DEMOGRAPHIC RESEARCH
A peer-reviewed, open-access journal of population sciences

DEMOGRAPHIC RESEARCH

VOLUME 42, ARTICLE 18, PAGES 513–548
PUBLISHED 13 MARCH 2020
https://www.demographic-research.org/Volumes/Vol42/18/
DOI: 10.4054/DemRes.2020.42.18

Research Article

Who responds to fertility-boosting incentives? Evidence from pro-natal policies in Australia

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Demographic Research: Volume 42, Article 18
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Abstract

BACKGROUND
In the wake of aging societies, pro-natalist policies have been used around the world to promote childbearing in developed countries. Very little research investigates the causal effect of the Australian government's baby bonus policy as a once-off, non-means tested incentive scheme on the observed individual fertility.

OBJECTIVE
We investigate the role of immigration in raising fertility beyond what could be achieved by the Australian-born population. The impact of this policy is heavily reliant on the effectiveness of monetary incentives in boosting fertility, yet it is not clear who drives this effect.

METHODS
We utilize triple difference-in-difference (DDD) strategy to evaluate the relationship between childbearing and introduction of the baby bonus in a quasi-experimental setting. We evaluate the quasi-experimental setting by using propensity score matching.

RESULTS
Our findings highlight the role of immigrant women in driving the success of the policy. Moreover, the impact is found to be highest among immigrant women with low levels of human capital, which diminishes with age.

CONCLUSION
The results imply that the role of immigrants, especially that of a young workforce, in aging societies may be greater than has been previously attributed.

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CONTRIBUTION

This paper not only provides scope for the analysis of pro-natal policies within the context of Australia but investigates its impact on immigrant women vis-à-vis native-born women. We find the immigrant contribution to be significant in driving the success of pro-natal monetary incentives. Australia is an exemplar for this analysis due to early adoption of pro-natalist incentives for childbearing amidst a population with high immigrant concentration.

1. Introduction

Fertility has become an increasingly policy-relevant topic in the wake of aging populations in many nations. Total fertility rates have significantly decreased in developed countries over the past two decades (OECD 2015) while many are experiencing the strain of an aging population on the long-term stability of the economy (Adserà 2011, 2004). The general concern around aging caused by low birth rates and low mortality rests on the implications for the fiscal demand on the government budget. To address the challenges of population aging, many governments have utilized policies such as monetary incentives for childbearing and immigration programs in an attempt to offset future demographic and economic challenges (see for example Milligan 2005; Akbari and MacDonald 2014). However, analysis of the interaction between these two policies has rarely been evaluated.

Australian policymakers have concentrated on addressing population aging in the early stages of demographic transition. To boost childbearing, the Australian government offered a pro-natalist monetary incentive for childbirth known as the 'baby bonus' from 2004 to 2014. Additionally, Australian immigration policies have been pivotal to the nation's demographic position. As fertility patterns for native-born and immigrant groups differ, this study utilizes causal identification tools to analyze the impact of the baby bonus on observed individual fertility rates and evaluates the contribution of Australia's immigrant subpopulation in the success of this policy.

Fertility choices are influenced by many factors, including educational attainment, labor force participation, and the opportunity cost of childbearing. The diminished total fertility rate in Australia reflects the increased labor market attachment, which echoes the increase in the rising education levels amongst women (Jain and McDonald 1997; Kippen 2004; McDonald 2006). As a result, government policies often focus on reducing the opportunity cost culminating from childbearing and child rearing. Pro-natalist policies seek to use both financial and social incentives to encourage reproduction (McDonald 2006).

Studies have focused on both direct and indirect policies such as tax incentives, cash payments, or other welfare support for childbearing (Gauthier and Hatzius 1997; Milligan 2005; Laroque and Salanié 2008). Empirical evidence from the analysis of monetary incentives identifies a positive impact on both childbearing and childbearing intentions; however, the results are heterogenous and dependent on the criteria surrounding the incentive. The existing literature has determined a strong positive relationship between financial incentives such as family allowances or taxation benefits and childbearing (Gauthier and Hatzius 1997; Laroque and Salanié 2008). However, the literature suggests that the response to such policies varies across countries (Laroque and Salanié 2008) and are likely to be heterogeneous across family structures (Milligan 2005).

Although not explicitly declared as such, the baby bonus was an open attempt at a pro-natalist social policy (Drago et al. 2011). The direct aim was to promote population growth by providing direct cash incentives (Risse 2010). At the time of the announcement of the baby bonus, the treasurer for the government, Peter Costello, implored Australian couples to have "one for your husband, one for your wife, and one for the country" (Australian Broadcasting Corporation 2004; Costello 2007). Total fertility rates increased steadily following its introduction, with the largest gain observed in 2007 and highest contributions from women aged 15–24 (Australian Bureau of Statistics 2013). Sinclair, Boymal, and de Silva (2012) provide evidence that within 10 months, Australia experienced a significant increase in the total number of births. Self-reported fertility intentions also increased after the implementation of the bonus (Risse 2010).

While some studies examine the impact of the bonus on total fertility rates, fertility intentions, birth timing, and the incidents of higher order births (Sinclair, Boymal, and de Silva 2015, 2012; Drago et al. 2011; Fan and Maitra 2010; Gans and Leigh 2009), to the best of our knowledge, the causal effect of the policy as a once-off, non-means tested incentive scheme on the observed individual fertility has not been evaluated. As the number of countries implementing pro-natalist policies have been increasing over the past decade, an analysis of efficacy is necessary (McDonald 2006). It is important to identify who responds to these incentives, thereby reducing the potential for unintended consequences. This paper not only provides scope for the analysis of pro-natal policies within the context of Australia but investigates its impact on immigrant women vis-à-vis native-born.[3]

[3] Those born within a country are often referred to as natives in the literature. However, in the Australian context, the term 'native' is commonly associated with the Indigenous population. Hence, we use the terms 'Australian-born' and 'native-born' to refer to those who are born in Australia in this paper.

According to the Australian Bureau of Statistics (ABS), recent immigration trends have raised the proportion of Australians who were born overseas to nearly a third of the population, the highest in 120 years (ABS 2015). Australian policymakers believe immigration has the potential to offset consequences of population aging by contributing to the workforce and via taxes paid to governments to support essential services (Intergenerational Report 2010). Notwithstanding the ongoing debate on whether immigration is an effective means of addressing population aging, migration of a primarily young workforce from overseas may be crucial in raising fertility rates. Currently, about 88% of migrants are aged under 40 years, as opposed to only 54% of the Australian-born cohort; and almost half are aged 20–34 years versus only one in five Australians (Intergenerational Report 2015). Although we do not directly evaluate the role of shifting age structure among immigrants, it is important to understand the extent to which immigrants contribute to the success of pro-natal monetary incentives. Australia is an exemplar for this analysis due to early adoption of pro-natalist incentives for childbearing amidst a population with high immigrant concentration.

Theories that link migration and fertility choices are ambivalent – split between fertility disruption and fertility assimilation theories. When migrants arrive, they bring their preferences for higher (or lower) levels of fertility. Evidence suggests that immigrant fertility levels are initially low upon arrival; however, immigrant women reveal a higher parity-progression rate over their lifetime to compensate for postponement of childbearing (Andersson 2004). In addition to this, immigrants from high-fertility source countries display higher fertility in the host country relative to natives (Blau 1992; Abbasi-Shavazi and McDonald 2002). However, the immigrant/native-born fertility differential has declined globally over the past decade (Blau, Kahn, and Papps 2011). A more recent study by Stichnoth and Yeter (2016) suggests that cultural influence on fertility choices is strongest with first-generation immigrants, in particular for women with low levels of education who are in a relationship with a partner who has the same home country.

Immigrants tend to have different outcomes compared to native-born individuals in the context of many economic measures, including employment and fertility (Mayer and Riphahn 2000; Andersson 2004; Fernández and Fogli 2006; Adserà and Ferrer 2016). Immigrant and native-born women have differing responses to monetary incentives within the welfare literature (Borjas 1999; Kaestner and Kaushal 2005). However, the role of these subpopulations in determining the effectiveness of fertility incentives has not been explored. The objective of this paper is to evaluate whether the increased fertility levels observed after the introduction of the baby bonus in Australia are attributable to a greater response from immigrants than Australian-born. Since the response to fertility-boosting incentives vary substantially across countries (Laroque and Salanié 2008), we postulate an increased responsiveness of the immigrant cohort to pro-natal polices such as the baby bonus would provide an additional channel to reduce the potential strain caused by population aging.

We utilize a triple difference-in-difference (DDD) strategy to evaluate the relationship between childbearing and introduction of the baby bonus in a quasi-experimental setting. Since all women were eligible to claim the bonus payment, we establish treatment status by identifying two criteria: (a) the subpopulation that is most likely to respond to the incentive and (b) the control groups that establish credible counterfactuals. Using the level of human capital for the first criterion, and age group for the second, we measure the effect of the bonus on fertility of potential compliers.

Our findings suggest a significant increase in fertility among women with low levels of educational attainment for both immigrant and Australian-born women. The impact is higher for younger women, aged under 25 years. Moreover, we find that the baby bonus led to a larger increase in the fertility of immigrant women, compared to Australian-born women, when using a triple difference-in-difference method for causal identification. When utilizing propensity score matching to evaluate our treatment assignment, we also find Australian-born (immigrant) women with low levels of educational attainment are more likely to respond to the baby bonus incentive in comparison to similar Australian-born (immigrant) women with higher levels of educational attainment. Whilst the matched results for Australian-born women are positive and point in the same direction as the results for immigrant women, the impact is found to be substantially higher for immigrant women who have low levels of educational attainment.

