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Abstract

Past research has suggested that Common Law restrictions may have prevented minors from obtaining oral contraceptives (the pill) without parental consent and that, thus, reductions in the age at which a woman became a legal adult could work through access to the pill to increase incomes, educational attainment, and participation in occupations previously dominated by men, but these age of majority changes are often confounded with other relevant legal changes. Because Australian states had similar reasons for lowering their ages of majority around the same time as US states did but did not enact other youth consent or pill access measures around the same time, we use state-specific variation in Australian age of majority laws and estimate effects on schooling attainment and life-cycle incomes, finding that living under an age of majority of 18 rather than 21 decreased women's weekly earnings slightly in their 20s and increased their earnings at later ages and increased their probability of bachelor degree attainment by around 1.5 percentage points (from a baseline of 14%).

Introduction

During the 1960s and 1970s, feminist and youth movements transformed western societies. At this time, every Australian state lowered its age of majority (AoM) from 21 to 18, granting broad new rights to many young adults. We estimate effects of these policy changes on educational attainment and life-cycle incomes for men and for women using data from the Australian Censuses of Population and Housing.

The economic contraceptive policy literature includes multiple articles that treat variation in policies related to legal adulthood as if they represent exogenous variation in the costs of obtaining and using oral contraceptives. 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. They and the subsequent literature on "Early Legal Access" (ELA) to the pill (Bailey 2006) estimate reduced form 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.[1]

The existing literature in this field identifies pill cost changes for young women not only with age of majority changes but with sets of diverse pill access policies including repeal of "Comstock"

[1] Estimated effects include that unmarried women in cohorts that gained the ability to legally make their own medical decisions in late adolescence and early adulthood used the Pill earlier and with greater frequency (Bailey, Hershbein, and Miller 2012; Cragun 2019; Goldin and Katz 2002) had later fertility (Ananat and Hungerman 2012; Bailey 2006; Guldi 2008), higher incomes later in life (Bailey 2013; Bailey, Hershbein, and Miller 2012), and more educational attainment (Bailey, Hershbein, and Miller 2012; Hock 2008); were less likely to experience poverty (Browne and LaLumia 2014); were more likely to participate in the labor force (Bailey 2006) and in careers that had been historically male (Bailey, Hershbein, and Miller 2012; Goldin and Katz 2002; Steingrimsdottir 2016); and had children that were more highly educated (Bailey 2013).

laws that banned the sale of the pill; legislative, judicial, and administrative "mature minor" doctrines that allowed minors to give medical consent if they were mature enough to understand the choice; family planning policies that explicitly granted the right to provide contraceptives to minors (and sometimes mandated provision of such services on request); and medical consent statutes that set legal minimum ages for giving consent for medical treatment that differed from the age of majority. The major weakness of using these reproductive policies as if they are valid instruments for pill access costs is that their exogeneity with respect to contraceptive desires is suspect. Not so for AoM reductions, which were the result of a nationwide push to lower the voting age rather than due to (for instance) court cases over contraception.

Although Cragun (2019) showed that the Australian AoM reductions did indeed increase the rate of starting pill use in early adulthood, the pill is not the only conceivable mechanism for effects of earlier legal adulthood. For instance, being considered an adult at a younger age might increase schooling attainment by lowering the cost of contracting for educational services or loans or might decrease schooling attainment by reducing liquidity available to young people by cutting them off from their parents' resources. These potential violations of the exclusion restriction represent the major danger in using AoM reductions as instruments for pill access costs. Our methodology does not allow us to differentiate these potential mechanisms, but it does differentiate age of majority changes from other ELA policies because, unlike in the US, Australian AoM changes did not coincide with other major contraceptive policy changes. Because of these concerns, we recommend caution in interpreting the results as effects of pill access, but the ability to consent to medical treatments (including the pill) is one likely mechanism for AoM reductions, so we discuss the implications of that mechanism.

