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FUTURE OF BONUS BABIES: EARLY ADULTHOOD OUTCOMES OF AUSTRALIA'S BABY BONUS CHILDREN

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Australia's Baby Bonus, announced in May 2004 and implemented from July 2004, provided a $3,000 payment for each newborn child, aiming to support families and boost fertility rates. In a previous note, Akyol and Vergili (2025) show that the policy increased births, concentrated among mothers with low or no taxable income. Twenty years later, these "bonus babies" are now young adults, raising a natural follow-up question: what happened to the children who were born as a result of this cash transfer?

This note tracks children born around February 2005 — the point after which births could first reflect conceptions made in response to the Baby Bonus announcement — and compares those born after February 2005 to those born just before, who would have arrived regardless of the policy. Using linked Australian administrative data, we follow both groups through early adulthood across education, labour market engagement, income support receipt, and disability support.

Our results show:

- Children born from February 2005 onward show no clear differences from those born just before in terms of their rates of high school dropout, engagement in education, training or employment, income support receipt, and NDIS participation.
- Two background measures, community housing and equivalised family income, also do not show clear differences at the February 2005 cutoff, although community housing rates are slightly higher for children born just after February 2005.

Across every outcome we examined, we find no evidence that the additional children born because of the Baby Bonus experienced worse early-adult trajectories than children who would have been born regardless of the policy.

Australia's total fertility rate has fallen to a historic low of 1.48 births per woman, and further declines are projected (Centre for Population, 2026). This has renewed policy interest in measures that lower the cost of having children. A central question in this debate is not only whether such policies increase births but also whether the additional children thrive.

Previous e61 work shows that the Baby Bonus led to a genuine increase in births, concentrated among mothers with low or no taxable income and among women with lower levels of education. These findings raise an important follow-on question: if the policy disproportionately encouraged births among disadvantaged families, did those children face worse outcomes when they grew up? Addressing this question is directly relevant to the design of future pronatalist policies, which must balance fertility objectives with equity and long-run fiscal sustainability.

The Baby Bonus was announced in May 2004. Since pregnancies take approximately nine months, the first births plausibly conceived in response would arrive around February 2005. Therefore, this note compares children born from February 2005 onward to children born just before that date, who would have been born regardless of the policy. We track both groups using Australia's Person Level Integration Data Asset (PLIDA), which links individual records across Centrelink welfare records, the National Disability Insurance Agency (NDIA), and the 2021 Census. We compare children born in the five months around this date—from September 2004 to June 2005—looking for a step change in outcomes at the February 2005 threshold.[1] We

1 This narrow window is deliberate. One important reason is school starting age. Australian states set different cutoff dates for school entry—ranging from 1 January in Tasmania to 31 July in NSW, with most states (QLD, WA, NT) using 30 June and VIC and ACT using 30 April. Within our window, children in NSW, QLD, WA, and the NT all fall into the same school cohort regardless of which side of February 2005 they are on. In Victoria and the ACT, however, children born in May or June 2005 miss the 30 April cutoff and start school a year later than those born just before—potentially placing them at a different stage of schooling when outcomes are measured at the 2021 Census. Keeping the window to five months limits this problem. A wider window would also risk introducing cohort differences driven by broader economic or social changes unrelated to the Baby Bonus.

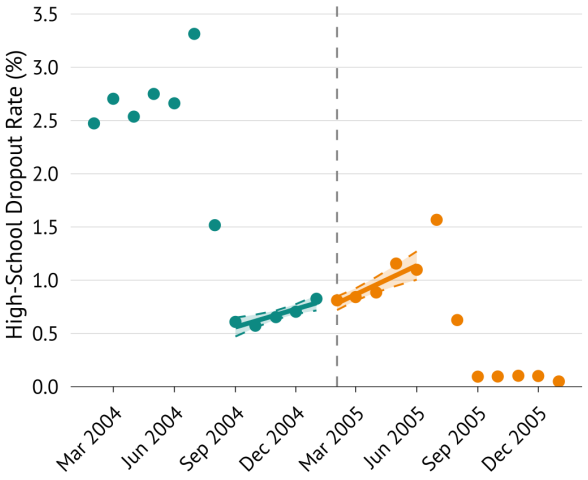
account for smooth underlying trends on either side of the February 2005 cutoff, so that our results capture any discrete jump at February 2005 rather than gradual changes over time.[2] To give a broader visual picture, we also present patterns across the full twelve-month window around February 2005.

Main results

High school completion

A first indicator of human capital accumulation is whether children complete secondary school. Children born just before and after the February 2005 cutoff benefited from the Baby Bonus payment. Those born just after may come from slightly more disadvantaged backgrounds. Even so, they are not more likely to leave school early than children born just before (Figure 1).[3]

Figure 1: Share Who Dropped Out of High School by Birth Month



* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

Education and labour market engagement

Beyond formal schooling, we examine whether young adults are engaged in education, training, or employment (fully engaged)—a broader measure of productive participation that captures early attachment to the labour market.