Given the results outlined above, we conclude both Australian-born and immigrant women with high levels of education are less likely to respond to the implementation of the bonus, supporting our initial hypotheses that women with low educational attainment are more likely to respond to monetary incentives for childbearing. Further to this, immigrants with low levels of education respond significantly to monetary incentives. In consideration of the Australian government's pro-natal policies to address population aging, these results are particularly relevant when considering the potential implications of future policies for Australia and similar nations with a significantly large immigrant population.

The paper is structured as follows: The next subsection provides an overview of the baby bonus policy in Australia. Section 2 describes the data and empirical methodology respectively. Results from the triple difference-in-difference and robustness analysis are reported in Section 3. Section 4 concludes with a discussion of the implications of our findings.

Background to the baby bonus

In an attempt to increase the fertility rate across the nation, the Australian government introduced a cash incentive scheme for childbirth, which offered a single payment of $3,000 to all women having a baby on or after July 2004. The program replaced an existing means-tested maternity tax offset scheme of $2,500 per annum that women could claim over a five-year period. However, unlike the maternity tax offset, the baby bonus offered immediate payment that was non-means tested, available to all citizens and permanent residents of Australia. The program was initially announced in May 2004 and implemented in July 2004. Therefore, we expect the bonus to impact on childbirth rates from 2005 onwards, accounting for the lag from conception to birth.

The baby bonus payment underwent a series of alterations since its implementation in 2004, to its dissolution in 2014. The bonus payment was subsequently raised to $4,000 in July 2006 whilst indexed to rise alongside the national consumer price index in the interim. In July 2008, the payment was increased further to $5,000, at the same time the Australian government also indicated the inclusion of an eligibility criteria resulted in means testing, which implied only families earning below $150,000 per annum were eligible to receive the bonus as of January 1, 2009. Given the increased stringency in eligibility criteria from 2009, the incentives of the bonus are likely to have a more selective impact on fertility from 2009 onwards. In 2012, the means testing criteria became stricter, with the eligibility threshold lowered to $75,000 and only available if the mother had no access to paid parental leave. Thereafter, the government reduced the payment amount to $3,000, staggered across 12 weeks in the 2013–2014 financial year. As of May 2014, the policy reverted to a means-tested maternity tax offset, with compulsory implementation of paid parental leave, thereby restricting the incentive to actively employed women.

2. Empirical strategy

2.1 Data

We use the *Household, Income and Labour Dynamics in Australia* (HILDA) Survey, a longitudinal survey of representative Australian households where all individuals over the age of 15 in a household are interviewed. We consider data from 2003–2008 for two reasons. First, it provides us with a natural experimental setting wherein fertility is observed prior to the introduction of the baby bonus. Second, since fertility incentives may have changed when the Australian government introduced means testing to the bonus recipients in 2008, we restrict our sample to 2008 to ensure clean identification of the initial influence the bonus exerted over individual fertility.[4]

Immigrant families who are Australian citizens or permanent residents were also eligible to receive the baby bonus. Therefore, the immigrant cohort experiences assimilation to local conditions over at least two years, which was at the time the minimum period of stay required to attain permanent residency in Australia. The composition of the sample consists of 5,295 Australian-born women and 1,369 immigrant women. The predominant regions of birth for the immigrant women in the sample is North-West European followed by South-East Asia. Fertility is measured as a binary indicator for childbearing in a given year. We focus on women of prime childbearing ages (18–40) but also include women with completed fertility (ages 40–60) as a robustness check.

2.2 Quasi-experimental design

Universal claim to the bonus payment following childbirth does not allow one to estimate the causal effect of the baby bonus, as the average treatment effect cannot be identified directly.[5] To address this, we develop quasi-experimental treatment groups similar to Kaestner and Kaushal (2005) by categorizing women based on their likelihood of responding to the incentive provided by the baby bonus. In order to overcome concerns regarding the validity of treatment assignment, we consider four alternative treatment groups and further define the control group for each treatment in multiple ways. The differential responses of Australian-born and immigrant women to the bonus is estimated using a triple difference-in-difference (DDD) strategy.

The choice of treatment group is motivated by the notion that childbearing among women with low levels of human capital is most likely to respond to the financial incentives offered by the baby bonus (Kaestner and Kaushal 2005; Parr and Guest 2011). Furthermore, greater attachment to the labor market predicts lower fertility response for women with higher levels of education (Rosenzweig 1999; Milligan 2005; Andersson 2004). Therefore, our ‘treated’ group comprises of women with the lowest

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[4] Means testing was announced in May 2008 and implemented in January 2009. We conduct a sensitivity analysis that includes 2009 in the post treatment period. However, the utilization of 2009 to ensure the clean identification of the impact of the policy is debatable due to the structure of the announcement of means testing. We find the results change slightly in economic magnitude; however, the direction and strength remain the same.

[5] The bonus was automatically available to women who gave birth without requiring the recipients to apply for it.

level of human capital – having attained a level of education below a high school (HS) qualification – among whom uptake is expected to be the highest.

Given our focus on investigating the effectiveness of incentives on observed fertility outcomes, we restrict the treatment group to various subgroups of ages under 40.[6] Considering the effect of age on the fertility decision is potentially heterogeneous, we specifically allow for four different variations in the treatment based on age – less than 25 years of age, less than 30, less than 35, and less than 40. It is important to note that instead of using mutually exclusive groups, we include more women of childbearing age to define the next treatment (Table 1). This design allows us to check whether the effect of the baby bonus on fertility is sensitive to women's age. We prefer this approach over using mutually exclusive age categories to ensure our sample size is sufficiently large for the immigrant subgroup.

As each treatment group is defined based on the education level and age of women, all remaining women – not belonging to that age group or education level – are assigned to the control group. Since these controls can differ when treated by age or education, we define four different ways to allocate women to the control group based on the cutoffs defined for the treated: by education only (control 1), by age only (controls 2 and 3), and by education and age (control 4). In order to discern between women who are in their prime childbearing ages and those who are not, we additionally set an upper bound at age 40 in control 2 while leaving age unrestricted in control 3.[7]

Table 1: Assignment to treatment and control groups

Treatments	Control 1	Control 2	Control 3	Control 4
A: < HS and age < 25	≥ HS and age < 25	< HS and 25 ≤ age < 40	< HS and age ≥ 25	≥ HS and age ≥ 25
B: < HS and age < 30	≥ HS and age < 30	< HS and 30 ≤ age < 40	< HS and age ≥ 30	≥ HS and age ≥ 30
C: < HS and age < 35	≥ HS and age < 35	< HS and 35 ≤ age < 40	< HS and age ≥ 35	≥ HS and age ≥ 35
D: < HS and age < 40	≥ HS and age < 40		< HS and age ≥ 40	≥ HS and age ≥ 40

Note: < HS refers to less than high school educational qualification.

Two points related to the choice of an optimal control group must be noted. First, in order to ensure comparability, each control group differs from the treatment group along one dimension; that is, education or age. Control 4 differs on both education and

[6] We use 40 years as an upper bound to capture all women within their prime childbearing age.
[7] Since Treatment D includes women up to age 40, this restriction only applies to Treatments A, B, C, leaving Control 2 blank for Treatment D.

age; however, its inclusion is for completeness in our analysis.[8] Second, our treatment variable is defined primarily by low educational attainment. Hence, although we define a range of controls for each treatment for more insight, we rely on control 1 for identification of baby bonus effects. Table 1 presents details of the assignment to treatment and control groups as described above.

2.3 Common trend assumption

Validity of the DDD estimator in a quasi-experimental setting rests crucially on the common trend assumption, which requires the treatment and control groups to follow a comparable trend before the introduction of the bonus. Based upon the timeline for the bonus scheme outlined in Section 1, we assign 2003–2004 as the pre-treatment years and 2005–2008 as the post-treatment years.[9] We compare average fertility trends for each of the four treatment groups (denoted by solid lines) with the control groups (denoted by dashed and dotted lines) over our sample period in Figure 1. Comparison of fertility levels for the four treatment groups confirms that women with low levels of human capital (< HS) respond prominently to the bonus as observed in the jump from 2005. This is true for women across all ages, as seen from the solid lines for all treated groups A–D. In contrast, fertility of women with higher levels of human capital remain stable over the period. Evidently, our quasi-experimental design seems appropriate as fertility levels and trends for treated groups A and B are observed to be similar in the pre-treatment years (2003–2004) to their closest controls (groups 1 and 2).[10]

We rely heavily on treated groups A and B for inference, as treatments C and D include older women whose fertility is expected to be less responsive to the bonus incentive. We note the trends for control groups 1 and 4 remain relatively unresponsive during the period and confirm our treatment assignment validity, as the impact of tax incentives are indeed highest among the women with low human capital (as found by

[8] Control 4 was included in the analysis to achieve completeness in the possible combinations of age and education. However, this group is not a valid control, as it leaves women who are in the same education but different age group (e.g., < HS and age > 25 for Treatment A) as well as those in same age but different education (e.g., > HS and age < 25 for Treatment A) out of the analysis.