Contraception and early human capital accumulation

Early human capital accumulation is important for building lifetime wealth because the time horizon over which that human capital can earn returns is longer than when a person is close to retirement and because delaying earnings growth until later reduces the present value of those earnings (Ben-Porath 1967). Societies have legal and social norms around adulthood that might drive patterns of investment, so we estimate effects of lowering the age of majority from 21 to 18 in Australian states on early human capital accumulation and subsequent earnings.

Early human capital accumulation has historically been particularly costly for young women. Female fertility is constrained to young ages by biology, so women who desire to have children must do so at ages when investments in skills can be particularly profitable. Human infants are notoriously fragile, and women have typically been the primary caregivers for infants, which means that fertility increases the opportunity cost of work and schooling for women. Thus, women give up valuable investments in skills to care for children.

Typical age-earnings profiles for women have shown very slow earnings growth in a pre-first-birth period followed by a period of low and non-growing earnings while children are young and then a period of positive earnings growth once children reach schooling age (Mincer, Polachek). These patterns are consistent with the hypothesis that a sustained period of detachment from the labor force when children are young makes pre-fertility investments in human capital have little value (because the human capital will be used for a short time and then depreciate while not being used).

Bailey (2006) showed that women born before the 1950s in the US typically left the labor force in their 20s and returned later (when we could expect their children to be at schooling age) but that later cohorts increased their labor force participation throughout their 20s and 30s. This increased exposure to the labor force likely increased the incentives for early human capital accumulation, which would show up as faster wage or earnings growth at young ages.[2] Much research has attempted to explain this revolution in women's labor force participation. Explanations include innovations in contraceptive technologies and laws decreasing the cost of timing births[3], cultural shifts (the women's movement), and improvements in household production technologies. Abortion and oral contraceptive (the pill) access, in particular, have received must interest. Cragun (2019) used variation in Australian states' age of majority reductions in the 1970s to show that an age of majority of 18 instead of 21 increased the probability that a young woman would start using the pill for the first time. This paper extends the literature by linking those age of majority reductions to accumulation of human capital and to age-earnings profiles.

If legal autonomy primarily decreases contraceptive costs (as opposed to other mechanisms discussed above) and the returns to schooling are high enough, a lower age of majority should increase early human capital accumulation for women. This implies higher educational attainment, decreased earnings at young ages, increased earnings at older ages, and increased lifetime wealth. However, another possibility is that a lower age of majority will increase early labor force attachment without increasing early human capital accumulation. A household consisting of a male and female partner may decide to have one partner invest heavily in labor earnings while the other specializes in home production, and the partner who specializes in home production is usually female. Because of liquidity constraints, some households will fund the husband's early human capital accumulation with the wife's market earnings. More effective contraception lowers the cost of this path by decreasing uncertainty over the wife's fertility. Figure 1 shows the life-cycle employment patterns of women in the Australian Family Project born during given years. Unlike the patterns shown by Bailey (2006) for the US, women born in the 1950s in Australia drop out of the labor force in their 20s like the women born earlier did. Estimates with the Labour Force Survey show the same pattern. The figure is consistent with little delayed fertility, so we might expect to see smaller increases in human capital accumulation for Australian women than for American women in this time period.

[2] In some cases, however, an early period of complete specialization in human capital accumulation can lead to an S-shaped age-earnings profile, which means that earnings would not grow at all at young ages (Ben-Porath 1967; Mincer 1997). However, this pattern of slow or no growth of earnings followed by accelerating earnings growth is distinct from the N-shaped profiles described by Mincer and Polachek in which earnings grow slowly at first and then fall to zero.

[3] Or, alternately, decreasing the cost of entering sexual relationships by providing low-cost and reliable means of avoiding pregnancy that are controlled by women.