Rates of full engagement and disengagement vary across birth cohorts in ways that track common seasonal and cohort patterns, but there is no step change at February 2005 (Figures 2A and 2B). Children born just after February 2005 are neither more nor less engaged in education and work than those born just before.

Income support receipt

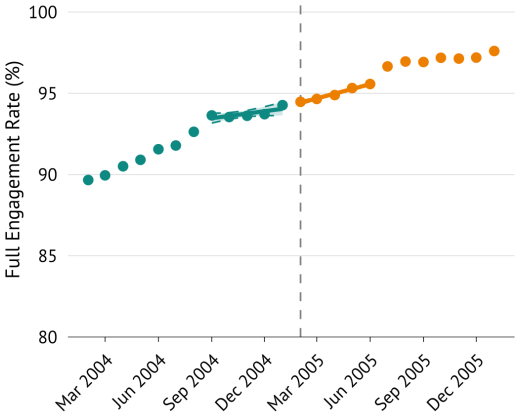
A common concern about pronatalist cash transfers is that they may encourage births in families with greater reliance on income support, raising questions about intergenerational patterns of income support receipt. This concern is particularly relevant here, given that children born just after the February 2005 cutoff come from families that appear slightly more disadvantaged on some measures.

Figure 3 presents patterns on whether Bonus Babies are more likely to access income support as young adults. Panel A focuses on receipt from age 16 onward—the age at which young people in Australia can independently claim income support such

2 We implement robustness checks across a range of comparison windows (Appendix A.2).
3 The sharp changes in dropout rates visible around June and September reflect school entry cutoff effects. In Australia, school entry dates vary by state, meaning children born around these months may have started school a full year apart despite being born only months apart. This creates discontinuities in educational attainment that are not related to the Baby Bonus policy.

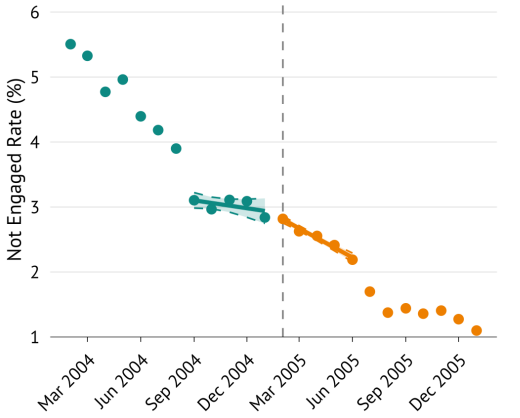
Figure 2: Engagement in Education, Training or Employment by Birth Month

A: Share who are fully engaged



* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

B: Share who are not engaged

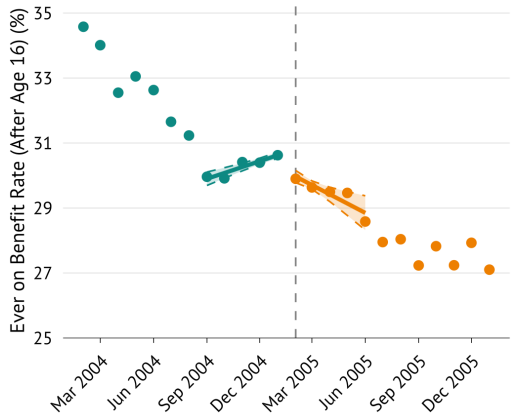


* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

as Youth Allowance—while Panel B captures receipt after age 18, when income support receipt would reflect labour market and education transitions rather than family circumstances. Both panels show a clear downward trend across birth months, reflecting broader cohort patterns, but no discontinuity at February 2005. Bonus Babies are no more likely — and if anything slightly less likely — to receive income support as young adults than children born just before February 2005.

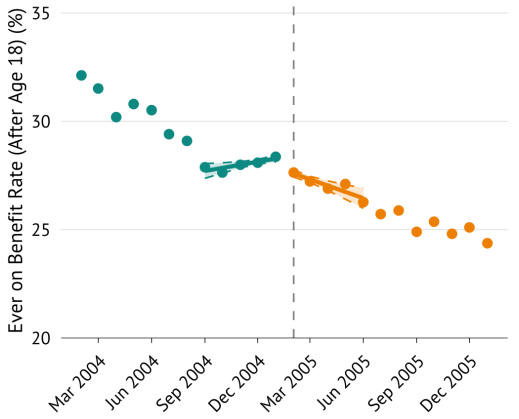
Figure 3: Income Support Receipt by Birth Month

A: Share ever on benefit (age 16 and over)



* Welfare data sourced from PLIDA, with the last observation from June 2025. Bands: robust 95% CIs.
Sources: e61 Institute; PLIDA

B: Share ever on benefit (age 18 and over)



* Welfare data sourced from PLIDA, with the last observation from June 2025. Bands: robust 95% CIs.
Sources: e61 Institute; PLIDA

