Age of majority and early accumulation of human capital by sex in Australia

Randy Cragun[*] and Ishita Chatterjee[**]

[*]*Department of Economics, Birmingham-Southern College. Contact: rc@rcragun.net*
[**]*Department of Economics, The University of Western Australia*

26 January 2024
First version: 2017

Abstract

We use state-specific variation in the timing of age of majority laws in Australia to estimate effects on human capital accumulation, marriage, and career outcomes. The data are the 1991, 1996, 2001, and 2006 Censuses of Population and Housing. Estimated effects of living under an age of majority of 18 rather than 21 differ by gender and age. Results are consistent with women marrying earlier, obtaining more post-secondary certifications, and earning more in middle age. Men, however, married later and earned and worked less in their 20s while obtaining more bachelor's degrees. Similar research using state-specific age of majority laws in the US has been interpreted as demonstrating effects of lower-cost access to oral contraception.

Introduction

In the 1970s, every Australian state lowered its age of majority (AoM) from 21 to 18, granting broad rights to young adults. As shown in Table 1, states lowered their AoM at different times spread out over seven years, so which birth cohort first reached legal adulthood at age 18 differs by state. We use this policy variation to estimate effects of earlier legal adulthood on educational attainment, labor force participation and employment, incomes over the life cycle, and marriage timing separately for women and men using data from the Australian Censuses of Population and Housing.

Relationship to "Power of the Pill" literature

Some readers may interpret this research as estimating effects of confidential access to oral contraceptives ("the pill") in late adolescence because we use the same methods, treatments, and outcomes used in the literature on "Early Legal Access" (ELA) to the pill (term from M. Bailey 2006). In a seminal article, Goldin & Katz (2002) argue that state-specific reductions in ages of majority from 21 to 18 in the US in the 1970s were independent of desires for contraception but inadvertently gave younger women the ability to consent to medical treatments, which made it easier for young women to obtain oral contraceptives ("the pill") without parental consent.[1] They and the subsequent ELA literature estimate reduced form

[1] The set of policies used in the ELA literature to identify variation in pill access also includes medical consent laws, mature minor doctrines, and various other laws and policies that affirmed that

effects of these differently-timed state-specific policies on life outcomes for women and interpret the results as effects of increased access to the pill (Ananat and Hungerman 2012; Bailey 2006; Bailey, Guldi, and Hershbein 2013; Bailey, Hershbein, and Miller 2012; Goldin and Katz 2002; Guldi 2008; Myers 2017). The only substantial difference between the present work and the existing ELA literature is that we study Australia.

Throughout the article, we avoid interpreting results as effects of pill access but recognize that some readers will prefer that interpretation. A lower AoM likely reduced the cost of using oral contraceptives, but there are many alternative mechanisms that could also explain effects of AoM reductions. While some of the ELA literature attempts to show that pill access is the primary mechanism, the evidence is weak. Bailey (2006) presents evidence that AoM reductions (and other policies related to pill access) in the US delayed fertility, but Myers (2017) shows that the evidence is not robust to controls for reasonable alternative operationalizations of abortion policy and procedures for sample selection. Bailey (2006) also presents estimates suggesting that effects on labor force participation only occurred among women who delayed fertility, but Cragun (2023) shows that these apparent effects are due to bias induced by a model restriction. Estimates from Bailey, Hershbein, and Miller (2012) suggest that women who lived under one of these policies in late adolescence were more likely to use the pill before age 22. However, this does not rule out other mechanisms. Even if AoM reductions increase pill use, it does not follow that downstream effects on earnings or labor force participation are effects of pill access.

There is one important difference between US and Australiana laws. In the US, laws that altered pill access sometimes changed the minimum age at which a person could marry without parental consent. Thus, estimates of effects of these laws may conflate effects of the minimum marriage age with effects of pill access. However, no state-level AoM in Australia determines the legal minimum marriage age. Thus, Australian laws may be even better candidates for identifying variation in pill costs than US laws are.

Potential mechanisms

Reaching the AoM confers a broad set of rights and responsibilities on a young person. We suggest that there are three primary mechanisms of effects: changes to medical consent rules, reductions in the cost of forming new households, and increases in the expectation that young people should be self-sufficient.

A lower AoM means that physicians could accept the consent of younger people to receive treatment. This change lowers the cost of obtaining oral contraceptives or abortion in late adolescence, thereby increasing certainty about fertility timing and lowering the costs of school and career investments and of engaging in sexual activity. These effects are

---

physicians could accept the consent of young women. The exact policy assumed to grant ELA differs between research articles, but AoM reductions are invariably a large fraction of the relevant policies. We find no evidence that any Australian state had such policies before its AoM reduction.

particularly important for women. We expect these mechanisms to lead to women delaying fertility, getting more schooling, earning less in youth (while investing in skills), earning more in middle age (when skills pay off), having fewer career interruptions, having more prestigious careers, and being more likely to work. There might also be some reduction in attachment to the labor force for a subset of women in their 30s because of delayed fertility.

Easier access to credit at ages 18–20 reduces the cost to young couples of forming new households. Access to mortgages and other financing for home goods might then hasten early marriage. Indeed, news coverage of AoM changes in the early 1970s often discussed the importance of hire-purchase agreements for couples forming new households. Earlier marriage may then lead to earlier fertility and to the opportunity for couples to specialize in household and market production. Because women are more likely than men to be household specialists, we expect this mechanism to decrease earnings, labor force participation, and educational attainment for women and increase those variables for men. However, there is an exception: some man-woman households that will eventually have the woman be a homemaker will first fund the man's investment in human capital with the woman's earnings on the formal labor market. For these households, employment by young women should increase temporarily. It is also possible that earlier fertility could mean that mothers will be younger when their children abandon the hive, leading to increased employment by these women at later ages. However, this effect could be offset by women having more children.

Expectations placed on young people might change. The lower AoM might make parents feel less responsibility for their offspring (a change in preferences). This could lead to a loss of liquidity in youth, as parents may be less willing to fund early investments in human capital. Furthermore, parents often subsidize particular goods and services for their offspring rather than giving cash bequests. This suggests that the parents' preferences do not always align with the offspring's: the parents want to achieve particular outcomes for their children's lives rather than wanting their children to achieve whatever outcomes the children want. Loss of control and reductions in the information available to parents may reduce the parents' incentives for investing in their children (for the same reason a bank is more willing to make a loan when it can monitor the finances of the debtor and can impose restraints on the behavior of the debtor). Young people would then face tougher liquidity constraints when trying to fund investments in human capital. Finally, earlier legal adulthood may change attitudes about when sexual activity is appropriate, leading to earlier sexual activity.