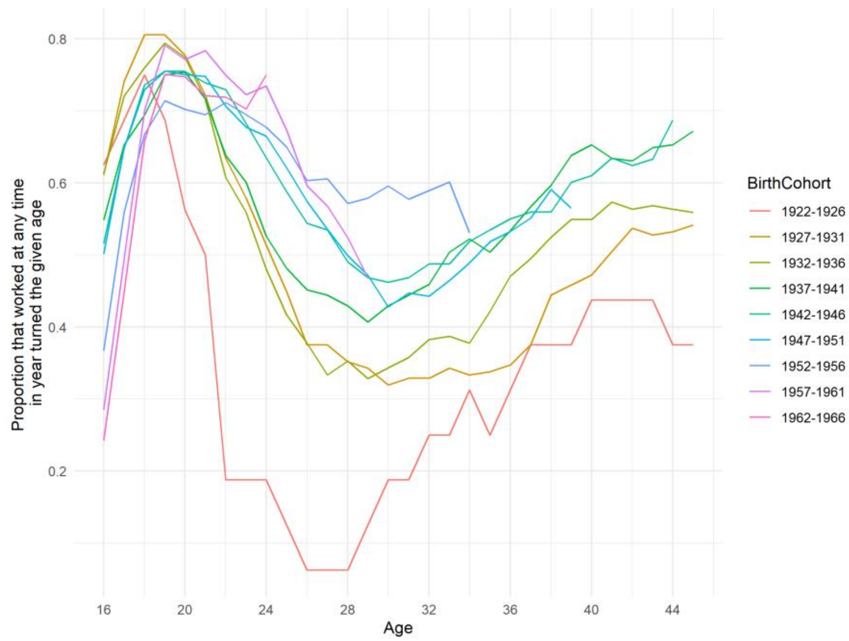

Figure 1: Proportion of women in the Australian Family Project who worked at any time in the year that they turned a given age separated by birth cohort

History of policy changes

Our identification strategy relies crucially on the assumption that the timing of AoM reductions was random so that sates that did not change their AoM at a given time represent a valid counterfactual for the states that did. Cragun (2019) shows that, as in the US, Australia's participation in the Vietnam War (including a draft) led to a nationwide effort to reduce voting ages and 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). Cragun (2019) further gives evidence that the timing was primarily 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 rights of Australian women at ages 18 – 20 (including the costs of obtaining the pill).

Every state and major territory has its own age of majority law (hereafter "AoM") lowering the age of majority from 21 to 18 years old. Table 1 gives the dates when each law came into force ("commenced"). One state (New South Wales) had separate statutory minimum age for medical consent.

Table 1: Dates of age of majority laws by state and territory

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

Methods for estimating effects of AoM on life outcomes

Our method for estimating the effect of age of majority (AoM) on life outcomes follows Bailey, Hershbein, and Miller (2012) with some modifications. We estimate the effect of a policy environment change (for instance, moving from an AoM of 21 to an AoM no higher than 19) on earnings at various ages. If we were interested in the effect at age a, we would limit the sample to only women aged a and estimate the difference-in-differences (DiD) model

$$W_i = \alpha_a + \delta_a Policy_i + \sum_c \beta_c C_{i,c} + \sum_s \gamma_s S_{i,s} + \eta_i \tag{1}$$

where

- i indexes individuals
- c indexes year of birth (in 5-year groups)
- s indexes state
- W is the wage or other outcome
- $Policy_i$ is 1 if person i was subject to the new policy and 0 otherwise
- $C_{i,c}$ is 1 if person i is in birth cohort c and 0 otherwise
- $S_{i,s}$ is 1 if person i lives in state s and 0 otherwise

and $\eta \sim N(0, \sigma)$.[4]

If we wanted to estimate effects at many ages, we could estimate Equation 1 separately for each age or we could pool the data and (more efficiently) estimate one regression with *every* right-hand-side term (other than the error) in Equation 1 interacted with age group dummies. This methodology maintains the DiD structure of the single-age version. The pooled (across age) version of Equation 1 is

$$W_i = \sum_a \delta_a Policy_i D_{i,a} + \sum_a \alpha_a D_{i,a} + \sum_a \sum_c \widetilde{\beta_{a,c}} C_{i,c} D_{i,a} + \sum_a \sum_s \widetilde{\gamma_{a,s}} S_{i,s} D_{i,a} + \widetilde{\eta_i} \tag{2}$$

where $D_{i,a}$ is an indicator equal to 1 if person i is in age group a (in 5-year groups) and 0 otherwise.

