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**Re-examining female labor supply responses to the
1994 Australian pension reform**

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Abstract:

Many governments are aiming to extend working lives by raising the age at which people can claim retirement pensions. This makes it vital to understand how these policies affect retirement decisions. In this paper, I revisit the labor supply effects of a major Australian reform that increased women's pension age from 60 to 65. Atalay and Barrett (2015) studied these effects using repeated household surveys and a differences-in-differences design in which male cohorts form the comparison group. They estimate that the reform increased female labor force participation by 12 percentage points. Using earlier data, I show that the parallel-trends assumption did not hold before the reform because of a strong female-specific trend in participation rates across the relevant cohorts. Accounting for this trend, the estimated effect on female participation falls by two-thirds and becomes statistically insignificant at conventional levels. This highlights the importance of carefully assessing and controlling for trends across cohorts when evaluating pension reforms, which are typically phased in across cohorts.

Zusammenfassung:

Viele Staaten versuchen durch eine Anhebung des Renteneintrittsalters eine Verlängerung der Lebensarbeitszeit zu erreichen. Daher ist es von entscheidender Bedeutung zu verstehen, wie solche Maßnahmen die Rentenentscheidungen beeinflussen. In diesem Beitrag untersuche ich die Auswirkungen einer großen australischen Reform auf das Arbeitsangebot, mit der das Rentenalter von Frauen von 60 auf 65 Jahre angehoben wurde. Atalay und Barrett (2015) haben diese Auswirkungen anhand von wiederholten Haushaltsbefragungen und einem „Difference-in-differences“-Design untersucht, bei dem Kohorten von Männern die Vergleichsgruppe bilden. Ihren Ergebnissen zufolge hat die Reform die Erwerbsbeteiligung von Frauen um geschätzte 12 Prozentpunkte erhöht. Auf der Grundlage weiter zurückreichender Daten kann ich eine Verletzung der zugrundeliegenden Annahme paralleler Trends vor der Reform – aufgrund eines starken Frauen-spezifischen Trends bei den Erwerbsquoten in den relevanten Kohorten – nachweisen. Unter Berücksichtigung dieses Trends sinkt der geschätzte Effekt auf die Frauenerwerbsbeteiligung um zwei Drittel auf ein statistisch nicht mehr signifikantes Niveau. Dieser Befund verweist auf die Wichtigkeit einer sorgfältigen Überprüfung und Kontrolle kohortenspezifischer Trends bei der Bewertung von Rentenreformen, die in der Regel über die Kohorten hinweg stufenweise eingeführt werden.

Keywords:

Retirement age, labor supply, cohort effects

JEL Classification:

H55, J26

Re-examining female labor supply responses to the 1994 Australian pension reform*

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Abstract

Many governments are aiming to extend working lives by raising the age at which people can claim retirement pensions. This makes it vital to understand how these policies affect retirement decisions. In this paper, I revisit the labor supply effects of a major Australian reform that increased women's pension age from 60 to 65. Atalay and Barrett (2015) studied these effects using repeated household surveys and a differences-in-differences design in which male cohorts form the comparison group. They estimate that the reform increased female labor force participation by 12 percentage points. Using earlier data, I show that the parallel-trends assumption did not hold before the reform because of a strong female-specific trend in participation rates across the relevant cohorts. Accounting for this trend, the estimated effect on female participation falls by two-thirds and becomes statistically insignificant at conventional levels. This highlights the importance of carefully assessing and controlling for trends across cohorts when evaluating pension reforms, which are typically phased in across cohorts.

Keywords: Retirement age; labor supply; cohort effects

JEL Classification: H55, J26, I38

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1 Introduction

In response to aging populations, many governments are reducing the generosity of retirement pensions for younger cohorts through increases in eligibility ages or reductions in benefit amounts. A key aim of these reforms is to encourage older workers to remain in the labor force, making it vital to understand how these policies affect retirement decisions. While several studies find large effects of these reforms on retirement decisions (e.g., Börsch-Supan and Schnabel, 1998; Snyder and Evans, 2006; Hanel and Riphahn, 2012; Staubli and Zweimüller, 2013; Vestad, 2013; Cribb, Emmerson and Tetlow, 2016; Lalive, Magesan and Staubli, 2017; Geyer and Welteke, 2019), it has been difficult to disentangle the key mechanisms as these reforms may affect retirement behavior through several channels. For example, these reforms may encourage people to delay retirement due to (i) a decrease in individuals' lifetime pension wealth, known as the wealth effect, (ii) the opportunity to work for a longer period and thus accrue a larger pension, known as the accrual effect, (iii) a change in social norms, (iv) the presence of liquidity constraints, or (v) a deferral of any income/earnings test, which discourages work among individuals who are eligible for a pension.

An important paper in this literature is Atalay and Barrett (2015). Atalay and Barrett (2015) study an Australian reform in 1994 that gradually raised women's eligibility age for the means-tested public retirement pension from 60 to 65. Atalay and Barrett (2015) argue that this reform is useful for understanding mechanisms because Australia's retirement pension is non-contributory. This means that there is no accrual effect — an effect which has been emphasized in the literature (Samwick, 1998; Gruber and Wise, 2004) — and any impact on retirement decisions must result from other mechanisms. Atalay and Barrett (2015) estimate the impact of the reform on female labor force participation at ages 60 to 64 using repeated household surveys ranging from 1994 to 2010 and a differences-in-differences design in which male cohorts form the comparison group. Their headline estimate is that each one-year increase in women's eligibility age increased female labor force participation by 12 percentage points, an effect they interpret as evidence of strong wealth effects.¹

In this paper, I start by replicating Atalay and Barrett's (2015) labor force participation estimates. Using the same source data, I apply the sample restrictions and methods described in Atalay and Barrett's (2015) paper and find that their estimates can be successfully replicated. Namely, due to the clarity of Atalay and Barrett's (2015) paper, I am able to closely match the sample and regression estimates without needing to use original program files. Then, I extend the analysis with earlier data, building on a literature that is increasingly scrutinising difference-in-difference methods (e.g., Bertrand, Duflo and Mullainathan, 2004; Wolfers, 2006; Donald and Lang, 2007; Cameron, Gelbach and Miller, 2008; Borjas, 2017; Goodman-Bacon, 2018). In the Australian context studied by Atalay and Barrett (2015), one concern with using men as

¹Atalay and Barrett (2015) also estimate significant spillovers on other government programs, such as the Disability Support Pension.

the comparison group is that, as in many countries, the participation rate for women has risen considerably over recent decades, while the rate for men has been relatively stable. With these gender-specific time trends in mind, later cohorts of women may have higher participation rates for reasons other than their higher pension age. If this is the case, Atalay and Barrett's (2015) estimates may be biased upwards. As Atalay and Barrett's (2015) sample period started after the reform was announced, they were unable to use their data to examine whether the parallel-trends assumption held before the reform for the cohorts in their sample.

I extend the analysis using two sources of data from before the reform was announced. First, I use aggregate data on participation rates by age and gender to examine whether the parallel-trends assumption held before the reform for the cohorts in Atalay and Barrett's (2015) sample. This builds on Figure 1 in Atalay and Barrett (2015), which showed similar pre-reform trends in male and female participation rates near the retirement age among earlier cohorts. For the relevant cohorts, I find opposing trends in male and female participation rates before the reform (at younger ages), with a strong upward trend in female participation rates and a weak downward trend in male participation rates. For example, at ages 45 to 49, women's participation rate increased by 18.6 percentage points in the fifteen years before Atalay and Barrett's (2015) sample period (from 52.5% to 71.1%), while men's participation rate decreased by 2.4 percentage points (from 93.5% to 91.1%). Moreover, I find similar gender-specific trends among these cohorts at both older and younger ages, suggesting that women would have reduced the gender gap in participation rates at ages 60 to 64 over Atalay and Barrett's (2015) sample period even if the reform had not occurred. This indicates that Atalay and Barrett's (2015) estimates, which attribute the convergence in male and female participation rates to the reform, may be biased upwards.

Second, I use repeated household surveys from 1982 to 1990 to examine whether the different trends of men and women can be explained by the control variables in Atalay and Barrett's (2015) regressions. Specifically, I estimate placebo regressions using Atalay and Barrett's (2015) differences-in-differences approach to test for an impact on the participation rates of female cohorts before the reform was announced. In these regressions, positive estimates of the (placebo) treatment effect would indicate that Atalay and Barrett's (2015) estimates are likely to be biased upwards. I find that the estimates are positive, highly statistically significant, and broadly consistent with the size of Atalay and Barrett's (2015) estimates in percentage terms. Thus, Atalay and Barrett's (2015) estimates are likely to confound women's responses to the reform with a persistent cohort trend in female participation rates.

In the final part of the paper, I use the replication sample to re-estimate the effects of the reform on female labor force participation. I follow the empirical strategy of several recent studies examining phased increases in pension-eligibility ages (Staubli and Zweimüller, 2013; Cribb, Emmerson and Tetlow, 2016; Oguzoglu, Polidano and Vu, 2020). This approach differs from Atalay and Barrett's (2015) approach by

directly controlling for the trend in female participation rates across cohorts. These controls may partially absorb any wealth effects from the reform but not effects from other mechanisms, such as a change in social norms, liquidity constraints or the pension’s income test. I find that the estimated increase in female labor force participation falls by two-thirds to 4.1 percentage points and becomes statistically indistinguishable from zero at the 10% level (with a p -value of 0.109). This estimate provides only weak evidence that the reform had any effect on female labor force participation. This modest impact is consistent with the raw data, which shows little evidence of a drop in female participation rates at the eligibility age (and only a small decrease in male participation rates). Moreover, the estimated effect is similar in magnitude to (unpublished) estimates of the reform by Morris (2019) based on more detailed survey data.² These impacts are not only smaller than Atalay and Barrett’s (2015) estimates but also, as discussed below, smaller than estimates of similar reforms in other countries.

More generally, the results in this paper have implications for studies estimating the impact of retirement reforms on female labor supply or other (potentially) interdependent outcomes such as welfare receipt and health.³ Over recent decades, female labor force participation rates have increased considerably across the developed world; from 1970 to 2016, the participation rate of women aged 25 to 64 in OECD countries increased from 46.3% to 68.1% (OECD, 2018). In the context of Australia, my results show that these trends can result in meaningful differences across cohorts in female participation rates near the retirement age. Even when these differences appear to reflect logical responses to a decrease in the generosity of retirement benefits for later cohorts — and even when differences across earlier cohorts are minimal near the retirement age — it is important, where possible, to examine trends across the relevant cohorts in earlier time periods.

2 Background and replication of Atalay and Barrett (2015)

Institutional background. Australia’s public retirement pension, the Age Pension, is received by around 70% of older Australians. Unlike in many other countries, employment history does not affect pension eligibility or payment rates. Instead, there is a means test based on the current income and assets of the household; from a maximum level, payments are gradually reduced to zero for households with higher income and assets. In practice, around two-thirds of pension recipients receive the maximum payment (Barrett and Tseng, 2008). In 2008, near the end of Atalay and Barrett’s (2015) sample period, this payment was \$546.80 every two weeks for singles (~\$14,300 per annum) and \$913.60 for couples combined (~\$23,800

²Using longitudinal survey data from 2001 to 2015, Morris (2019) estimates a 3.1-percentage-point increase in female labor force participation, an effect that is statistically significant at the 10% level. These estimates correspond to the average impact of the reform on a slightly different set of cohorts at slightly different ages.

³Recent studies examining the impact of retirement reforms on female labor supply in other countries include the following: Hanel and Riphahn (2012); Staubli and Zweimüller (2013); Vestad (2013); Cribb, Emmerson and Tetlow (2016); Lalive, Magesan and Staubli (2017); and Geyer and Welteke (2019).

per annum).⁴ For perspective, the median equivalized household income in Australia in 2008 was \$692 per week. Thus, a single household receiving the maximum payment and no other income would have income equal to 40% of the median household income in the Australian population (44% for couples).⁵

Households are eligible for the maximum payment if their assessable income and assets are below the respective income and asset disregards.⁶ Otherwise, payments are determined by either the income test or the assets test, whichever implies a lower payment. In 2008, each dollar of assessable income above the income disregard reduced Age Pension payments by 40 cents (20 cents each for couples) and each \$1,000 of assessable assets above the asset disregard reduced payments by \$1.50. In practice, the income test binds for many more pensioners than the assets test (Chomik et al., 2018).

To be eligible for the Age Pension, individuals must reach the Age Pension Age (APA). The APA is most similar to an Early Retirement Age (ERA); it is the first age individuals can claim the Age Pension. However, there is a stronger incentive to start claiming at the APA because payment rates do not depend on the age at which individuals start claiming. Prior to the Social Security Legislation Amendment Act (No. 2) 1994, the APA was 60 for women and 65 for men. This reform, proposed in 1993, legislated that women's APA would increase to 65. As shown in Figure 1, the increases were phased in based on women's date of birth. For example, the APA increased to 60.5 for women born between July 1935 and December 1936, and it increased to 61 for women born between January 1937 and June 1938. The APA continued increasing in this manner until it reached 65 — the APA of men — for women born after December 1948.

Other sources of retirement funding exist besides the Age Pension. For example, many Australians receive other government payments before they reach the APA. Oguzoglu, Polidano and Vu (2020) show that, depending on the cohort, around 30–45% of women receive a regular payment before they reach the APA, with the most common payment being the Disability Support Pension. Like the Age Pension, other payments are non-contributory and means tested, and many have the same payment rates. These payments have additional eligibility criteria but no age conditions. Typically, recipients of other payments are transferred onto the Age Pension at the APA (Oguzoglu, Polidano and Vu, 2020). Australians can also fund their retirement privately through voluntary savings and a mandatory scheme called superannuation, in which employers make contributions to their employees' private retirement accounts. However, the Age Pension is the main source of retirement income for older women.⁷

⁴The average exchange rate in 2008 was 1 AUD = 0.85 USD (ranging 0.60–0.98 USD). Thus, at the average exchange rate, the maximum payment was equivalent to US\$12,155 per annum for singles and US\$20,230 for couples.

⁵For this calculation (and other measures of household income in the paper), I adjust for household size using an equivalence scale of 1.5 for couples.

⁶Most forms of income are assessed. In 2008, the income disregard was \$138 for singles and \$240 for couples. The assets test excludes the value of the primary residence but assesses most other assets. The asset disregard depends on homeownership. In 2008, the asset disregard for homeowners was \$171,750 for singles and \$243,500 for couples. For non-homeowners, the respective disregards were around \$120,000 higher.

⁷Most older women have little saved in superannuation. In 2013/14, the median woman aged 60–64 had just \$28,000 of

Sample. Atalay and Barrett’s (2015) sample comes from 11 cross-sections of the nationally representative Australian Bureau of Statistics Income and Housing Costs Surveys (IHCS). The first survey was in 1994/95 and the last in 2009/10, with surveys conducted over the course of a financial year and a gap of at most one year between surveys.⁸ Atalay and Barrett (2015) restrict their sample to men and women aged 60–64. They also exclude immigrants who have lived in Australia for less than ten years, who would not satisfy the residency condition for the Age Pension. Using the same source data and the information in Atalay and Barrett’s (2015) paper, I am able to closely replicate their sample. Appendix Table B1 shows that the replication sample is similar to Atalay and Barrett’s (2015) sample in terms of the number of observations and the means of the key variables.⁹ Overall, women in the sample have low rates of labor force participation (LFP), with just 27% of women participating compared to 52% of men. A relatively high proportion of women in the sample receive a government payment (56%),¹⁰ with 39% of women receiving a payment before they are eligible for the Age Pension. Due to the means-testing of payments, payment recipients are typically in lower income households. In 2008 dollars, female payment recipients have average equivalized household income of \$441 (including pension income),¹¹ compared to \$860 for non-recipients.

Mechanisms affecting the impact on female labor force participation. Before describing Atalay and Barrett’s (2015) estimates, it is worth discussing the relevant factors that may affect the size of the reform’s impact on female LFP. The reform may increase female LFP at ages 60–64 for two main reasons. First, it may be optimal for some women to permanently leave the labor force (i.e., retire) exactly at the APA, and thus we would expect some women to remain in the LFP longer until they reach the new APA. This optimality may arise due to (i) liquidity constraints, since individuals cannot borrow against their future Age Pension wealth, or (ii) the income test for the Age Pension, which increases the effective marginal tax rate on earnings by up to 40 percentage points for many individuals above the APA.¹² Second, the reform

superannuation wealth (Clare, 2015). Historically, older women would have had even less, since superannuation contributions only became mandatory in 1992. Moreover, superannuation wealth is likely to be highest among wealthier women who are not eligible for the Age Pension.

⁸Between 1994/95 and 2009/10, the IHCS was conducted in 11 out of 16 years. It was not conducted in 1998/99, 2001/02, 2004/05, 2006/07 and 2008/09.

⁹One point where there may be minor differences is the exclusion of immigrants who arrived in Australia in the last ten years. There is some discretion in implementing this restriction as the variable for ‘year of arrival in Australia’ is often only available in five-year windows (e.g., 1985–89). To be conservative, I exclude anyone with a non-zero probability of arriving in Australia in the last ten years. Nonetheless, I still have a slightly different number of observations (88 more respondents, or 0.7% of the sample) than in Atalay and Barrett (2015).

¹⁰This proportion is slightly higher than reported in Atalay and Barrett (2015), which appears to be due to differences in the payments included in this definition (which are not listed in Atalay and Barrett (2015)). I include recipients of the following payments: the Age Pension (34.9% of women), the Disability Support Pension (7.8%), Partner Allowance (3.4%), Widow Allowance (2.4%), Carer Payment (2.1%), Newstart Allowance (1.8%), Service Pension (1.1%), Mature Age Allowance (0.7%), Veteran’s Disability Pension (0.6%), War Widow’s Pension (0.5%), Wife Pension (0.3%), Parenting Payment (0.05%), Sickness Allowance (0.03%), Special Benefit (0.02%), and ‘other government cash pensions and allowances’ (0.15%).

¹¹Excluding women’s income from government payments, payment recipients have equivalized household income of \$341.

¹²The APA may also influence social norms by providing a signal as an appropriate age to retire. Cribb, Emmerson and Tetlow (2016) find evidence of this mechanism in the context of a similar reform in the U.K.

reduces the present value of women's expected lifetime income from the Age Pension. This negative wealth effect is the key mechanism emphasized by Atalay and Barrett (2015). Atalay and Barrett (2015) construct a model that indicates that women's optimal retirement age will increase as long as leisure is a normal good. The model implies that the reform will affect retirement decisions at all ages, including below the old APA and above the new APA. However, studies of comparable reforms indicate that most of the impact is likely to result from a shift in retirement decisions at ages in which pension eligibility is affected (Staubli and Zweimüller, 2013; Cribb, Emmerson and Tetlow, 2016; Geyer and Welteke, 2019).¹³

There are also several factors that may attenuate the impact of the reform on female LFP. First, women in these cohorts had very low rates of LFP, with many women having left (or never entered) the labor force before age 60. Second, women's access to other government payments provided a substitute form of retirement income for many women until they reached the new APA (Atalay and Barrett, 2015; Oguzoglu, Polidano and Vu, 2020). Using administrative data on all welfare-receipt spells from 1998 to 2013, Oguzoglu, Polidano and Vu (2020) estimate that the reform decreased women's receipt of government payments by only 12.6 percentage points, since a 51.9-percentage-point decrease in Age Pension receipt was offset by a 39.3-percentage-point increase in other payments.¹⁴ The decrease in the receipt of government payments was even smaller for single women (7.8 percentage points, Oguzoglu, Polidano and Vu, 2020), who are more likely to be financially constrained and faced a larger reduction in pension income (due to the higher per-person payment rates for singles). Third, the Age Pension is unavailable to many older women due to the means test, with Oguzoglu, Polidano and Vu's (2020) estimates implying that the reform had no effect on Age Pension receipt for 48.1% of women.