Contributions of this research

The findings of this research help discriminate between these potential mechanisms. For example, if AoM reductions mainly increase access to contraception, we should clear evidence that the laws caused women to invest more in human capital and earn returns on that human capital in the labor market.

Furthermore, this research can aid in predicting outcomes of potential real-world policies. For example, the age of majority in New Zealand is 20, and New Zealanders debate whether to lower the AoM. To the extent that preferences and constraints (e.g., institutions) are similar in Australia and New Zealand, Australia's experience with AoM reductions can inform New Zealand's choice.

The case for exogeneity of AoM reductions

Our identification strategy requires that the timing of AoM reductions was random (specifically, independent of potential outcomes for the untreated conditional on time invariant, state-specific factors and state-invariant time-specific factors) so that sates that did not change their AoM at a given time represent a valid counterfactual for the states that did. We argue that, as in the US (Goldin and Katz 2002), Australia's participation in the Vietnam War (including a draft) led to a nationwide effort to reduce voting ages, which prompted states to consider also reducing ages of majority, and that this effort passed through a federalist political system to produce state-specific timing of age of majority reductions (from age 21 to age 18). We provide further evidence that the timing was determined by bureaucratic factors rather than by pre-treatment preferences for youth rights, suggesting that reduced state ages of majority represent plausibly-exogenous shocks to the cost of participating in adult activities for Australians who were ages 18–20 in the period when states were changing their laws.

Balance

Table 1 gives the dates when each state's AoM of 18 came into force. More liberal states (South Australia and Victoria) are represented at both the early and late end of legal changes. This is somewhat true for more conservative states (Western Australia and Queensland, for instance). States or territories with very different cultural climates have legal changes around the same time (e.g. Northern Territory and Australian Capital Territory). For further discussion of the legal environment by state, see the Appendix.

AoM changes as a federalist response to the Vietnam War

"Votes for troops"

The case for exogeneity of these legal changes is strengthened by their genesis from the war in Vietnam. This is the same argument Goldin and Katz (2002) make for the exogeneity of AoM laws in the US. Like the US, Australia conscripted soldiers to fight in Vietnam in the 1960s and 70s, and like in the US this led to public opposition. As in the US, males under the age of majority were eligible for conscription in Australia but were ineligible to vote due to their age. This was also the first time conscripts were required to participate in a conflict that was not seen as defense of Australia. These conscripts were asked to do something extraordinary despite not having the full rights of Australian adults. The apparent injustice

of this contradiction led to the extension of voting rights to members of the armed forces under age 21 deployed to South Asia starting in 1966 (Commonwealth Electoral Act 1966 and other state-level Acts), and this move was the origin of changes in the general voting age. Although there was a little discussion in newspapers and parliament of lowering the voting age to 18 as early as 1962, a search for "voting age" and related terms in the archives of the Sydney Morning Herald and the Canberra Times shows that votes for military members overseas broke into public discourse in 1965 right when the government was transitioning into sending men to war. The Sydney Morning Herald reported on 1 April 1966 that "the move to reduce the voting age has been given top priority and the Government hopes to introduce a bill amending the Electoral Act immediately after the Easter recess" ("Fast action on votes for troops"). There was even some concern that soldiers would need parental consent to be stationed overseas ("A Time for Concern", Sydney Morning Herald, 3 April 1966), but the government reasoned that most drafted 20-year-olds would reach 21 before being sent overseas ("A.L.P Fails to Bar Overseas Service", Sydney Morning Herald, 13 May 1965).

53% of men turning 20 in the first half of 1965 (the first "birthday ballot") were selected by a randomization mechanism for potential mandatory National Service ("balloted in"), and approximately 25% of 20-year-olds were balloted in in each subsequent draft until 1972 (Ville and Siminski 2011). 63,735 of these National Servicemen served in the Army from 1965 to 1972, around 15,381 (Langford 1997) or 18,654 (Ville and Siminski 2011) served in Vietnam (usually for one year), and 200 were killed and 1,279 wounded. Ville and Siminski (2011) estimate from Australian Censuses that 868,605 men turned 20 during this period. This means that slightly more than 2% of 20-year-old men were conscripted and served in Vietnam. Not all military members in Vietnam were National Servicemen, so the proportion of all 20-year-old men who served in Vietnam is higher than this 2% figure.

Although the numbers of Australians who were stationed or died in Vietnam were low relative to comparable numbers of Americans, around a quarter of men born within an 8-year span either were in the military or experienced a non-trivial risk of conscription. Further, the fact that this was the first time that the government compelled National Servicemen to participate in an overseas conflict[2] and the fact that opposition parties campaigned on opposition to the draft (and particularly to compelling drafted men to serve overseas), the war could not have been a minor public issue. We conducted a search of Australian newspapers throughout the 1960s and 1970s for results related to synonyms for "conscription", "voting age", and "age of majority". Articles came mostly from The Canberra Times, The Australian Women's Weekly, and the Sydney Morning Herald but included some smaller publications. Debates about lowering the voting age and the age of majority often

[2] The only exception was a minor case from WWII where conscripts were required to serve in the South-West Pacific in Dutch territories, but this was still viewed as defense of Australian territory because Japanese forces there were close to Australian territory.

mentioned military service but never mentioned a desire by young women to obtain contraception or consent to medical treatment (although the right to consent to marriage was discussed). Thus, perceptions of an unjust conflict between conscription at age 20 and voting at age 21 likely led to lowering the voting age and age of majority.

"Constructive federalism"

The Australian age of majority changes started as a nationwide effort to lower the voting age (although only Tasmania used the same Act for both purposes). On 11 March 1966, the Attorney General of NSW requested that the NSW Law Reform Commission consider the question of lowering both the voting age and age of majority. At a conference of state Premiers and a conference of Attorneys General in 1968, the Attorneys General of the various states and of the Commonwealth agreed that voting age laws would need to be uniform throughout the nation. A recommendation for a lower voting age from the NSW Law Reform Commission was then considered by the Standing Committee of Commonwealth and State Attorneys-General, and it was agreed that *all states would adopt the recommendations of the report if there was overall agreement to the terms of the report*, which the Attorney General of Australia called "constructive federalism" ("Age for voting, marrying to drop to 18").