Suppose that instead of estimating Equation 2, we estimate

$$W_i = \sum_a \delta_a Policy_i D_{i,a} + \sum_a \alpha_a D_{i,a} + \sum_c \beta_c C_{i,c} + \sum_s \gamma_s S_{i,s} + \eta_i \tag{3}$$

[4] We also allow for heteroskedasticity and error correlation within states.

This is the method employed by Bailey, Hershbein, and Miller (2012). Notice that this does not condition on the full set of dummies that the age-specific DiD model (Eq. 2) does, as it is missing the interaction terms between age and state and age and birth cohort. We can imagine threats to this specification because of that fact. Suppose, for instance, that South Australia had unusually high wages (among states) for women in their 30s. The age-specific DiD models control for such variation by having one dummy for each state for *each* age group, but in Equation 3, there is only one SA dummy, and this variation would not be fully captured by either γ_{SA} or δ_{30-34}. Some of this variation would be captured by the coefficient on the $Policy$. The best available counterfactual to the difference between treated and untreated 30-year-olds in SA is the difference between 30-year-olds in other states who did not change their policies when SA did who would have been treated and untreated had they lived in SA. Equation 3 instead compares the difference between treated and untreated 30-year-olds in SA to the income change for *all* age groups in other states. This imposes the strong parametric assumption that the shape of the age-earnings profiles must be the same for all birth cohorts and states (only the levels are allowed to differ). Yzerbyt, Muller, and Judd (2004) demonstrate the size of bias when a treatment is interacted with a moderator but other controls are not.

We estimate Equation 2, where the policy is a reduction in the state age of majority from 21 to 18 (see the Appendix for estimates of Equation 3). The outcomes are weekly earnings and hourly wages in log and non-log forms, and we test if using only women with positive earnings matters. We do not include zero earnings whenever the dependent variable is the log of earnings. We always exclude infinite apparent weekly earnings (cases where annual wage and salary earnings were positive but the respondent reported working zero weeks in the last year).

Effect of AoM on life-cycle incomes

Australian Censuses of Population and Housing

We use repeated cross-sections from the 1986, 1991, 1996, 2001, and 2006 Australian Censuses of Population and Housing.

We define the treatment variable $\widetilde{AoM}19$ for an individual as 1 if the woman's state of residence lowered its age of majority before her 19th birthday and 0 otherwise.[5] We do not observe $\widetilde{AoM}19$ because we observe ages in five-year bins rather than knowing exact birth dates. We thus calculate a probability of treatment with an AoM no higher than 19 instead of 21 ($AoM19$). We calculate the number of days on which the person could have been born given her age group on Census day. $AoM19$ is the fraction of those days that would have given the person $\widetilde{AoM}19 = 1$ if she had been born on that day.[6] 83% of census year by age group cells have a value of $AoM19$ of 0 or 1, and 88% have either

[5] Alternative specifications with age 18 or 20 yield similar results.

[6] 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 (or about .9) for the $AoM19$ variable. The table shows the dates of the census nights and examples of how we encode the $AoM19$ variable.

fewer than 11% or over 89% of their possible birth dates treated (i.e., there are few cells where treatment status is not nearly universal).

In 1996, 2001, and 2006, we use the usual state of residence five years before census night to determine $AoM19$. In 1986 and 1991, we observe only state of current residence. The time when we observe residency is clearly a long way from the time when some people in the sample were 19-years-old (and when age of majority laws changed). We should expect that this will attenuate estimates of effects of youth consent. While it is possible that a lower AoM led to an environment that was desirable to immigration by high earners (inducing an upward bias in the estimates), it seems likely that 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.