Finally, there is a mismatch between women who are likely to claim the Age Pension at the APA and women who are most likely to be in the labor force at ages 60–64. Appendix Figure B1 shows how Age Pension receipt and LFP varies across the distribution of household income for women in the sample. I sort women into quintiles based on their (current) equivalised household income, excluding their own

¹³Using register data, Staubli and Zweimüller (2013) examine increases in the ERA (and stronger penalties for early claiming) for Austrian men and women. They find large effects on employment rates at ages where eligibility was affected due to a spike in labor force exits at the ERA. However, they find little effect on employment rates at younger or older ages (see Table 6 and Figures 4 and 5 of their paper). Cribb, Emmerson and Tetlow (2016) study a similar reform in the UK that raised women's ERA from 60 to 62. Despite clear effects on employment rates at ages 60 and 61, there is no sign of an effect on employment rates at ages 59 and 62 (see Figure 3 of their paper). Moreover, the authors test for wealth effects by omitting cohort controls from their regressions and find that this does not materially affect their results. Finally, Geyer and Welteke (2019) exploit a sharp increase in the ERA in Germany from 60 to 63 for women born on or after January 1, 1952 using a regression discontinuity design. Although the reform was well anticipated and represented "a strong negative wealth shock" (Geyer and Welteke, 2017, p. 7), they find precisely estimated zero effects on employment rates prior to age 60 despite a large increase at ages 60–61 (14.4 percentage points).

¹⁴Atalay and Barrett (2015) estimate a larger decrease in women's receipt of government payments (27.0 percentage points) due to smaller estimates of the increase in the receipt of other government payments (21.2 percentage points). However, Oguzoglu, Polidano and Vu (2020, p.76) note that Atalay and Barrett's (2015) estimates "may not have adequately dealt with cohort differences" in the receipt of government payments and come from "survey data that may under-report the receipt of disability and unemployment benefits". Graphically, both papers show clear cohort differences in payment receipt well before the APA, with higher rates among earlier cohorts (consistent with their lower LFP rates).

income from the Age Pension (and other payments). In Appendix Figure B1a, I present Age Pension receipt for women in the sample who are above the APA, and in Appendix Figure B1b I present the LFP rates of women at age 59, before all women have reached the APA. As expected, Age Pension receipt is much higher for women in lower income households, while LFP is much higher for women in high-income households. Thus, the group of women with the most capacity to remain in the labor force at ages 60–64 were likely to be relatively unaffected by the reform.

Atalay and Barrett’s (2015) estimates. Atalay and Barrett (2015) estimate the causal effect of the reform on female LFP using a differences-in-differences design in which male cohorts form the comparison group. Figure 2 shows this approach graphically. Figure 2 replicates Figure 1 in Atalay and Barrett (2015), showing the trends in male and female LFP at ages 60–64 over time. The black lines plot aggregate time series data from the Australian Bureau of Statistics Labour Force Survey, while the gray lines show the calculations based on the IHCS data (with the X indicating each survey).¹⁵ I also include a dashed line denoting the counterfactual change in female LFP; this line shows how female LFP would have changed over Atalay and Barrett’s (2015) sample period if it followed the same trend as male LFP. Evidently, female LFP increased by 13.2 percentage points more than male LFP over the sample period. If men are a suitable control group, this comparison indicates that the reform has caused a substantial increase in female LFP.¹⁶

Atalay and Barrett (2015) formalise their approach using a regression framework. In their regressions, they estimate the impact of the reform on the LFP of women who were affected by the reform, those born after June 30, 1935 (the “after” cohort). Their approach compares the LFP of women in the after cohort with the LFP of women born before July 1, 1935 (the “before” cohort) — whose APA remained at 60 — using the equivalent male cohorts as the comparison group. Atalay and Barrett (2015) estimate the following regressions to implement this approach:

$$LFP_i = \beta x_i + \alpha_0 Female_i + \alpha_1 \Pr(CohortA_i^* = 1) + \delta[Female_i \times \Pr(CohortA_i^* = 1)] + u_i \quad (1)$$

where LFP_i is an indicator for individual i participating in the labor force; $Female_i$ is an indicator for individual i being female; $\Pr(CohortA_i^* = 1)$ is the probability that individual i is in the after cohort; and x_i includes controls for age, marital status, an indicator for having a bachelor’s degree or higher, household

¹⁵As noted by Atalay and Barrett (2015), the LFP trends in the IHCS sample are broadly consistent with the aggregate data, and similar old-age LFP trends have been observed in many countries over recent decades. For example, Coile, Milligan and Wise (2018, p. 28) note a “U-shaped pattern for men that generally reaches its minimum in the mid-1990s and a much shallower U or continuously rising pattern for women”.

¹⁶Figure 2 provides an opportunity to assess the plausibility of this assumption by comparing the LFP rates of men and women aged 60–64 before the reform. While male and female LFP rates followed different trends from 1978 to 1985, with a large decline in male LFP rates and a little change in female LFP rates, the trends were broadly similar from 1985 to 1993, with male and female LFP rates both increasing by around 5 percentage points. However, this analysis corresponds to earlier birth cohorts that were not affected by the reform. I focus on the relevant cohorts in detail below.

size and state of residence.¹⁷ The key explanatory variable is $Female_i \times \Pr(CohortA_i^* = 1)$. Its coefficient, δ , identifies the effect of the reform on the LFP rates of women in the after cohort under the assumption that, if not for the reform, the difference in the LFP rates of women in the before and after cohorts would have been equal to the corresponding difference among men.

Atalay and Barrett’s (2015) estimate of δ is 7.7 percentage points ($p < 0.01$). This estimate indicates that, on average, the reform increased the LFP of women in the after cohort by 7.7 percentage points at ages 60–64. This estimate implies a large effect, given that it is an average impact among women in the affected cohorts and 41% of women in these cohorts had already reached the APA when surveyed. For example, if we were to assume that this impact only applied to women below the APA (consistent with the findings of similar studies), this estimate implies an increase in LFP of 13.1 percentage points.¹⁸ Overall, Table 1 shows that I am able to closely replicate this estimate. I find an estimate of δ of 7.8 percentage points, and the standard error is almost identical to the one reported in Atalay and Barrett (2015) to three decimal places.

In addition, I am able to closely replicate Atalay and Barrett’s (2015) other LFP estimates (see Appendix Table B2); these estimates come from modified versions of equation (1) that allow the treatment effect to vary based on women’s APA cohort (and APA cohort and age). For example, I am able to closely replicate Atalay and Barrett’s (2015) headline estimate that each one-year increase in women’s APA increased female LFP by 12.0 percentage points ($p < 0.05$).

For robustness, Atalay and Barrett (2015) also estimate their regressions without men as a control group (dropping the female dummy and interaction terms from their regressions). These estimates are larger, which Atalay and Barrett (2015) argue validates the use of male cohorts as a control group. However, this analysis provides no evidence that male cohorts are *correctly* accounting for any unobserved cohort and time factors affecting female LFP. Atalay and Barrett (2015) also estimate regressions on the subsample of single men and women.¹⁹ Atalay and Barrett (2015) motivate this analysis as allowing them to assess the robustness of the results to potential within-family spillovers. Atalay and Barrett (2015) estimate larger effects on this subsample (replicated in Appendix Table B2 for equation (1)). I examine whether single men are a suitable comparison group for single women in the next section.

From here on, I focus on the validity of the baseline specification (equation (1)). I do this because

¹⁷The IHCS data includes people’s age in years and the survey year and quarter. Hence, each person’s date of birth lies within a fifteen-month window. To construct $\Pr(CohortA_i^* = 1)$, Atalay and Barrett (2015) assume that each person’s birthdate is uniformly distributed within the possible window.

¹⁸Some of Atalay and Barrett’s (2015) estimates indicate that the reform significantly increased the LFP of women who had already reached the APA. This is inconsistent with the findings of studies examining similar reforms (see footnote 13) and difficult to reconcile with (i) the strong incentive to start claiming the Age Pension at the APA and (ii) the difficulty of supplementing pension income with labor income because of the income test. This suggests that Atalay and Barrett’s (2015) estimates may be picking up factors other than the reform, such as cohort trends among women in high-income households.

¹⁹Although not explicitly discussed, Atalay and Barrett (2015) appear to define their single group as all persons who are currently single. I use this definition and closely match the proportion of single men and women in their sample.

at least one of the identifying assumptions underpinning the expanded specifications will not hold if the baseline parallel-trends assumption does not hold. See Appendix A.1 for more details.

3 Robustness to cohort trends in female participation

In this section, I extend Atalay and Barrett's (2015) analysis with earlier data. Starting with aggregate time series data on labor force participation rates by age and gender, I examine whether the parallel-trends assumption held before the reform for the cohorts in Atalay and Barrett's (2015) sample.

Figures 3a and 3b show the trends in male and female LFP at ages 45–49 and 50–54 respectively. For now, focus on the black lines, which plot aggregate time series data from the Australian Bureau of Statistics Labour Force Survey. These lines show opposing LFP trends for men and women. For example, at ages 45–49, female LFP increased from 52.1% to 79.7% over the period from 1978 to 2010, while male LFP decreased from 94.0% to 89.8%. Figures 3a and 3b also include vertical dashed lines to denote Atalay and Barrett's (2015) sample period shifted back fifteen and ten years respectively. Between these lines, the sample consists of individuals born between 1929 and 1950, the cohorts in Atalay and Barrett's (2015) sample. During these periods, male and female LFP rates follow very different trends, with a large increase in female LFP rates and a small decrease in male LFP rates. Therefore, the parallel-trends assumption did not hold for the relevant cohorts before the reform.²⁰

A critical question is whether these gender-specific trends are driven by transitory factors or factors likely to persist through to Atalay and Barrett's (2015) sample period. To assess this question, I compare the trends in LFP rates across the relevant cohorts at several points in time. In Figure 4a, I present LFP rates at ages 50–54 over the period from 1983 to 1994 against the five-year lag of LFP rates at ages 45–49 (here, both series correspond to individuals born between 1928 and 1944). In Figure 4b, I present LFP rates at ages 45–54 over the period from 1978 to 1994 against the ten-year lag of LFP rates at ages 35–44 (here, both series correspond to individuals born between 1923 and 1949).²¹ In each figure, there are strong positive correlations for men and women between (i) the LFP rate in a given year and (ii) the lagged LFP rate of the same cohorts at a younger age. For example, there are strong upward trends in (i) female LFP rates at ages 45–54 from 1978 to 1994 and (ii) female LFP rates at ages 35–44 from 1968 to 1984, while the corresponding trends for men are weaker and in the opposite direction. Hence, persistent cohort factors can

²⁰I do not focus on the trends at ages 55–59 because the reform was announced before most of the relevant cohorts reached this age. As such, trends across female cohorts at this age could partially reflect anticipatory responses to the reform. Nonetheless, the gender-specific trends across cohorts at this age are similar (see Appendix Figure B3), with much higher LFP rates for younger female cohorts and fairly constant rates for male cohorts. Moreover, these gender differences are apparent for the relevant cohorts before the reform was announced in 1993 (and legislated in 1994).

²¹To construct Figure 4, I combine data from the Labour Force Historical Timeseries, the precursor to the Labour Force Survey, over the period from 1968 to 1977 with data from the Labour Force Survey over the period from 1978 to 1994. I cannot present LFP rates by age in five-year groups in Figure 4b because they were not reported until 1978.

explain a large part of the gender-specific trends in Figure 3.

To further understand whether these trends would have persisted if not for the reform, it is useful to understand the characteristics of women behind the rise in female LFP. For example, if the rise in female LFP was entirely driven by women in low-income households, many of these women may have dropped out of the labor force once they turned 60 and became eligible for the Age Pension. To examine this, I use three earlier surveys administered by the Australian Bureau of Statistics: the 1982, 1986 and 1990 Income Distribution Surveys. These surveys are appropriate for this analysis because they (i) provide a nationally representative snapshot of LFP by age and gender at several points in time prior to the reform and (ii) have information on household income. Below, I also use these surveys to check whether the different trends of men and women can be explained by the controls in Atalay and Barrett's (2015) regressions and whether single men are likely to be an appropriate comparison group for single women.²² As outlined by Siminski et al. (2003), these surveys differ from the surveys used by Atalay and Barrett (2015) in important ways. For example, age information in the pre-reform surveys is in five-year groups rather than single years, and the pre-reform surveys were conducted over a shorter period of time (less than three months) than the post-reform surveys, which were conducted over the course of a financial year. Nonetheless, I show in Appendix A.3 that the placebo regression estimates below are only marginally affected by these and other differences in the pre-reform surveys.

With these surveys, I exclude immigrants who have lived in Australia for less than ten years (to be consistent with Atalay and Barrett (2015)), and I restrict the sample to people aged 45–49 or 50–54.²³ The gray lines in Figures 3a and 3b plot the LFP rates of men and women in each survey (with the X indicating each survey). As the graphs demonstrate, the LFP rates in these surveys are broadly consistent with the aggregate data at the time of each survey.²⁴

Figure 5 shows that the increase in female LFP rates has come from women in both high- and low-income households. Using the pre-reform surveys, I divide women in half based on the median equivalised household income among women of the same age in the relevant survey. Figure 5 shows that although women in high-income households had much higher baseline rates of LFP, they exhibited a similar (and slightly larger) percentage-point increase in LFP rates between 1982 and 1990 at ages 45–49 and 50–54.²⁵

²²Other surveys administered by the Australian Bureau of Statistics during this period, namely the Household Expenditure Surveys in 1984 and 1988–89, lack information on relevant control variables such as education. I do not use the 1993–94 round of the IHCS as the LFP rates in this survey could have been affected by the announcement of the reform, though the results are robust to the inclusion of this survey (available on request).

²³I also exclude the 2.1% of individuals who live in non-private dwellings because the post-reform surveys used by Atalay and Barrett (2015) only include residents of private dwellings. The estimates are similar without this restriction (available on request).

²⁴The LFP rates in the survey data are also consistent with those in the sample files for the 1981, 1986 and 1991 Censuses, which contain a 1% random sample of all records in each Census (see Appendix Figure B2).

²⁵Unsurprisingly, men in high-income households did not experience the same increase in LFP rates at these ages, with LFP rates close to 100% in all three surveys (not shown).

Assuming that most of these women would have remained in high-income households, they would have had limited access to government support at ages 60–64 even if the reform had not occurred. As such, it seems likely that women would have reduced the gender gap in LFP rates at ages 60–64 over Atalay and Barrett’s (2015) sample period even if the reform had not occurred.

One key question remains: can the control variables in Atalay and Barrett’s (2015) regressions explain the different trends of men and women across the relevant cohorts? To examine this question, I use the pre-reform sample to estimate placebo regressions with Atalay and Barrett’s (2015) baseline regression approach. That is, I estimate equation (1) on the pre-reform sample to test whether the LFP of women in the after cohort was affected before the reform. To estimate these regressions, I have to define $\Pr(\text{Cohort}A_i^* = 1)$, the probability that individual i is in the after cohort. With age information in five-year groups and surveys conducted over a three-month period, each individual’s date of birth lies within a window of five years and three months. This relatively large window makes it less appealing to follow Atalay and Barrett’s (2015) assumption that each individual’s date of birth is uniformly distributed within the possible window. Hence, I construct $\Pr(\text{Cohort}A_i^* = 1)$ in a more refined way that accounts for age-specific mortality and differences in the size of individual birth cohorts: using annual population counts by gender and single year of age from the Australian Bureau of Statistics (see Appendix A.2 for details).²⁶ This approach is validated by the fact that these surveys are strongly representative of the Australian population in terms of age (see Appendix Figure B4).²⁷

For these regressions, positive and statistically significant estimates of δ , the placebo treatment effect, would indicate that Atalay and Barrett’s (2015) estimates are likely to be biased upwards by unobservable gender-specific differences in LFP across the relevant cohorts. Table 2 presents the results. In Panel A, I present the results for the full sample of men and women. Columns (1) and (2) show positive, large and statistically significant estimates of δ at ages 45–49 and 50–54 of 24.6 and 15.7 percentage points respectively (both $p < 0.01$).²⁸ In column (3), I remove anyone with a positive probability of being misclassified into the before and after cohorts. While this restriction precludes estimation on the age 45–49 sample (as there is no one who is definitely in the before cohort), the estimated placebo effect at ages 50–54 remains similarly large

²⁶As it turns out, the estimates are similar if I assume that birthdates are uniformly distributed within the possible windows (see Appendix Table B3).

²⁷In Appendix Figure B4, I calculate the fraction of male and female respondents in different age groups in each survey. The surveys allow for a consistent definition of 12 age groups, including 45–49 and 50–54. Then, I use estimates of the total population by sex and single year of age from the Australian Bureau of Statistics to construct the fraction of the 15+ population in each age group in the relevant years (1982, 1986, 1990). The figure shows that the population shares at each age closely align with the shares in each survey for men and women. The fit is especially good at the key ages of 45–49 and 50–54. As such, it is likely that the survey would also be representative within these five-year age groups. This justifies the use of population data to assign the probability that individuals are in the after cohort.

²⁸Note that these regressions do not include individual-level controls for age, unlike Atalay and Barrett’s (2015) regressions, as the pre-reform surveys only report age in five-year groups. However, in Appendix A.3, I show that the age distribution changed in very similar ways over time for men and women and, as such, the estimates are similar with controls for the average age of men and women at the time of the relevant survey.

and statistically significant (16.1 percentage points, $p < 0.01$). In Panel B, I present the results when the sample is restricted to men and women with household income above the median (for their age and gender in the relevant year). These results are useful because high-income women would have had limited access to government payments at age 60 even if the reform had not occurred (and thus the pre-reform trends for this group may be especially likely to reflect counterfactual LFP trends at ages 60–64). The results are similar, with large and highly statistically significant placebo estimates ranging from 12.7 to 24.2 percentage points. Overall, this analysis suggests that Atalay and Barrett’s (2015) estimates are likely to be biased upwards.

