Power of the Spillover: The Effects of Contraception and Abortion Legalization on Male Outcomes

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Abstract:

Did men benefit from the legalization of oral contraception and abortion? While a large literature has documented how the expansion of access to these reproductive technologies in the 1960s and 1970s reduced women's fertility and improved their subsequent socioeconomic outcomes, less is known about how these policies affected the women's sexual partners. In this paper, we explore whether men's socioeconomic outcomes later in life were impacted by their female partner's access to contraception and abortion as teenagers. We leverage state by birth cohort changes in whether men's partners could access contraception and abortion at certain ages to identify the effects this access had on men's marital patterns, education, and earnings. We find that the legalization of abortion and contraception resulted in men marrying at later ages and changed the characteristics of the women they married. Men whose states legalized abortion during their teenage years were 2 p.p. or 8 % more likely to later marry a woman more educated than them and those in states which extended legal confidential access to the Pill to women under the age of 18 had 6% smaller age gaps with their wives. We find no effect of the roll out of the Pill or abortion on men's educational attainment, but find that access to abortion as a teenager increased earnings later in life, with men earning 7% more at ages 30-35.

JEL Codes: J13, J18, N32

Key Words: Contraception, Abortion, Marriage, Gender

Did men benefit from the wide-spread expansion of access to contraception and abortion in the latter half of the twentieth century? Despite the mechanical fact that an avoided birth reduces the lifetime fertility of a both man and a woman, birth control and abortion are often depicted as tools that solely benefit women. In a nationally representative poll of Americans, 52 percent of men surveyed said that affordable birth control would not benefit them personally (Chicago: Perry Undem, 2017). Men's ambivalence towards the benefits of accessible reproductive technology does not mean, however, that such benefits do not exist. While the first-order effects on women undoubtedly would be stronger, one might think that men whose partners avoid pregnancy or births may have different life trajectories than men whose partners have a child.

A large literature has documented the fact that changes in access to reproductive healthcare such as oral contraception and abortion has changed whether and when women have children (Goldin and Katz, 2002; Bailey, 2006; Ananat, Gruber, and Levine, 2007; Guldi, 2008; Bailey, 2010; Myers, 2017), influencing a number of social and economic outcomes for women ranging from timing of marriage (Goldin and Katz, 2002; Myers, 2017), educational attainment and occupational choice (Hock, 2008; Bailey, Hershbein, and Miller, 2012), labor force participation (Bailey, 2006; Bailey, Hershbein, and Miller, 2012), and long-term well-being of their children (Ananat and Hungerman, 2012). However, less is known about how these policy changes have impacted men's economic outcomes.

Only a limited number of papers have explored the potential spill-overs of technologies that allow women to control their fertility onto their partner's socio-economic outcomes. Hock (2008) explores the effects of late adolescent parental consent laws for contraception on both women's and men's educational attainment using a differences-in-differences strategy and lagging the law effects by two-years for male specifications to mirror the gender age gap in partnerships. He finds that male college degree attainment is significantly higher in the presence of these laws, with these laws increasing male degree attainment by about 0.7 percentage points. This paper does not, however, explore the effects of abortion access on male educational attainment. One paper, Steingrimsdottir (2016) looks at the effects of the Pill and abortion access on male and female college students' career plans and shows that while the Pill and abortion shifted women towards higher paying and more male-dominated occupations, there were primarily null effects on men, with only black males being positively affected in terms of occupational prestige or earnings.

A related literature has explored how teenage fatherhood impacts socio-economic outcomes for young men. Past research uses panel data from the National Longitudinal Study of Adolescent Health (Add Health) to show that young men with partners who experienced a birth have lower educational attainment relative to those whose partners are pregnant and do not carry to term due to miscarriage (Fletcher and Wolfe, 2012) or abortion (Everett, Myers, Sanders, and Turok, 2019). Fletcher and Wolfe (2012) show that having a child as a teenager is associated with a higher likelihood of being married at age 21-22. The limited number of studies on the impacts of early fatherhood stems in part from limited data on fathers' fertility. While Add Health asks about sexual partners and their pregnancies, most surveys of men do not ask about men's fertility histories and instead only ask about number of children within their household. Even in surveys that do collect this data, the quality of the data is linked to survey design with retrospective histories being more likely to miss early births (Joyner et al., 2012). Additionally, these surveys are more likely to experience non-response bias from populations more likely to have children outside their household (Monte and Fields, 2020) with systematic demographic differences in which births are not captured; younger and non-marital births are less likely to be reported.