Respondents reported their usual weekly gross income (Australian Bureau of Statistics 1999; Castles 1986, 1994; Dennis Trewin 2001). The questionnaires also provide an 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 the top bin, where we assign 150% of the lower bound of the bin, or any bin that includes negative values. We assign an income of zero to any person whose income is in a bin with negative values.[7] 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.[8] We deflate earnings and wages by the all groups CPI with 1986 as the base year.

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. 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

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		AoM19 treatment probability						
Census night	18th birthday for someone age 30–34	NSW	SA	WA	Tas	ACT/NT	Qld	Vic
1986: Aug 12	Aug 13, 1969 – Aug 12, 1974	$\frac{1320}{1826}$	$\frac{1216}{1826}$	$\frac{650}{1826}$	$\frac{377}{1826}$	0	0	0
1991: Aug 13	Aug 14, 1974 – Aug 13, 1979	1	1	1	1	$\frac{1747}{1826}$	$\frac{1627}{1826}$	$\frac{559}{1826}$
1996: Aug 13	Aug 14, 1979 – Aug 13, 1984	1	1	1	1	1	1	1
2001: Aug 14	Aug 15, 1984 – Aug 14, 1989	1	1	1	1	1	1	1
2006: Aug 15	Aug 16, 1989 – Aug 15, 1994	1	1	1	1	1	1	1

[7] 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.

[8] 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.

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 rely on typical weekly earnings where possible.

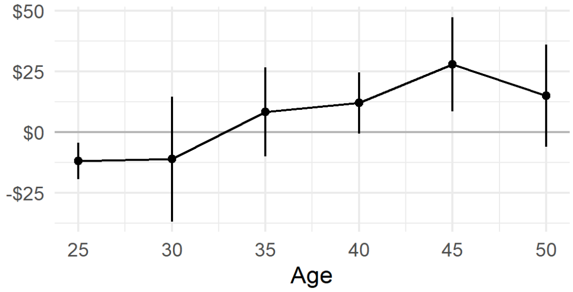
For all years except 1991, we combine the Australian Capital Territory and the Northern Territory because the 1986 Census reports combined values for these territories. Conveniently for our analysis, their legal changes were coincident. Inconveniently, ACT is a mostly-urban area (mostly consisting of 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.

Estimates of age-earnings profile effects for Australia

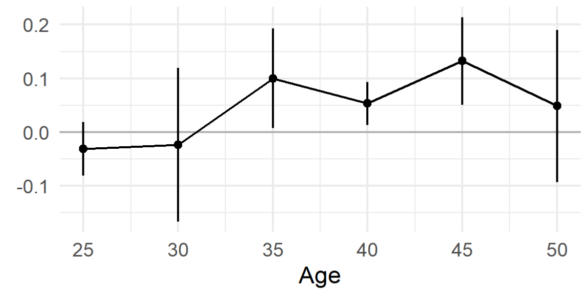
Estimates of Equation 2 for women in both level and log forms are in Figure 2. Like BHM, we see negative coefficients in the 20s and positive coefficients at later ages. The estimates are close to but lower than those from Bailey, Hershbein, and Miller (2012). We should expect some attenuation of the marginal effects because of measurement error in state of residence at age 18. Note that our estimates start at ages 25–29 due to data limitations, whereas theirs start at 20–24. Their estimated effect on the log of earnings at ages 25–29 is positive (approximately 4 to 19 percentage points), whereas our point estimate is slightly negative. We also show estimates for hourly wages. BHM used only women with observed wages for their estimates. We keep in women who reported working but earning no income for comparison but emphasize that their estimated wages of 0 probably reflect measurement error.