Using the pre-reform sample, I also examine whether single men are likely to be an appropriate control group for single women. Appendix Figure B5 replicates Figure 3 for this sample. I also present LFP rates from the Labour Force Survey for unmarried men and women, since the Labour Force Survey does not report LFP rates by single status. While ‘unmarried’ is an imperfect proxy for single status, this data provides a continuous time series over a longer period (and allows the cohorts in Atalay and Barrett’s (2015) sample to be matched exactly). Evidently, the relevant cohorts of single men and women had very different LFP trends before the reform. Single women’s LFP rates increased by approximately 15 percentage points at ages 45–49 and 50–54 between the 1982 and 1990 surveys, while there was little change in the LFP rates of single men at ages 45–49 and a decrease of 10 percentage points at ages 50–54. There are also strong gender-specific trends for the unmarried sample over the relevant years. Moreover, the gender-specific trends cannot be explained by the control variables in Atalay and Barrett’s (2015) regressions. For the pre-reform sample of single men and women, the placebo estimates are positive, large and mostly statistically significant (ranging from 16.2 to 24.6 percentage points, see Appendix Table B4). Thus, Atalay and Barrett’s (2015) estimates on single women are also likely to be biased upwards.

Discussion. It is worth making several points here. First, while the placebo estimates in Table 2 are larger than Atalay and Barrett’s (2015) estimates of women’s actual labor supply response at ages 60–64, women had much higher LFP rates at ages 45–49 and 50–54. Thus, in percentage terms, the placebo estimates imply a similar increase in female LFP to Atalay and Barrett’s (2015) estimates (37.0–55.6% compared to 35.3%).

Second, the probabilistic assignment of the after cohort means that the estimates in column 1 for the age 45–49 sample are particularly sensitive to differences in LFP rates between the 1982 survey (cohorts 1932–37) and subsequent surveys. In the 1982 survey, there is a 48.4% chance that individuals are in the after cohort, and this uncertainty magnifies the placebo estimate. A conservative way to address this is to assume that everyone in the 1982 survey is in the before cohort (i.e., by reducing the probability that these individuals are in the after cohort to 0). Appendix Table B5 shows that this adjustment reduces the placebo estimate by 44% to 13.7 percentage points. However, as the standard error also falls by the same percentage,

the t-statistic is unchanged (6.3). It is worth emphasizing that this approach is likely to be conservative. The aggregate data in Figure 3a shows that female LFP rates are slightly higher for the 1932–37 cohorts than the 1930–35 cohorts, while the reverse is true for male LFP rates; this is evident by comparing the LFP rates in 1982 (cohorts 1932–37) and 1980 (cohorts 1930–35). Thus, if it were possible to exactly match the before cohort (i.e., if there had been a survey in 1980), it is likely that the placebo estimate would be larger than 13.7 percentage points.

Third, there is little change in the placebo estimates with the inclusion of controls. As shown in Appendix Table B6, the placebo estimates fall by less than 5% with controls. Hence, the control variables in Atalay and Barrett’s (2015) regressions explain only a small fraction of the different trends of men and women prior to the reform.²⁹

Fourth, as Australia experienced recessions in 1982 and 1990, a possible concern is that the placebo estimates may be affected by macroeconomic shocks. However, the aggregate time series data in Figures 3a and 3b show no evidence of any meaningful break in the LFP trends of the relevant cohorts in any of the three survey years. Moreover, Figures 3 and 4 show that there is nothing specific about the 1982 to 1990 period. The aggregate data in Figure 3a shows different trends in the LFP rates of men and women in the relevant cohorts both throughout and beyond these years, and Figure 4b shows similar gender-specific trends for the relevant cohorts ten years earlier, during a different time period.

Fifth, as the pre-reform surveys are less frequent and contain age information in five-year groups rather than single years, it is not possible to exactly match the cohorts in Atalay and Barrett’s (2015) sample.³⁰ However, an exact match is possible in the aggregate data; between the vertical dashed lines in Figures 3a and 3b, where the cohorts match those in Atalay and Barrett’s (2015) sample, men and women have opposing trends. As such, we would expect to find similar results if it were possible to exactly match the cohorts in Atalay and Barrett’s (2015) sample in the survey data.

Sixth, given the slightly different cohorts in Atalay and Barrett’s (2015) sample, controls may have a greater effect on their estimates. However, controls have a similarly minor impact on Atalay and Barrett’s (2015) estimates, reducing their baseline estimate by just 10% (see Appendix Table B8).

Finally, in Appendix A.3, I verify the robustness of the placebo estimates further. Specifically, I show that the estimates are only marginally affected by certain idiosyncrasies of the pre-reform surveys, which are discussed in detail by Siminski et al. (2003) and include (i) changes in the measurement of labor force status across surveys, (ii) seasonality in LFP rates, and (iii) the lack of individual-level age controls.

²⁹Similarly, control variables explain only a small fraction of the rise in female LFP prior to the reform. Estimating the placebo regressions without men as the control group, I find that controls only reduce the placebo estimates by around 15%, and the estimates remain positive and highly statistically significant (ranging from 11.5 to 19.8 percentage points, see Appendix Table B7).

³⁰In the pre-reform sample, the age 45–49 sample consists of individuals born from 1932 to 1945, and the age 50–54 sample consists of individuals born from 1927 to 1940. Atalay and Barrett’s (2015) sample consists of individuals born from 1929 to 1950.

4 Re-estimating the impact on female participation

In this section, I re-estimate the reform’s impact on female LFP using a different approach. I follow the approach of several recent studies examining phased increases in pension-eligibility ages (Staubli and Zweimüller, 2013; Oguzoglu, Polidano and Vu, 2020; Cribb, Emmerson and Tetlow, 2016). Specifically, I compare the LFP of women who are just below the Age Pension Age (APA) to those who are just above, with fixed effects for women’s age and cohort. The benefit of this approach is that it directly controls for differences in female LFP across cohorts, which, as shown above, cannot be explained by controls.

For women in the replication sample, I estimate regressions based on the following specification:

$$LFP_i = \beta x_i + \delta \mathbf{1}(age_i < APA_i) + FE_age_yrs_i + FE_APA_Cohort_i + u_i \quad (2)$$

where $FE_age_yrs_i$ is a set of fixed effects for woman i ’s age in years; $FE_APA_Cohort_i$ is a set of fixed effects for woman i ’s APA cohort; $\mathbf{1}(age_i < APA_i)$ is an indicator variable for woman i being below her APA; and x_i includes the same variables as in equation (1) (except age). As age is only available in years, there is potential misclassification for some women of (i) the cohort fixed effects, $FE_APA_Cohort_i$, and (ii) the key variable, $\mathbf{1}(age_i < APA_i)$. I address this issue by replacing these variables with (i) the probabilities that woman i is in each APA cohort, $\Pr(APA_Cohort_i)$, and (ii) the probability that woman i is below her (expected) APA, $\Pr(age_i < \mathbb{E}[APA_i])$.³¹ In these regressions, δ estimates the effect on female LFP of women remaining below the APA because of the reform if the following assumption holds: different cohorts would have had identical age-related trends in LFP if not for the reform.

Figure 6 shows graphical evidence in support of this assumption; it shows the age-related trends in LFP for different cohorts of women. While there are considerable differences in LFP rates across cohorts, the trends with age are similar at ages 55–59, when all cohorts are below the APA. Moreover, in Appendix A.4, I show that — after controlling for age, cohort and individual factors — there is no evidence of any trend in female LFP based on the number of years until the APA (and no significant change at the APA).

Table 3 presents estimates of the treatment effect from equation (2). Column 1 shows a similar estimate to Atalay and Barrett’s (2015) preferred estimate when cohort controls are excluded (13.4 vs. 12.0 percentage points). However, when cohort controls are included, column 2 shows that the estimate falls to 4.1 percentage points and becomes statistically indistinguishable from zero at the 10% level ($p = 0.109$).

Note that the cohort controls may partially absorb any wealth effects of the reform, the main mechanism

³¹To construct these variables, I first calculate the probabilities that woman i is in each APA cohort, $\Pr(APA_Cohort_i)$, under the assumption that woman i ’s birth date is uniformly distributed within the possible fifteen-month window, as per Atalay and Barrett’s (2015) approach. Then, I use these probabilities to calculate woman i ’s expected APA, $\mathbb{E}[APA_i]$. Finally, I calculate $\Pr(age_i < \mathbb{E}[APA_i])$ under the assumption that each woman’s exact age is uniformly distributed on the interval $[age_y, age_y + 1)$, where age_y is her age in years.

offered by Atalay and Barrett (2015) to explain their estimates.³² Specifically, the cohort controls will absorb any wealth effects on female LFP at ages where women's pension eligibility is not affected, which, as discussed above, are likely to be small based on studies of similar reforms (Staubli and Zweimüller, 2013; Cribb, Emmerson and Tetlow, 2016; Geyer and Welteke, 2019).³³ However, the treatment effect may capture wealth effects at ages where women's pension eligibility is affected. For example, the estimates are likely to capture the response of a woman who decides to retire at age 62 rather than 60 because of the wealth effects of her APA increasing from 60 to 63. Given that changes in pension wealth may be particularly salient at ages where women's pension eligibility is affected, the estimates may capture a sizeable proportion of any wealth effects from the reform.

I also show that the estimates remain similar if I use time fixed effects rather than cohort controls in equation (2). Specifically, I add fixed effects for the quarter-year that the interview took place. This specification reduces measurement error (since women's exact cohort is unknown) and potentially allows δ to better capture differences in LFP rates across cohorts stemming from wealth effects. Cribb, Emmerson and Tetlow (2016) use a similar approach to examine the importance of wealth effects in their context. Appendix Table B15 presents the estimates. The estimates are similar and actually slightly smaller than the baseline estimates (3.9 percentage points, $p = 0.075$). The estimates are also similar with survey-wave or year fixed effects (not shown).

I also assess the robustness of the results to other concerns. First, I examine whether the lack of exact information on age causes attenuation in the effects. In this context, there are two sources of measurement error: (i) in the assignment of women's APA cohort and (ii) in the assignment of whether women are below the APA. In Appendix Table B15, I examine the sensitivity of the estimates to each source of measurement error in columns 3–5. In column 3, I restrict the sample to women who can be classified as either above or below the APA with certainty, which reduces the sample size by 13%. The treatment effect remains similar and falls slightly compared to the baseline estimates (from 4.1 to 3.8 percentage points). In column 4, I restrict the sample to women who can be assigned to their APA cohort with certainty, which reduces the sample size much more, by 55%. The estimated treatment effect remains at 4.1 percentage points but becomes less precise. In column 5, I combine both restrictions, which reduces the sample size slightly more (56%). In this case the treatment effect falls to 3.1 percentage points. Thus, the estimates are relatively stable and there is little evidence that measurement error leads to attenuated estimates.

³²It is worth pointing out that even if Atalay and Barrett's (2015) identification strategy were valid, their estimates cannot isolate the wealth effects of the reform from other mechanisms. As discussed above, there are several other mechanisms that could make it optimal for women to retire exactly at the APA, and Atalay and Barrett (2015) do not control for whether women have reached the APA. In Appendix Table B9, I add such controls and find that Atalay and Barrett's (2015) baseline estimate falls by 54% and becomes statistically indistinguishable from zero at the 10% level.

³³These effects are also likely to be small in the Australian context due to (i) the low LFP rates and high reliance on other government payments among women in low-income households prior to the APA, (ii) the strong incentive to start claiming the Age Pension at the APA, and (iii) the difficulty of supplementing pension income with labor income because of the income test.

Second, since Atalay and Barrett (2015) interpret their LFP estimates as a decline in the probability of retirement, it is worth discussing under what circumstances the estimated effects on LFP are likely to reflect a change in retirement decisions. Based on the analysis in Section 3, one concern with using “not in the labor force” as a proxy for retirement is that women in earlier cohorts were more likely to have never worked (or dropped out of the labor force well before the APA), as documented in Figures 3 and 4. This is problematic for Atalay and Barrett’s (2015) identification strategy and suggests that their estimates are more likely to reflect pre-existing differences in LFP rates across cohorts rather than changes in retirement decisions near the APA. In contrast, equation (2) relies on within-cohort changes in LFP around the APA. This variation is more likely to result from decisions to retire, given the low re-entry into the labor market at these ages.³⁴

Third, a further valid concern is reform endogeneity. There are two potential sources of endogeneity. First, it is clear that the reform was implemented in such a way that there was a strong positive correlation between women’s pre-reform rates of LFP and the size of the increase in APA. Unlike Atalay and Barrett’s (2015) estimates, which rely on cross-cohort comparisons, my preferred estimates account for this source of endogeneity by relying on within-cohort changes in female LFP around the APA. A second possible concern is that the reform may have been implemented in response to changes in social norms about the appropriate age to retire. This would imply that later cohorts would have retired later even if the reform had not occurred, biasing both estimates upwards. However, the trends in LFP rates prior to age 60 in Figure 6 show little evidence that later cohorts systematically favored later retirement (conditional on their LFP rates at age 55). It also seems unlikely that my estimates are strongly biased upwards, given that (i) the 90% confidence interval for my estimate contains zero and (ii) economic theory suggests we should expect at least some increase in female LFP rates.

Discussion. The modest effects on female LFP are consistent with other evidence. First, the raw data shows little evidence of a drop in female LFP at the APA. Figure 7 plots the average LFP rate of women with respect to the number of years until their pension age.³⁵ While female LFP decreases with age, there is little evidence of a stronger drop at the APA. Moreover, Figure 4c of Morris (2019) shows a near-identical pattern in female LFP based on longitudinal survey data from 2001 to 2015 that contains precise information

³⁴I assess this further in Appendix A.5 by examining whether the decrease in LFP rates around the APA coincides with an increase in the receipt of government payments. Specifically, I show that, around the APA, women mainly transition from being in the labor force and not receiving a government payment to being out of the labor force and receiving a government payment. Meanwhile, there is little change in the proportion of women who are in the labor force and receiving a government payment. This indicates that within-cohort changes in LFP around the APA are likely to mainly reflect decisions to retire with the support of government payments.

³⁵I assign each woman’s number of years to pension age based on which group is most likely, assuming birth dates are uniformly distributed within the possible 15-month windows. I omit 16% of women who are equally likely to be in neighboring groups. The remaining women can be assigned to the correct group with an average probability of 0.83.

on birthdates and survey dates.³⁶ Second, with this data, Morris (2019) finds an increase in female LFP of similar magnitude (3.1 percentage points, $p < 0.1$) to the preferred estimate in Table 3 (4.1 percentage points). Although the two estimates correspond to the average impact of delayed pension eligibility on slightly different birth cohorts at slightly different ages, both estimates suggest that the reform had modest effects. Third, these impacts are consistent with similarly modest changes in male LFP at the APA. Using the same data as Morris (2019), Appendix Figure B6 shows that the decrease in male LFP from ages 64 to 65 is only 5.5 percentage points larger than the decreases from 62 to 63 and 63 to 64.³⁷ This decline is smaller than the decline observed at the ERA in most countries.

Overall, the estimates suggest that the Australian reform had a smaller impact on female LFP than similar reforms in other countries. For example, studies of similar reforms in the U.K. (Cribb, Emmerson and Tetlow, 2016), Austria (Staubli and Zweimüller, 2013) and Germany (Geyer and Welteke, 2019; Geyer et al., 2020) estimate increases in female employment of 6.4 to 14.4 percentage points. The smaller impacts found here may partly result from the fact that around one third of women do not satisfy the means test — Atalay and Barrett (2015) estimate this proportion to be 37.5%. If we assume that all of the increase in LFP occurs among women who satisfy the means test, the estimated effect on LFP increases from 4.1 to 6.6 percentage points, similar to Cribb, Emmerson and Tetlow’s (2016) estimate of 6.4 percentage points for the impact of raising women’s State Pension Age from 60 to 62. However, it is worth remembering that the estimates here are statistically indistinguishable from zero at the 10% level.

It is somewhat surprising that the Australian reform did not have a larger impact given that the means test discourages work beyond the APA. This lies in contrast to the reform examined by Cribb, Emmerson and Tetlow (2016) in which there is no income test for the pension and thus a much weaker incentive to retire at the State Pension Age. While the absence of an accrual effect is one explanation for the modest effects, it may not be the most important one. Relative to other reforms, the Australian reform targeted a group of women with much weaker attachment to the labor market and a very high reliance on other government transfers. As shown by several studies (Atalay and Barrett, 2015; Oguzoglu, Polidano and Vu, 2020; Morris, 2019), many women extended their receipt of other means-tested government transfers in response to the reform. Recent estimates by Oguzoglu, Polidano and Vu (2020) with administrative data emphasize how large these responses were; Oguzoglu, Polidano and Vu’s (2020) estimates indicate that 76% of women who were delayed from receiving the Age Pension ended up receiving another government payment instead. These spillovers are much larger than has been found in other countries and help explain the relatively modest labor supply responses.

³⁶Morris (2019) uses waves 1–14 of the Household Income and Labour Dynamics in Australia (HILDA) survey.

³⁷Unfortunately, it is not possible to construct a similar graph using the IHCS data, as age is only available in five-year groups at ages 65 and above.

5 Conclusion

In this paper, I replicate Atalay and Barrett's (2015) labor supply estimates and extend their analysis. Atalay and Barrett (2015) estimate the effects of an Australian reform in 1994 that gradually raised women's pension-eligibility age from 60 to 65. Using a differences-in-differences design in which male cohorts form the comparison group, Atalay and Barrett (2015) estimate that each one-year increase in women's eligibility age increased female labor force participation by 12 percentage points. While I am able to successfully replicate this estimate, I show that the underlying parallel-trends assumption did not hold before the reform for the relevant cohorts. Specifically, there is a strong upward trend in female participation rates and a weak downward trend in male participation rates. After accounting for these gender-specific trends, I find that the estimated increase in female labor force participation falls by two-thirds to 4.1 percentage points and becomes statistically indistinguishable from zero at the 10% level. This suggests that the Australian reform had a smaller impact on female labor supply than estimates of similar reforms in other countries.

More generally, the results have implications for other studies examining the impact of retirement reforms on female labor supply or other (potentially) interdependent outcomes such as welfare receipt and health. This paper demonstrates the importance of cross-cohort trends in female labor supply in the Australian context, and given the widespread increase in female labor supply over recent decades, similar trends may also exist in other countries. Hence, studies evaluating the impact of retirement reforms on female outcomes should examine trends across cohorts before these reforms and control for them if necessary.

Data availability

The data used in this study can be obtained from the Australian Bureau of Statistics and the Australian Government's Department of Social Services, but restrictions apply to the availability of these data. Instructions for how other researchers can obtain the data, and all the information needed to proceed from the raw data to the results of the paper (including code) are available from the author upon request.