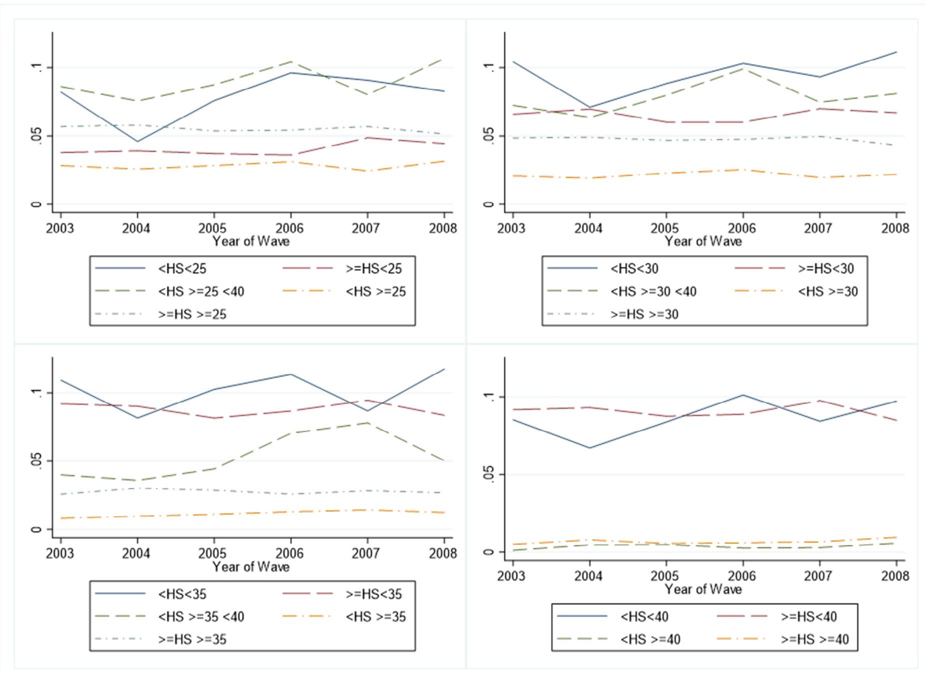
[9] We note that those children born in early 2005 may have been conceived before the implementation of the policy, and thus these children may belong to the pre-bonus period rather than the post-bonus period. Consequently, we conduct a robustness check, moving 2005 between pre and post treatment in our analysis in Section 4. We find the DDD estimates are similar in direction and magnitude, however slightly less significant across the various controls. Results can be found in Tables A-3 and A-4 in the appendix.

[10] Note that control group 3 displays lower average fertility levels, due to the presence of older women up to age 60, in addition to the set of women in control group 2, where the number of older women is nearly three times that of the younger women present in control 2.

Kaestner and Kaushal 2005). The fertility levels in the last pre-treatment year, 2004, for each of the four treatment groups are similar, and in fact lower than at least one control group as seen in each panel of Figure 1.

We further examine the relevance of the common trend assumption between native-born and immigrant women when discussing the results in Table 3. The use of a triple difference model makes the comparison of pre-treatment trends easier to interpret using the regression model rather than using figures.

Figure 1: Fertility trends for treated (A to D) with controls (1 to 4) for each treatment



Notes: < HS refers to less than high school educational qualification. The numbers (e.g., < 25, < 30) refer to age limits. The top left and right panels denote treatments A and B while the bottom left and right panels denote treatments C and D respectively. The solid line denotes treated while the dashed lines represent the four controls for that treatment.

2.4 Triple difference estimates

We examine the differential responses of native-born and immigrant women to the bonus utilizing a triple DDD strategy. We begin by estimating the response of immigrant and native-born women to the baby bonus incentive scheme using the following linear probability model:

$$F_{i,t} = \alpha_0 + \alpha_1 \boldsymbol{X_{i,t}} + \beta_1 I_{i,t} + \beta_2 \tau_{i,t} + \beta_3 T_{i,t} + \beta_4 (I_{i,t} \times \tau_{i,t}) + \beta_5 (I_{i,t} \times T_{i,t}) + \beta_6 (\tau_{i,t} \times T_{i,t}) + \gamma (I_{i,t} \times \tau_{i,t} \times T_{i,t}) + \epsilon_{i,t} \quad (1),$$

where $F_{i,t}$ takes a value of 1 if an individual had given birth in the previous year and 0 otherwise. $\boldsymbol{X}_{i,t}$ is a vector of individual level characteristics. $I_{i,t}$, and $\tau_{i,t}$ are indicators for immigrant and treatment status respectively. $T_{i,t}$ is an indicator for the post-bonus period (2005–2008). Since education is used to determine treatment assignment, it is not included in the vector $\boldsymbol{X}_{i,t}$. The controls selected for vector $\boldsymbol{X}_{i,t}$ are well known in the literature to impact fertility. These include the number of children, marital status, log gross income from all sources for each survey respondent, labor force status, and partner's education.[11]

We note that childbirth and fertility are different concepts. Childbirth refers to how many children are born within a given period of time, whereas fertility is the total number of children a woman has over her lifetime. Our paper investigates the impact of the baby bonus on the revealed preferences for childbearing during the previous year. However, the implications for fertility may be different, as the baby bonus may have lifted childbirth rates by bringing forward the timing of births without affecting total fertility.

While identification of causal effects using the DDD estimator, γ, is valid when unobserved factors influencing both treatment assignment as well as the outcome, fertility $(F_{i,t})$, are time-invariant, we further evaluate our quasi-experimental design. Since immigrant women are likely to differ from Australian-born women along time-varying factors as well, we evaluate our empirical strategy by utilizing propensity score matching (PSM) to estimate the average treatment effect on the treated (ATT). This allows us to verify whether the DDD inference based on our quasi-experimental treatment assignment is valid despite potential heterogeneity in immigrant and native-born unobservable characteristics. As PSM only allows for matching on one treatment indicator, we estimate the effect of being in the treatment group for our two

[11] For covariates related to partner's information, if the individual could not be matched via the household identifier to a partner, we allocate that observation to an additional 'missing' category to retain potentially single women.

subpopulations, immigrant and Australian-born, separately for the pre- and post-bonus periods. This allows us to determine if women with low educational attainment have a higher probability of fertility in the post-bonus period. Within each treatment category, we can then compare the ATT for the subsample of immigrants to that of the native-born counterparts to determine if the bonus was indeed more effective in raising fertility of immigrant women above the response observed in Australian-born women. This comparison between the responsiveness of immigrant women with low human capital to Australian-born women with low human capital is crucial for policy development to address long-term population aging challenges.

3. Results

Summary statistics for women in our sample are reported in Table 2. Additionally, Table A-1 shows the descriptors for partner's education. The immigrant sample closely represents the proportion of women in the population relative to Australian-born women during our sample period, which is roughly 20%.[12] We find significant differences in the pooled data across all covariates apart from log gross income, the likelihood of being unemployed and undefined certificate qualifications. Immigrants in the sample tend to be approximately four years older on average in comparison to the Australian-born women within the sample, which is also approximately four years older than the general immigrant population age, suggesting a slightly older cohort of immigrants. Fertility represents the unconditional probability of having a child in a given year, which is approximately 5% for Australian-born women both before and after the bonus. The probability of giving birth over the pre- and post-treatment period is persistently lower for immigrant women compared to Australian-born women. We also observe that immigrant women have more children on average suggesting immigrant women have more children on average. Immigrant women are also found to be less likely to be employed and more likely to be in a relationship (either married or de facto) in comparison to their native-born counterparts. Finally, we also observe that

[12] According to the 2006 Australian Census, 24% of all females are born overseas (http://www.abs.gov.au/AUSSTATS/abs@.nsf/DetailsPage/3412.02013-14?OpenDocument). However, in light of the increased inflow of immigrants to Australia in recent years, the lack of resampling in subsequent waves of the HILDA will provide a lower (upper) bound of our estimates if the baby bonus is more (less) effective in raising the fertility levels of immigrant than Australian-born women. Furthermore, given that the eligibility criteria for receiving the bonus excludes temporary residents, we check the robustness of our results by restricting the sample of immigrants to permanent residents and citizen. We find the results are similar in direction but are no longer significant. It must be noted that reporting of citizenship status was not consistent over the years, leading to an attenuated sample of 17% of immigrants, which likely explains the reduced power.

immigrant women are more likely to have attained a university level education compared Australian-born women.

Table 2: Summary statistics for sample women

Variable	All			Pre-bonus			Post bonus		
	Australian-born	Immigrant	Mean diff.	Australian-born	Immigrant	Mean diff.	Australian-born	Immigrant	Mean diff.
	(1)	(2)	(1)–(2)	(3)	(4)	(3)–(4)	(5)	(6)	(5)–(6)
Fertility	0.049	0.042	0.007*	0.048	0.042	0.007	0.049	0.042	0.008
	(0.216)	(0.200)	(0.003)	(0.215)	(0.200)	(0.005)	(0.217)	(0.200)	(0.004)
Log gross	10.057	10.040	0.017	9.913	9.909	0.004	10.130	10.111	0.019
Income	(1.007)	(1.063)	(0.016)	(1.024)	(1.059)	(0.027)	(0.991)	(1.059)	(0.019)
Age	38.140	42.497	–4.357***	38.264	42.024	–3.760***	38.077	42.756	–4.680***
	(12.020)	(10.995)	(0.180)	(11.827)	(10.855)	(0.300)	(12.117)	(11.063)	(0.226)
No. children	1.569	1.679	–0.110***	1.644	1.713	–0.068	1.531	1.660	–0.129***
	(1.467)	(1.413)	(0.022)	(1.505)	(1.439)	(0.038)	(1.445)	(1.398)	(0.027)
Married / de facto	0.648	0.726	–0.078***	0.647	0.720	–0.073***	0.649	0.730	–0.081***
	(0.477)	(0.446)	(0.007)	(0.478)	(0.449)	(0.012)	(0.477)	(0.444)	(0.009)
Labor force status									
Employed	0.736	0.688	0.047***	0.707	0.661	0.046***	0.750	0.703	0.047***
	(0.441)	(0.463)	(0.007)	(0.455)	(0.473)	(0.012)	(0.433)	(0.457)	(0.008)
Unemployed	0.033	0.032	0.000	0.033	0.038	–0.005	0.032	0.029	0.003
	(0.178)	(0.177)	(0.003)	(0.179)	(0.191)	(0.005)	(0.177)	(0.168)	(0.003)
Not in LF	0.232	0.279	–0.048***	0.260	0.301	–0.041***	0.217	0.268	–0.050***
	(0.422)	(0.449)	(0.007)	(0.439)	(0.459)	(0.011)	(0.413)	(0.443)	(0.008)
Observations	21,448	5,366	26,814	7,240	1,898	9,138	14,208	3,468	17,676