A lower age of majority seems to push down earnings for women early in their lives and increase those earnings later in their lives (relative to the wages they would have without access). This is consistent with the intuition that young women living under the lower AoM invest more intensively and extensively in human capital that will support higher-earning careers, and those investments require foregone wages early in life but pay off later (as in Ben-Porath 1967). However, although early human capital accumulation is the likely mechanism for the observed effect, it does not follow that Figure 2 shows effects of access to the pill or increased certainty over fertility because AoM laws apply to broad sets of rights and responsibilities.

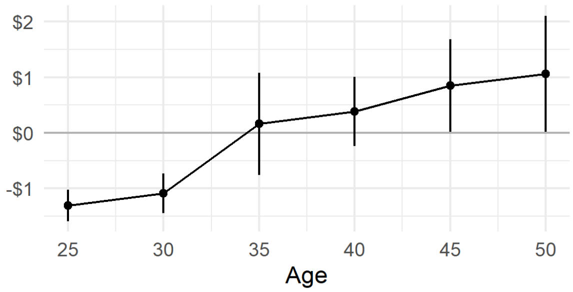
Estimates for men are in Figure 3. Unlike women, men do not see a later-life increase in earnings because of the lower AoM.



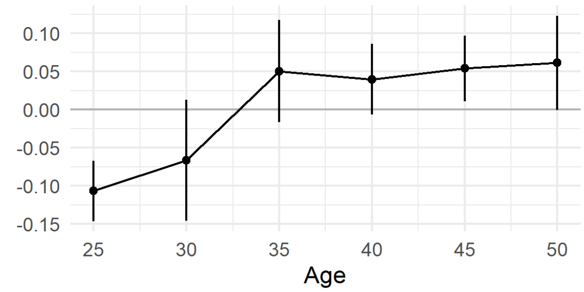
(a) Weekly earnings (1986 AUD)



(b) Log of weakly earnings

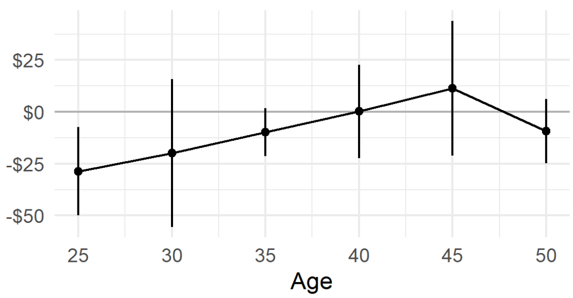


(c) Hourly wages (1986 AUD)

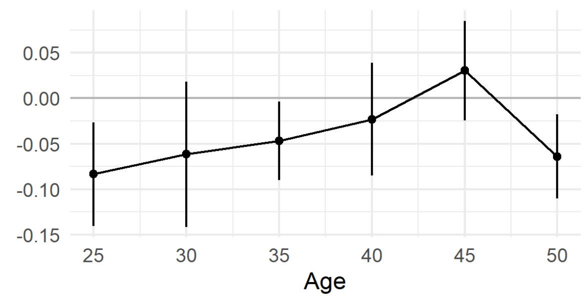


(d) Log of hourly wages

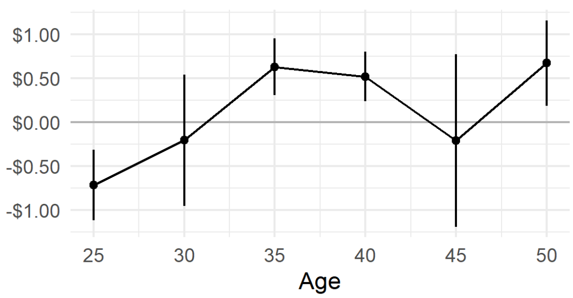
Figure 2: Estimates of age-specific increase in weekly earnings and hourly wages for women due to lower AoM (Equation2). Vertical bars indicate 90% confidence intervals based on standard errors allowing arbitrary error correlation within states.