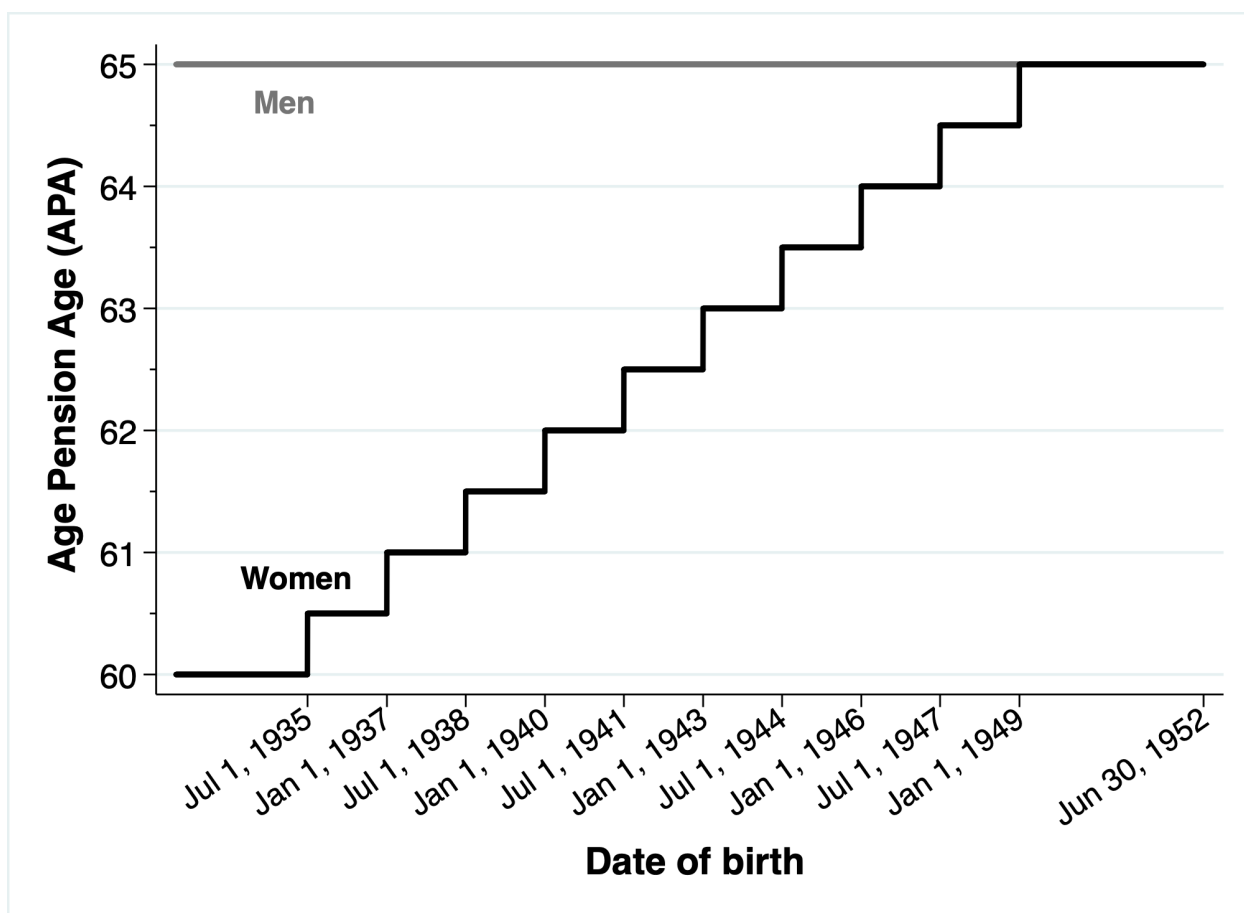
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Working Paper No. 25018.

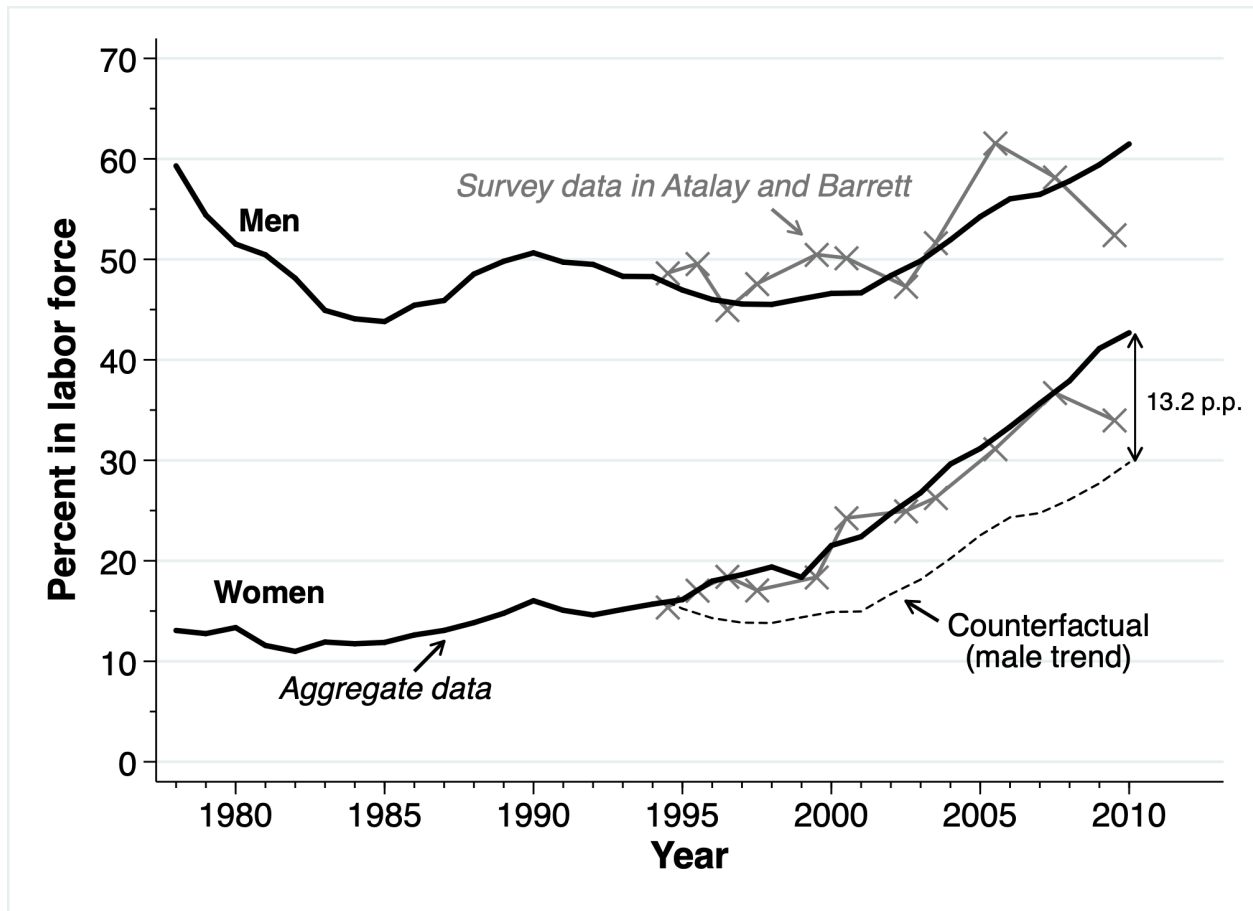
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Figure 1: Age Pension Age (APA) by date of birth and gender



Notes: This figure shows the Age Pension Age (APA) of Australian men and women based on their date of birth. The phased increases in women's APA from 60 to 65 were legislated in 1994.

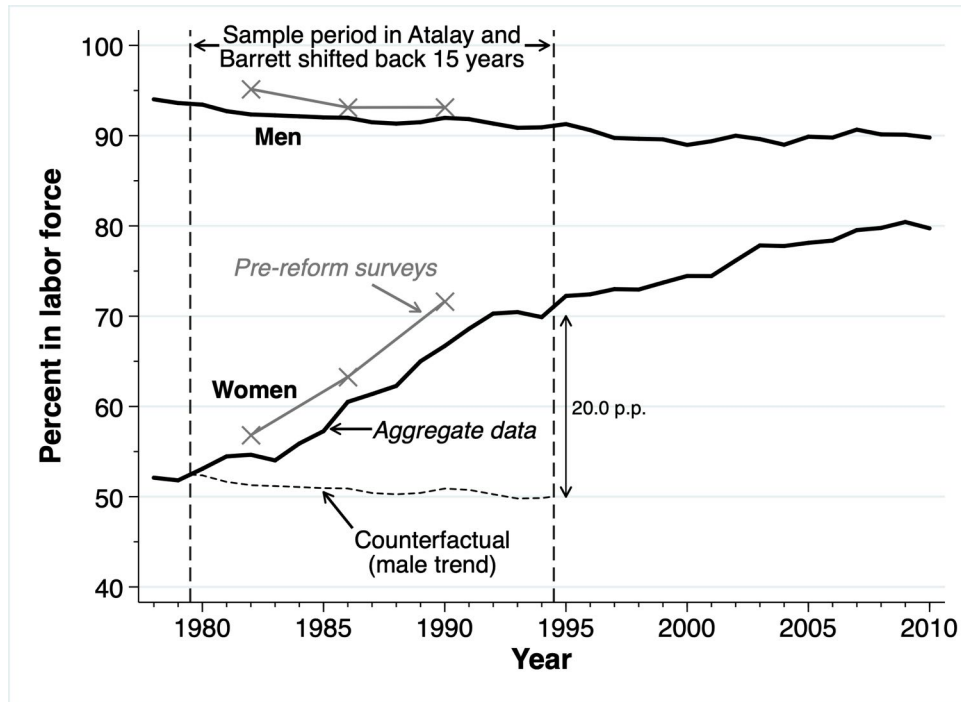
Figure 2: Labor force participation rates of men and women aged 60–64 over time



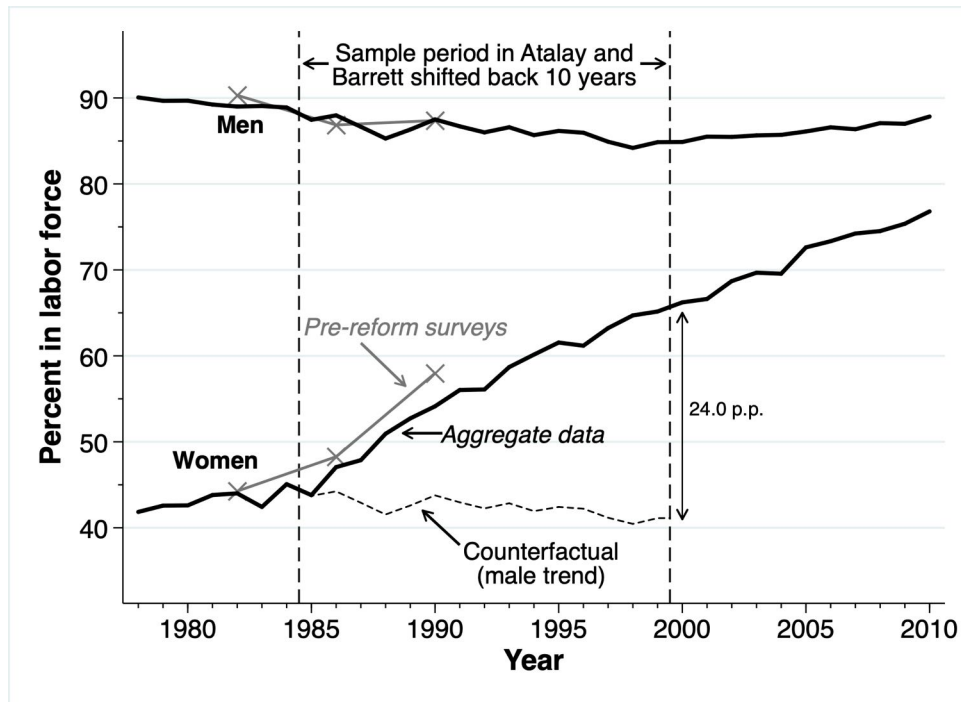
Notes: This figure replicates Figure 1 in Atalay and Barrett (2015). The black lines plot aggregate time series data from the Labour Force Survey, while the gray lines show the calculations based on the Income and Housing Costs Surveys (with the X indicating each survey). I also include a dashed line denoting the counterfactual change in female labor force participation (LFP) over Atalay and Barrett's (2015) sample period (1994–2010); this line shows how female LFP would have changed if it followed the same trend as male LFP over the sample period. Evidently, female LFP increased considerably more than male LFP over this period.

Figure 3: Labor force participation rates of the relevant cohorts at younger ages

(a) Ages 45–49



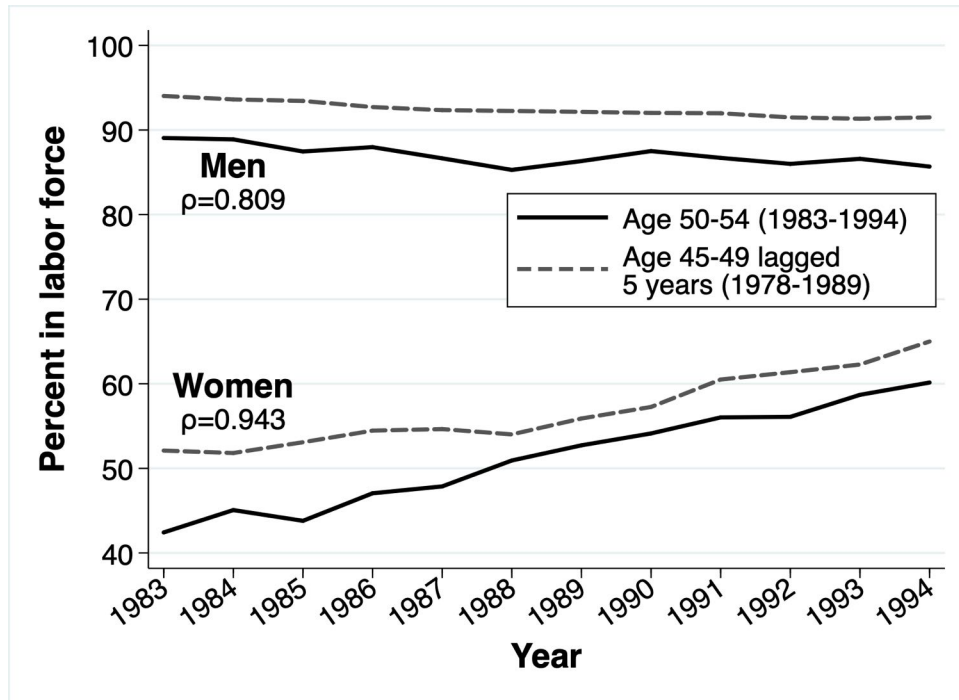
(b) Ages 50–54



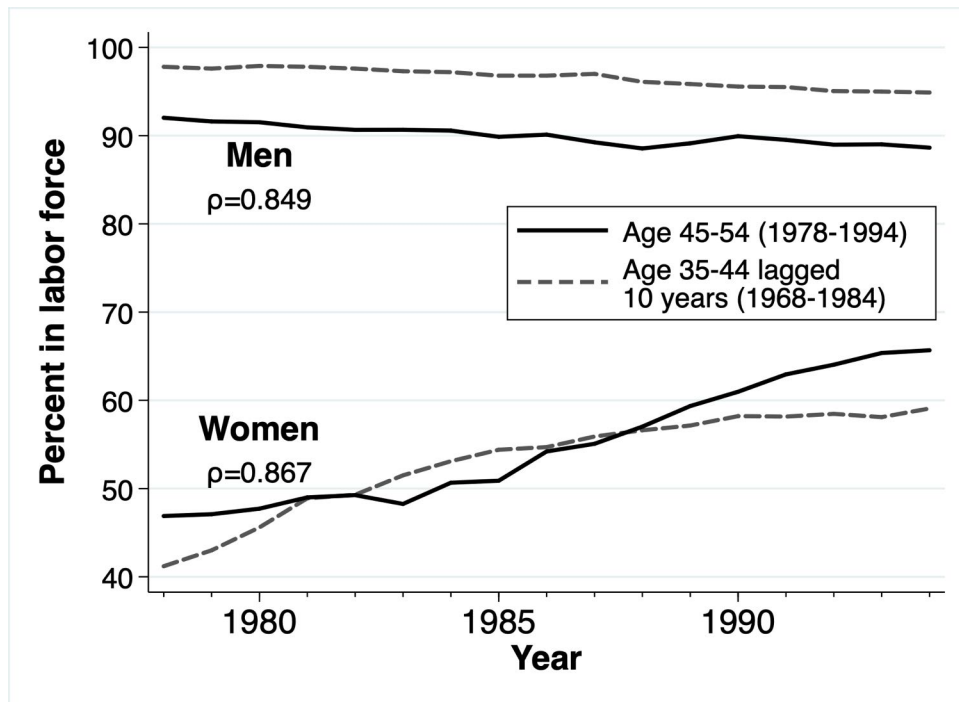
Notes: These figures plot labor force participation (LFP) rates from the Labour Force Survey (in black) and the Income Distribution Surveys (in gray). Between the vertical dashed lines, the cohorts in Atalay and Barrett's (2015) sample were aged 45–49 in (a) and 50–54 in (b). During these periods, male and female LFP rates follow opposing trends. Thus, the parallel-trends assumption did not hold for the relevant cohorts before the reform.