Table 2: (Continued)

Variable	All			Pre-bonus			Post bonus		
	Australian-born	Immigrant	Mean diff.	Australian-born	Immigrant	Mean diff.	Australian-born	Immigrant	Mean diff.
	(1)	(2)	(1)–(2)	(3)	(4)	(3)–(4)	(5)	(6)	(5)–(6)
Educational qualification									
Postgraduate	0.027	0.054	–0.027***	0.023	0.047	–0.024***	0.029	0.058	–0.029***
	(0.162)	(0.226)	(0.003)	(0.151)	(0.213)	(0.004)	(0.167)	(0.234)	(0.003)
Grad diploma or	0.065	0.080	–0.015***	0.061	0.076	–0.016*	0.068	0.082	–0.015**
Grad cert.	(0.247)	(0.272)	(0.004)	(0.239)	(0.266)	(0.006)	(0.251)	(0.275)	(0.005)
Bachelor or	0.152	0.198	–0.046***	0.144	0.185	–0.041***	0.156	0.206	–0.049***
Honours	(0.359)	(0.399)	(0.006)	(0.351)	(0.388)	(0.009)	(0.363)	(0.404)	(0.007)
Adv. diploma	0.093	0.102	–0.009*	0.091	0.101	–0.011	0.094	0.102	–0.008
Diploma	(0.290)	(0.303)	(0.004)	(0.287)	(0.302)	(0.007)	(0.292)	(0.303)	(0.006)
Cert. III or IV	0.149	0.132	0.017**	0.135	0.127	0.008	0.156	0.134	0.022**
	(0.356)	(0.338)	(0.005)	(0.342)	(0.333)	(0.009)	(0.363)	(0.341)	(0.007)
Cert. I or II	0.019	0.012	0.007***	0.019	0.013	0.006	0.019	0.012	0.007**
	(0.137)	(0.110)	(0.002)	(0.136)	(0.112)	(0.003)	(0.137)	(0.109)	(0.003)
Cert. not defined	0.005	0.008	–0.002	0.006	0.009	–0.003	0.005	0.007	–0.002
	(0.074)	(0.087)	(0.001)	(0.077)	(0.094)	(0.002)	(0.072)	(0.083)	(0.001)
High school	0.184	0.199	–0.015*	0.185	0.208	–0.023*	0.184	0.195	–0.011
	(0.388)	(0.399)	(0.006)	(0.388)	(0.406)	(0.010)	(0.387)	(0.396)	(0.007)
Year 11 and below	0.305	0.214	0.091***	0.336	0.234	0.102***	0.289	0.204	0.086***
	(0.461)	(0.411)	(0.007)	(0.472)	(0.423)	(0.012)	(0.454)	(0.403)	(0.008)
Observations	21,448	5,366	26,814	7,240	1,898	9,138	14,208	3,468	17,676

Notes: Standard deviations reported in parentheses. Standard errors are reported for mean differences. * / ** / *** denote statistical significance at the 0.10 / 0.05 / 0.01 levels respectively.

For each of the four treatment groups, we present the DDD estimation results in panels A to D of Table 3, respectively, using controls 1 and 2 while those using controls 3 and 4 are reported in the Appendix (Table A-2). The results are also summarized visually in Figure 2. While we do not present the pre- and post- treatment trends separately for immigrant and Australian-born women in Figure 1, the results in Table 3 support the common trend assumption between the Australian-born and immigrant groups in the pre- bonus period. Our findings suggest that prior to the introduction of the baby bonus, immigrant fertility is not different from natives in the control groups, as identified by the coefficient of $Immigrant$.

When considering fertility of the native-born cohorts in the four treatment groups, we find that the fertility levels of the treated are higher than women in control group 2 but lower than women in control group 1. Immigrant women in the treatment groups A, B, and C were between 7.7% to 11.9% less likely to have a child than an Australian-born in the treatment group in the pre-bonus period, as captured by the coefficient of $Imm \times \tau$. So, while the pre-treatment trends were not similar, we find they were declining for the immigrants. Thus, the difference-in-difference (DDD) estimates in the pre-treatment trends indicate that Australian-born women with less than high school attainment had lower fertility than those with greater education in the same age group, which were further lower for immigrant women. However, when compared to older women aged up to 40 with similar levels of education, Australian-born women were more likely while immigrant women were less likely to have children.

The DDD estimate, denoted by the coefficient of $Imm \times \tau \times Post\ Bonus$, is positive and significant in panels A and B, implying that the baby bonus provided a boost to fertility of immigrants. However, according to the DDD coefficient of $\tau \times Post\ Bonus$, a similar impact of the bonus on Australian-born women is not evident. Our findings support our main hypothesis that the effect of the bonus diminishes with both age and human capital levels. This is evident from first comparing the magnitude of the DDD estimate in each column across panels A and B. The coefficients in each column is higher in panel A than the corresponding column in panel B, supporting the hypothesis that the impact of the bonus on raising immigrant fertility was concentrated among younger women. Specifically, women with low human capital under age 25 are 18.6% more likely than those aged between 25 and 40 (in column 2 of panel A) to have a baby, while the corresponding effect size is 15.7% for women under age 30 (in column 2 of panel B). Likewise, women with low human capital under age 25 are 13% more likely to have a baby than similar aged women with higher human capital (in column 1 of panel A), while the effect size is 9.8% for women under age 30.

Second, the coefficients in column 1 of each of panel A and B are positive, indicating women with lower levels of human capital are more like to have a child than their educated counterparts. While the effects are not significant, they imply similar effects after including older women in panel C. Additionally, on comparing across columns within each panel, the effect is higher when the control is composed of older women as against more educated women. That is, women below age 25 with low levels of human capital are 13% more likely to have a child than those with higher human capital levels, but 18.6% more likely than older women with similar (low) levels of human capital. As we incorporate more women outside their prime childbearing age into our treatment group in Panel C, the effects in both columns decrease but remain positive. The implications discussed for panels A and B above remain unchanged when including panel C for comparison. Similar results are observed when using controls 3 and 4 as reported in the Appendix (Table A-2).[13]

Furthermore Table 3 shows a noticeable difference in the age pattern responses. The DDD coefficient suggests the post-bonus period was stronger for women over 25; this may be due to young women in their childbearing prime bringing forth their fertility choice. The DDD coefficient, however, diminishes in magnitude. This may be attributable to the increased labor force attachment of immigrant women as assimilation to the local labor market occurs over time. In addition to this, it may also reflect Australia's positive selection policy for immigration, focusing on skilled labor. The focus on skilled labor may represent increased labor force attachment for older immigrant women.

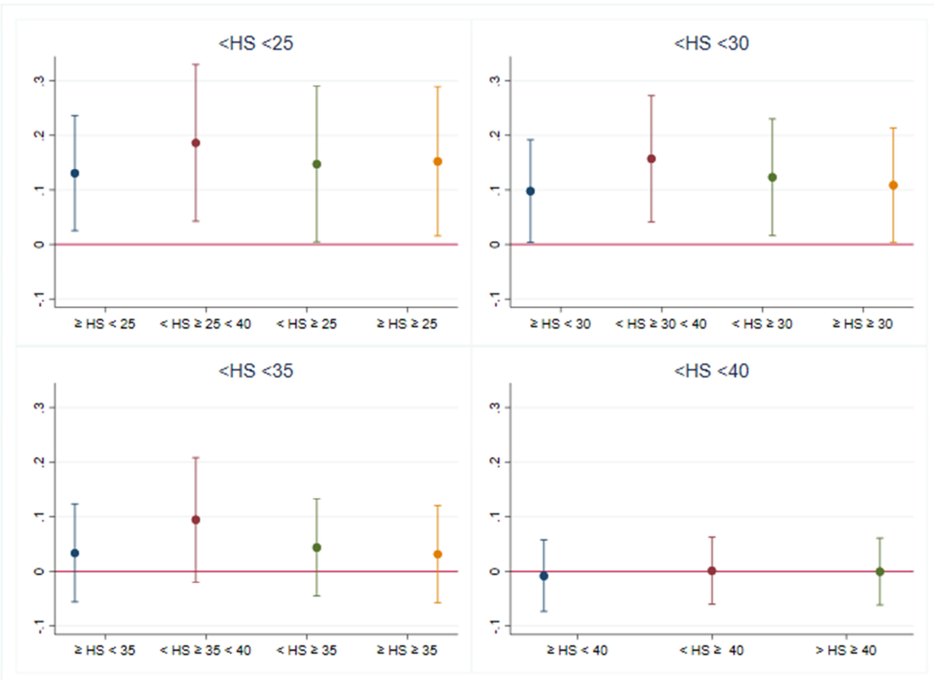
In addition to the above outlined DDD specification, we estimated three alternative models to determine whether the results are sensitive to the cutoff year used to define the pre- and post-treatment periods in our analysis. This sensitivity check is required, as the announcements of the bonus and its subsequent alterations were declared in the federal budget in each relevant year. If even some women are expected to adjust their fertility decision right after the announcement while the majority of women may take longer, our results may be sensitive to the allocation of years to pre- and post-bonus periods. Towards that end, we allow for three variations in determining the bonus period cutoffs – (a) switch 2005 to be included in the pre-treat period, (b) extend the duration of analysis to include 2009 in the post-treat period, (c) include 2005 in pre-treat and 2009 in post-period simultaneously.[14] The results from these specifications are reported in Appendix Tables A-3–A-8. The findings for DDD estimates are qualitatively similar to those reported in Table 3; that is, immigrant women are found to be a stronger contributor to the success of the baby bonus policy. Additionally, we now find significant effects for treatments C and D in some cases, implying that older women have different planning horizons for fertility compared to younger women and may respond at a different pace to such incentives – consistent with the results found in Parr and Guest (2011).