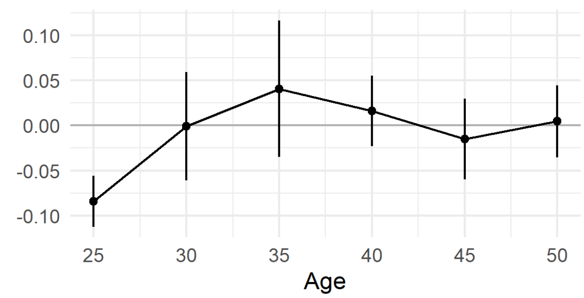
(a) Weekly earnings (1986 AUD)



(b) Log of weakly earnings



(c) Hourly wages (1986 AUD)



(d) Log of hourly wages

Figure 3: Estimates of age-specific increase in weekly earnings and hourly wages for men due to lower AoM (Equation2). Vertical bars indicate 90% confidence intervals based on standard errors allowing arbitrary error correlation within states.

Effect of AoM on educational attainment

Theory suggests that one of the ways pill access or increased youth autonomy could induce higher wages is by decreasing the cost of schooling attainment.

Table 2 reports linear probability model estimates for the probability that a woman received a bachelor degree (or higher) and also for the probability that the woman received any tertiary certification with women over age 25 in the Australian Censuses.[9] We again include state and birth cohort fixed effects, but we do not estimate separate effects of $AoM19$ for each age group because college education (at least at the bachelor level) should be completed for almost everyone in the age groups we observe.[10]

Table 2: Linear probability model estimates of the effect of AoM on educational attainment for Australian women

	Dependent variable:	
	Bachelor or higher	**Any certification**
	(1)	**(2)**
Proportion with the specified degree	0.149	0.354
Marginal effect of AoM18	0.016	0.045
SEs clustered at state	(0.006)	(0.004)
Observations	126,572	126,572
Notes:	*Using probit produces no substantial changes*	
	All regressions include state and cohort fixed effects	

The effect of a lower AoM on tertiary education is large in each specification, with an overall 1.2 percentage point increase in the probability of getting a bachelor degree (from a base of 14%). This is substantially larger than estimates of .78 percentage points for the US by Hock (2008), but we did not include here controls for abortion laws because we are not able to separately identify abortion and contraceptive access. The effect for any certification is larger than the effect for bachelor degrees. This might give us some indication of the size of the fertility delays we should expect to see (because most certificates takes less time than a bachelor degree).

Table 3 shows the estimates for men. The effect on college graduation is slightly larger for men than for women, whereas the effect on receiving and certification is much smaller for men (if there even is an effect) than for women.

[9] We use a linear probability model for ease of exposition and because the dependent and independent variables are all theoretically binary. Using probit estimates produces almost identical marginal effects.

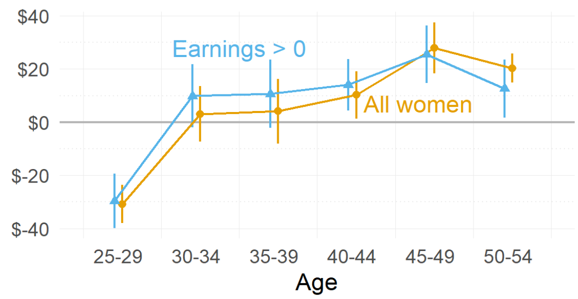
[10] If educational attainment increases for many women after age 25, the estimates may be biased. Hock (2008) shows that women in the US not treated with early legal access to the pill did not exhibit any catching up effect in educational attainment after age 30. Furthermore, we estimated the full interaction specification with age groups on micro data from the 1963–2018 Annual Social and Economic supplement to the Current Population Surveys from the US Census and find that the estimated effect does not depend on age.