For this reason, we focus not on the direct effect of the historical roll-out of women's reproductive control on men's fertility, but instead consider the spill-over effects of these changes on men's social and economic outcomes including age at marriage, spousal qualities, educational attainment, and earnings. As past research suggests that the advent of the oral contraceptive pill and abortion allowed women to delay fertility and marriage, one would expect that there would be a corresponding increase in the age that men get married as well. Similarly, the mechanisms driving changes in spousal qualities are driven directly by the effects of these policies on women: if access to the Pill and abortion increase women's age at marriage and their education, we would expect to find that men are more likely to have older and more educated wives. However, while men's marriage arguably has a mechanical relationship to how women's marital patterns respond to new reproductive technologies, it is not clear a priori how much one would expect men's educational attainment and earnings to respond to women's access to reproductive technologies. On one hand, unintended fatherhood may interrupt a man's education and cause him to enter the workforce sooner to provide for a child, resulting in lower lifetime earnings. On the other hand, fertility typically interrupts women's careers more than men's, regardless of intendedness (Kleven, Landais, Sogard, 2020) and early entry into the laborforce due to the need to support a family may

result in higher earnings earlier in life. Our analyses will allow us to test which of these hypotheses hold.

We leverage state of birth by birth cohort changes in whether men's partners could access contraception and abortion at certain ages to identify the effects this access had on men's marital patterns, education, and earnings. Using legislative coding from Myers (2021), we categorize a man as treated if his potential sexual partners were able to legally and confidentially obtain either oral contraceptives or an abortion at the age of 16 or 18, as well as using a continuous measure of access between the ages of 15 and 21.

We find that the legalization of abortion is associated with men marrying later, with men whose partners could access abortion at age 18 having a 15% lower likelihood of being married by age 20, a 9% lower likelihood of being married by age 25, and a 3% higher likelihood of being married by age 30. We also show that growing up in an era with access to abortion changed the characteristics of the women that men married, increasing the likelihood that their wife has more education than them by 2 p.p. or 8%. Access to contraception has no significant impact on the age at which men marry or the education of their spouse, but does significantly impact the relative age of spouses. Extending legal confidential access to the Pill to women under the age of 21 decreased the age gap by 1.4 months or 6%.

Turning to men's own economic outcomes, we next look at how the legalization of these reproductive technologies changed men's educational attainment and earnings. In our analysis of educational attainment, we find an inconsistently signed and not statistically significant at a $p<0.05$ level relationship between access to abortion and college attainment/ years of education. We find a positive relationship between access to contraception and college attainment, but the relationship is also only significant at a $p<0.10$ level. The magnitudes of the effects are consistent with previous findings from Hock (2008) and our results demonstrate that the size of the findings in Hock are not sensitive to the legislative coding discrepancies documented Myers (2021), though the level of significance is.

Access to abortion does have a significant relationship with men's earnings later in life however. In particular, laws that allowed their partners to access legal and confidential abortion at age 16 are associated with 7% higher earnings for men at ages 30 to 35. While we do see these significant impacts on men's earnings, they are much smaller in magnitude than that seen for

women in past papers. Together, these findings suggest that the primary ways in which the changes in access to reproductive technologies impacted men was through changing the characteristics of the partners that they married.

The remainder of the paper is organized as follows. Section 2 discusses the data and identification strategy. Section 3 reports the results of our analyses. Section 4 concludes.

Data and Methods

Policy Environment

In this paper, we consider two types of reproductive control – access to the Pill and access to abortion – and the laws governing where these reproductive tools were available.

The first oral contraceptive, Enovid, was approved for purchase by the US Food and Drug Administration the US in 1960, but was not available to all women until many years later due to Comstock laws, which restricted the advertisement and sale of contraceptives on the basis of a number of criteria including marital status, age, and provider type. Many states did change laws regulating access through the 1960s and early 1970s, but contraception was not legally accessible across all states until a series of Supreme Court rulings granted the right to access contraception to married women in 1965 (*Griswold v. Connecticut*, 1965) and to unmarried women in 1972 (*Eisenstadt v. Baird*, 1972). Though these decisions granted confidential access for all women over the age of 21, states still varied in what they considered the age of majority, resulting in differences in confidential access to the Pill by state and age depending on if the age of majority was 18, 21, et cetera. Similarly, abortion access varied by state until the Supreme Court ruling *Roe v. Wade* (1973). Prior to *Roe v. Wade*, five states and the District of Columbia had legalized abortion and thirteen states allowed abortion due to health risks to the mother, health risks to the fetus if born, or in cases of rape or incest.