[13] As fertility may be implicitly different between settled migrants and those who have recently arrived in Australia, we estimated a model including years since migration. We note that the results remain qualitatively similar across all specifications, with the effect sizes and significance levels found to be similar.

[14] We also estimated the DDD model using a logit specification for robustness. The marginal effects are found to be significant for treatments C and D in addition to treatments A and B. We note that a triple difference specification for nonlinear dichotomous variable models should be interpreted with caution as outlined in Puhani (2012) and therefore report the OLS results in this paper.

Figure 2 provides a graphical representation of the results for treatments A–D across all specifications. As observed in the figure, we observe a statistically significant effect of the bonus on the fertility of immigrant women for treatments A and B, which is consistent across all controls. This effect decreases as we incorporate more women of childbearing age (panels C and D). This further supports our hypothesis that the effect of the bonus diminishes with both age and human capital levels.[15]

Figure 2: Visualization of triple difference (DDD) results



Notes: This figure summarizes the results for the DDD estimates reported in Table 3 and Table A-2. The top left and right panels denote treatments A and B, while the bottom left and right panels denote treatments C and D respectively. Each line represents the results against the corresponding control identified on the x-axis. About 90% confidence intervals provided. < HS refers to less than high school educational qualification. The numbers refer to age limits on the controls.

[15] We also investigated the inclusion of women with a high school education in our sample. We note that the economic magnitude of the effect is smaller and no longer statistically significant. We note that the sample size in our analysis is large enough to provide a robust analysis. For example, within the group < HS & age < 25, we report 965 observations – broken down into 900 Australian-born and 65 immigrant women.

Overall, Table 3 and Figure 2 support our two main hypotheses. Firstly, we find the uptake of the bonus is higher for women with the lowest levels of human capital (less than high school equivalence). Secondly, we find the baby bonus provided a boost to fertility of immigrants. Both findings are important in the context of an aging population where both immigration and pro-natal policies can be utilized to address the economic implications of the changing demographics in Australia and other nations facing the aging crisis.

Table 3: Differential impact of baby bonus on immigrants: Triple difference (DDD) estimates (using controls 1 and 2)

	Panel A Treated: < HS and age < 25		**Panel B** Treated: < HS and age < 30		**Panel C** Treated: < HS and age < 35		**Panel D** Treated: < HS and age < 40	
	≥ *HS* and age < 25	**< *HS* and 25 ≤ age < 40**	**≥ *HS* and age < 30**	**< *HS* and 30 ≤ age < 40**	**≥ *HS* and age < 35**	**< *HS* and 35 ≤ age < 40**	**≥ *HS* and age < 40**	**< *HS* and age ≥ 40**
Immigrant	–0.006	0.009	0.008	0.023	0.005	0.047	–0.004	–0.008**
	(0.012)	(0.029)	(0.014)	(0.032)	(0.015)	(0.036)	(0.013)	(0.003)
Treatment (*τ*)	–0.027*	0.093***	–0.053***	0.103***	–0.059***	0.113***	–0.065***	0.084***
	(0.015)	(0.022)	(0.015)	(0.020)	(0.013)	(0.017)	(0.010)	(0.009)
Post-bonus	–0.001	0.016	0.002	0.018	0.002	0.034**	–0.001	0.001
	(0.007)	(0.014)	(0.007)	(0.015)	(0.007)	(0.016)	(0.006)	(0.002)
Imm**τ*	–0.077**	–0.119***	–0.078**	–0.104**	–0.053	–0.095**	–0.016	–0.005
	(0.038)	(0.041)	(0.037)	(0.046)	(0.034)	(0.047)	(0.027)	(0.024)
Imm*Post-bonus	0.000	–0.034	–0.012	–0.053	–0.003	–0.061	0.004	–0.001
	(0.016)	(0.039)	(0.017)	(0.039)	(0.019)	(0.044)	(0.015)	(0.004)
*τ**Post-bonus	0.011	–0.008	0.032*	–0.004	0.023	–0.019	0.029**	0.018
	(0.019)	(0.023)	(0.018)	(0.022)	(0.016)	(0.021)	(0.013)	(0.011)
Imm**τ**Post-bonus	0.130**	0.186**	0.098*	0.157**	0.033	0.094	–0.008	0.001
	(0.064)	(0.087)	(0.057)	(0.070)	(0.054)	(0.069)	(0.040)	(0.037)
Constant	–0.017	–0.375***	–0.006	–0.364***	–0.107***	–0.371***	–0.173***	–0.226***
	(0.033)	(0.065)	(0.032)	(0.059)	(0.032)	(0.058)	(0.030)	(0.029)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,089	2,829	6,710	2,829	9,721	2,829	13,151	7,470
Adj. R-squared	0.345	0.098	0.244	0.105	0.175	0.106	0.130	0.091

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

4. Average treatment effects: Treatment selection using propensity score matching

To assess our quasi-experimental treatment assignment, we use propensity scores as a means of examining treatment selection effects. We implement the matching method within each subsample of Australian-born and immigrant women. The estimates of average treatment effect on the treated (ATT) for each of the two groups are then compared to verify if the baby bonus was indeed more effective in boosting fertility of immigrant women. The results of the propensity score matching method are summarised in Table 4 for controls 1 and 2, while those using controls 3 and 4 are reported in Table A-9. In order to make the results comparable to those obtained for our DDD design, we compute the ATT separately for the pre- and post-bonus periods. We match on the number of children, marital status, log gross income, employment status, and partner's education in all specifications for direct comparison with Table 3.

As our treatment is defined by educational attainment and age, Table 4 provides insights into the fertility responses for immigrants and Australian-born separately. We interpret the coefficients in Table 4 differently to the results in Table 3. The top half of Table 4 reports the ATT for Australian-born women in the treatment group matched to each of the controls 1 and 2 separately in the pre- and post-bonus periods. Likewise, the ATT for immigrants are reported in the bottom half of Table 4. We first compare the ATT in post-bonus to that in pre-bonus period for Australian-born women, followed by a comparison of ATT in post-bonus to that in pre-bonus period for immigrants. This allows us to verify if indeed the baby bonus led to an increase in fertility for the treated women. Next, a cross comparison of the magnitude of ATTs between Australian-born and immigrant women confirms if the increase in fertility was higher for immigrants. The main purpose of matching within each group is to allow for differences in the selection mechanisms for Australian-born and immigrant fertility; that is, each subpopulation may have different contributing characteristics that influence their fertility decision.

Similar to our discussion of DDD results, we defined a range of controls for each treatment predominantly rely on controls 1 and 2 for identification of baby bonus effects. For example, Panel A shows in the pre-bonus period an Australian-born in the < HS and < 25 treatment is 2.92% more likely to have a baby than an Australian-born in the ≥ HS and < 25 control. Comparing this pre-bonus to the post-bonus result for the same control, we can see that the coefficient increases in the post-bonus period. This suggests that there is an increase in the fertility for Australian-born women in our treatment group when compared to a comparable Australian-born woman in the control group. Overall for the post-bonus period, we find fairly consistent evidence of a positive effect of the bonus on the individual fertility for the Australian-born treatment groups, and the magnitude is mostly higher for the coefficients reported in the pre-bonus period.

In order to verify the validity of our DDD results, we should observe not only higher magnitudes in the post-bonus period compared to the pre-bonus period for immigrants but also higher ATTs in the post-bonus period than observed for Australian-born women. In the pre-bonus period for the immigrant subgroup, we observe some negative but insignificant coefficients in panels A, B, and C. In the post-bonus period, we observe in many cases, statistically significant increase in the fertility of immigrants – when matched with control 1 for treatment groups A and B (panels A and B) and control 2 for treatment groups B and D (panels B and D). More importantly, the ATTs for immigrant women are lower than Australian-born women in the pre-bonus period but higher in the post-bonus period, thereby reaffirming our findings using the DDD methodology on the stronger role played by immigrant women in boosting fertility due to monetary incentives provided by the baby bonus policy.