While the general trend toward reducing the AoM came from national-level motivations, the differential timing of the state laws might be due to bureaucratic issues. For instance, NSW passed a law lowering the voting age to 18 in 1970, but the law did not come into force until 1973—*after* the Commonwealth lowered the voting age for Federal elections. The reason for the delay is that NSW—like other states—used the federal elections rolls for state elections and did not want to bear the cost of maintaining separate rolls ("Votes at 18: No Move before next Election" 1972). After New South Wales passed the lower voting age in 1970, the Canberra Times reported that the Attorney General of Tasmania, Max Bingham, predicted that a voting age of 18 would be "Australia-wide policy within 18 months" ("NSW in Favour of Voting Age Cut" 1970). Although this prediction was wrong, it was not due to poor support among state governments. All state premiers expressed support for the policy. See the Appendix for a list of voting age laws by state and when they commenced.

Changes to other youth rights

Although our focus is on age of majority laws, mature minor doctrines, medical consent laws, and laws and court decisions regarding abortion are relevant to our analysis because they may moderate or confound effects of AoM reductions.[3]

[3] Readers familiar with the "early legal access" or "power of the pill" literature from the US may be concerned about Comstock-style anti-obscenity laws. We have found no evidence of anti-obscenity laws in Australia that would have made sales of oral contraceptives illegal during the period studied. Multiple states had bans on advertisement of contraceptives (Siedlecky and Wyndham 1990), which

Medical consent by minors

A "mature minor doctrine" (MM) is a legal rule that allows minors to consent to receiving medical care when the minors understand the choice. A state could also have a medical consent law (MC) that sets the minimum age for consent to medical care below the AoM. Changes to MM or MC rules may confound or moderate effects of AoM reductions. Fortunately for our research design, MM and MC were usually not in force in Australia until much later than the age of majority changes. Two states (New South Wales and South Australia) have separate statutory minimum ages for medical consent. SA's was passed over a decade after the age of majority change, and NSW's was built into the age of majority reduction. Although extending medical consent to younger people was not an unintended side effect of NSW's law, it was not part of the original intent of the reform process. The law was a result of a years-long effort to identify what rights an adult should have and when someone should be considered an adult. As in other states, the parliament of NSW asked its Law Reform Commission to report on the rights and obligations associated with adulthood and whether those should be extended to any minors (New South Wales Law Reform Commission 1969).

Abortion policy

Another potential confounder or moderator is abortion policy. Many of the likely mechanisms and outcomes of AoM reductions are shared with abortion policies (Ananat and Hungerman 2012; Bailey et al. 2011; Bailey, Guldi, and Hershbein 2013; Bailey, Hershbein, and Miller 2012; Guldi 2008; Myers 2017). Furthermore, AoM reductions will have different effects when abortion is legal than when it is not. For example, a lower AoM allows women below age 21 to consent to medical treatments—including abortion.

However, changes in abortion policy are likely a much smaller issue than in similar research on the US. Around 1970, a few US states (sometimes called "repeal states") legalized abortion for most reasons. *Roe v Wade* then legalized abortion for most purposes nationwide in 1973. Although there were some court decisions about abortion in Australia over this time, there was nothing comparable to the broad liberalization in the US. At most, courts clarified that abortion was legal to protect the life (sometimes physical "health") of the pregnant person (comparable to what are sometimes called "reform states" in the US). See the appendix for further details on abortion laws, regulations, and practices.

Empirical strategy

We get difference-in-difference estimates of effects of living under an AoM of 21 as opposed to an AoM of 18 with OLS with fixed effects for state and birth cohort:

---

probably raised the costs of obtaining them, but Bailey (2010), Myers (2017b), and others treat advertising bans in the US as not preventing access.

$$W_i = \delta \times AoM18at18_i + \beta_b + \gamma_s + X + \eta_i \quad (1)$$

where

- i indexes individuals
- b indexes year of birth (in 5-year groups)
- s indexes state
- W is the wage or other outcome
- $AoM18at18_i$ is 1 if person i was subject to the new policy and 0 otherwise
- β_c is a set of birth cohort fixed effects
- γ_s is a set of state fixed effects
- X includes indicators for being Catholic, being non-Catholic Christian, having no religion, and being fluent in English

The outcomes are four measures of earnings or wages, two measures of labor force or employment activity, and two measures of schooling. We estimate separate effects on earnings and labor force participation for various age groups. We estimate a single effect for educational attainment at ages 35–44 (the ages at which we observe every cohort) because age patterns are less important for that outcome. When estimating effects at particular ages, we estimate the model separately for each age, following Cragun (2023), who demonstrates that imposing that fixed effects are shared across ages induces substantial bias in a similar application.

Figure 2 illustrates the treatment rate by state and cohort in the Census data. In the earliest cohort shown, everyone turned 18 before their state lowered the AoM. Thus, they were all untreated. Everyone is treated in the latest cohort shown. There are three pairs of consecutive cohorts over which the treatment rate changes in some state.

Data: Census of Population and Housing

We use repeated cross-sections from the 1986, 1991, 1996, 2001, and 2006 Australian Censuses of Population and Housing and limit the sample to people born no earlier than 1946 and no later than 1966. Deaths and movement into and out of Australia mean that the data are not exactly representative of the set of people who lived in Australia around ages 18–20.

A weakness of the Census data is that we observe ages only in five-year bins rather than knowing exact birth dates. Thus, birth cohorts are also in five-year bins: August 1946 – August 1951, August 1951 – August 1956, August 1956 – August 1961, and August 1961 – August 1966.[4] Table 2 shows when each cohort would have turned 18 and 21.

[4] Census day was between 12 August and 15 August during these years, and ages are reported rounded down to the nearest year by convention. The oldest possible person aged 35–39 on Census day in 1991 (13 August) was born on 14 August 1951, while the youngest possible was born on 13 August 1956.