Participation in the National Disability Insurance Scheme (NDIS)

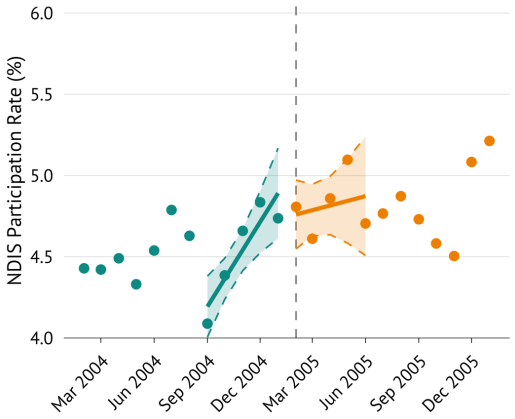
A separate concern is whether children born just after February 2005 might have higher rates of disability or developmental conditions—whether due to the characteristics of families who responded to the policy, or to constrained resources in early childhood—and so be more likely to access the NDIS, or to access it with greater need.[4] Around 4 to 5 percent of children in these cohorts are NDIS participants, with an average severity score of around 7.3 on a 15-point scale where 15 indicates the highest support needs. Neither of these measures shows any difference at the February 2005 threshold (Figures 4 and

4 The latter channel is most plausible for environmentally responsive conditions such as language delays or mental health, rather than the primarily genetic or neurological conditions like autism and intellectual disability that dominate the NDIS caseload at these ages.

A.6),[5] and self-reported long-term mental health conditions from the 2021 Census, presented in Appendix A.8, are likewise smooth across the threshold.

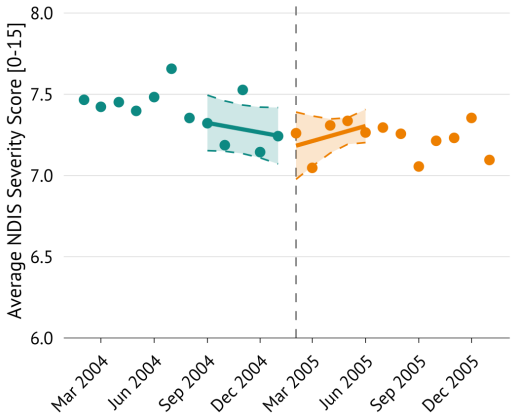
Figure 4: NDIS Outcomes by Birth Month

A: Share of NDIS participants



* NDIS data sourced from PLIDA, with the last observation from June 2025. Bands: robust 95% CIs.
Sources: e61 Institute; PLIDA

B: Average severity score (0–15)



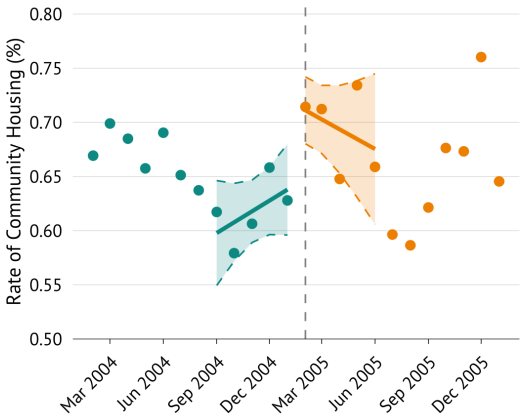
* NDIS data sourced from PLIDA, with the last observation from June 2025. Bands: robust 95% CIs.
Sources: e61 Institute; PLIDA

Housing and Family Income

Two background measures do not show clear differences at the February 2005 cutoff. Community housing rates are slightly higher for children born just after February 2005 (Figure 5A), though the confidence bands overlap across the cutoff and the estimate is sensitive to bandwidth. Equivalised family income shows no visible shift at the cutoff (Figure 5B). Neither measure points to a clear discontinuity when we focus on the months closest to the cutoff.

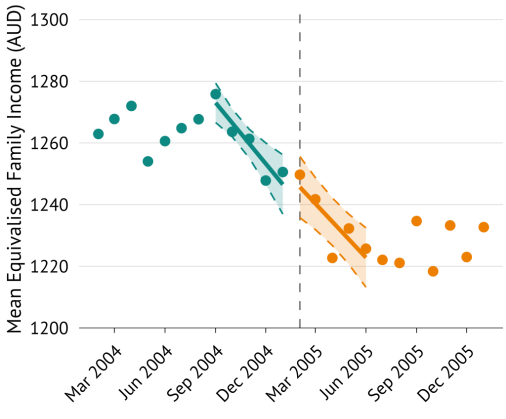
Figure 5: Community Housing and Equivalised Family Income

A: Share Living in Community Housing by Birth Month



* Census data from the ABS 2021 Census conducted in August 2021. Bands: Bonferroni 97.5% CIs.
Sources: Census 2021; e61 Institute

B: Equivalised Family Income (Weekly)



* Income data from the Census 2021. Equivalised income adjusts for household size using the OECD-modified equivalence scale. Bands: Bonferroni 97.5% CIs.
Sources: Census 2021; e61 Institute

5 NDIS application rates—capturing a broader measure of contact with the scheme that includes unsuccessful applications—are presented in Appendix A.7 and confirm this null finding.