Table 4: Average treatment effect for immigrants and native subsamples using propensity score matching (using controls 1 and 2)

	Panel A < HS and age < 25		**Panel B**: < HS and age < 30		**Panel C**: < HS and age < 35		**Panel D**: < HS and age < 40	
Natives	≥ *HS* and age < 25	< *HS* and 25 ≤ age < 40	≥ *HS* and age < 30	< *HS* and 30 ≤ age < 40	≥ *HS* and age < 35	< *HS* and 35 ≤ age < 40	≥ *HS* and age < 40	< *HS* and age ≥ 40
Pre-bonus	0.0292*	–0.0080	0.0286**	0.0293*	0.0097	0.0675***	–0.0122	0.0746***
	(0.0150)	(0.0193)	(0.0143)	(0.0175)	(0.0129)	(0.0184)	(0.0105)	(0.0074)
	1145	920	1923	953	2826	960	3793	2365
Post-bonus	0.0485***	–0.0144	0.0358***	0.0096	0.0235**	0.0466***	0.0072	0.0904***
	(0.0105)	(0.0154)	(0.0102)	(0.0147)	(0.0096)	(0.0160)	(0.0081)	(0.0062)
	2526	1537	4020	1587	5626	1587	7422	3999
Immigrants	≥ *HS* and age < 25	< *HS* and 25 ≤ age < 40	≥ *HS* and age < 30	< *HS* and 30 ≤ age < 40	≥ *HS* and age < 25	< *HS* and 25 ≤ age < 40	≥ *HS* and age < 30	< *HS* and 30 ≤ age < 40
Pre-bonus	–0.0294	–0.0706	–0.0418	–0.0541	–0.0217	–0.0237	–0.0327	0.0625***
	(0.0372)	(0.0564)	(0.0434)	(0.0491)	(0.0393)	(0.0466)	(0.0304)	(0.0142)
	123	106	226	111	388	111	697	406
Post-bonus	0.1071**	0.0588	0.1032***	0.0862*	0.0082	0.0464	–0.0175	0.0890***
	(0.0421)	(0.0572)	(0.0335)	(0.0488)	(0.0323)	(0.0461)	(0.0259)	(0.0132)
	234	154	402	154	786	165	1170	679

Note: Heteroskedasticity-consistent analytical standard errors proposed by Abadie and Imbens (2006) are reported parentheses. * / ** / *** denote statistical significance at the 0.10 / 0.05 / 0.01 levels respectively. We match on the number of children, marital status, log gross income, employment status, and partner's education in all specifications for direct comparison with Table 3. Nearest neighbour matching algorithm with replacement used. In each panel heading, HS refers to high school.

5. Discussion and conclusion

An investigation of the role of pro-natal policies in boosting fertility within the backdrop of an aging Australia makes a twofold contribution. First, we analyze the effectiveness of once-off payments to incentivize childbearing. Second, we assert the role of immigrants as the effective drivers of the success in this policy. Specifically, this study focuses on the Australian government's 'baby bonus' scheme as a means of increasing the nation's fertility rate.

While the overarching goal as signaled by the treasurer was to improve the total fertility rate of the nation, we uncover unintended heterogeneous impact of the bonus incentives. Using a triple difference methodology (DDD) to analyze the revealed childbearing preferences of Australian women, we observe an increase in fertility for women with low levels of educational attainment. With the increasing postponement of childbearing and rearing among women with high levels of education, an unintended consequence of the policy may lead to a widening disparity in the fertility of these groups.

Our findings support the importance of pro-natal policies in addressing the challenges imposed by an aging population. The strain of growing fiscal demand on the government budget, particularly to service public health on the one hand and low productivity on the other, in addition to a shrinking tax base for an aging workforce, could potentially be alleviated by such fertility-boosting policies. However, if the mechanism of fertility growth is reliant on immigrants with low educational qualifications, as our findings suggest, then the incentives need to be supplemented by human-capital-augmenting programs to enhance the productivity of their children as well as supports for retirement planning to ensure financial self-sufficiency during old age.

We note the limitations of our study in the context of the HILDA data collection methods over our period of study. First, we use a broad definition of an immigrant – those not born in Australia, which does not account for the diversity within the immigrant population. Given the increasing emphasis on skill-based immigration in Australia, understanding the role of culture on the fertility responsiveness of immigrants constitutes an interesting avenue of future research. Second, the HILDA survey was designed to reflect the composition of Australian residents starting in 2001. Every year people arrive and leave Australia, changing the composition of the Australian population. Therefore, the HILDA would not be representative of immigrants who arrived after the initial wave in 2001 until the top-up sample was conducted in 2011 (see Watson 2006). Thus, results of our analysis may not hold for Australia's contemporary migrant population. Furthermore, over the past ten years, there has been an increase in the number of migrants who initially arrive on a temporary visa prior to attaining permanent residence. This group will be underrepresented in our analysis, suggesting that the fertility choice of permanent residents may be brought forward compared to recent immigrant women who arrive on a temporary visa.

We also note that discrepancies in the representation of migrant population in the HILDA may lead to an overstatement of the responsiveness of immigrants to the baby bonus. For example, on average our immigrant cohort is four years older than the Australian-born cohort, suggesting that immigrants would be closer to childbearing and rearing ages upon arrival in Australia. Likewise, the difference in relationship status may impact the results, as immigrant women in our sample are more likely to be married. Furthermore, limited information is collected on the citizenship and residency status of immigrants, which determine eligibility for receiving the bonus, thereby restricting a clear evaluation of the policy for eligible immigrant women.

Finally, we highlight that in more recent years, the decline in the total fertility rate for immigrant women in Australia has been less severe than the decline for Australian-born women. Specifically, we observe a steeper decline in the total fertility rate of Australian-born women from 2008 to 2012, dropping by 0.18 compared to a decline of 0.13 for immigrant-born women (Australian Bureau of Statistics 2017). Whether this difference can be attributed to the increasingly stringent eligibility criteria for receiving the bonus would be an interesting avenue of further study. Further consideration of the eligibility criteria is needed, as reported income alone does not indicate whether the bonus was received.

Our findings yield optimistic predictions of the impact of immigration policy in combatting aging, as migration of a young overseas workforce is often seen as an effective vehicle for boosting fertility and addressing the distributional issues of an aging population. Overall, the interaction between pro-natal incentives and immigration policies may help offset the economic and demographic consequences of an aging population.

6. Acknowledgements

We are thankful to Anu Rammohan, Xiaohui Zhang, Lionel Page, Jayanta Sarkar, Robert Breunig, participants at the 2015 Australian Conference of Economists, and an anonymous referee for helpful insights. All remaining errors are our own.

This paper uses unit record data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey. The HILDA Project was initiated and is funded by the Australian Government Department of Social Services (DSS) and is managed by the Melbourne Institute of Applied Economic and Social Research

(Melbourne Institute). The findings and views reported in this paper, however, are those of the author and should not be attributed to either DSS or the Melbourne Institute.

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Appendix

Table A-1: Summary statistics for partner's education

Variable	All			Pre-bonus			Post-bonus		
	Australian-born	Immigrant	Mean diff.	Australian-born	Immigrant	Mean diff.	Australian-born	Immigrant	Mean diff.
	(1)	(2)	(1)–(2)	(3)	(4)	(3)–(4)	(5)	(6)	(5)–(6)
Educational qualification									
Postgraduate	0.026	0.058	−0.032***	0.025	0.051	−0.026***	0.027	0.062	−0.035***
	(0.161)	(0.234)	(0.003)	(0.156)	(0.219)	(0.004)	(0.163)	(0.242)	(0.003)
Grad diploma or	0.032	0.043	−0.010***	0.031	0.037	−0.005	0.033	0.046	−0.013***
Grad cert.	(0.177)	(0.203)	(0.003)	(0.175)	(0.189)	(0.005)	(0.178)	(0.210)	(0.004)
Bachelor or	0.081	0.114	−0.033***	0.077	0.112	−0.035***	0.083	0.115	−0.033***
Honours	(0.273)	(0.318)	(0.004)	(0.267)	(0.315)	(0.007)	(0.276)	(0.319)	(0.005)
Adv. diploma	0.053	0.081	−0.028***	0.051	0.079	−0.028***	0.054	0.082	−0.028***
Diploma	(0.224)	(0.273)	(0.004)	(0.220)	(0.270)	(0.006)	(0.226)	(0.275)	(0.004)
Cert. III or IV	0.192	0.163	0.030***	0.191	0.165	0.026**	0.193	0.161	0.032***
	(0.394)	(0.369)	(0.006)	(0.393)	(0.372)	(0.010)	(0.394)	(0.367)	(0.007)
Cert. I or II	0.008	0.003	0.005***	0.007	0.002	0.005*	0.009	0.004	0.005**
	(0.090)	(0.056)	(0.001)	(0.085)	(0.046)	(0.002)	(0.093)	(0.061)	(0.002)
Cert. not defined	0.002	0.000	0.002**	0.002	0.000	0.002*	0.002	0.000	0.002*
	(0.044)	(0.000)	(0.001)	(0.047)	(0.000)	(0.001)	(0.043)	(0.000)	(0.001)
High school	0.064	0.071	−0.007*	0.061	0.071	−0.009	0.065	0.071	−0.006
	(0.244)	(0.257)	(0.004)	(0.240)	(0.256)	(0.006)	(0.246)	(0.257)	(0.005)
Year 11 and below	0.135	0.117	0.018***	0.145	0.131	0.014	0.130	0.109	0.021***
	(0.342)	(0.321)	(0.005)	(0.352)	(0.337)	(0.009)	(0.336)	(0.312)	(0.006)
No partner info	0.406	0.350	0.056***	0.408	0.353	0.055***	0.405	0.349	0.056***
Reported	(0.491)	(0.477)	(0.007)	(0.492)	(0.478)	(0.013)	(0.491)	(0.477)	(0.009)
Observations	21,448	5,366	26,814	7,240	1,898	9,138	14,208	3,468	17,676

Notes: Standard deviations are reported in parentheses. Standard errors are reported for mean differences. * / ** / *** denote statistical significance at the 0.10 / 0.05/ 0.01 levels respectively.