As a result of the pooled birth years, our primary treatment variable $AoM18at18$ is a noisy measure of whether the observation's state of residence lowered its age of majority before her 18th birthday.[5] $AoM18at18 = 1$ if everyone in her state and cohort would have turned 18 after the state lowered the age of majority and 0 if everyone would have turned 18 before the state lowered the age of majority. However, most of the cohort×state groups do not fit into one of these categories because the cohorts span five years of birth dates. In these cases, we use the probability of being treated with a lower age of majority before turning 18 assuming equal probabilities of being born on each possible birth date.[6]

Figure 2 illustrates the patterns of treatment by state and birth cohort. All of the 1946–51 cohort turned 18 before any state lowered its AoM, and all of the 1961–66 cohort was treated with an AoM of 18 before they turned 18.

Table 3 gives the samples used to estimate separate effects by age. Comparing the table to Figure 2 shows the treatment variation available to estimate effects at each age.[7] For example, observing ages 25–29 for only the latest two cohorts means that effects on earnings at that age are identified almost exclusively with treatment variation in Victoria.

In 1996, 2001, and 2006, we use the usual state of residence five years before census night to determine $AoM18at18$. In 1986 and 1991, we observe only state of current residence. In neither case do we observe state of residence at age 18. We should expect that this measurement error will attenuate estimates of effects. Random migration between states would dissipate the observed effect of early legal access as people who were treated move into other states where they would not have been treated and people who were not treated move into states where they would have been treated had they been there at age 18. If AoM changes induced non-random migration, that could bias estimates in either direction. For example, if a lower AoM creates an environment that encourages immigration by high earners, that would induce upward bias in estimates of effects on earnings.

Respondents reported their usual weekly gross income (Australian Bureau of Statistics 1999; Castles 1986, 1994; Dennis Trewin 2001). The questionnaires also provide an

[5] Alternative specifications with age 19 or 20 yield similar results.

[6] More formally, we define the unobserved treatment variable $\widehat{AoM18at18}$ for an individual as 1 if her state of residence lowered its age of majority before her 18th birthday and 0 otherwise. We calculate the number of days on which the person could have been born given her age group on Census day. $AoM18at18$ is the fraction of those birth days that would have given the person $\widehat{AoM18at18} = 1$. For example, if a woman's age is 30–34 on Census night in 1991 (August 13), then she could have been born 30 years earlier on 13 August or one day less than 35 years earlier on 14 August or on any date in between. Thus, she turned 18 between late 1974 and early 1979. If she lived in Queensland, which changed its law on 1 March 1975, then she has 1627 days when she could have turned 18 after the legal change and 1826 total potential 18th birthdays. Thus, we would assign a value of 1627/1826 for the $AoM18at18$ variable.

[7] Although the 1956–61 cohort is observed at ages 20–24 in the 1981 Census, the 1981 Census does not allow us to determine state of residence, so we cannot estimate effects below age 25.

annualized version of the weekly categories (e.g. an option on the survey might be "$1,000 – $1,299 per week ($52,000 – $67,599 per year)"), and there is likely variation in whether respondents reported perceived annual salary or earnings in a typical (possibly modal) week. The income data are in bins, and we replace these categories with the midpoint from each bin except as follows. We assign 150% of the lower bound of the bin to anyone in the top bin, and we assign an income of zero to any person whose income is in a bin with negative values.[8] Because the nil income bins in 1986 and 1991 included positive incomes as well, we cannot use the earnings data to differentiate earners from non-earners.[9] We deflate earnings and wages by the all groups CPI with 1986 as the base year. For analyses with the log of earnings, we drop observations with earnings of zero.[10]

We construct an estimate of the person's hourly wage by dividing her usual gross weekly income by the number of hours she worked in the week before the Census.11 However, because respondents will not have worked their usual hours in the previous week, this is a noisy measure. The wage is undefined for people who did not work in the previous week, but this is no different from wages missing for people who did not work in any other data set. However, the wages for people who worked for a small but positive number of hours in the previous week could be artificially inflated if working few hours in a given week occurs because of an unusually low random draw. Similarly, for respondents who earn no income in a typical week but worked last week, this measure implies a wage of $0/hour, which is unlikely. Thus, we use only the set of respondents with both positive earnings and positive hours worked for our analysis of effects on hourly wages and instead emphasize results for typical weekly earnings where possible.

---

[8] Starting in 1996, an option for negative income was added to the questionnaire. The 1991 wording also allowed for negative incomes, as the smallest income category was "Less than $58 per week". The 1986 questionnaire had no response category that could allow negative incomes. It is hard to say what negative income meant to respondents. The form told respondents to not deduct "tax, superannuation, health insurance," but respondents were instructed to report business or farm income (and, starting in 1996, rent income) less expenses, and the majority of those reporting negative income owned their own business. Because this option was not present in all years, there is some concern that the presence of this option in some years may have changed how respondents thought about what "gross income" means. For instance, business owners may not have subtracted expenses before 1996.

[9] Suppose, for instance, that we wanted to estimate a tobit model of earnings. We would not observe the zero values for the most important Census years (the ones closest to the time of the treatment in question). Hours worked last week, on the other hand, always has zero values reported.

[10] We also check these estimates against ones where we assign 0 to the log of earnings when earnings are zero and the results are similar. We further test if using only women with positive earnings matters in the non-log cases and find that it does not.

[11] Approximately 2% of respondents declined to report hours worked despite reporting being employed.

For all years except 1991, we combine the Australian Capital Territory and the Northern Territory because the 1986 Census reports combined values for these territories. Conveniently for our analysis, their legal changes were coincident. Inconveniently, ACT is a mostly-urban area (mostly consisting of the city of Canberra) close to the major urban centers of Australia and contained within the borders of NSW, while NT is a large, mostly-rural area far from urban centers. Although we could identify residence in ACT and NT separately in 2001 and 2006, we leave them combined both for continuity with previous years and because each age group cell in the NT sample would have fewer than 30 women. In 1991, the Census combined ACT with Tasmania, and NT was combined with remote areas of SA and WA, and those groups do not share timing of legal changes, so we omit NT, ACT, Tasmania, and those remote areas in 1991. These areas represent a small fraction of the sample.

Table 4 gives descriptive statistics. As expected (see Table 3), there are more observations in the middle of the age range. Women are only slightly less likely than men to have a bachelor's degree, while men are far more likely to have some post-secondary certification. Women are more likely to be married, but the gap shrinks with age. Men earn and work far more than women at any age.