Figure 4: Persistence of labor force participation trends across cohorts

(a) Participation rates at ages 50–54 and lagged rates at 45–49

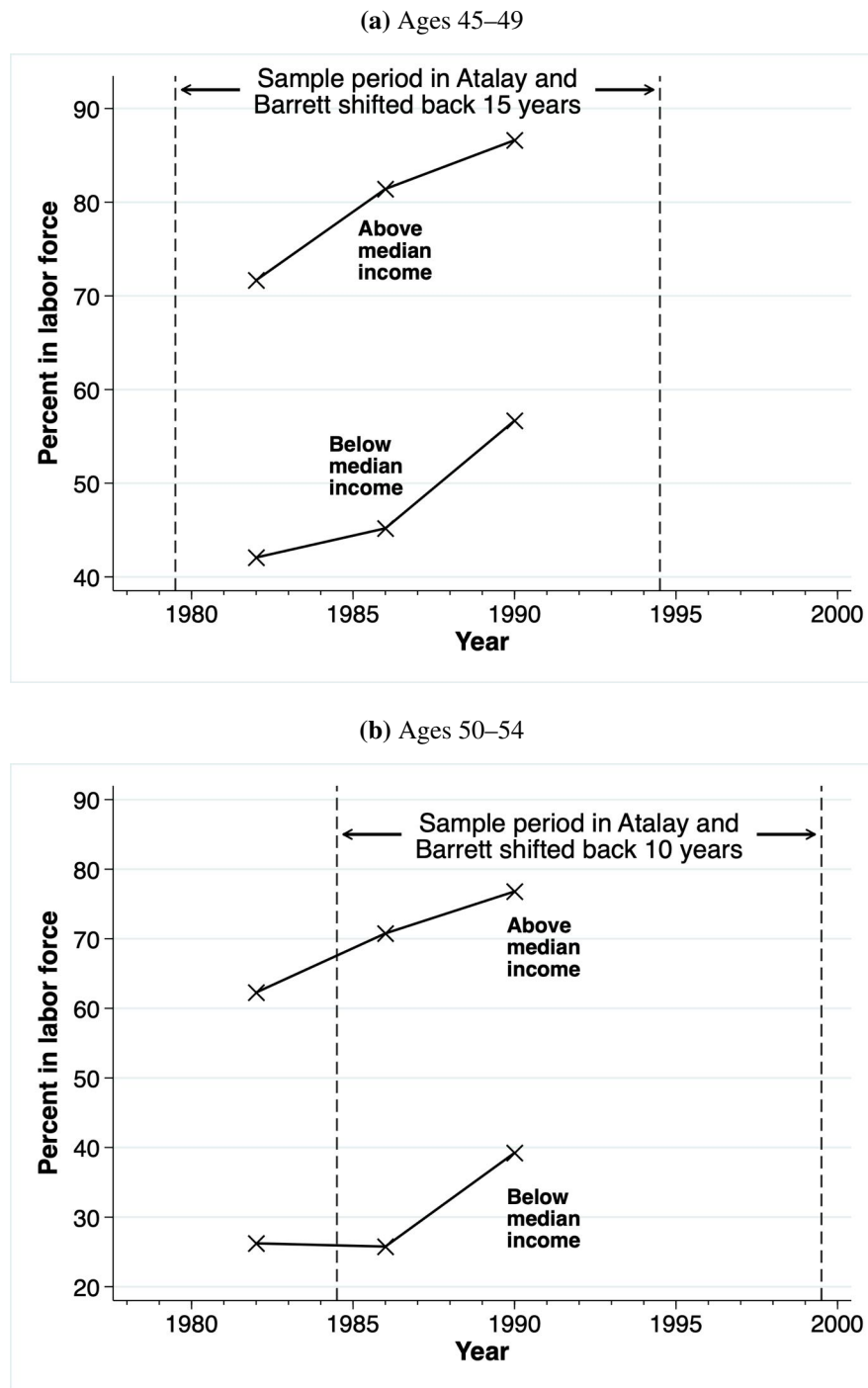


(b) Participation rates at ages 45–54 and lagged rates at 35–44



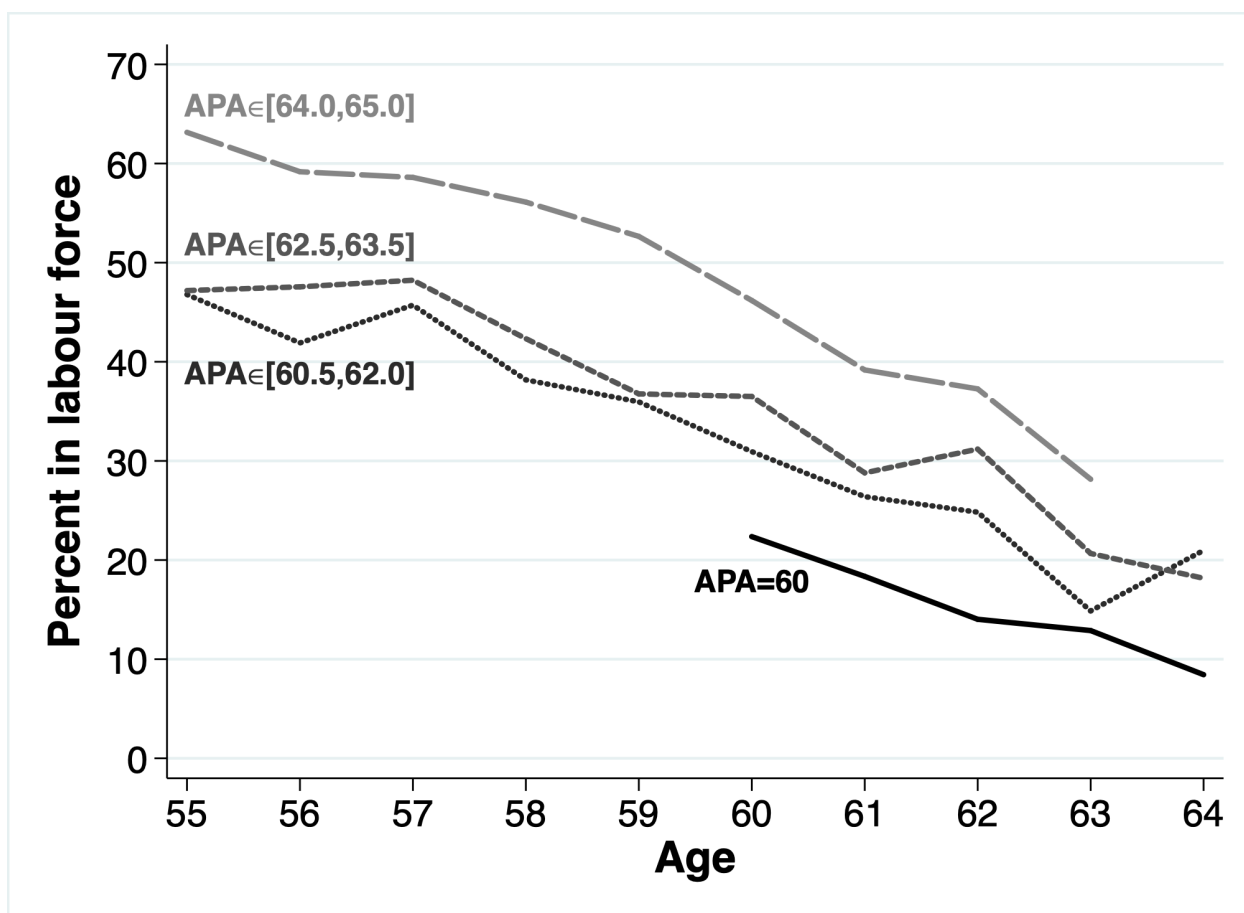
Notes: These figures combine data from the Labour Force Historical Timeseries (1968–1977) with the Labour Force Survey (1978–1994). (a) plots labor force participation (LFP) rates at ages 50–54 from 1983 to 1994 against the five-year lag of LFP rates at ages 45–49 (here, both series correspond to the 1928–1944 cohorts). (b) plots LFP rates at ages 45–54 from 1978 to 1993 against the ten-year lag of LFP rates at ages 35–44 (here, both series correspond to the 1923–1949 cohorts).

Figure 5: Pre-reform female labor force participation rates by household income



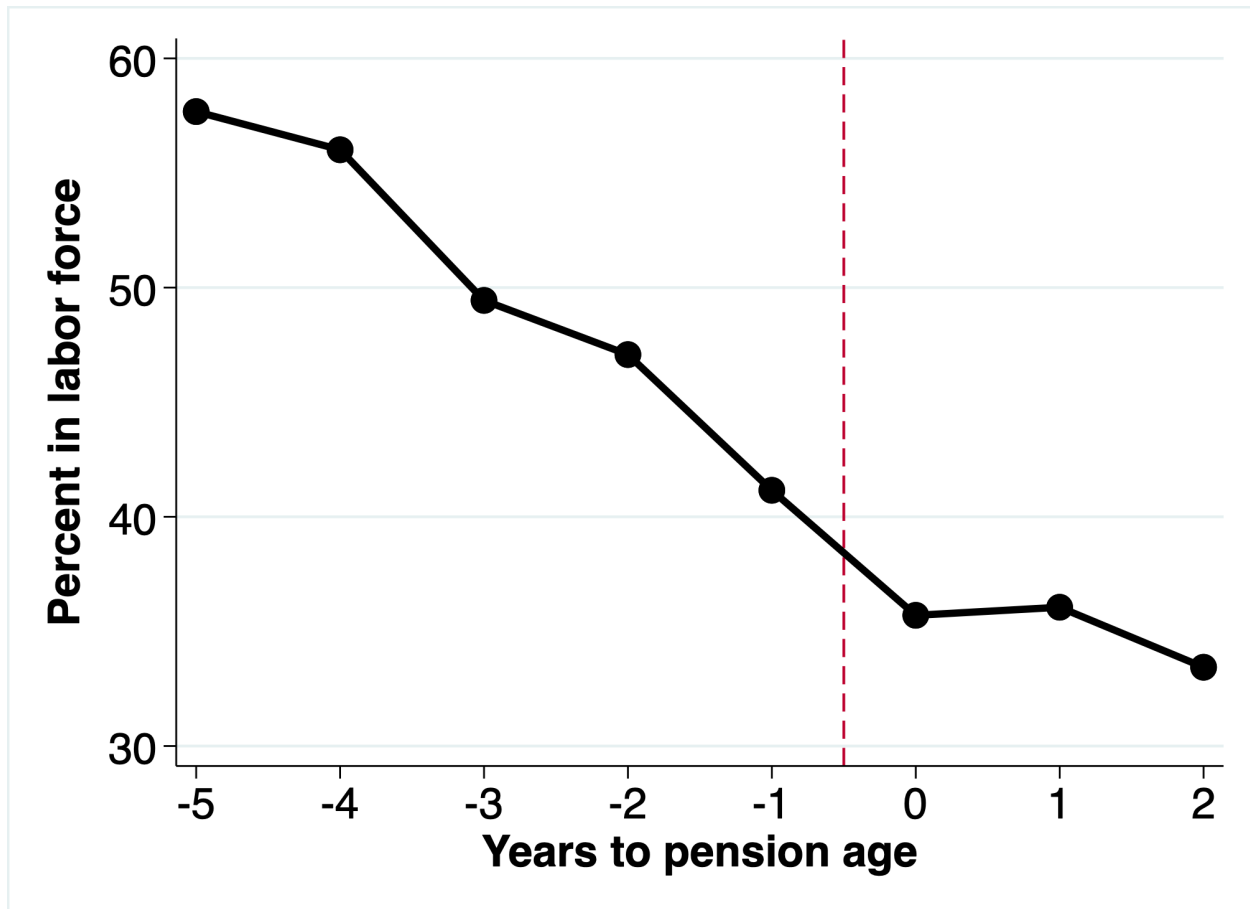
Notes: These figures plot the labor force participation (LFP) rates of women from the 1982, 1986 and 1990 Income Distribution Surveys. I divide women in half based on the median equivalised household income among women of the relevant age in the relevant survey. Between the vertical dashed lines, the cohorts in Atalay and Barrett's (2015) sample were aged 45–49 in (a) and 50–54 in (b). These graphs show that women in low- and high-income households contributed to the higher LFP rates among later cohorts prior to the reform. See Section 3 for more details.

Figure 6: Female labor force participation rates by age and cohort



Notes: This figure shows female labor force participation rates by age and cohort. I assign women to their most likely cohort. The sample comes from the eleven Income and Housing Costs Surveys used by Atalay and Barrett (2015) from 1994 to 2010.

Figure 7: Female labor force participation around the pension age



Notes: This figure shows the average labor force participation of women by the number of years to their pension age. I assign women's number of years to pension age based on their most likely group, assuming birth dates are uniformly distributed within the possible 15-month windows. The sample comes from the eleven Income and Housing Costs Surveys used by Atalay and Barrett (2015) from 1994 to 2010.

Table 1: Replication of baseline estimates

	Atalay & Barrett (1)	Replication (2)
Treatment effect (Female $\times$ After)	0.077*** (0.021)	0.078*** (0.020)
Female	-0.288*** (0.019)	-0.305*** (0.018)
After cohort	0.030* (0.017)	0.039** (0.017)
Mean: Women in after cohort		0.297
Implied percent increase in female participation		35.3%
R-squared		0.121
Observations	11,409	11,497

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows that I am able to successfully replicate Atalay and Barrett's (2015) baseline estimates on female labor force participation. The estimates (reported in Table 3 of Atalay and Barrett (2015)) come from estimating equation (1). The sample consists of men and women aged 60–64 from the eleven Income and Housing Costs Surveys between 1994/95 and 2009/10.

Table 2: Placebo regression estimates on the pre-reform sample

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)	Age 50–54 sample 2 (3)
Panel A: All men and women			
Placebo effect (Female $\times$ After)	0.246*** (0.038)	0.157*** (0.027)	0.161*** (0.027)
Female	-0.492*** (0.033)	-0.452*** (0.016)	-0.460*** (0.019)
After cohort	-0.057*** (0.019)	-0.032** (0.014)	-0.034** (0.015)
Mean: Women in after cohort	0.688	0.580	0.580
Implied percent increase in female participation	55.6%	37.0%	38.6%
R-squared	0.163	0.206	0.207
Observations	5,085	4,763	3,733
Panel B: Men and women with household income above median			
Placebo effect (Female $\times$ After)	0.242*** (0.045)	0.127*** (0.030)	0.136*** (0.032)
Female	-0.382*** (0.040)	-0.338*** (0.020)	-0.351*** (0.023)
After cohort	-0.017 (0.012)	0.001 (0.011)	0.002 (0.011)
Mean: Women in after cohort	0.848	0.768	0.768
Implied percent increase in female participation	39.8%	19.8%	21.5%
R-squared	0.128	0.176	0.183
Observations	2,537	2,377	1,862

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table presents placebo regression estimates on female labor force participation using data from the 1982, 1986 and 1990 Income Distribution Surveys. In Panel B, I restrict the sample to men and women with equivalised household income above the median (for their age and gender in the relevant survey). See equation (1) and Section 3 for details on the regression specification.

Table 3: Re-estimating the impact on female labor force participation

	(1)	(2)
Treatment effect (probability of being below pension age)	0.134*** (0.016)	0.041 (0.026)
Cohort controls	No	Yes
R-squared	0.086	0.093
Observations	5,863	5,863

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table re-estimates the effects of the reform on female labor force participation using equation (2). The regression sample consists of all women in the replication sample: women aged 60–64 in the eleven Income and Housing Costs Surveys between 1994/95 and 2009/10.

Web Appendix for “Re-examining female labor supply responses to the 1994 Australian pension reform”

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A Data Appendix

A.1 Other specifications in Atalay and Barrett (2015)

Atalay and Barrett's (2015) other specifications are modifications of their baseline regressions that allow the treatment effect to vary based on women's Age Pension Age (APA) cohort (and APA cohort and age). In these specifications, Atalay and Barrett (2015) divide the after cohort into five 'AC' cohorts, AC_1 ($APA \in \{60.5, 61.0\}$) to AC_5 ($APA \in \{64.5, 65.0\}$), and estimate separate treatment effects for women in each AC cohort. Like in Atalay and Barrett's (2015) baseline regressions, these specifications compare the labor force participation (LFP) of women in each AC cohort at ages 60–64 with the LFP of women in the before cohort, using the corresponding male cohorts as comparison groups. As such, these specifications rely on five parallel-trend assumptions. These assumptions are closely related to the baseline parallel-trends assumption in equation (1); the counterfactual for women in the after cohort in the baseline regressions is a weighted average of the counterfactuals for women in the five AC cohorts in the expanded specifications. As such, if the baseline parallel-trends assumption does not hold, at least one of the parallel-trend assumptions in the expanded specifications must not hold.

A.2 Constructing the probability that individuals are in the after cohort

For the pre-reform sample, I construct the probability that each individual is in the after cohort, $\Pr(CohortA_i^* = 1)$, in four steps. *Step 1:* I estimate the probability that each individual is a certain age in years (i.e., $\Pr(a_i)$) at the time of the survey, for each possible age, using annual population counts by single year of age and gender from the Australian Bureau of Statistics.^{A1} For example, for a woman aged 45–49, I construct their probability of being 45 years old as the total number of 45-year-old women in Australia in the relevant year divided by the total number of women aged 45–49. *Step 2:* For each possible age, I estimate an individual's conditional probability of being in the after cohort (i.e., $\Pr(CohortA_i^* = 1|a_i)$) under the assumption that their date of birth is uniformly distributed within the possible window (of approximately fifteen months),

^{A1}As the population data is collected in June and the survey data is collected between September and December, I use linear interpolation to estimate annual population counts by single year of age and gender at the midpoint of each survey period.

as per Atalay and Barrett's (2015) approach. *Step 3*: For each possible age, I multiply the probability that the individual is that age (i.e., the $\Pr(a_i)$ terms from *Step 1*) by the probability that an individual of that age is in the after cohort (i.e., the $\Pr(\text{Cohort}A_i^* = 1|a_i)$ terms from *Step 2*). *Step 4*: I sum up the terms in *Step 3* across each possible age to get $\Pr(\text{Cohort}A_i^* = 1)$, the overall probability that individual i is in the after cohort. That is:

$$\Pr(\text{Cohort}A_i^* = 1) = \sum_{a_i \in \mathbb{A}_i} \Pr(a_i) \times \Pr(\text{Cohort}A_i^* = 1|a_i)$$

where $\mathbb{A}_i$ is the set of possible ages for individual i .

A.3 Additional robustness checks of the placebo estimates in Table 2

In this section, I show that the placebo estimates in Table 2 are only marginally affected by certain idiosyncrasies of the pre-reform surveys, which are discussed in detail by Siminski et al. (2003) and include (i) changes in the measurement of labor force status, (ii) seasonality in labor force participation (LFP) rates, and (iii) the lack of individual-level age controls.

Changes in the measurement of labor force status across surveys. In the 1982 survey, labor force status was categorized differently to the surveys in 1986 and 1990. Table A1 shows how labor force status was categorized in each survey:

Table A1: Categorization of labor force status in each survey

1982	1986 & 1990
Current employment status:	Labor force status in current main and second jobs:
(i) Wages & salaries full-time worker	(i) Not applicable
(ii) Own business (limited liability) full-time worker	(ii) Permanently unable to work
(iii) Wages & salaries part-time worker	(iii) Still at school
(iv) Own business (limited liability) part-time worker	(iv) Studying full time
(v) Self employed	(v) Unpaid voluntary worker
(vi) Unemployed	(vi) Employed full-time
(vii) Not in labor force	(vii) Employed part-time
	(viii) Unemployed
	(ix) Not in the labor force

Notes: I define individuals as in the labor force if they are in any of the categories in boldface: categories (i)–(vi) in the 1982 survey and categories (vi)–(viii) in the 1986 and 1990 surveys.

Due to the different categories in each survey wave, it is not possible to construct a measure of LFP that is perfectly consistent across survey waves.^{A2} For example, categories (i)–(v) in the 1986 and 1990

^{A2}For the 1982 survey, I define individuals as in the labor force if they are in categories (i)–(vi) (i.e., any category but ‘not in labor force’). For the 1986 and 1990 surveys, I define individuals as in the labor force if they are in categories (vi)–(viii) (i.e., employed full-time, employed part-time or unemployed).

surveys — people categorized as ‘not applicable’, ‘permanently unable to work’, ‘still at school’, ‘studying full time’ or ‘unpaid voluntary worker’ — do not have an obvious analogue in the 1982 survey. However, as just 0.6% of the sample are in any of these categories, the impact of these inconsistencies is likely to be minor compared to the size of the placebo estimates in Table 2 (15.7–24.6 percentage points).

A more important concern is that the pre-reform surveys may either over-represent or under-represent labor force participants in different ways over time for men and women. For example, if the later surveys over-represent female labor force participants but not male participants, the placebo estimates in Table 2 may be biased upwards. While Figures 3a and 3b show that the LFP rates in the pre-reform surveys are broadly consistent with the aggregate data over time, Siminski et al. (2003) present evidence that female labor force participants are slightly over-represented in later surveys. Table A2 shows the ratio of labor force totals from the pre-reform surveys to the totals in the aggregate data from the Labour Force Survey for men and women of all ages (from Table 3 in Siminski et al. (2003)); numbers less than one suggest that labor force participants are under-represented in the survey data, while numbers greater than one suggest that labor force participants are over-represented in the survey data. The table shows that (i) labor force participants are under-represented in the 1982 survey and over-represented in the 1986 and 1990 surveys, and (ii) the degree of under/over-representation is larger among women.