Table A-2: Differential impact of baby bonus on immigrants: Triple difference (DDD) estimates (using controls 3 and 4)

	Panel A: Treated: < HS and age < 25		Panel B: Treated: < HS and age < 30		Panel C: Treated: < HS and age < 35		Panel D: Treated: < HS and age < 40
	< HS and age ≥ 25	≥ HS and age ≥ 25	< HS and age ≥ 30	≥ HS and age ≥ 30	< HS and age ≥ 35	≥ HS and age ≥ 35	≥ HS and age ≥ 40
Immigrant	–0.012	–0.017**	–0.005	–0.015**	0.000	–0.006	–0.003
	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)	(0.006)	(0.003)
Treatment (τ)	0.085***	0.041**	0.108***	0.058***	0.115***	0.077***	0.072***
	(0.018)	(0.017)	(0.016)	(0.015)	(0.013)	(0.012)	(0.009)
Post-bonus	0.002	–0.005	0.004	–0.004	0.005	–0.003	–0.002
	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
Imm*τ	–0.079***	–0.095***	–0.068**	–0.069**	–0.041	–0.035	–0.008
	(0.022)	(0.028)	(0.032)	(0.032)	(0.033)	(0.033)	(0.025)
Imm*Post-bonus	–0.008	0.001	–0.013	0.003	–0.011	0.002	0.001
	(0.010)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.004)
τ*Post-bonus	0.013	0.020	0.006	0.016	0.006	0.015	0.021*
	(0.020)	(0.019)	(0.018)	(0.017)	(0.014)	(0.015)	(0.011)
Imm*τ*Post-bonus	0.147*	0.152*	0.123*	0.108*	0.044	0.031	–0.000
	(0.087)	(0.083)	(0.065)	(0.064)	(0.054)	(0.054)	(0.037)
Constant	–0.241***	–0.161***	–0.260***	–0.198***	–0.268***	–0.214***	–0.179***
	(0.032)	(0.029)	(0.030)	(0.029)	(0.030)	(0.025)	(0.023)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,470	16,536	7,470	14,731	7,470	12,946	10,821
Adjusted R-squared	0.058	0.075	0.077	0.073	0.098	0.073	0.088

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-3: Sensitivity analysis of triple difference (DDD) estimates (using controls 1 and 2) with 2005 included in the pre-bonus period

	Panel A Treated: < HS and age < 25		**Panel B Treated: < HS & and age < 30**		**Panel C Treated: < HS and age < 35**		**Panel D Treated: < HS and age < 40**	
	≥ *HS* and age < 25	**< *HS* and 25 ≤ age < 40**	**≥ *HS* and age < 30**	**< *HS* and 30 ≤ age < 40**	**≥ *HS* and age < 35**	**< *HS* and 35 ≤ age < 40**	**≥ *HS* and age < 40**	**< *HS* and age ≥ 40**
Immigrant	–0.001	–0.013	–0.004	–0.001	–0.004	0.035	–0.012	–0.007*
	(0.009)	(0.025)	(0.010)	(0.029)	(0.011)	(0.035)	(0.010)	(0.004)
Treatment (*τ*)	–0.023*	0.093***	–0.049***	0.099***	–0.053***	0.115***	–0.061***	0.088***
	(0.012)	(0.020)	(0.013)	(0.019)	(0.011)	(0.015)	(0.009)	(0.008)
Post-bonus	0.002	0.016	0.001	0.014	0.001	0.041**	–0.004	0.001
	(0.006)	(0.014)	(0.006)	(0.015)	(0.006)	(0.017)	(0.006)	(0.002)
Imm*τ	–0.036	–0.058	–0.047	–0.067	–0.052*	–0.094**	–0.022	–0.016
	(0.037)	(0.044)	(0.033)	(0.041)	(0.028)	(0.043)	(0.024)	(0.021)
Imm*Post-bonus	–0.009	0.008	0.009	–0.017	0.016	–0.055	0.023	–0.003
	(0.017)	(0.042)	(0.015)	(0.041)	(0.017)	(0.050)	(0.015)	(0.004)
τ*Post-bonus	0.006	–0.010	0.034**	0.001	0.020	–0.032	0.031**	0.017
	(0.018)	(0.022)	(0.017)	(0.022)	(0.015)	(0.022)	(0.012)	(0.011)
Imm*τ*Post-bonus	0.091*	0.118	0.069	0.133**	0.049	0.129*	0.007	0.030
	(0.046)	(0.077)	(0.049)	(0.067)	(0.049)	(0.071)	(0.040)	(0.039)
Constant	–0.020	–0.372***	–0.005	–0.359***	–0.105***	–0.367***	–0.171***	–0.226***
	(0.034)	(0.065)	(0.032)	(0.059)	(0.032)	(0.058)	(0.030)	(0.029)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,089	2,829	6,710	2,829	9,721	2,829	13,151	7,470
Adjusted R-squared	0.344	0.098	0.244	0.105	0.175	0.107	0.130	0.091

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-4: Sensitivity analysis of triple difference (DDD) estimates (using controls 3 and 4) with 2005 included in the pre-bonus period

	Panel A: Treated: < HS and age < 25		Panel B: Treated: < HS and age < 30		Panel C: Treated: < HS and age < 35		Panel D: Treated: < HS and age < 40
	< HS and age ≥ 25	≥ HS and age ≥ 25	< HS and age ≥ 30	≥ HS and age ≥ 30	< HS and age ≥ 35	≥ HS and age ≥ 35	≥ HS and age ≥ 40
Immigrant	–0.015**	–0.023***	–0.009	–0.018***	–0.000	–0.010*	–0.003
	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.005)	(0.003)
Treatment (*τ*)	0.088***	0.044***	0.108***	0.059***	0.119***	0.080***	0.076***
	(0.017)	(0.014)	(0.014)	(0.013)	(0.011)	(0.011)	(0.008)
Post-bonus	0.002	–0.008*	0.002	–0.007	0.006*	–0.006*	–0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
Imm*τ	–0.041	–0.050	–0.053*	–0.051*	–0.054**	–0.044*	–0.017
	(0.035)	(0.037)	(0.029)	(0.029)	(0.025)	(0.025)	(0.021)
Imm*Post-bonus	–0.003	0.014*	–0.010	0.011	–0.014*	0.009	0.003
	(0.010)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.003)
τ*Post-bonus	0.012	0.020	0.009	0.020	–0.000	0.013	0.020*
	(0.018)	(0.018)	(0.017)	(0.016)	(0.014)	(0.014)	(0.011)
Imm*τ*Post-bonus	0.121*	0.114	0.138**	0.113*	0.092*	0.068	0.024
	(0.074)	(0.071)	(0.061)	(0.060)	(0.052)	(0.051)	(0.039)
Constant	–0.241***	–0.161***	–0.259***	–0.197***	–0.267***	–0.214***	–0.179***
	(0.032)	(0.029)	(0.030)	(0.029)	(0.029)	(0.025)	(0.023)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,470	16,537	7,470	14,732	7,470	12,947	10,822
Adjusted R-squared	0.058	0.075	0.078	0.073	0.099	0.073	0.088

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-5: Sensitivity analysis of triple difference (DDD) estimates (using controls 1 and 2) with 2009 included in the post-bonus period

	Panel A Treated: < HS and age < 25		Panel B Treated: < HS and age < 30		Panel C Treated: < HS and age < 35		Panel D Treated: < HS and age < 40	
	≥ *HS* and age < 25	< *HS* and 25 ≤ age < 40	≥ *HS* and age < 30	< *HS* and 30 ≤ age < 40	≥ *HS* and age < 35	< *HS* and 35 ≤ age < 40	≥ *HS* and age < 40	< *HS* and age ≥ 40
Immigrant	–0.007	0.009	0.007	0.023	0.005	0.047	–0.004	–0.008**
	(0.012)	(0.029)	(0.014)	(0.032)	(0.014)	(0.036)	(0.013)	(0.003)
Treatment (*τ*)	–0.025	0.094***	–0.054***	0.103***	–0.059***	0.114***	–0.066***	0.084***
	(0.015)	(0.022)	(0.015)	(0.020)	(0.013)	(0.017)	(0.010)	(0.009)
Post-bonus	–0.003	0.013	0.002	0.016	0.001	0.030*	–0.000	–0.000
	(0.006)	(0.013)	(0.006)	(0.014)	(0.006)	(0.015)	(0.006)	(0.002)
Imm*τ	–0.074**	–0.118***	–0.077**	–0.104**	–0.052	–0.094**	–0.016	–0.005
	(0.037)	(0.040)	(0.037)	(0.046)	(0.034)	(0.047)	(0.027)	(0.024)
Imm*Post-bonus	–0.004	–0.020	–0.024	–0.046	–0.015	–0.059	–0.005	0.004
	(0.015)	(0.037)	(0.016)	(0.038)	(0.019)	(0.043)	(0.015)	(0.005)
τ*Post-bonus	0.007	–0.010	0.026	–0.008	0.021	–0.019	0.025**	0.015
	(0.018)	(0.022)	(0.017)	(0.021)	(0.015)	(0.020)	(0.012)	(0.011)
Imm*τ*Post-bonus	0.130**	0.163**	0.111**	0.157**	0.050	0.102	0.007	0.004
	(0.060)	(0.082)	(0.052)	(0.064)	(0.050)	(0.064)	(0.038)	(0.035)
Constant	–0.019	–0.366***	–0.001	–0.352***	–0.093***	–0.362***	–0.157***	–0.221***
	(0.030)	(0.060)	(0.029)	(0.054)	(0.030)	(0.053)	(0.027)	(0.027)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,808	3,196	7,919	3,196	11,363	3,196	15,306	8,468
Adjusted R-squared	0.340	0.095	0.247	0.102	0.176	0.104	0.132	0.089

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-6: Sensitivity analysis of triple difference (DDD) estimates (using controls 3 and 4) with 2009 included in the post-bonus period