Conclusion and Policy Discussion

The evidence across all the outcomes we examined paints a clear picture: children born as a result of the Baby Bonus do not appear to have experienced different early-adult trajectories from children born just before the policy cutoff. This holds even though the policy encouraged more births among lower-income families.

A payment that successfully increased fertility among disadvantaged families did not lead to higher rates of school dropout, lower engagement in work or study, greater receipt of income support, or higher disability support needs in early adulthood. The Baby Bonus appears to have increased births among more disadvantaged families without leading to any clear evidence of worse early-adult outcomes or greater use of government support, at least over the first two decades of life.

Children born just after February 2005 may come from slightly more disadvantaged backgrounds, based on the direction of the community housing pattern, though this finding is not statistically definitive and equivalised family income shows no corresponding shift. If such differences were to persist, their effects may accumulate over time or become more visible when this generation forms families of its own. Tracking these cohorts into their late twenties and thirties, particularly through the transitions of sustained employment and parenthood, will be important for a fuller assessment of the policy's longer-run legacy.

References

Akyol, P., & Vergili, A. (2025). How financial incentives shape fertility in Australia. https://e61.in/how-financial-incentives-shape-fertility-in-australia/

Centre for Population. (2026). *2025 population statement*. Australian Government. https://population.gov.au/publications/statements/2025-population-statement

A. Appendix

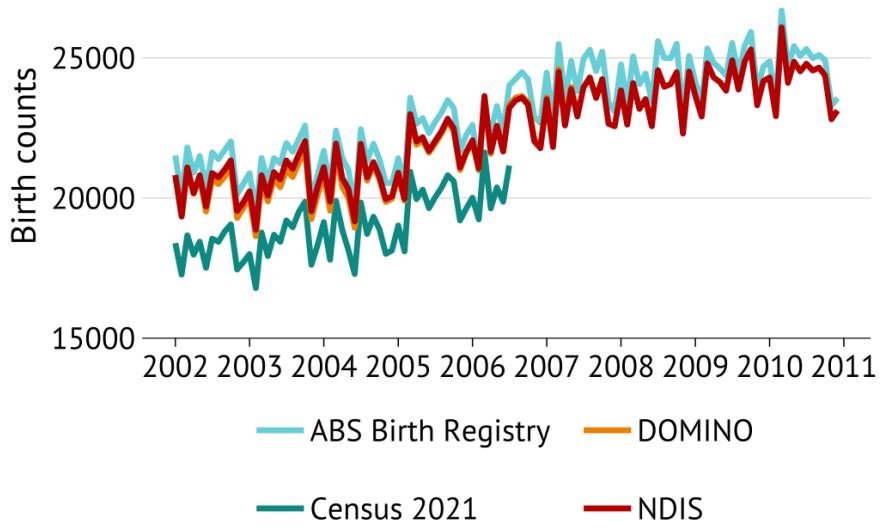
A.1 Data Source Validation

Figure A.1 plots monthly birth counts from each of the four data sources used in this note — the ABS Birth Registry, DOMINO (perinatal records), 2021 Census, and NDIS administrative records — over the period 2002 to 2011.

The ABS Birth Registry serves as the benchmark. DOMINO and NDIS track the registry closely throughout, confirming that both sources achieve near-complete coverage of births with no unusual gaps, spikes, or differential trends around the February 2005 Baby Bonus cutoff. This is reassuring for our identification strategy: any discontinuity we estimate in outcomes derived from these sources reflects a genuine change in the population at the cutoff, not a change in administrative coverage.

The 2021 Census count is structurally lower than the other sources, as expected. The Census captures individuals present in Australia on census night in August 2021. Children born in Australia who subsequently emigrated, or who were otherwise absent on census night, are excluded. This coverage gap is stable over time and does not vary discontinuously around the February 2005 cutoff, so it does not threaten our Census-based estimates. For Census outcomes — including SEIFA, housing tenure, and school engagement — our estimates should be interpreted as applying to the share of the bonus baby cohort that remained resident in Australia until 2021.