For the purposes of this paper, we define access to contraception and abortion based on the legislative coding in Myers (2021). Myers (2021) defines a state as providing legal confidential access to contraception by age group (15-17, 18-20, and 21+) if physicians and pharmacists were

legally allowed to distribute contraceptives to unmarried women and that women in that age range could consent to these services without a parent's knowledge. Abortion access is coded similarly.[1]

Determining whether a man was 'treated' by these policies requires some assumptions about the ages of the man's sexual partners. Because these policies restricted access based on women's ages, the relevant treatment group for a policy that granted 18-year-olds access are the male partners of 18-year-olds. We therefore define treatment in three different ways. In the first, we assign a man as 'treated' if women of his same age had access to contraception (abortion) based on the policy in the man's state of birth when he was 16 and when he was 18. In the second, we assign a man based on the average age difference between men and their spouses, where a man is treated is women two years younger than him had access to contraception (abortion) in the man's state of birth when he was 18 and 20. In the third, we use a continuous measure: the proportion of years between ages 15 and 20 that a man's partner of the same age would have access to contraception (abortion). For example, California had legal confidential access to contraception for 18 year olds in 1972 and for 16 year olds in 1976. Based on the first definition of treatment, a man born in 1959 would be considered treated based on laws granting access to women at age 18, but not at age 16. Based on the second definition, he would be treated for both types of laws since when he is 18 in 1977, 16-year old women have confidential access to contraception. In the third definition, his treatment value would be 0.67, based on women aged 17, 18, 19, or 20 having access when he was those ages.

Outcome Data

Our primary outcomes of interest are likelihood of marriage, spouse age, spouse education, own educational attainment, and own earnings. To measure these outcomes, we use IPUMS data from the 1% sample of the 1970 Census, and the 5% samples of the 1980 Census, 1990 Census, and 2000 Census (Ruggles et al., 2020). We restrict the sample to men born between 1934 and 1982, ensuring that the youngest members of the sample are at least 18 in the 2000 Census and that the

[1] Myers (2021)'s legislative coding for contraceptive access and abortion differs from the legislative changes in the four other papers which use the roll out of the Pill or abortion as variation (Goldin and Katz (2002), Bailey (2006), Hock (2008), and Guldi (2008). The primary discrepancies come from differences in coding of age of consent laws, as well as some errors in dates recorded.

oldest respondents were 15 by 1960, the earliest year legal contraceptive access was recorded in the policy data set.

Our first outcomes of interest are measures that capture how early in life a man was first married. Respondents in the Census are asked the age at which they first were married and the age at which their current spouse was first married. We create three indicator variables, 'married by 20' and 'married by 25', 'married by 25' equal to 1 if the respondent is observed to be older than that age and married. These variables allow us to include in the analysis men who never marry or marry post- the Census. Men who are younger than the age of interest receive a missing value (regardless of whether they are or are not currently married).

In addition to being interested in when men marry, one might also think that these reproductive technologies changed the type of women men marry. For example, men may be more likely to exhibit positive assortative matching in their marriage, marrying women who are more similar to them in terms of age or education. To test this, we look at three outcomes. The first is age of the spouse. Because we condition on Census sample and birth-year, this regression tells us if men marry older spouses conditional on their own age in the presence of these policies. The second is a similar measure – age gap between the man and their spouse. Finally, we test for positive assortative matching in terms of education with an outcome based on men's education relative to their spouse: an indicator equal to one if their spouse has more years of education than them.

Next, we turn to economic outcomes, such as educational attainment and earnings. Our first measure is an indicator for having a four-year college degree. For our second measure, we use number of years of education to try to capture variation in not just degree attainment, but also attendance in post-secondary education that does not lead to a degree. Finally, we look at the long-term effects of women's access to contraception on men's earnings, measured as log earnings from wages and salary. For all regressions on economic outcomes, we restrict the sample to men between ages 30 to 35 so that we are capturing men who are likely to have completed their education and not capturing differences in earnings in age composition by Census sample.

Identification Strategy

To identify the effects of these reproductive technology roll outs on men's outcomes, we use variation by state of birth and men's ages at the time of policy implementation in a difference-in-

difference style design. Using the first definition of treatment described above, our regression specification is as follows:

$$Y_{ist} = \beta_0 + \beta_1 Treat^{C16}_{s(i),t} + \beta_2 Treat^{C18}_{s(i),t} + \beta_3 Treat^{A16}_{s(i),t} + \beta_4 Treat^{A18}_{s(i),t} + X_i'\beta_4 + \alpha_{s(i)} + \alpha_t + \epsilon_{ist}$$