Table A2: Ratio of labor force totals in pre-reform surveys to totals in Labour Force Survey

	Men	Women
1982	0.991	0.979
1986	1.023	1.036
1990	1.004	1.019

Source: Siminski et al. (2003).

To examine the extent of any bias caused, I re-estimate the placebo estimates in Table 2 in a way that accounts for the degree of under/over-representation of male and female labor force participants in each survey. Specifically, I re-estimate the placebo regressions by weighted least squares, where the weight for individual i , w_i , is defined as:

$$w_i = \begin{cases} 1 & \text{if } LFP_i = 0 \\ 1/r_i & \text{if } LFP_i = 1 \end{cases}$$

where r_i is the relevant ratio in Table A2. Intuitively, this reduces (increases) the weight of male and female labor force participants by the degree to which these types of individuals are over-represented (under-represented) in the survey data relative to the aggregate data.

Table B10 shows that this adjustment has only a modest impact on the placebo estimates. While the

estimates fall by 5–10%, they remain positive, large and statistically significant at the 1% level.^{A3}

Seasonality in labor force participation rates. The pre-reform surveys were conducted over a shorter period of time (less than three months) than the post-reform surveys used by Atalay and Barrett (2015), which were conducted over the course of one year. As shown in Table A3 below, each of the pre-reform surveys was conducted between September and December of the relevant year.

Table A3: Exact dates of pre-reform surveys

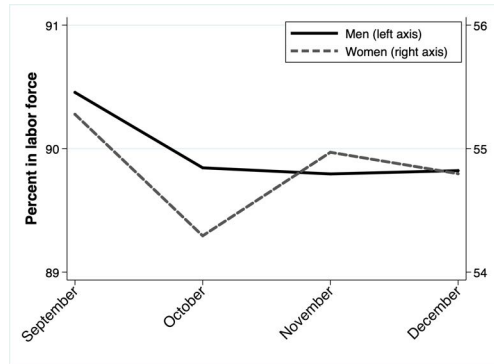
1982	Sometime between September and November
1986	September 14 to December 8
1990	October 7 to December 15

Source: Siminski et al. (2003).

Notes: Exact dates are not available for the 1982 survey.

Given the dates of the pre-reform surveys, seasonality may affect the LFP rates in these surveys. However, this impact is likely to be modest; among individuals aged 45–54 from 1982 to 1990, the average LFP rate was 1.30 percentage points higher between September and December than the rest of the year for women and 0.18 percentage points higher for men.^{A4} Thus, the impact of seasonality on the LFP rates in the pre-reform surveys is likely to be much smaller than the size of the placebo estimates in Table 2, which range from 15.7 to 24.6 percentage points. Moreover, seasonality is likely to have an even smaller impact on the *trends* in male and female LFP rates between surveys, given that each survey was conducted over similar months of the year. Figure A1 shows that there is little seasonality within the survey months. Figure A1 shows the average LFP rates of men and women aged 45–54 by month in the Labour Force Survey from 1982 to 1990. Unsurprisingly, the LFP rates are similar across the survey months, varying by less than one percentage point. Overall, the impact of seasonality on the placebo estimates is likely to be minor.

Figure A1: Average labor force participation rates by month within the survey months (1982–1990)



^{A3}The estimates also remain similar if I use the sample weights included in each survey (see Table B11).

^{A4}For this calculation, I use monthly data from the Labour Force Survey.

Lack of individual-level age controls. Another possible concern is the absence of controls for age. Since age information is only available in five-year groups in the pre-reform surveys, it is not possible to control for the age of individuals in these regressions. There are two reasons why controls for age may be useful in Atalay and Barrett’s (2015) differences-in-differences set-up: (i) as older individuals are generally less likely to be in the labor force, including age controls may improve the precision of estimates of the treatment effect and (ii) the age distribution may change in different ways over time for men and women, biasing estimates of the treatment effect. However, age controls have only a minor impact on the estimates. For example, with age controls, Atalay and Barrett’s (2015) baseline estimate falls by less than 3% (from 8.0 percentage points to 7.8 percentage points, see Table B12), and the standard error is almost identical to three decimal places. Moreover, I find that the placebo estimates are similar with gender-specific controls for the average age of individuals in the population (of the relevant ages) at the time of each survey; as shown in Table B13, the placebo estimates fall by just 2%.^{A5} This minor impact of controlling for age is not surprising given that (i) the identification strategy relies on the corresponding male cohorts as a comparison group for female cohorts and (ii) among individuals aged 45–49 and 50–54, the average age of men and women changed in similar ways over time, as shown in Table A4 below.^{A6}

Table A4: Average age of men and women at the time of each (pre-reform) survey

	1982	1986	1990
Average age (among those aged 45–49): Women	47.45	47.44	47.39
Average age (among those aged 45–49): Men	47.45	47.43	47.40
Average age (among those aged 50–54): Women	52.52	52.45	52.45
Average age (among those aged 50–54): Men	52.52	52.45	52.44

Notes: This table presents the average age of men and women aged 45–49 and 50–54 at the time of each survey. I construct these averages using annual populations counts by single year of age and gender from the Australian Bureau of Statistics.

A.4 Examining the key identifying assumption in Section 4 (“Re-estimating the impact on female participation”)

To test the validity of the parallel-trends assumption underpinning equation (2), I examine how women’s labor force participation (LFP) changes leading up to their Age Pension Age (APA). If the parallel-trends assumption is suitable, we would not expect to see any trend in women’s LFP leading up to the APA after controlling for age, cohort and individual factors. That is, after controlling for age, cohort and individual factors, women who are one year below the APA should have a similar level of LFP to women who are two,

^{A5}Due to perfect multicollinearity, it is not possible to include age controls for the sample in column 3 of Table 2, where there is no misclassification of individuals into the before and after cohorts.

^{A6}Unsurprisingly, the age distribution also changed in similar ways over time for men and women (see Table B14).

three or four years below the APA. To test whether this is true, I estimate regressions based on the following specification for all women aged 55–64 in the surveys used by Atalay and Barrett (2015):

$$LFP_i = \beta x_i + \delta_{-5} \mathbf{1}(age_i < APA_i - 4) + \sum_{\substack{j=-4 \\ j \neq -1}}^0 \delta_j \mathbf{1}(age_i \in [APA_i + j, APA_i + j + 1)) \\ + \delta_1 \mathbf{1}(age_i \geq APA_i + 1) + FE_age_yrs_i + FE_APA_cohort_i + u_i \quad (A1)$$

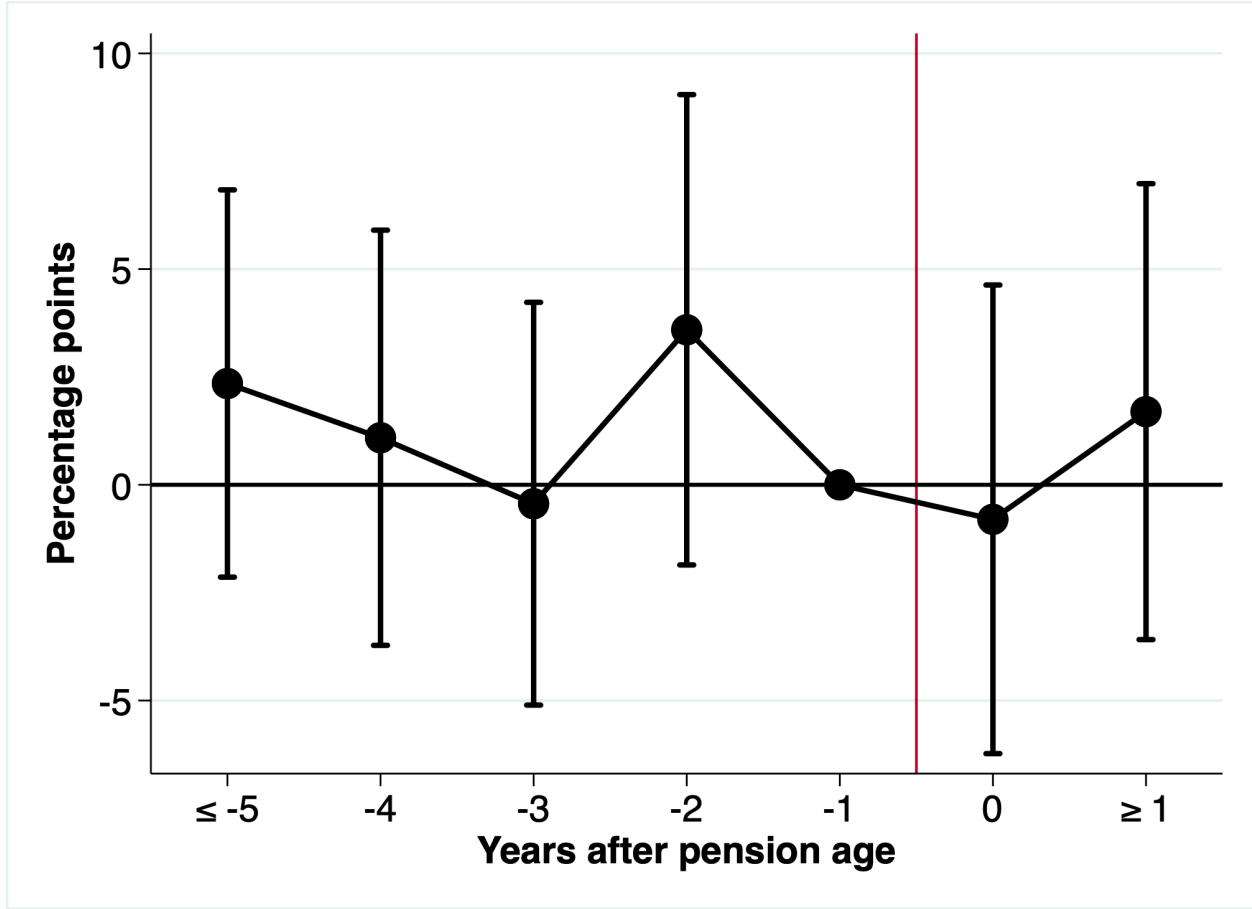
where these regressions modify equation (2) by replacing $\mathbf{1}(age_i < APA_i)$ with six indicator variables that each specify that a woman is a certain number of years above or below the APA. This divides women into seven mutually exclusive and exhaustive categories based on how many years they are above or below the APA. As information on age is only available in years, there is potential misclassification for some women of (i) the cohort fixed effects, $FE_APA_Cohort_i$, and (ii) the indicator variables specifying that a woman is a certain number of years above or below the APA. I address this issue by replacing these variables with (i) the probabilities that woman i is in each APA cohort, $\Pr(APA_Cohort_i)$, and (ii) the probabilities that woman i is in each of the six categories in equation (2).^{A7}

Each of the δ coefficients in equation (A1) should be interpreted as an effect relative to the omitted category, which is women who are less than or equal to one year below the APA. For example, δ_{-2} estimates the effect on LFP from women being 1 to 2 years below the APA, i.e. $age_i \in [APA_i - 2, APA_i - 1)$, relative to them being less than or equal to one year below the APA, i.e. $age_i \in [APA_i - 1, APA_i)$. If the identification strategy is suitable, we would expect the δ_j coefficients to be close to zero and statistically insignificant for $j < 0$. We would also not expect to find large estimates of the δ_0 and δ_1 coefficients given that equation (2) provides only weak evidence that the reform caused any increase in female LFP.

Figure A2 presents the estimated δ coefficients with 95% confidence intervals. The estimates support the identification strategy on both dimensions: (i) the estimated δ_j coefficients are close to zero and statistically insignificant at the 10% level for $j < 0$, both individually and jointly,^{A8} providing no evidence of any trend in women's LFP leading up to the APA, and (ii) the estimates of δ_0 and δ_1 are not significant at the 10% level, providing little evidence of any drop in female LFP from women reaching the APA.

^{A7}I construct these variables using the same approach as discussed below equation (2). First, I calculate the probabilities that woman i is in each APA cohort, $\Pr(APA_Cohort_i)$, under Atalay and Barrett's (2015) assumption that birth dates are uniformly distributed within the possible window of fifteen months. Then, I use these probabilities to calculate woman i 's expected APA, $\mathbb{E}[APA_i]$. Finally, I calculate the probabilities that woman i is in each category under the assumption that her exact age is uniformly distributed on the interval $[age_y, age_y + 1)$, where age_y is her age in years.

^{A8}The p -value on the joint significance test is 0.615.

Figure A2: Estimated effects on female labor force participation relative to year before the pension age

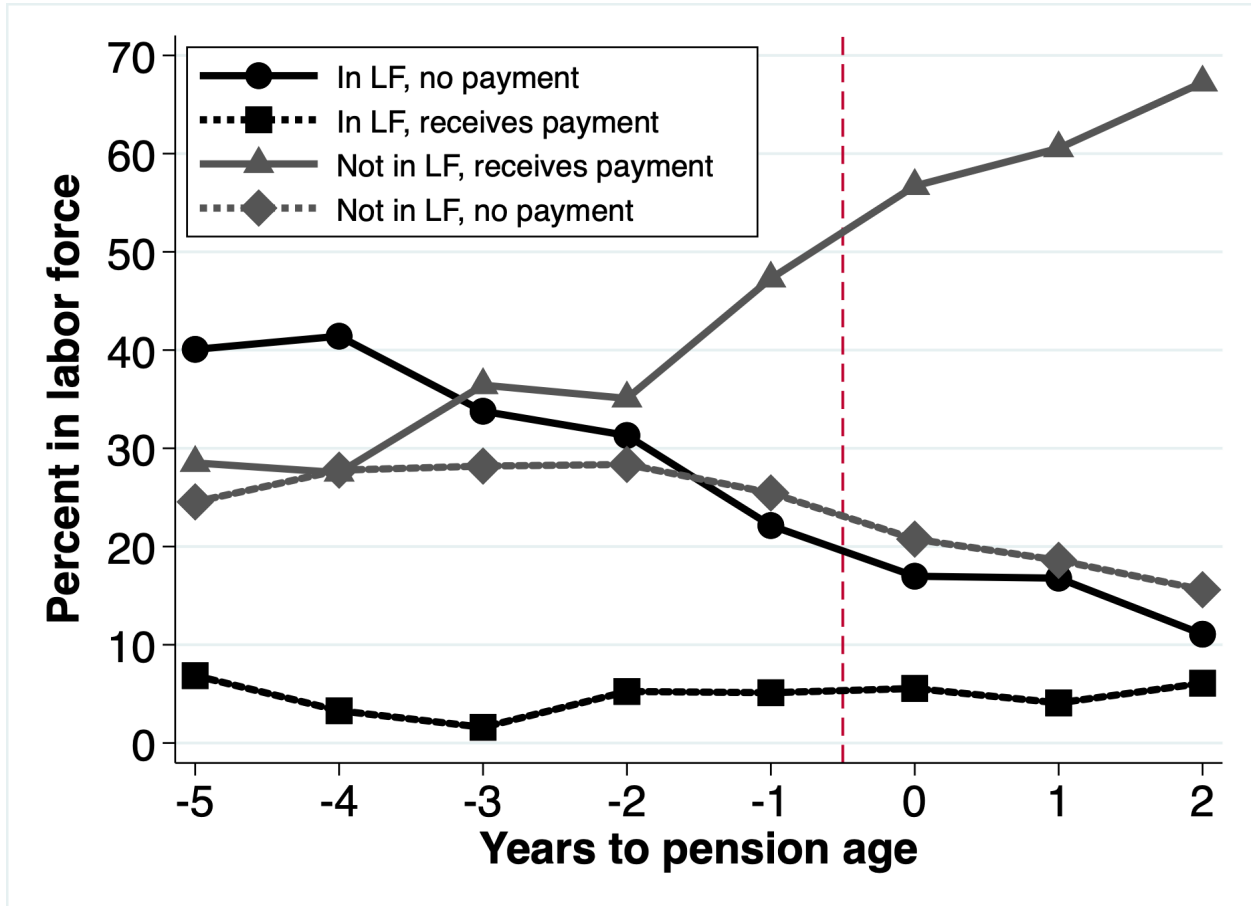
Notes: These figures plot the estimated δ coefficients, with 95% confidence intervals, from estimates of equation (A1). The regression sample consists of women aged 55–64 from the surveys used by Atalay and Barrett (2015) — the eleven Income and Housing Costs Surveys from 1994/95 to 2009/10.

A.5 Assessing whether “not in the labor force” is a suitable proxy for retirement

I follow Atalay and Barrett (2015) and use labor force participation (LFP) as the key variable for analysis. Atalay and Barrett (2015) implicitly use “not in the labor force” as a proxy for retirement behavior. For example, in the abstract, they discuss a “decline in the probability of retirement by 12 to 19 percentage points”. While I am slightly more cautious in the interpretation of my estimates, it is worth discussing whether the estimates in this paper are likely to reflect retirement decisions or other factors affecting female LFP. As discussed in the paper, my preferred identification strategy picks up within-cohort changes in LFP around the APA. This variation seems likely to result mainly from decisions to retire, given the low re-entry into the labor market at these ages. To assess this further, I examine whether the decrease in LFP rates around the APA coincides with an increase in the receipt of government payments. Specifically, I divide women into four groups: (i) in labor force and not receiving a government payment (21.9% of the sample); (ii) in

labor force and receiving a government payment (4.9%); (iii) not in labor force and receiving a government payment (51.2%); (iv) not in labor force and not receiving a government payment (22.0%). Evidently, most women are either in the labor force or receiving income support but not both. Moreover, Figure A3 shows that, around the APA, women mainly transition from group (i) ‘in labor force, no payment’ to group (iii) ‘not in labor force, receives payment’. Thus, within-cohort changes in LFP around the APA are likely to mainly reflect decisions to retire with the support of government payments.