Table 3: Linear probability model estimates of the effect of AoM on educational attainment for Australian men

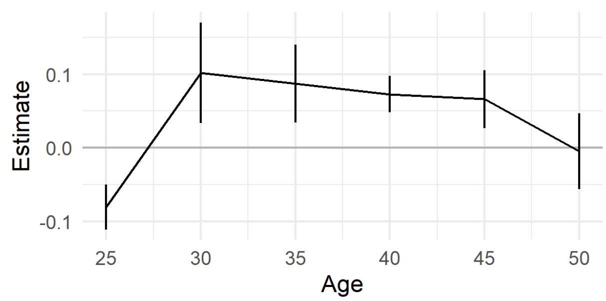
	Dependent variable:	
	Bachelor or higher	**Any certification**
	(1)	**(2)**
Proportion with the specified degree	0.142	0.493
Marginal effect of AoM18	0.018	0.009
SEs clustered at state	(0.003)	(0.009)
Observations	123,306	123,306
Notes:	*Using probit produces no substantial changes*	
	All regressions include state and cohort fixed effects	

Appendix

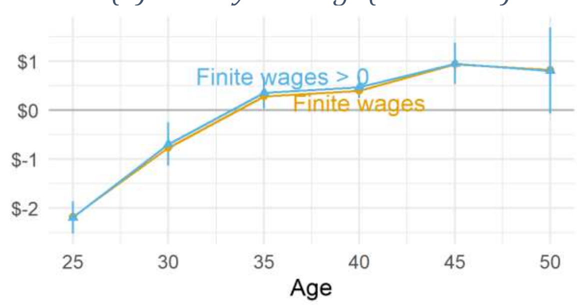
Estimates of Equation 3 for women in both level and log forms are in Figure 4. Like BHM, we see negative coefficients in the 20s and positive coefficients at later ages. The estimates are close to but lower than those from Bailey, Hershbein, and Miller (2012). We should expect some attenuation of the marginal effects because of measurement error in state of residence at age 18. Note that our estimates start at ages 25–29 due to data limitations, whereas theirs start at 20–24. Their estimated effect on the log of earnings at ages 25–29 is positive (approximately 4 to 19 percentage points), whereas ours is negative. We also show estimates for hourly wages. BHM used only women with observed wages for their estimates. We keep in women who reported working but earning no income in some specifications for comparison but emphasize that their estimated wages of 0 probably reflect measurement error.



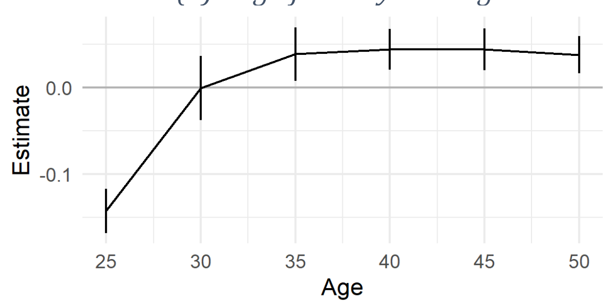
(a) Weekly earnings (1986 AUD)



(b) Log of weakly earnings



(c) Hourly wages (1986 AUD)



(d) Log of hourly wages

Figure 4: Estimates of age-specific increase in weekly earnings and hourly wages due to lower AoM using the BHM specification (Equation 3). Vertical bars indicate 90% confidence intervals based on standard errors allowing arbitrary error correlation within states.

Our estimated effects might be smaller than the BHM estimates based on a broader policy set because some effects of AoM changes offset increased incentives for human capital accumulation due to increased pill access. Loss of liquidity from parental resources is a likely candidate. But it could also be that AoM reductions simply have a smaller impact on pill costs than policies directed specifically at contraception do. Cragun (2019) showed that women in the US who had ELA were around 10 percentage points more likely to start using the pill in a given month at age 20, whereas Australian women who lived under the lower age of majority were only 2 percentage points more likely to start. However, we remind readers that leaving out the interactions of age with state and birth cohort controls introduces bias into the regression estimates.

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