Y_{it} is the outcome of interest for person *i* born in state*s* in year *t*. The treatment variables, $Treat^{P}_{s(i),t}$, correspond to four types of policy P= {C16, C18, A16, A18}, where C16 refers to women aged 16 having access to contraception, A16 refers to women aged 16 having access to abortion, and similar for access at age 18 for C18 and A18. The treatment varies by state of birth, *s(i)*, and year of man's birth, *t*. We also control for non-time-varying characteristics such as dummies for race and ethnicity and indicators for Census sample (*X*). Finally, we include state of birth and year of birth fixed effects. For our second definition of treatment, the specification has a similar form but women's ages are lagged two-years, and for the third definition, there is a single treatment variable for contraception and a single treatment variable for abortion, equal to the percent of years a partner of the same age would be able to access the reproductive technology.

Our identification relies on the assumption that when the policy 'turns on', it turns on for a given age cohort in a given state and nothing else is changing simultaneously with the policy for this age cohort in this state. In practice, the policies are monotonic in age, meaning that if a state allows 16 year olds to access the reproductive technology, it also allows access for those older and there are no repeals of these laws in this time period, meaning that once a law turns on for an age group, it stays on for future generations. This implies that the control group for a given treated group can be thought of as men whose partners received access at a later age, rather than at the age of 16 or 18. By including state fixed effects, we control for any non-time-varying characteristics of a state related to men's socio-economics outcomes that might influence their likelihood of passing more lenient reproductive laws, such as political leanings which may impact not only reproductive laws but also educational investment or welfare generosity. By including year fixed effects, we control for any cohort level trends in outcomes that do not vary across state lines.

Results

Age at Marriage

Table 1 reports the effects of the roll out of the Pill and abortion on the likelihood that men were married by age 20, married by age 25, or married by age 30. All regressions include birth year fixed effects, birth state fixed effects, dummies for white, Black, and Hispanic, and dummies for Census sample. The samples are restricted by age to exclude men who are not yet 20, 25, or 30 respectively. Panel A uses measures of treatment assuming that the policy is relevant for women of the same age as the men; Panel B uses measures of treatment assuming that the policy is relevant for women two years younger than the men; Panel C uses the continuous measure of access based on the percent of years between 15 to 20 that men's same-age partners were treated.

For all three outcomes, we do not see significant effects of contraceptive roll out on men's age at marriage. The effects are also non-significant in a practical sense, with no effect sizes large than a 0.3 p.p. increase in the likelihood of being married by a given age. We do, however, find a statistically significant effect of abortion policy. For both the lagged and non-lagged measure of women's access to abortion at age 18, it is associated with a lower likelihood of men marrying by age 20 or 25 and a higher likelihood that they have married by age 30. For comparison, the base likelihoods for untreated men aged 30 are that 6.5 percent have married by age 20, 18.3 percent have married by age 25, and 22.3 percent have married by age 30. This means that the abortion policy is associated with a 15% lower likelihood of being married by age 20, a 9% lower likelihood of being married by age 25, and a 3% higher likelihood of being married by age 30.

Both the measures of access at younger ages and the continuous measure suggest that earlier access attenuates the effects of access of abortion on age at marriage. The continuous measure does show positive effects of abortion access on likelihood of marriage at age 30, but null effects for earlier ages. We see small positive coefficients on women's access to abortion at age 16.[2] Together, this suggests that the primary policies which delayed men's age marriage were the laws which legalized abortion rather than the laws which allowed confidential access to abortion for minors.

Spouse Qualities

[2] Note that while the coefficient is positive, it is smaller than the coefficient for age 18. Because of the monotonicity of the policy, a policy for 16-year-olds implies that that state also has a policy for 18-year-olds. This means that the interpretation of a positive, but smaller coefficient is that marriage rates are lower for those in a state which allows abortion for 16-year olds relative to one which only allows abortion for 21+, but higher for those in a state which allows it for 16-year olds relative to one which only allows abortion for 18+.

Table 2 reports the effects of these same policies on men's spouse's characteristics. The specification is the same as in table 1 but is restricted to the sample of men who are married at the time of the Census. Columns 1-3 report the effects of the policy on the spouse's age. Columns 4-6 report the effects of the policy on the age gap between the man and is spouse. Columns 7-9 report the effects of the policy on the likelihood that the man's spouse has more years of education than him.