Because we are thinking of schooling as a means of gaining valuable human capital that will increase earnings, Figure 2 shows estimates of how much more people with various degrees earn at each age than people without the degree. Later cohorts are more likely to have degrees and also earn more due to technological innovation, so we first calculate the differences in mean earnings by age separately for each cohort and then average across cohorts. The estimates almost certainly overestimate the causal impact of schooling for other reasons. However, the estimates may help readers make sense of sizes of effects of AoM reductions on degree completion and earnings.

Results

Results are in Tables 5 through 8. Table 5 shows effects on marital status. Table 6 gives estimates for educational attainment. Tables 7 and 8 contain estimates of effects on work and earnings by single age group for women and men (respectively).

Table 5 shows that living under a lower AoM is associated with earlier marriage—especially for women. Negative estimates at ages 25–29 and positive estimates at 30–34 are consistent with people who would have married in their early 30s instead marrying in their 20s (or earlier). However, the estimates cannot rule out that living under a lower AoM also caused some women to delay marriage until after their early 30s.

Table 6 demonstrates large positive effects of a lower AoM on achievement of post-secondary certifications for women and bachelor's degrees for men. There is no evidence for an increase in total post-secondary certifications for men. The "any post-secondary certification" outcome includes bachelor's degrees, so it is possible that the AoM reductions

caused men to switch from less costly certifications to bachelor's degree programs. There is weak evidence that AoM reductions made women more likely to get bachelor's degrees.

Table 7 gives some evidence that a lower AoM increased participation in the labor force by the youngest women in our data (column 7). However, they also appear to earn less on average (column 1). If the only effect was that women with lower earning potential joined the labor force, we would expect to see larger negative effects when omitting $0 earnings (columns 2 and 3), but we do not (although we also cannot rule out a larger negative effect in column 2 than in column 1). However, the estimates are consistent with increased employment among women with lower earning potential and decreased employment among women with higher earning potential. For example, women with lower potential wages may marry earlier and then work to support a husband who is in school, while women with greater potential wages might be induced to work less when young to invest in human capital.

Table 7 also shows greater earnings for women in middle age, consistent with positive returns to investments in human capital.

Finally, Table 7 shows that women who lived under a lower AoM retired earlier (final row of columns 4 through 7). A possible explanation is that earlier marriage meant that these women were younger when their first grandchildren were born. Like their own children, grandchildren increase the opportunity cost of work.

Table 8 shows a clear reduction in work time and earnings for young men. This is consistent with their increased attainment of college degrees. There is weak evidence that these early investments in human capital paid off in middle age.

We can compare the estimates in Tables 7 and 8 to how we should expect the increases in educational attainment to impact earnings. Suppose that a lower AoM caused 2% of men to switch from earning no certification to earning bachelor's degrees (as in Table 6) and that all such men completed their degrees before age 25. Suppose further that the estimates in Figure 2 are the actual causal schooling wage premiums. If the increase in bachelor's degrees were the only mechanism of effects of the AoM reduction, we would expect to see increases in men's average earnings of $3.48 at ages 25–29 that rises to an increase of $7.77 at age 45–49. However, the increases in earnings in Table 8 are smaller than that at young ages and greater at older ages, which suggests that the AoM reduction caused men to invest in human capital through mechanisms other than bachelor's degrees. The delayed payout is also consistent with increased college completion for men above age 25.

The same sort of analysis for women yields similar results. Suppose that a lower AoM caused 3% of women to switch from earning no certification to earning a certification other than a bachelor's degrees (as in Table 6) and that all such women completed their degrees before age 25. If the increase in certifications were the only effect of the AoM reduction, mean earnings for women would increase by at most $3 per week at any age. This effect is tiny compared to the earnings increases in Table 7. Thus, the increases in earnings for women are likely driven by mechanisms other than formal schooling. For example, lower-

cost access to contraception should lower the risk that pregnancy or birth will occur at times that are inconvenient for a woman's career.

Discussion

This research estimates effects of living under an age of majority of 18 rather than 21. The policies appear to have increased educational attainment, early marriage, and earnings in middle age for both women and men, but there are nuanced differences by gender, and there are hints of evidence for heterogenous effects of different signs on different groups of women.

The laws seem to have induced both women and men to avoid marrying in their early 30s and to instead marry at both younger and older ages. However, the movement toward earlier marriage dominates for women but not for men.

Women and men started with nearly-identical rates of attainment of bachelor degrees and then the AoM reductions led to an increase in degrees for men but not for women. On the other hand, women started with a far lower rate of attainment of other post-secondary certifications than men, and then the AoM reductions led to more women getting these certifications.

The results contain some paradoxes. Earlier marriage and earlier retirement by women are consistent with increased specialization in home production by women, but the large increases in earnings by women in middle age suggest an increased focus on careers by some women. The increased labor force participation by women in their late 20s combined with lower average earnings is also consistent with this heterogenous effects interpretation; women with higher potential earnings likely worked less in their late 20s while investing in their skills. A paradox that is harder to explain is that the laws increased college completion by men but did not clearly increase their earnings until very late in their careers.

Some of these effects are in line with similar research from the US but others are not. Goldin and Katz (2002) present evidence that similar legal changes in the US made women less likely to be married by age 23, but their sample included only women who had completed college. (M. Bailey 2006; M. J. Bailey, Hershbein, and Miller 2012) and substantially increased women's earnings and accumulation of human capital (M. J. Bailey, Hershbein, and Miller 2012), but Cragun (2023) finds no evidence for these effects when relaxing a constraint on the regression model. Somewhat surprisingly, we find evidence for those effects in Australia when using the same methodology as Cragun (2023).

Should we expect similar effects from AoM reductions today? The results highlight the potential for youth rights to have different effects on different people in different cultural contexts. Among women who already have greater career opportunities, the policy may enhance those opportunities, but where women have fewer career opportunities, the policy may instead increase sex-based specialization in home production and market labor.

References

"A Time for Concern", Sydney Morning Herald, 3 April 1966

"Age for voting, marrying to drop to 18", Canberra Times, 22 April 1969, page 1

"A.L.P Fails to Bar Overseas Service", Sydney Morning Herald, 13 May 1965

Ananat, E. O., & Hungerman, D. M. (2012). The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics. Review of Economics and Statistics, 94(1), 37–51.