	Panel A: Treated: < HS and age < 25		**Panel B: Treated: < HS and age < 30**		**Panel C: Treated: < HS and age < 35**		**Panel D: Treated: <HS and age <40**
	< HS and age ≥ 25	**≥ HS and age ≥ 25**	**< HS and age ≥ 30**	**≥ HS and age ≥ 30**	**< HS and age ≥ 35**	**≥ HS and age ≥ 35**	**≥ HS and age ≥ 40**
Immigrant	–0.011	–0.017**	–0.005	–0.015**	0.001	–0.007	–0.003
	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)	(0.006)	(0.003)
Treatment (*τ*)	0.085***	0.040**	0.108***	0.058***	0.116***	0.077***	0.071***
	(0.018)	(0.017)	(0.016)	(0.015)	(0.013)	(0.012)	(0.009)
Post-bonus	–0.001	–0.004	0.001	–0.004	0.003	–0.003	–0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
Imm*τ	–0.078***	–0.095***	–0.068**	–0.068**	–0.041	–0.035	–0.008
	(0.022)	(0.028)	(0.032)	(0.032)	(0.033)	(0.033)	(0.024)
Imm*Post-bonus	–0.002	–0.002	–0.008	0.002	–0.007	0.001	0.001
	(0.010)	(0.008)	(0.009)	(0.008)	(0.007)	(0.007)	(0.004)
τ*Post-bonus	0.008	0.012	0.002	0.009	0.003	0.009	0.016
	(0.019)	(0.018)	(0.017)	(0.017)	(0.014)	(0.014)	(0.011)
Imm*τ*Post-bonus	0.131	0.146*	0.128**	0.119**	0.052	0.043	0.007
	(0.080)	(0.077)	(0.058)	(0.057)	(0.049)	(0.049)	(0.035)
Constant	–0.235***	–0.150***	–0.252***	–0.189***	–0.262***	–0.209***	–0.174***
	(0.030)	(0.027)	(0.028)	(0.027)	(0.027)	(0.024)	(0.021)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,468	19,429	8,468	17,238	8,468	15,140	12,615
Adjusted R-squared	0.055	0.076	0.075	0.071	0.097	0.070	0.084

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-7: Sensitivity analysis of triple difference (DDD) estimates (using controls 1 and 2) with 2005 pre-bonus period and 2009 post-bonus

	Panel A Treated: < HS and age < 25		Panel B Treated: < HS and age < 30		Panel C Treated: < HS and age < 35		Panel D Treated: < HS and age < 40	
	≥ *HS* and age < 25	< *HS* and25 ≤ age < 40	≥ *HS* and age < 30	< *HS* and30 ≤ age < 40	≥ *HS* and age < 35	< *HS* and35 ≤ age < 40	≥ *HS* and age < 40	< *HS* and age ≥ 40
Immigrant	–0.002	–0.014	–0.005	–0.001	–0.004	0.035	–0.012	–0.007*
	(0.009)	(0.025)	(0.010)	(0.029)	(0.011)	(0.035)	(0.010)	(0.004)
Treatment (*τ*)	–0.021*	0.094***	–0.049***	0.100***	–0.054***	0.116***	–0.062***	0.088***
	(0.012)	(0.020)	(0.013)	(0.018)	(0.011)	(0.015)	(0.009)	(0.008)
Post-bonus	–0.000	0.010	0.001	0.010	0.000	0.033**	–0.003	–0.001
	(0.005)	(0.013)	(0.006)	(0.014)	(0.006)	(0.016)	(0.005)	(0.002)
Imm**τ*	–0.034	–0.057	–0.046	–0.068	–0.052*	–0.094**	–0.021	–0.016
	(0.037)	(0.044)	(0.033)	(0.041)	(0.028)	(0.043)	(0.024)	(0.021)
Imm*Post-bonus	–0.014	0.022	–0.009	–0.010	–0.001	–0.053	0.008	0.003
	(0.016)	(0.038)	(0.015)	(0.038)	(0.016)	(0.047)	(0.014)	(0.004)
*τ**Post-bonus	0.002	–0.011	0.025	–0.004	0.017	–0.030	0.025**	0.012
	(0.016)	(0.020)	(0.016)	(0.020)	(0.014)	(0.020)	(0.011)	(0.010)
Imm**τ**Post-bonus	0.091**	0.089	0.085**	0.131**	0.067	0.133**	0.025	0.030
	(0.042)	(0.069)	(0.043)	(0.058)	(0.042)	(0.063)	(0.036)	(0.035)
Constant	–0.022	–0.363***	0.000	–0.347***	–0.092***	–0.358***	–0.156***	–0.221***
	(0.030)	(0.060)	(0.029)	(0.054)	(0.030)	(0.053)	(0.027)	(0.027)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,808	3,196	7,919	3,196	11,363	3,196	15,306	8,468
Adjusted R-squared	0.339	0.094	0.246	0.102	0.176	0.104	0.132	0.089

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-8: Sensitivity analysis of triple difference (DDD) estimates (using controls 3 and 4) with 2005 pre-bonus period and 2009 post-bonus

	Panel A: Treated: < HS and age < 25		Panel B: Treated: < HS and age < 30		Panel C: Treated: < HS and age < 35		Panel D: Treated: < HS and age < 40
	< HS and age ≥ 25	≥ HS and age ≥ 25	< HS and age ≥ 30	≥ HS and age ≥ 30	< HS and age ≥ 35	≥ HS and age ≥ 35	≥ HS and age ≥ 40
Immigrant	–0.015**	–0.023***	–0.009	–0.019***	–0.000	–0.010*	–0.003
	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.005)	(0.003)
Treatment (τ)	0.087***	0.043***	0.108***	0.059***	0.119***	0.080***	0.075***
	(0.016)	(0.014)	(0.014)	(0.013)	(0.011)	(0.011)	(0.008)
Post-bonus	–0.002	–0.006	–0.001	–0.007	0.003	–0.005	–0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
Imm*τ	–0.041	–0.050	–0.053*	–0.051*	–0.054**	–0.044*	–0.017
	(0.035)	(0.037)	(0.029)	(0.029)	(0.025)	(0.025)	(0.021)
Imm*Post-bonus	0.005	0.008	–0.003	0.008	–0.008	0.007	0.003
	(0.010)	(0.007)	(0.008)	(0.007)	(0.007)	(0.006)	(0.003)
τ*Post-bonus	0.007	0.010	0.003	0.010	–0.003	0.006	0.013
	(0.016)	(0.016)	(0.015)	(0.015)	(0.013)	(0.013)	(0.010)
Imm*τ*Post-bonus	0.098	0.104	0.138***	0.122**	0.095**	0.078*	0.031
	(0.065)	(0.064)	(0.051)	(0.051)	(0.044)	(0.044)	(0.035)
Constant	–0.235***	–0.150***	–0.251***	–0.189***	–0.261***	–0.208***	–0.174***
	(0.030)	(0.027)	(0.028)	(0.027)	(0.027)	(0.024)	(0.021)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,468	19,429	8,468	17,238	8,468	15,140	12,615
Adjusted R-squared	0.055	0.076	0.076	0.071	0.098	0.070	0.085

Notes: Cluster-robust standard errors shown in parentheses; * / ** / *** denote significance at 0.10 / 0.05 / 0.01 levels respectively. All models include controls for the number of children, marital status, log gross income, employment status, and partner's education. Each panel corresponds to a treatment, and each column corresponds to an alternative control group for that treatment as summarized in Table 1. In each panel heading, HS refers to high school.

Table A-9: Average treatment effect for immigrants and native subsamples using propensity score matching (using controls 3 and 4)

	Panel A: < HS and age < 25		Panel B: < HS and age < 30		Panel C: < HS and age < 35		Panel D: < HS and age < 40
Natives	**< HS and age ≥ 25**	**≥ HS and age ≥ 25**	**< HS and age ≥ 30**	**≥ HS and age ≥ 30**	**< HS and age ≥ 35**	**≥ HS and age ≥ 35**	**≥ HS and age ≥ 40**
Pre-bonus	0.0406***	0.0093	0.0734***	0.0429***	0.0911***	0.0710***	0.0725***
	(0.0118)	(0.0154)	(0.0097)	(0.0121)	(0.0081)	(0.0088)	(0.0068)
	2214	3679	2338	3452	2365	3218	2735
Post-bonus	0.0563***	0.0345***	0.0754***	0.0518***	0.0954***	0.0814***	0.0906***
	(0.0090)	(0.0100)	(0.0077)	(0.0084)	(0.0068)	(0.0065)	(0.0050)
	3795	8064	3999	7547	3999	6565	5475

	Panel A: < HS and age < 25		Panel B: < HS and age < 30		Panel C: < HS and age < 35		Panel D: < HS and age < 40
Immigrants	**< HS and age ≥ 25**	≥ HS and age ≥ 25	< HS and age ≥ 30	≥ HS and age ≥ 30	< HS and age ≥ 35	≥ HS and age ≥ 35	≥ HS and age ≥ 40
Pre-bonus	–0.0181	–0.0361	0.0100	–0.0082	0.0396**	0.0437**	0.0660***
	(0.0292)	(0.0407)	(0.0230)	(0.0308)	(0.0189)	(0.0212)	(0.0113)
	353	880	389	917	389	889	834
Post-bonus	0.1231***	0.1033***	0.1368***	0.1120***	0.1003***	0.0801***	0.0875***
	(0.0278)	(0.0340)	(0.0214)	(0.0263)	(0.0159)	(0.0182)	(0.0088)
	592	1781	592	1664	679	1939	1614

Notes: Heteroskedasticity-consistent analytical standard errors proposed by Abadie and Imbens (2006) are reported parentheses. * / ** / *** denote statistical significance at the 0.10 / 0.05 / 0.01 levels respectively. We match on the number of children, marital status, log gross income, employment status, and partner's education in all specifications for direct comparison with Table 3. Nearest neighbour match with replacement was used. In each panel heading, HS refers to high school.