Figure A3: Changes in female labor force participation and government support around the pension age

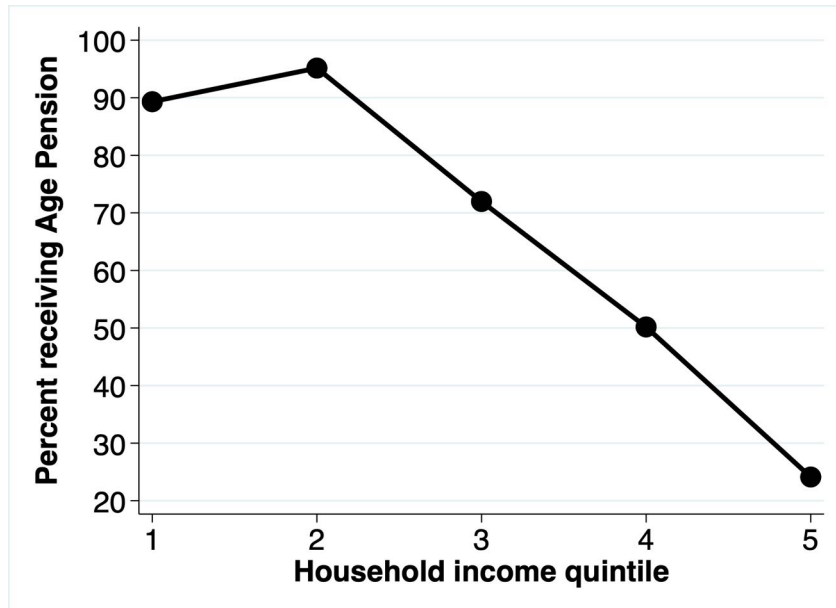


Notes: This figure shows the proportion of women in each of the four groups (based on their labor force participation and receipt of government payments) by the number of years to their pension age. I assign women's number of years to pension age based on their most likely group, assuming birth dates are uniformly distributed within the possible 15-month windows. The sample comes from the eleven Income and Housing Costs Surveys used by Atalay and Barrett (2015) from 1994 to 2010.

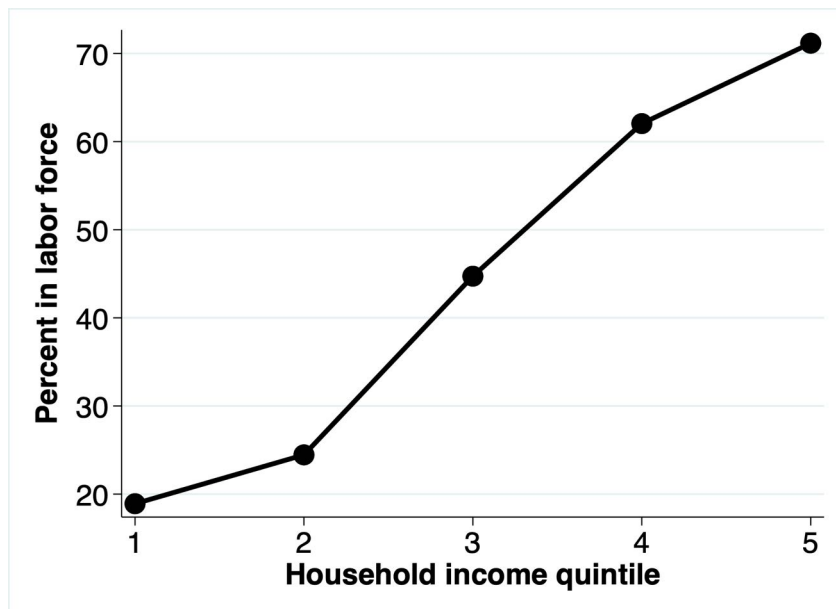
B Additional figures and tables

Figure B1: Age Pension receipt and labor force participation by household income

(a) Age Pension receipt: Pension-age women



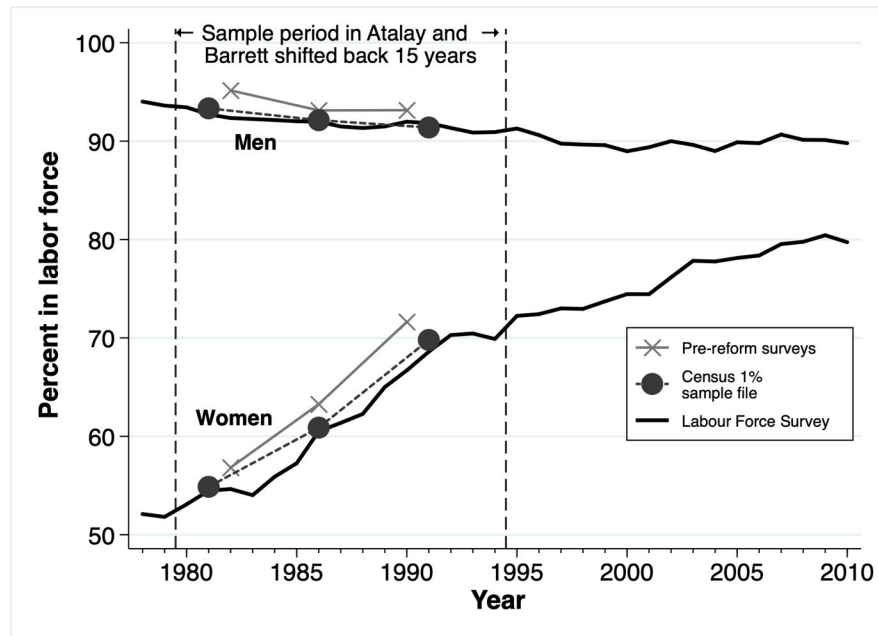
(b) Labor force participation: Women at age 59



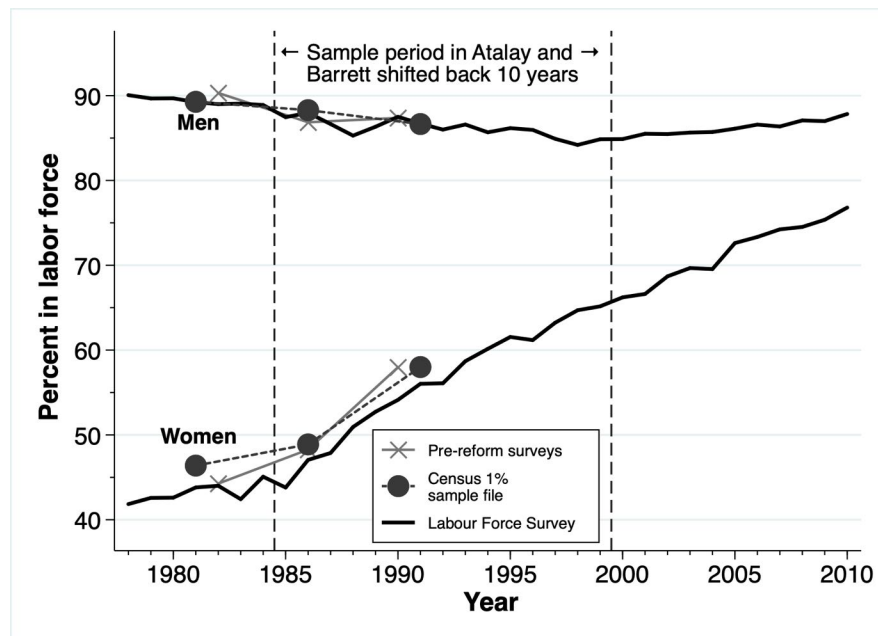
Notes: These figures show how Age Pension receipt and labor force participation vary among women based on their household income. I divide women into quintiles based on their equivalised household income, after excluding their own income from the Age Pension and other transfers. In (a), I restrict the sample to women who are above the pension age. In (b), I restrict the sample to women aged 59, who are all below the pension age. The overall sample consists of women aged 59–64 from the eleven Income and Housing Costs Surveys used by Atalay and Barrett (2015) from 1994 to 2010.

Figure B2: Comparison of the participation rates in the pre-reform surveys with Census data

(a) Ages 45–49

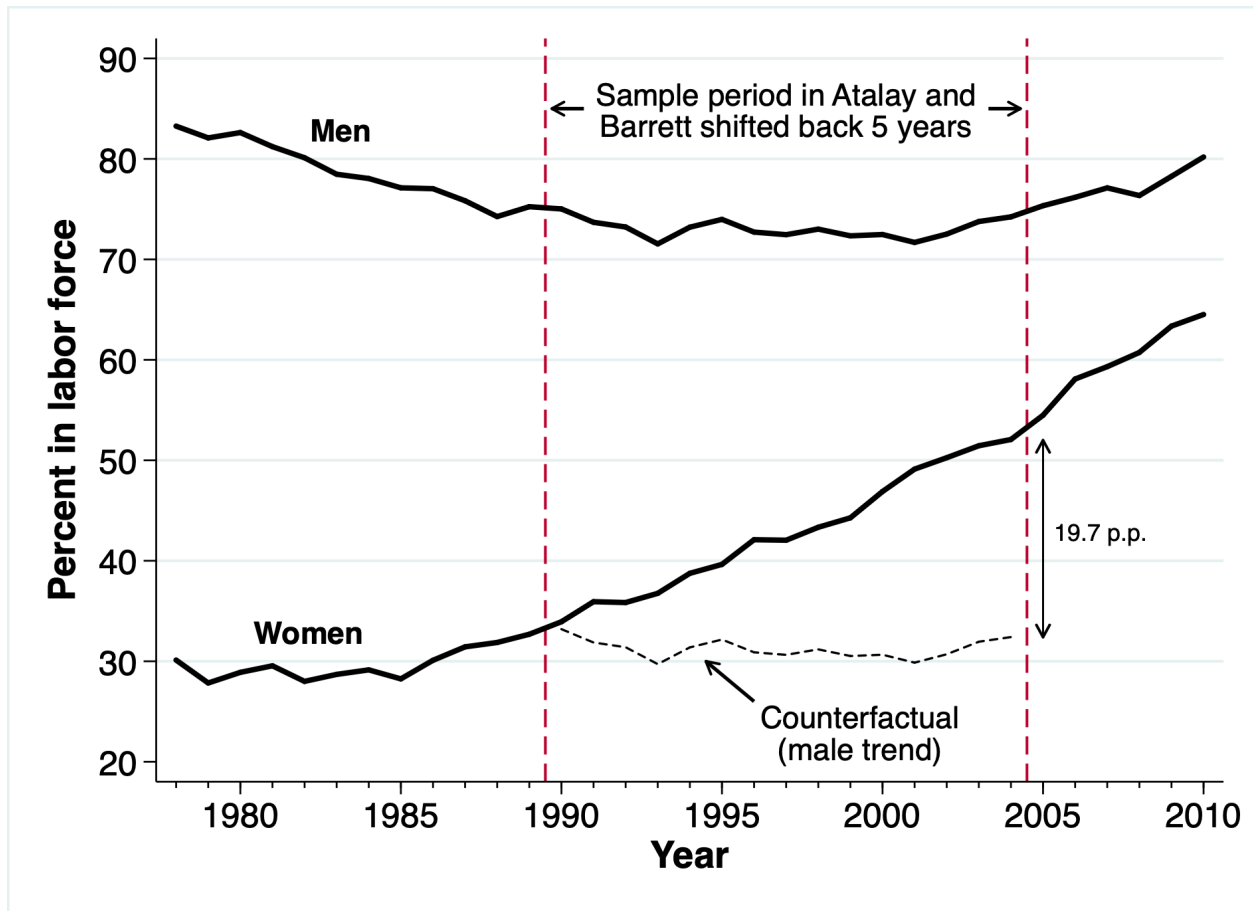


(b) Ages 50–54



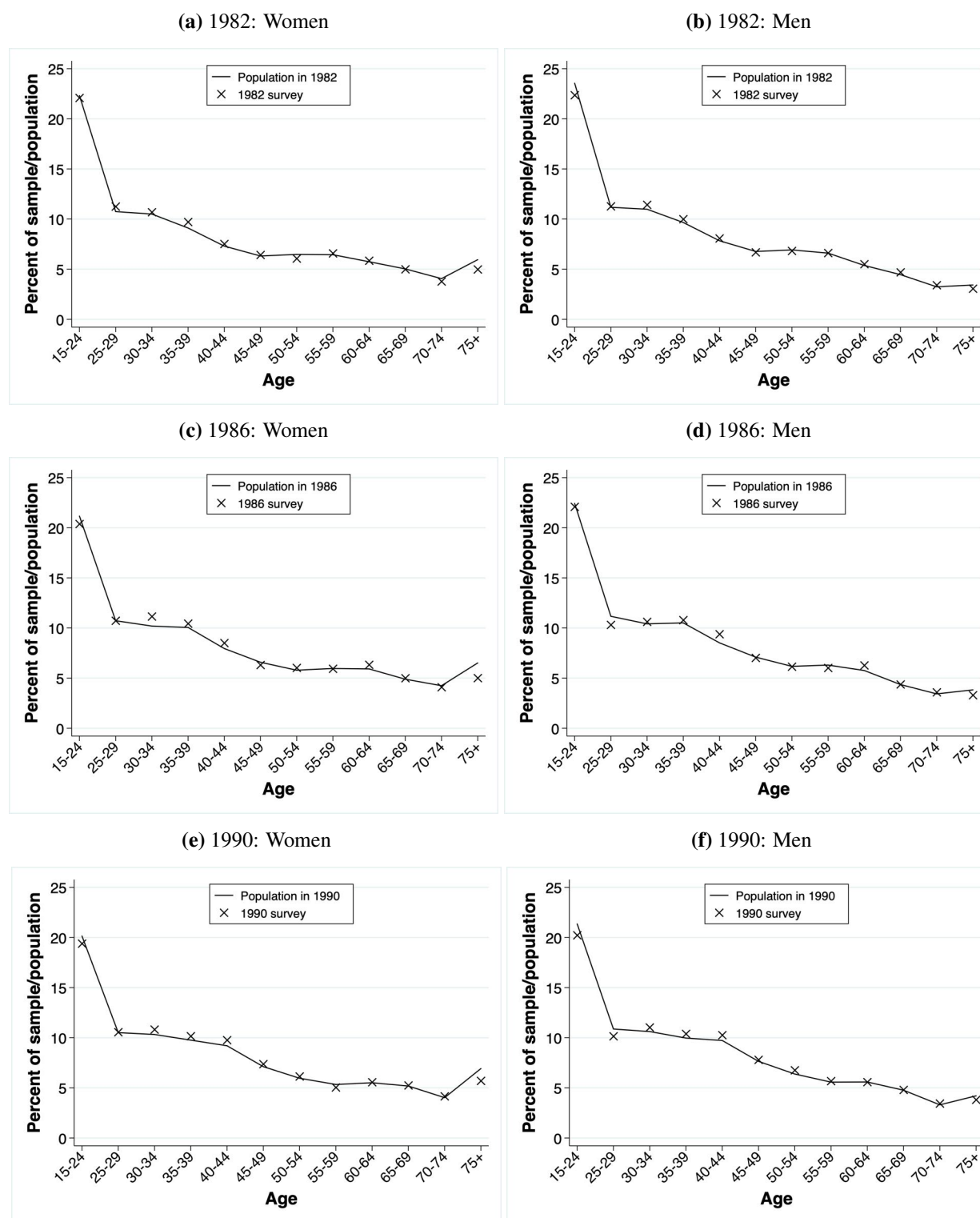
Notes: These figures augment Figures 3a and 3b with the labor force participation (LFP) rates in the 1% sample files for the 1981, 1986 and 1991 Censuses. These figures show that the LFP rates in the pre-reform surveys are consistent with the aggregate data in both the Census and the Labour Force Survey.

Figure B3: Labor force participation rates of the relevant cohorts at ages 55–59



Notes: This figure plots labor force participation (LFP) rates from the Labour Force Survey. Between the vertical dashed lines, the cohorts in Atalay and Barrett's sample were aged 55–59. During these periods, male and female LFP rates follow opposing trends.

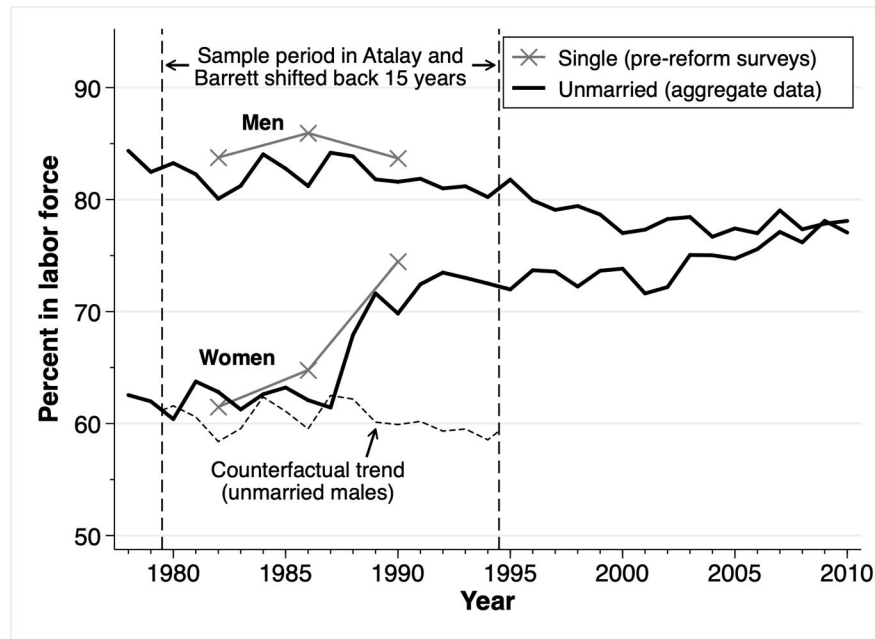
Figure B4: Representativeness of the pre-reform surveys with respect to age



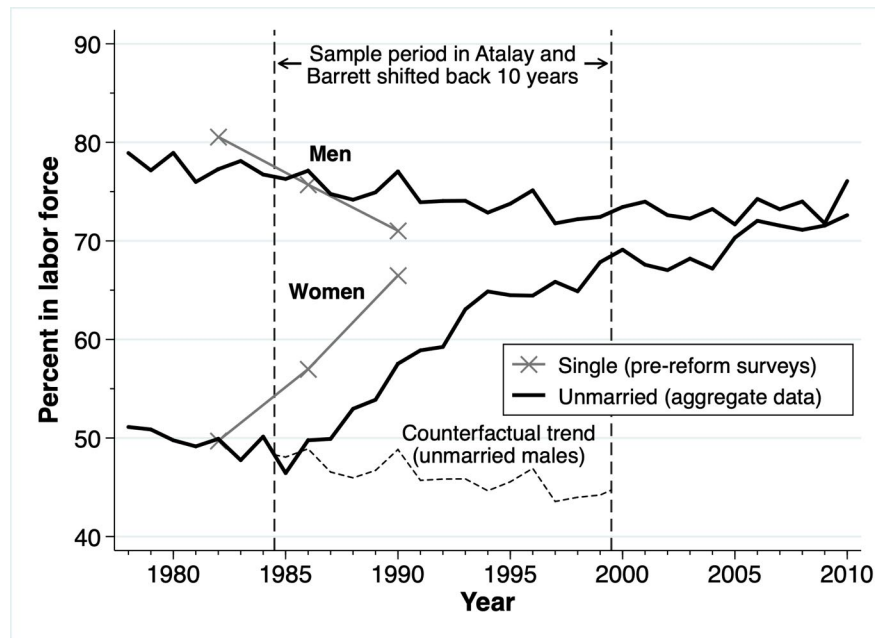
Notes: This graph examines the representativeness of the pre-reform surveys with respect to age. It shows the fraction of male and female respondents from the 1982, 1986 and 1990 Income Distribution Surveys in different age groups. The surveys allow for a consistent definition of the age groups shown above. I use estimates of the total population by single year of age and gender from the Australian Bureau of Statistics to construct the fraction of the 15+ population in each age group in the relevant years (1982, 1986, 1990) for men and women.