Figure A.1: Birth Counts by Data Source, 2002--2011



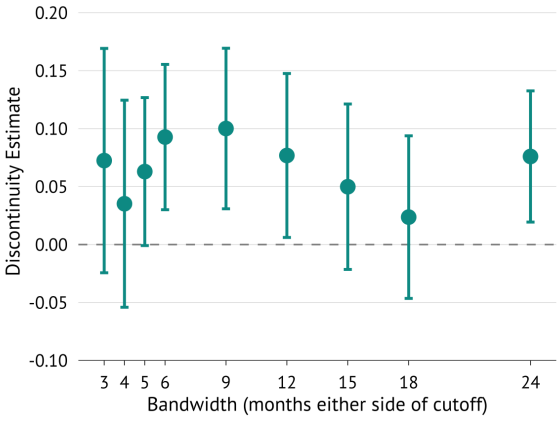
Source: ABS Birth Registry, Census 2021, NDIS, DOMINO, e61 Institute

A.2 Bandwidth Sensitivity

Each figure below replicates the main-body regression discontinuity estimates across different bandwidths either side of the February 2005 cutoff. Each point is the coefficient on the post-cutoff indicator from a local linear regression with separate slopes on each side of the threshold, with heteroscedasticity-robust 95% confidence intervals. For community housing and equivalised family income, confidence intervals are Bonferroni-adjusted (97.5%) to account for testing across two background measures. Stable estimates across bandwidths support a causal interpretation; estimates that move substantially at wider bandwidths may reflect cohort trends unrelated to the Baby Bonus.

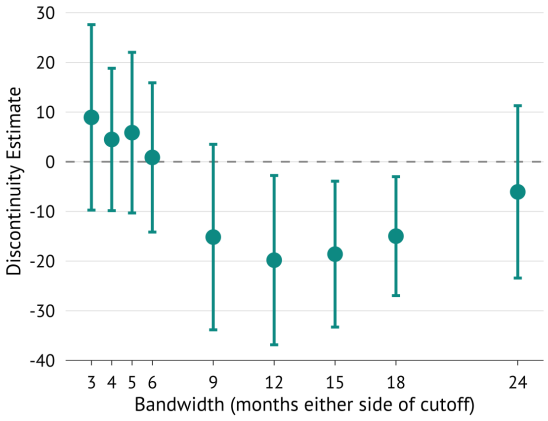
Figure A.2: Bandwidth Sensitivity: Community Housing and Equivalised Family Income

A: Community housing rate



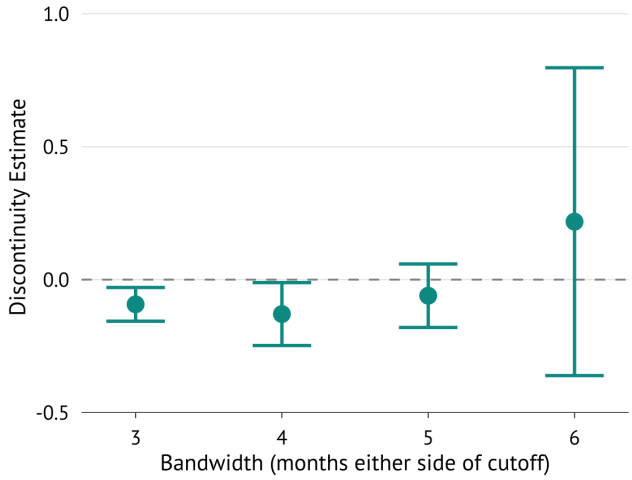
* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Bonferroni-adjusted 97.5% CIs.
Sources: Census 2021; e61 Institute

B: Mean equivalised family income



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Bonferroni-adjusted 97.5% CIs.
Sources: Census 2021; e61 Institute

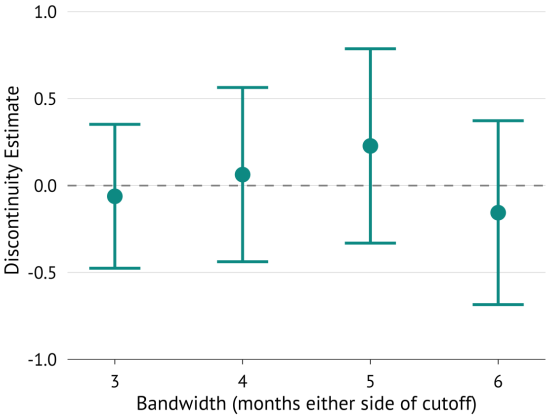
Figure A.3: Bandwidth Sensitivity: High School Dropout



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: Census 2021; e61 Institute

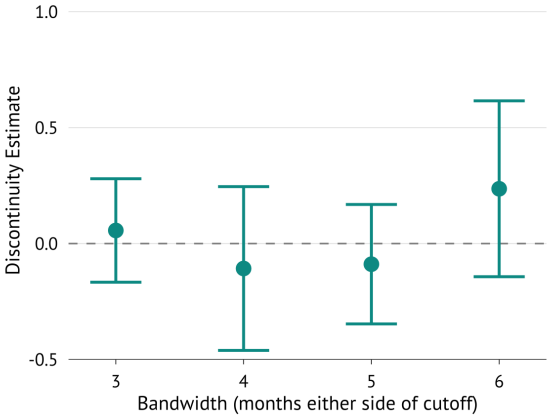
Figure A.4: Bandwidth Sensitivity: Education and Employment Engagement

A: Fully engaged rate



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: Census 2021; e61 Institute