Australian Bureau of Statistics. 1999. Census Dictionary, 1996. Commonwealth of Australia. Catalogue.

Bailey, Martha. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." The Quarterly journal of Economics 121(1).

———. 2010. "'Momma's Got the Pill': How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing." American Economic Review 10: 98–129.

Bailey, M., Guldi, M., Davido, A., & Buzuvis, E. (2011). Early Legal Access: Laws and Policies Governing Contraceptive Access, 1960–1980.

Bailey, M. J., Guldi, M., & Hershbein, B. J. (2013). Further Evidence on the Internal Validity of the Early Legal Access Research Design. Journal of Policy Analysis and Management: [The Journal of the Association for Public Policy Analysis and Management], 32(4), 899–904.

Bailey, Martha J, Brad Hershbein, and Amalia R Miller. 2012. "The Opt-In Revolution? Contraception and the Gender Gap in Wages." American Economic Journal: Applied Economics 4(3): 225–54.

Castles, Ian. 1986. The 1986 Census Dictionary. C.J. Thompson, Commonwealth Government Printer, Canberra. Catalogue.

———. 1994. 1991 Census of Population and Housing Technical Documentation. Australian Bureau of Statistics. Catalogue.

Cragun, Randy. 2023. "Effects of Confidential Access to Oral Contraception in Late Adolescence on Work and Earnings." Journal of Labor Economics.

Dennis Trewin. 2001. 2001 Census Dictionary. Australian Bureau of Statistics, Commonwealth of Australia. Catalogue.

"Fast action on votes for troops", Sydney Morning Herald, 1 April 1966

Goldin, Claudia, and Lawrence F. Katz. 2002. "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions." Journal of Political Economy 110(4): 730–70.

Melanie Guldi. (2008). Fertility Effects of Abortion and Birth Control Pill Access for Minors. Demography, 45(4), 817–827.

Langford, Sue. 1997. "The National Service Scheme, 1964-1972." In A Nation at War: Australian Politics, Society and Diplomacy during the Vietnam War 1965–1975, ed. Peter Geoffrey Edwards. St Leonards, N.S.W.: Allen & Unwin in association with the Australian War Memorial, 355–80.

Myers, Caitlin Knowles. 2017. “The Power of Abortion Policy: Reexamining the Effects of Young Women’s Access to Reproductive Control.” Journal of Political Economy 125(6): 2178–2224.

New South Wales Law Reform Commission. 1969. Report of the Law Reform Commission on Infancy in Relation to Contracts and Property.

“NSW in favour of voting age cut” (May 1970). In: Canberra Times (ACT: 1926 - 1995), p. 3.

Siedlecky, S., & Wyndham, D. (1990). Populate and Perish: Australian women’s fight for birth control. Allen & Unwin Australia Pty Ltd.

Ville, Simon, and Peter Siminski. 2011. “A Fair and Equitable Method of Recruitment? Conscription by Ballot into the Australian Army during the Vietnam War.” Australian Economic History Review 51(3): 277–96.

“Votes at 18: No move before next election” 1972. Sydney Morning Herald, 25 February 1972.

Table 1: Date each state lowered the age of majority to 18

State or territory	AoM of 18 Commenced
South Australia	15 Apr 1971
New South Wales	1 Jul 1971*
Western Australia	1 Nov 1972
Tasmania	1 Aug 1973
Australian Capital Territory	1 Nov 1974
Northern Territory	1 Nov 1974
Queensland	1 Mar 1975
Victoria	1 Feb 1978

**NSW also allowed people aged 14 and over to give medical consent starting on 1 July 1971 in Minors (Property and Contracts) Act 1970*

Table 2: Our sample of birth cohorts from the Australian Censuses

Born		Turn 18		Turn 21	
Earliest	Latest	Earliest	Latest	Earliest	Latest
August 1946	August 1951	1964	1969	1967	1972
August 1951	August 1956	1969	1974	1972	1977
August 1956	August 1961	1974	1979	1977	1982
August 1961	August 1966	1979	1984	1982	1987

Table 3: Relationship between birth year, age, and year of observation

Age	1986	1991	1996	2001	2006
25–29	Born 1956–61	1961–66			
30–34	1951–56	1956–61	1961–66		
35–39	1946–51	1951–56	1956–61	1961–66	
40–44		1946–51	1951–56	1956–61	1961–66
45–49			1946–51	1951–56	1956–61
50–54				1946–51	1951–56

Table 4: Descriptive statistics

Sex	Age	People	AoM 18 before age 18	Bachelor degree	Any certification	Married now	Married ever	Weekly income	In the labor force	Hours worked per week
Male	25-29	8,860	0.893	0.106	0.450	0.467	0.512	346	0.949	33.3
	30-34	12,937	0.709	0.132	0.487	0.620	0.706	405	0.945	34.4
	35-39	16,698	0.567	0.142	0.498	0.678	0.796	448	0.934	35.0
	40-44	16,403	0.562	0.161	0.527	0.681	0.831	482	0.913	35.0
	45-49	11,965	0.378	0.163	0.526	0.684	0.860	495	0.896	34.3
	50-54	7,411	0.152	0.167	0.521	0.697	0.891	506	0.862	33.1
Female	25-29	8,658	0.893	0.096	0.321	0.576	0.666	217	0.681	20.0
	30-34	12,864	0.713	0.123	0.333	0.680	0.807	210	0.642	17.0
	35-39	16,826	0.576	0.138	0.334	0.697	0.863	231	0.690	18.4
	40-44	16,792	0.572	0.161	0.364	0.676	0.883	274	0.773	21.9
	45-49	12,370	0.385	0.167	0.375	0.673	0.901	295	0.777	23.4
	50-54	7,677	0.151	0.168	0.366	0.672	0.927	297	0.737	22.3

People column gives counts of cases. Other columns give means.
Data: 1986, 1991, 1996, 2001, and 2006 Censuses of Population and Housing

Table 5: Effect of AoM reduction on marital status

	Married now	Married ever	Married now	Married ever
Age 25-29	0.0445**	0.0448***	0.00705	0.0097*
	(0.016)	(0.006)	(0.013)	(0.004)
30-34	-0.0224*	-0.00114	-0.0655***	-0.0543**
	(0.009)	(0.014)	(0.017)	(0.017)
35-39	0.0127	0.0122	-0.0204*	-0.0254
	(0.009)	(0.008)	(0.008)	(0.018)
40-44	0.0137	-0.00701	-0.0086	-0.00819
	(0.013)	(0.011)	(0.012)	(0.012)
45-49	0.0167	0.0183*	0.00652	-0.0143
	(0.019)	(0.009)	(0.031)	(0.015)
50-54	-0.0146	-0.0335**	-0.011	-0.0293
	(0.015)	(0.012)	(0.023)	(0.024)
Sex	Female	Female	Male	Male

** p≤0.10; ** p≤0.05; ** p≤0.01*

All regressions include controls for religion, English proficiency, and whether parents were born in Australia.