Figure B5: Participation rates in the pre-reform surveys of single/unmarried men and women

(a) Ages 45–49

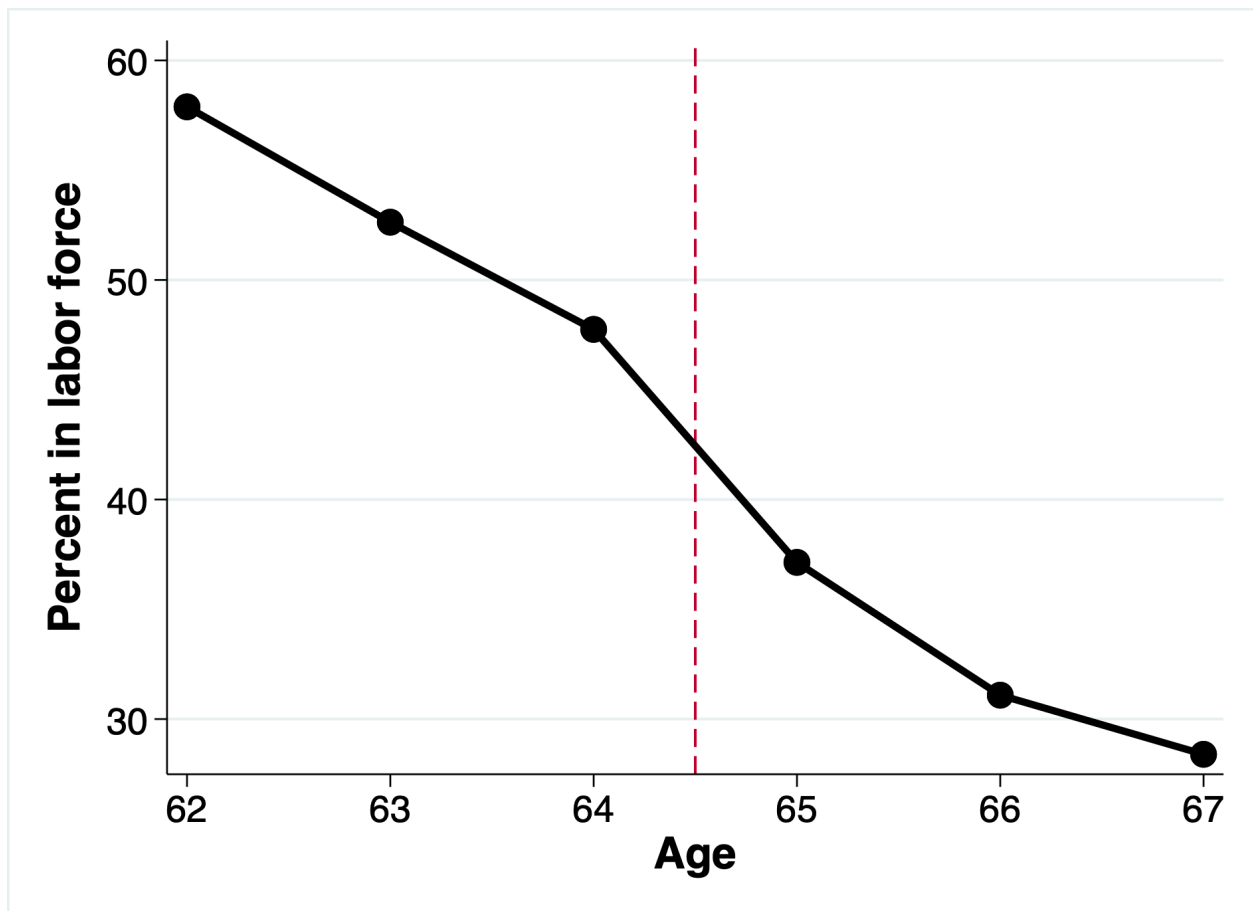


(b) Ages 50–54



Notes: These figures plot labor force participation (LFP) rates from the Labour Force Survey (in black) and the Income Distribution Surveys (in gray). The gray line shows the LFP rates of single men and women, while the black line shows the LFP rates for unmarried men and women. Between the vertical dashed lines, the cohorts in Atalay and Barrett’s (2015) sample were aged 45–49 in (a) and 50–54 in (b). During these periods, male and female LFP rates follow opposing trends for both the single and unmarried samples.

Figure B6: Male labor force participation rates around the pension age



Notes: This figure shows how male labor force participation rates change around the pension age. The sample comes from waves 1–14 of the Household Income and Labour Dynamics in Australia Survey (2001–2015).

Table B1: Comparison of the number of observations and means of key variables in the replication sample to Atalay and Barrett's (2015) sample

	Women		Men	
	Atalay & Barrett (1)	Replication (2)	Atalay & Barrett (3)	Replication (4)
Labor force participation	0.27	0.27	0.52	0.52
Age (years)	61.9	61.9	61.9	61.9
Single	0.29	0.30	0.19	0.19
Bachelor degree +	0.17	0.17	0.19	0.20
Household size	1.73	1.75	1.90	1.91
Receives any government payment				
Full sample	0.49	0.56	0.32	0.37
Below APA		0.39		0.37
Receives payment other than the Age Pension				
Below APA		0.39		0.37
Above APA		0.06		N/A
Equivalised household income per week (2008 AUD)				
Receives any government payment		441		454
Not receiving any government payment		860		894
Observations	5,822	5,863	5,587	5,634

Notes: This table shows that I am able to closely replicate Atalay and Barrett's (2015) sample. The replication sample is similar to Atalay and Barrett's (2015) sample in terms of the number of observations and the means of the key variables.

Table B2: Replication of Atalay and Barrett's (2015) other labor force participation estimates

	Atalay & Barrett (1)	Replication (2)
<u>Estimates in Table 3 on single sample</u>		
Treatment effect (Female $\times$ After)	0.135*** (0.043)	0.138*** (0.043)
Female	-0.218*** (0.038)	-0.226*** (0.038)
After cohort	-0.011 (0.037)	-0.005 (0.037)
R-squared		0.090
Observations	2,809	2,825
<u>Key estimates in Table 4</u>		
Female $\times$ Pension age $\in \{60.5, 61.0\}$	0.032 (0.026)	0.031 (0.033)
Female $\times$ Pension age $\in \{61.5, 62.0\}$	0.068** (0.029)	0.067** (0.029)
Female $\times$ Pension age $\in \{62.5, 63.0\}$	0.028 (0.026)	0.030 (0.028)
Female $\times$ Pension age $\in \{63.5, 64.0\}$	0.103*** (0.028)	0.106*** (0.028)
Female $\times$ Pension age $\in \{64.5, 65.0\}$	0.128*** (0.033)	0.127*** (0.032)
R-squared		0.126
Observations	11,409	11,497
<u>Key estimate in Table 5</u>		
Hypothetical one-year increase in the pension age	0.120** (0.049)	0.121** (0.050)
R-squared		0.131
Observations	11,409	11,497

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table replicates Atalay and Barrett's (2015) estimates in Table 3 for the single sample and the key estimates in Tables 4 and 5. Table 4 presents estimates where the baseline regressions are modified to allow the effect on female labor force participation to vary based on women's pension age. Table 5 presents estimates where the regressions are modified further to allow the treatment effect to vary based on women's pension age and age; Atalay and Barrett (2015) estimate the average response to a one-year increase in the pension age as a weighted average of these estimates. I use the delta rule to construct the standard error of this estimate.

Table B3: Comparison of the placebo estimates in Table 2 to those using the uniform distribution to construct the probability that individuals are in the after cohort

Panel A: Baseline estimates in Table 2		
	Age 45–49 sample (1)	Age 50–54 sample 1 (2)
Placebo effect (Female $\times$ After)	0.246*** (0.038)	0.157*** (0.027)
R-squared	0.163	0.206
Observations	5,085	4,763
Panel B: Estimates using uniform distribution		
Placebo effect (Female $\times$ After)	0.246*** (0.040)	0.156*** (0.026)
R-squared	0.163	0.206
Observations	5,085	4,763

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows how the placebo estimates in Table 2 change if $\Pr(\text{Cohort}A_i^* = 1)$, the probability that individual i is in the after cohort, is constructed under the assumption that each person's birthdate is uniformly distributed within the possible window, rather than the more accurate approach in Table 2 (and described in Appendix A.2), which uses annual population counts by gender and single year of age.

Table B4: Placebo regression estimates on the pre-reform sample of single men and women

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)	Age 50–54 sample 2 (3)
Placebo effect (Female $\times$ After)	0.162 (0.113)	0.240*** (0.068)	0.246*** (0.071)
Female	-0.297*** (0.095)	-0.324*** (0.045)	-0.337*** (0.050)
After cohort	-0.028 (0.082)	-0.095* (0.050)	-0.098** (0.050)
Mean: Women in after cohort	0.714	0.665	0.665
Implied percent increase in female participation	29.3%	56.3%	58.7%
R-squared	0.088	0.108	0.121
Observations	759	824	661

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table presents placebo regression estimates on single women's labor force participation using data from the 1982, 1986 and 1990 Income Distribution Surveys. See equation (1) and Section 3 for details on the regression specification.

Table B5: Impact of reducing the sensitivity of the placebo estimates at ages 45–49 to differences between the 1982 survey and subsequent surveys

	Baseline estimate (1)	No excess sensitivity (2)
Treatment effect (Female $\times$ After)	0.246*** (0.040)	0.137*** (0.022)
R-squared	0.163	0.163
Observations	5,085	5,085

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows the impact of reducing the sensitivity of the placebo estimates at ages 45–49 to differences between the 1982 survey and subsequent surveys. Column 1 shows the baseline estimate, where I follow Atalay and Barrett (2015) by using the probability of individuals being in the after cohort in the regressions. Column 2 reduces this probability to zero for individuals in the 1982 survey to reduce the sensitivity of the placebo estimates to differences in LFP rates between this survey and subsequent surveys.

Table B6: Impact of controls on the placebo estimates

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)	Age 50–54 sample 2 (3)
Panel A: Controls excluded from regressions			
Placebo effect (Female $\times$ After)	0.251*** (0.040)	0.162*** (0.028)	0.166*** (0.028)
R-squared	0.143	0.181	0.184
Observations	5,085	4,763	3,733
Panel B: Controls included in regressions			
Placebo effect (Female $\times$ After)	0.246*** (0.038)	0.157*** (0.027)	0.161*** (0.027)
R-squared	0.163	0.206	0.207
Observations	5,085	4,763	3,733

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows the impact of controls on the placebo estimates in Table 2, which test for an effect of the reform on the labor force participation of women in the after cohort before the reform was announced. Individual-level age controls are not available, though the estimates are similar with gender-specific controls for the average age of individuals in the sample at the time of each survey (see Table B13). The sample comes from the 1982, 1986 and 1990 Income Distribution Surveys. See equation (1) and Section 3 for details on the sample and regression specification.

Table B7: Placebo regression estimates without men as a comparison group

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)	Age 50–54 sample 2 (3)
Panel A: Controls excluded from the regressions			
Placebo effect (After cohort)	0.232*** (0.038)	0.137*** (0.023)	0.137*** (0.022)
Mean: Women in after cohort	0.688	0.580	0.580
Implied percent increase in female participation	50.9%	31.0%	31.0%
R-squared	0.015	0.015	0.019
Observations	2,538	2,343	1,823
Panel B: Controls included in regressions			
Placebo effect (After cohort)	0.198*** (0.039)	0.115*** (0.023)	0.119*** (0.024)
Mean: Women in after cohort	0.688	0.580	0.580
Implied percent increase in female participation	40.5%	24.8%	25.7%
R-squared	0.045	0.062	0.060
Observations	2,538	2,343	1,823

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table presents placebo regression estimates on women's labor force participation using data from the 1982, 1986 and 1990 Income Distribution Surveys. The regressions modify equation (1) by dropping men from the sample and omitting the female dummy and the interaction term from the regression.

Table B8: Impact of controls on Atalay and Barrett's (2015) estimates

	Without controls (1)	With controls (2)
Treatment effect (Female $\times$ After)	0.087*** (0.022)	0.078*** (0.020)
R-squared	0.076	0.121
Observations	11,497	11,497

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows the impact of controls on Atalay and Barrett's (2015) baseline estimate of the effect of the reform on female labor force participation.

Table B9: Impact of controls for Age Pension eligibility on Atalay and Barrett's (2015) estimates

	(1)	(2)
Treatment effect (Female $\times$ After)	0.078*** (0.021)	0.036 (0.023)
Probability of being above pension age		-0.069*** (0.015)
R-squared	0.121	0.122
Observations	11,497	11,497

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows that Atalay and Barrett's (2015) estimates fall by over 50% and become statistically insignificant if equation (1) is augmented with the probability that individuals have reached the APA. To construct this variable, I first calculate the probabilities that woman i is in each APA cohort, $\Pr(\text{APA_Cohort}_i)$, under the assumption that woman i 's birth date is uniformly distributed within the possible fifteen-month window, as per Atalay and Barrett's (2015) approach. Then, I use these probabilities to calculate woman i 's expected APA, $\mathbb{E}[\text{APA}_i]$. Finally, I calculate $\Pr(\text{age}_i \geq \mathbb{E}[\text{APA}_i])$ under the assumption that each woman's exact age is uniformly distributed on the interval $[\text{age}_y, \text{age}_y + 1)$, where age_y is her age in years.

Table B10: Impact of adjusting the placebo estimates for differential over-representation of female labor force participants over time

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)	Age 50–54 sample 2 (3)
Panel A: Baseline estimates in Table 2			
Placebo effect (Female $\times$ After)	0.246*** (0.038)	0.157*** (0.027)	0.161*** (0.027)
R-squared	0.163	0.206	0.207
Observations	5,085	4,763	3,733
Panel B: With adjustment			
Placebo effect (Female $\times$ After)	0.228*** (0.040)	0.148*** (0.027)	0.153*** (0.027)
R-squared	0.163	0.208	0.207
Observations	5,085	4,763	3,733

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows the impact on the placebo estimates in Table 2 of adjusting for the degree of over-representation of male and female labor force participants in each survey relative to the aggregate data. In Panel B, I re-estimate the placebo regressions using weighted least squares, where I reduce (increase) the weight of male and female labor force participants by the degree to which these types of individuals are over-represented (under-represented) in the relevant survey (see Appendix A.3 for details). The sample comes from the 1982, 1986 and 1990 Income Distribution Surveys. See equation (1) and Section 3 in the text for details on the sample and regression specification.

Table B11: Impact of sample weights on the placebo estimates

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)	Age 50–54 sample 2 (3)
Panel A: Without sample weights			
Placebo effect (Female $\times$ After)	0.246*** (0.038)	0.157*** (0.027)	0.161*** (0.027)
R-squared	0.163	0.206	0.207
Observations	5,085	4,763	3,733
Panel B: With sample weights			
Placebo effect (Female $\times$ After)	0.221*** (0.043)	0.135*** (0.028)	0.147*** (0.029)
R-squared	0.158	0.203	0.208
Observations	5,085	4,763	3,733

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Notes: This table shows the impact of using the sample weights on the placebo estimates in Table 2, which test for an effect of the reform on the labor force participation of women in the after cohort before the reform was announced. The sample comes from the 1982, 1986 and 1990 Income Distribution Surveys. See equation (1) and Section 3 in the text for details on the sample and regression specification.

Table B12: Impact of age controls on Atalay and Barrett's (2015) estimates

	No age controls (1)	With age controls (2)
Treatment effect (Female $\times$ After)	0.080*** (0.021)	0.078*** (0.022)
R-squared	0.103	0.121
Observations	11,497	11,497

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows the impact of individual-level age controls on Atalay and Barrett's (2015) baseline estimate of the effect of the reform on female labor force participation.

Table B13: Impact of age controls on the placebo estimates

	Age 45–49 sample (1)	Age 50–54 sample 1 (2)
Panel A: Without age controls		
Placebo effect (Female $\times$ After)	0.246*** (0.038)	0.157*** (0.027)
R-squared	0.163	0.206
Observations	5,085	4,763
Panel B: With age controls		
Placebo effect (Female $\times$ After)	0.241*** (0.039)	0.154*** (0.027)
R-squared	0.164	0.206
Observations	5,085	4,763

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table shows the impact of age controls on the placebo estimates in Table 2, which test for an effect of the reform on the labor force participation of women in the after cohort before the reform was announced. In Panel B, I include gender-specific controls for the average age of individuals in the sample at the time of each survey. The sample comes from the 1982, 1986 and 1990 Income Distribution Surveys. See equation (1) and Section 3 in the text for details on the sample and regression specification.

Table B14: Age distribution of men and women at the time of each (pre-reform) survey

	1982	1986	1990
<u>Age 45–49 sample:</u>			
Proportion aged 45: Women	0.210	0.214	0.223
Proportion aged 45: Men	0.210	0.215	0.221
Proportion aged 46: Women	0.208	0.205	0.213
Proportion aged 46: Men	0.208	0.207	0.212
Proportion aged 47: Women	0.199	0.198	0.195
Proportion aged 47: Men	0.198	0.197	0.195
Proportion aged 48: Women	0.191	0.193	0.185
Proportion aged 48: Men	0.192	0.192	0.187
Proportion aged 49: Women	0.192	0.190	0.184
Proportion aged 49: Men	0.192	0.189	0.185
<u>Age 50–54 sample:</u>			
Proportion aged 50: Women	0.190	0.215	0.211
Proportion aged 50: Men	0.192	0.215	0.213
Proportion aged 51: Women	0.204	0.201	0.203
Proportion aged 51: Men	0.202	0.201	0.206
Proportion aged 52: Women	0.201	0.195	0.200
Proportion aged 52: Men	0.201	0.195	0.200
Proportion aged 53: Women	0.201	0.196	0.195
Proportion aged 53: Men	0.202	0.195	0.194
Proportion aged 54: Women	0.203	0.193	0.191
Proportion aged 54: Men	0.203	0.194	0.187

Notes: This table shows the age distribution among men and women aged 45–49 and 50–54 at the time of each survey. I construct this table using annual populations counts by single year of age and gender from the Australian Bureau of Statistics.

Table B15: Robustness of the updated estimates of the impact on female labor force participation

	Baseline estimate (1)	Time controls, not cohort (2)	Baseline without measurement error in		
			Pension eligibility (3)	Pension cohort (4)	Both (5)
Treatment effect	0.041 (0.026)	0.039* (0.022)	0.038 (0.027)	0.041 (0.040)	0.031 (0.042)
Cohort controls	X		X	X	X
Year-quarter fixed effects		X			
No misclassification of pension eligibility			X		X
No misclassification of pension cohort				X	X
R-squared	0.093	0.097	0.100	0.090	0.093
Observations	5,863	5,863	5,101	2,655	2,577

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Bootstrapped standard errors, based on 999 replications, in parentheses.

Notes: This table examines the robustness of the preferred estimates of the impact of the reform on female labor force participation from equation (2). The regression sample consists of all women in the replication sample: women aged 60–64 in the eleven Income and Housing Costs Surveys between 1994/95 and 2009/10.