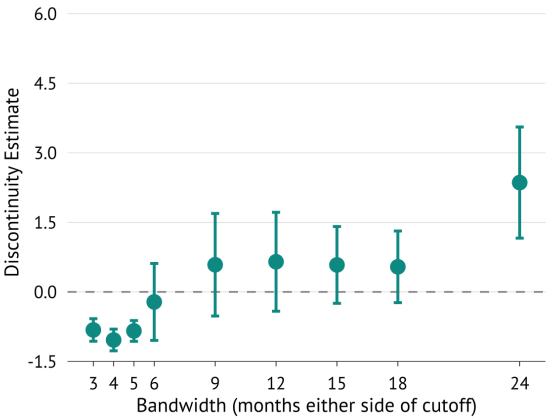
B: Not engaged rate



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: Census 2021; e61 Institute

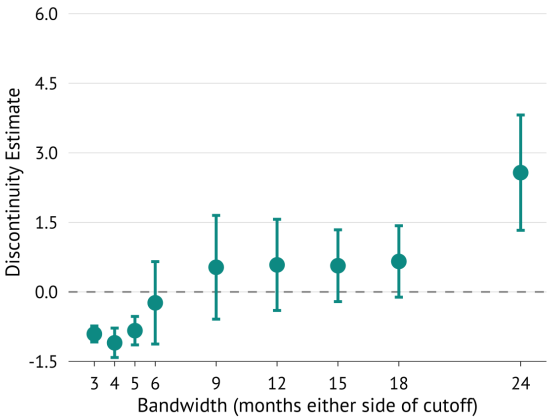
Figure A.5: Bandwidth Sensitivity: Income Support Receipt

A: Ever on benefit (age 16 and over)



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: e61 Institute; PLIDA

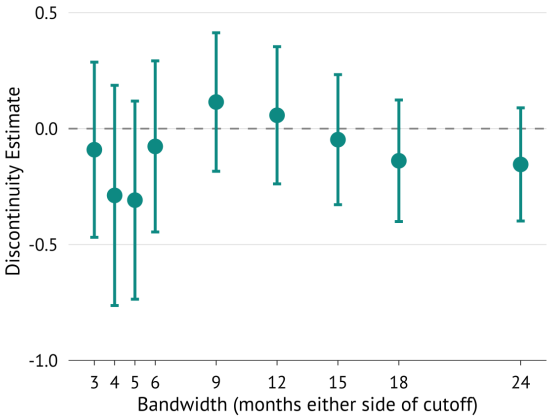
B: Ever on benefit (age 18 and over)



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: e61 Institute; PLIDA

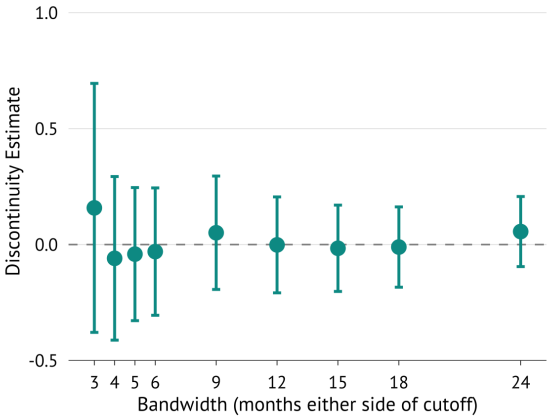
Figure A.6: Bandwidth Sensitivity: NDIS Outcomes

A: NDIS eligible participant rate



* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: e61 Institute; PLIDA

B: Average severity score



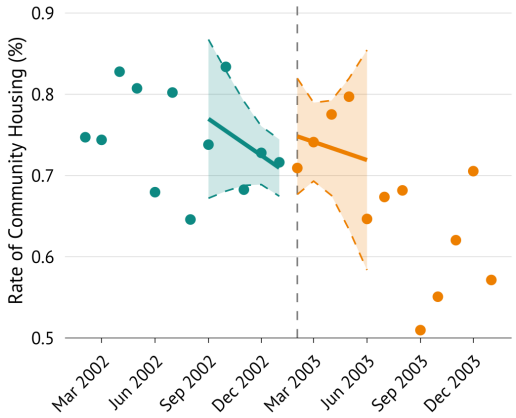
* Each point shows the RD coefficient on the post-cutoff indicator from a local linear regression. Heteroscedasticity-robust 95% CIs.
Sources: e61 Institute; PLIDA

A.3 Placebo cutoff tests: community housing

A potential concern with the community housing result in Figure 5A is that the small discontinuity at February 2005 might reflect seasonal variation in housing tenure rather than a genuine compositional shift. To assess this, Figure A.7 repeats the same analysis using placebo cutoff dates—February 2003, and February 2006—where no policy change occurred. None of the placebo cutoffs produces a discontinuity comparable to that observed at February 2005, supporting the interpretation that the pattern at February 2005 reflects a change in the composition of births encouraged by the Baby Bonus rather than a seasonal artefact.