Table 6: Effect of AoM reduction on education

	Any post-secondary certification	Bachelor's degree	Any post-secondary certification	Bachelor's degree
5-year age groups				
25–29	0.102**	0.0199*	-0.00943	0.0272**
	(0.035)	(0.008)	(0.008)	(0.011)
30–34	0.0195**	0.0178	-0.0156	0.0179
	(0.006)	(0.022)	(0.030)	(0.018)
35–39	0.0357*	-0.00697	0.0195	0.0039
	(0.016)	(0.008)	(0.021)	(0.010)
40–44	0.0141	0.00442	0.0174	0.0479***
	(0.009)	(0.010)	(0.015)	(0.008)
45–49	0.0588**	0.00666	-0.00257	-0.00114
	(0.023)	(0.025)	(0.010)	(0.011)
50–54	0.0228	0.0364**	0.00738	0.0115
	(0.023)	(0.011)	(0.008)	(0.012)
Pooled ages				
25–54	0.0351***	0.0109	0.000584	0.0193***
	(0.006)	(0.007)	(0.007)	(0.004)
30–49	0.0338***	0.00674	0.00777	0.021**
	(0.003)	(0.009)	(0.011)	(0.006)
35–44	0.0268**	-0.00158	0.0153	0.0236**
	(0.008)	(0.006)	(0.012)	(0.008)
Sex	Female	Female	Male	Male

** p≤0.10; ** p≤0.05; ** p≤0.01*

All regressions include controls for religion, English proficiency, and whether parents were born in Australia.

Table 7: Effect of AoM reduction on earnings and work by women

	Weekly income	Weekly income (no $0s)	Log of weekly income	Hours worked per week	Hours (no 0s)	Log of hours	In the labor force
Age 25–29	-7.94**	-5.90	0.0211	0.669	0.683	0.0174	0.052**
	(2.984)	(3.60)	(0.038)	(1.263)	(0.387)	(0.023)	(0.019)
30–34	-0.946	1.32	0.0291	1.48	0.826	0.0365	0.00767
	(13.363)	(12.64)	(0.083)	(1.260)	(0.516)	(0.025)	(0.028)
35–39	15.3	17.30	0.13**	0.0279	-0.249	0.00514	0.019
	(9.857)	(10.62)	(0.050)	(0.794)	(0.302)	(0.017)	(0.011)
40–44	11.90	14.70**	0.0469*	0.0739	-0.108	-0.0103	0.00346
	(6.154)	(5.89)	(0.023)	(0.705)	(0.358)	(0.014)	(0.023)
45–49	25.90*	31.70**	0.11***	1.040	-0.139	-0.0196	0.0128
	(12.34)	(10.40)	(0.029)	(1.438)	(0.780)	(0.031)	(0.028)
50–54	-4.21	-1.79	0.00979	-1.40***	-1.351*	-0.0443*	-0.0328*
	(13.11)	(12.76)	(0.074)	(0.373)	(0.574)	(0.023)	(0.016)

** p≤0.10; ** p≤0.05; ** p≤0.01*

All regressions include controls for religion, English proficiency, and whether parents were born in Australia.

Table 8: Effect of AoM reduction on earnings and work by men

	Weekly income	Weekly income (no $0s)	Log of income	Hours worked per week	Hours (no 0s)	Log of hours	In the labor force
Age 25–29	-26.85**	-26.85**	-0.103***	-1.63***	-0.877**	-0.0324**	0.00225
	(7.97)	(7.97)	(0.013)	(0.421)	(0.246)	(0.010)	(0.013)
30–34	-8.48	-9.46	-0.0388	-0.873	0.290	0.0125	-0.0249*
	(16.97)	(17.65)	(0.045)	(1.719)	(0.555)	(0.024)	(0.012)
35–39	-3.35	-5.78	-0.0344	-2.03	-0.282	-0.00203	-0.0095
	(9.26)	(9.37)	(0.019)	(1.15)	(0.557)	(0.022)	(0.007)
40–44	0.89	1.78	-0.0126	-0.274	0.022	-0.007	0.0068
	(11.06)	(11.52)	(0.039)	(1.008)	(0.270)	(0.011)	(0.019)
45–49	22.82	20.91	0.0593	0.716	-0.151	0.00419	0.0112
	(14.06)	(15.67)	(0.036)	(0.533)	(0.309)	(0.011)	(0.008)
50–54	22.97	28.32*	0.0470	-0.854	-0.262	-0.0268**	0.0228*
	(12.02)	(13.15)	(0.031)	(1.117)	(0.256)	(0.010)	(0.011)

** p≤0.10; ** p≤0.05; ** p≤0.01*

All regressions include controls for religion, English proficiency, and whether parents were born in Australia.

