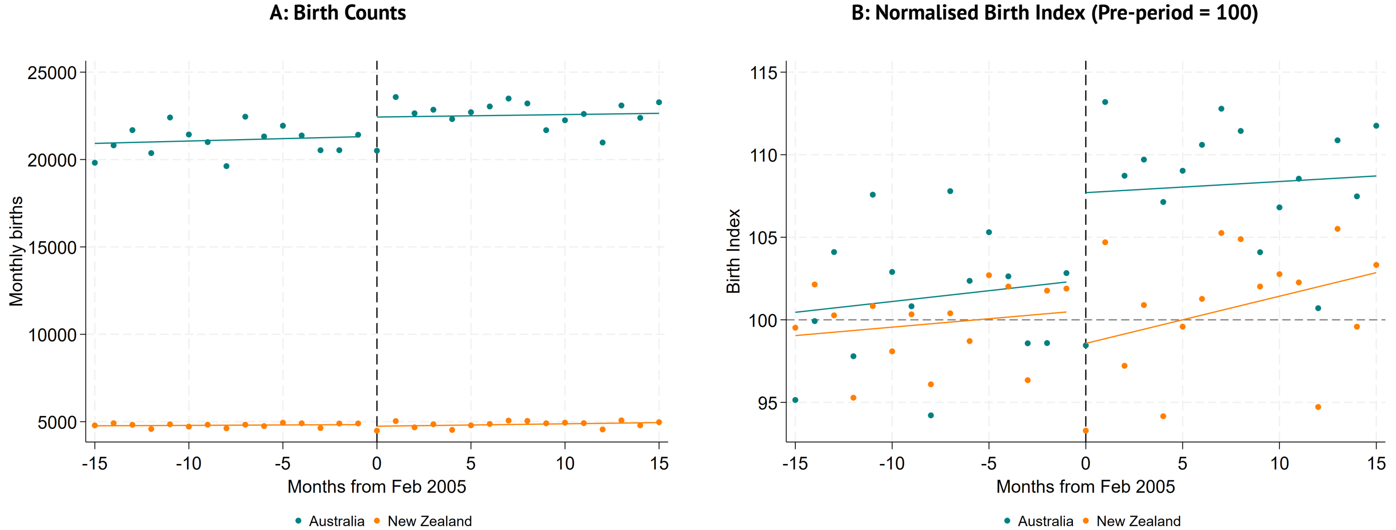
The local nature of RDiT estimates is both a strength and limitation. While the approach provides cleaner identification by focusing on a narrow window around the policy change, it captures only the immediate effect rather than the full dynamic response. Our ITS analysis suggests that fertility effects persisted and even grew over time, particularly following subsequent payment increases. Nevertheless, the RDiT estimates provide crucial validation that the initial discontinuity represents a genuine causal effect rather than a statistical artefact or confounding trend.

A.6.3 Differences-in-Discontinuities

A key concern with our ITS estimates is that the observed fertility increase might reflect broader trends affecting similar countries rather than the causal effect of Australia's Baby Bonus. To address this, following equation 6 we implement a differences-in-discontinuities design using New Zealand as a control country.

Figure A.8 presents visual evidence of the differential fertility response. Panel A displays raw monthly birth counts for both countries centred at February 2005—the month when conceptions responding to the Baby Bonus announcement would first appear in birth data. While the absolute difference appears modest given Australia's larger population, Panel B normalises both series to their pre-policy means (set to 100), revealing the economic magnitude of the discontinuity.

Figure A.8: Birth Discontinuity: Australia vs New Zealand



Notes: Monthly births centred at February 2005, when births would first reflect conceptions following the May 2004 Baby Bonus announcement. Sample includes ±15 months. Panel B normalises each country's births to 100 based on the average of months -3 to -1. Fitted lines from separate linear regressions on each side of the discontinuity.

The normalised panel strikingly illustrates our key finding: Australian births jump approximately 7% at precisely the expected date, while New Zealand births continue their smooth evolution. This visual evidence strongly suggests a causal policy effect rather than a regional fertility shock.

Table A.6 presents formal regression estimates. Column 1 shows that Australian births increased by 1,197 relative to New Zealand after February 2005. Given Australia's pre-policy mean of 21,115 monthly births, this represents a 5.7% increase. The log specification in Column 2 indicates a 7.0% differential effect—remarkably consistent with our main ITS estimate of 6.3%.

Table A.6: Differences-in-Discontinuities Estimates

	(1) Births	(2) Log(Births)
Australia × Post-Feb 2005	1,197.181***	0.070***
	(344.217)	(0.020)
Country FE	Yes	Yes
Post-Feb 2005 FE	Yes	Yes
Country-specific trends	Yes	Yes
Month-of-year FE	Yes	Yes
Observations	62	62
R^2	0.998	0.999
Pre-period AU mean	21,115	–

Notes: Sample includes monthly births ±15 months around February 2005. The bandwidth is chosen to maximise observations while avoiding contamination from Australia's subsequent policy change at month 24. Country-specific linear trends are estimated separately for pre- and post-discontinuity periods. Robust standard errors in parentheses. *** p<0.01.

The validity of this approach rests on the parallel trends assumption—that absent the Baby Bonus, fertility patterns would have evolved similarly in both countries. Several factors support this assumption. First, as clearly visible in Figure A.8, the pre-policy trends are strikingly parallel, with both countries showing similar fertility trajectories in the 15 months before February 2005. Second, the two countries exhibited strongly correlated fertility trends over the longer 2000-2004 period (r = 0.61). Third, New Zealand implemented no comparable fertility policies during our sample period. Fourth, both countries share similar demographic structures, cultural backgrounds, and economic cycles.

The differences-in-discontinuities estimates thus provide compelling validation of our main results. The Baby Bonus generated a substantial, immediate increase in Australian fertility that cannot be attributed to regional trends or global shocks affecting comparable countries.

Disclaimer

Person Level Integrated Data Asset (PLIDA)

The results of these studies are based, in part, on data supplied to the ABS under the Taxation Administration Act 1953, A New Tax System (Australian Business Number) Act 1999, Australian Border Force Act 2015, Social Security (Administration) Act 1999, A New Tax System (Family Assistance) (Administration) Act 1999, Paid Parental Leave Act 2010 and/or the Student Assistance Act 1973. Such data may only be used for the purpose of administering the Census and Statistics Act 1905 or performance of functions of the ABS as set out in section 6 of the Australian Bureau of Statistics Act 1975. No individual information collected under the Census and Statistics Act 1905 is provided back to custodians for administrative or regulatory purposes. Any discussion of data limitations or weaknesses is in the context of using the data for statistical purposes and is not related to the ability of the data to support the Australian Taxation Office, Australian Business Register, Department of Social Services and/or Department of Home Affairs' core operational requirements.

Legislative requirements to ensure privacy and secrecy of these data have been followed. For access to PLIDA and/or BLADE data under Section 16A of the ABS Act 1975 or enabled by section 15 of the Census and Statistics (Information Release and Access) Determination 2018, source data are de-identified and so data about specific individuals has not been viewed in conducting this analysis. In accordance with the Census and Statistics Act 1905, results have been treated where necessary to ensure that they are not likely to enable identification of a particular person or organisation.