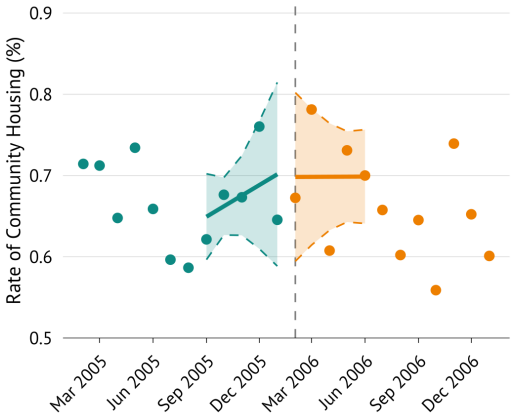
Figure A.7: Placebo Cutoff Tests: Community Housing Rate

A: Placebo: Feb 2003



* Placebo test using Feb 2003 as cutoff instead of Feb 2005. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

B: Placebo: Feb 2006



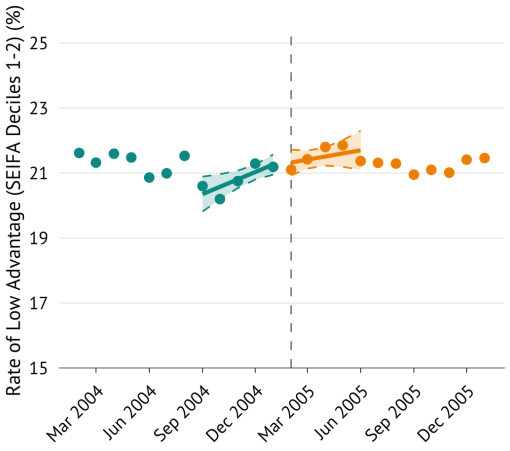
* Placebo test using Feb 2006 as cutoff instead of Feb 2005. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

A.4 Neighbourhood disadvantage (SEIFA)

The Socio-Economic Indexes for Areas (SEIFA) measure the relative socioeconomic advantage or disadvantage of neighbourhoods based on Census data. Figure A.8 presents the share of individuals living in a high-advantage area (top two SEIFA deciles) and the share living in a low-advantage area (bottom two deciles), by birth month. Around 21 per cent of children from both cohorts reside in bottom-decile areas and around 18 per cent in top-decile areas, with no clear difference between those born before and after February 2005.

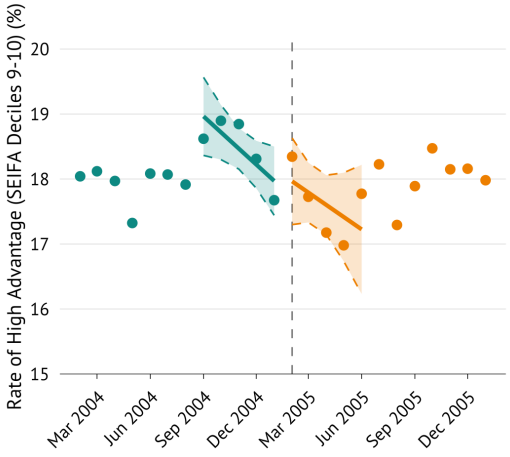
Figure A.8: Neighbourhood Disadvantage (SEIFA) by Birth Month

A: Share in low-advantage area (SEIFA deciles 1–2)



* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

B: Share in high-advantage area (SEIFA deciles 9–10)



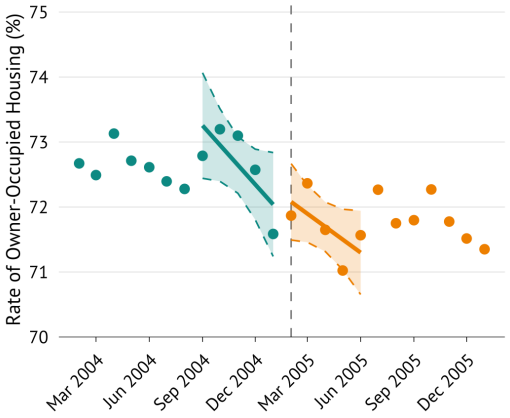
* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

A.5 Housing tenure

Figure A.9 shows housing tenure by birth month: the share of individuals living in owner-occupied housing and the share renting privately. These outcomes are measured from the 2021 Census and reflect the living arrangements of the cohorts as young adults.