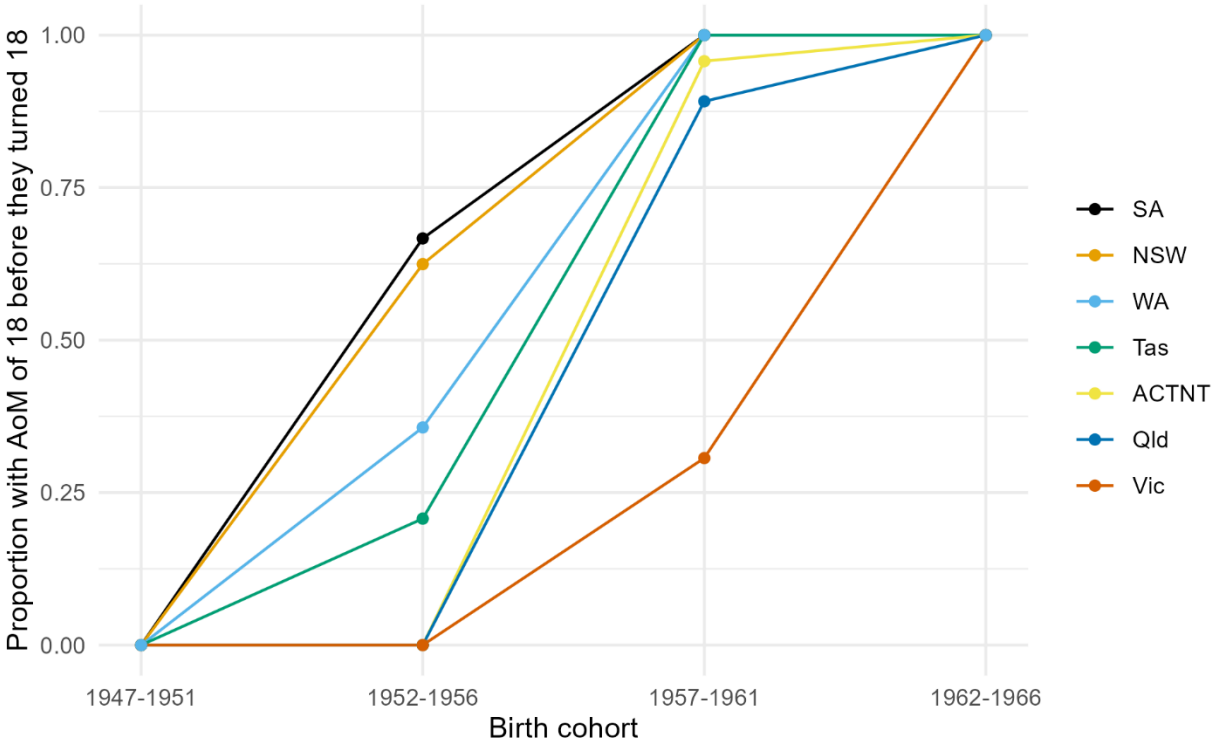

Figure 1: Rate of treatment with an AoM of 18 by state and cohort

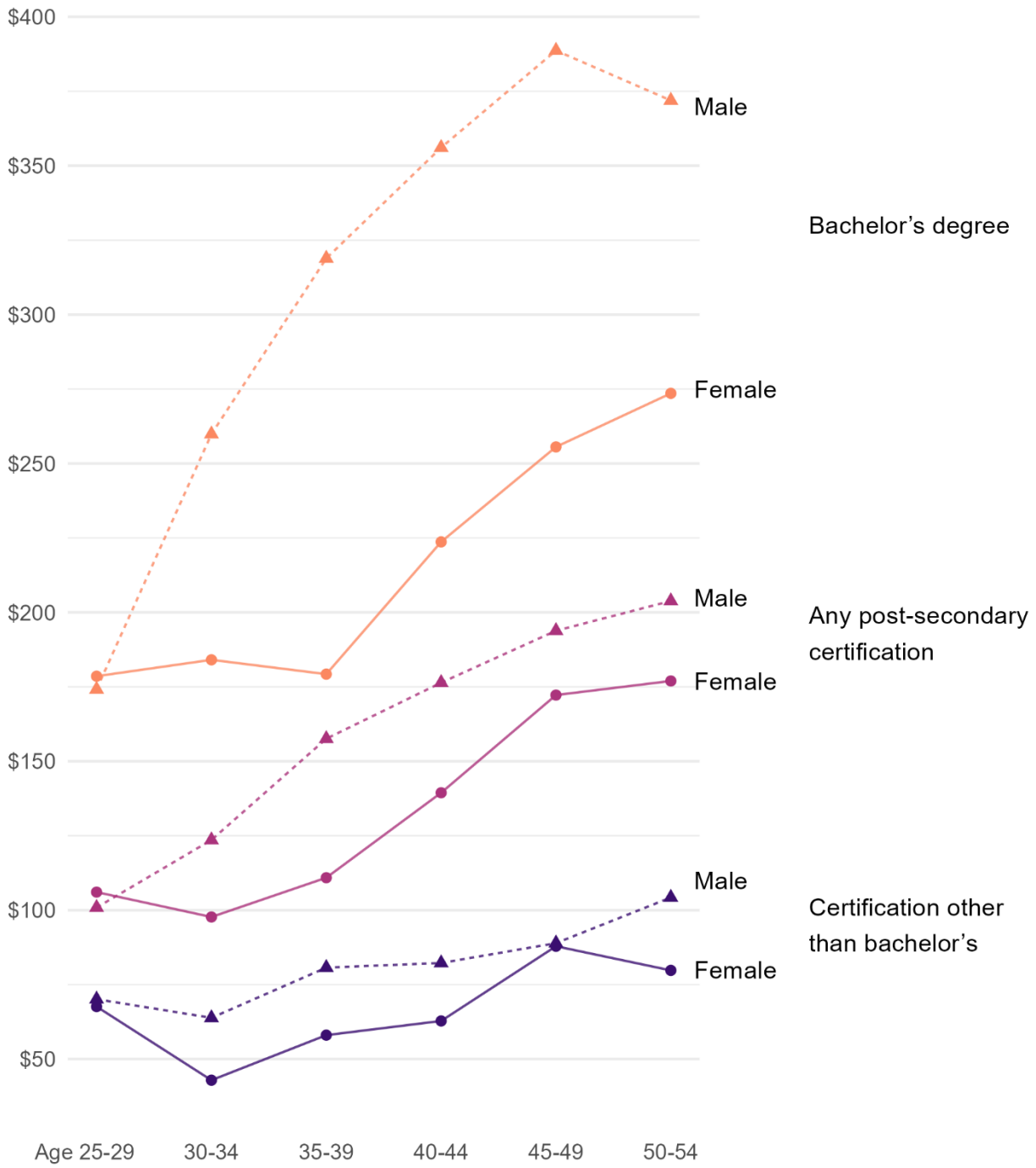

Figure 2: Difference between mean weekly earnings of people with the given degrees and people without the degrees (averages of differences within birth cohorts). Data: 1986, 1991, 1996, 2001, and 2006 Censuses of Population and Housing. Omitting cases with $0 earnings does not substantially alter estimates.