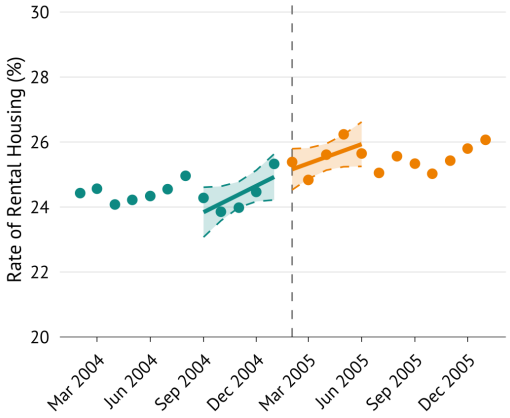
Figure A.9: Housing Tenure by Birth Month

A: Share in owner-occupied housing



* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

B: Share in private rental

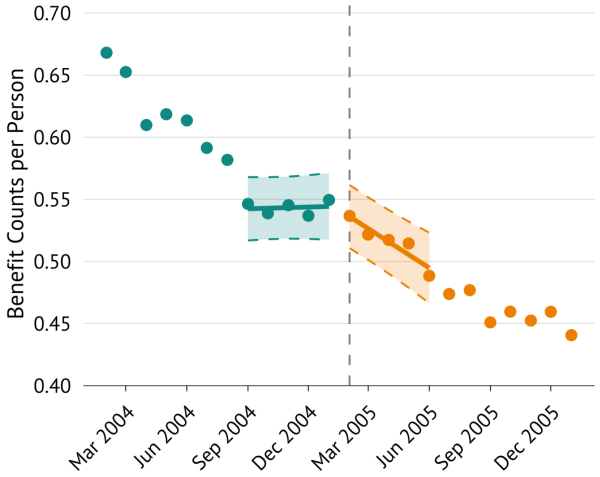


* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

A.6 Additional welfare results

Figure A.10 presents total benefit counts received per person by birth month, measuring the frequency of welfare contact rather than its prevalence. This outcome is smooth across the February 2005 threshold.

Figure A.10: Total Benefit Counts per Person by Birth Month

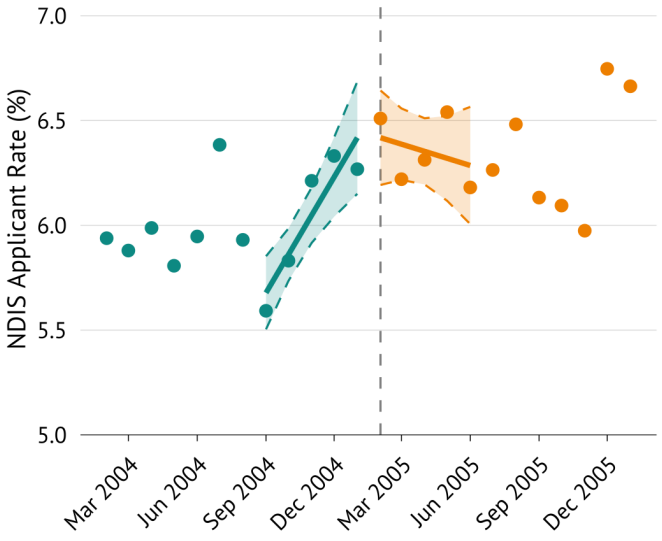


* Welfare data sourced from PLIDA, with the last observation from June 2025. Bands: robust 95% CIs.
Sources: e61 Institute; PLIDA

A.7 NDIS applications

Figure A.11 presents NDIS application rates by birth month. Around 6 per cent of children from these cohorts have applied to the NDIS at some point. Application captures contact with the scheme prior to any eligibility determination, and therefore provides a broader measure of disability support need than eligibility alone. As with the eligibility and severity results reported in the main text, the application rate is smooth across the February 2005 threshold.

Figure A.11: NDIS Application Rate by Birth Month

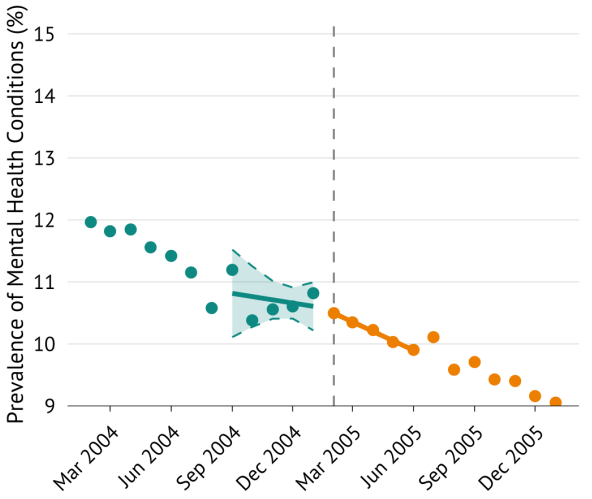


* NDIS data sourced from PLIDA, with the last observation from June 2025. Bands: robust 95% CIs.
Sources: e61 Institute; PLIDA

A.8 Self-reported mental health conditions

Figure A.12 presents the share of individuals who reported having a long-term mental health condition in the 2021 Census. Around 10 to 11 per cent of children from these cohorts report a long-term mental health condition. This provides a complementary picture to the NDIS results: children born just after February 2005 are no more likely to report mental health conditions than those born just before.

Figure A.12: Share with a Long-Term Mental Health Condition by Birth Month



* Census data from the ABS 2021 Census conducted in August 2021. Bands: robust 95% CIs.
Sources: Census 2021; e61 Institute

Data Disclaimers

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.