These regressions provide evidence that abortion and contraception access resulted in men marrying women who are older and closer in age to themselves. Access to contraception for 16-year-old women and access to abortion for 18-year old women are the primary policies associated with older spouses, though the effects are small in a practical sense translating to approximately one-month older spouses. The effects of contraceptive access remain significant when using a continuous measure of access (panel C), though the continuous measure of the effects of abortion access no longer has a significant effect. Though the effects are statistically significant for contraception access, they are small. On average, men born in states without access to contraception for 16-year-olds are 2.3 years older than their spouse. Going from no access to contraception before age 21 to access for all ages 15 to 21 decreases the age gap by 0.117 years, which is a 1.4 month or 6% decrease in the age gap between men and their spouses.

The third set of regressions suggest that abortion policy, but not contraceptive policy, increased the average education of men's spouses. In particular, while overall legalization of abortion had no significant effect, the extension of confidential access to abortion for minors increased the likelihood that a man's spouse has more education than him by about 1 p.p. off a base rate of 28.5 percent or a 4 percent increase. The continuous measure suggests that going from no access to contraception before age 21 to access for all ages 15 to 21 increases the likelihood by 2 p.p. or 8 percent. This is consistent with the fact that teenage pregnancies are more likely to be disruptive of women's educational completion than pregnancies following high school completion, making policies that extend access to abortion to minors more strongly associated with men facing a more educated marriage market.

Economic Outcomes

Table 3 reports the effects of these policies on men's own educational attainment and earnings. The specification is as described above, but the sample is restricted to men ages 30 to 35. Columns

1 and 2 report the effects on likelihood of obtaining a four-year college degree; columns 3 and 4 report the effects on years of educational attainment, and columns 5 and 6 report the effects on log earnings from wages and salary.

The effects of these policies on men's educational attainment are small and not significant at the $p<0.05$ level. Though the effects of abortion inconsistent in sign across measures, there is a consistent positive impact of access to oral contraception on college attainment. We find a marginally significant increase of 0.8 p.p. in the likelihood of having a college degree associated with access to contraception at sixteen. This is of a very similar magnitude to what was found in Hock (2008), though with our legislative coding and controls for abortion policy, the results are only significant at a $p< 0.10$ level. Nonetheless, this is suggestive evidence of a small positive relationship between men's education and contraceptive access.

We do, however, find that abortion access positively impacts earnings later in life. Abortion access for 16-year-olds is associated with men earning approximately 7 percent higher incomes between the ages of 30 and 35. The continuous measure also shows a significant positive relationship between access to abortion and men's earnings; one-additional year of access to abortion before the age of 21 increases men's earnings by 2.8%. The effects of contraception policies and abortion access at age 18 also have positive coefficients, but we cannot reject the null for these policies. For comparison, Bailey, Hershbein, and Miller (2012) find that 'early legislative access', measured as treated if a woman's birth cohort would have had access to oral contraception before age 21 in her state of residence at age 21, is associated with 13.6% higher log annual wages.

Robustness Check

To check that our results are robust to different interpretations of the laws governing access to reproductive technologies, we re-run our primary specifications (i.e., assuming same age treatment) using the Bailey, Guldi, Davido, and Buzuvis (2011) coding of access to contraception. This coding diverges from Myers (2021) in how it interprets age of consent laws, with Bailey et al. (2011) having more conservative assumptions about how age of consent laws written to allow women to marry younger were implemented for consent over health care procedures. Additionally, the measure in Bailey et al. (2011) is what they define as "early legal access to the Pill" (ELA) or laws that allowed physicians to prescribe oral contraception to those who were younger than 21. They do not distinguish between laws that allowed prescription without parental consent to minors

(i.e., laws that would allow a 16-year-old to access oral contraception) and laws that lowered the age of majority to 18. We therefore include just one measure of contraceptive access, access at age 18, rather than access at age 16.

Table 4 reports the results of our regressions using this coding, with panel A showing the results using the Bailey at al. coding and panel B showing the results with the Myers coding excluding the indicator for access to contraception at 16. The measures do not change the results substantively for the marital outcomes. Access to abortion is still associated with lower likelihoods of marriage at younger ages, higher likelihoods of marriage at age 30, smaller age gaps with the spouse, and slightly more educated spouses. The one specification where the measure matters is the effects of these roll outs on men's earnings at age 30-35 (column 9). While access to contraception was positively but not significantly associated with higher earnings for men under the legislative coding from Myers (2021), the effects are significant and of similar magnitude to the effects of abortion using the legislative coding from Bailey at al. (2011).

Conclusion

This paper contributes to the literature on the impacts of the historical roll out of the Pill and abortion across the United States in three ways. First, it shows that these reproductive technologies effects on men's marital patterns, delaying the age at which they married, decreasing the age gap between them and their spouse, and increasing the likelihood that they were married to woman with more education than them. Second, it the first paper to explore how the legalization of the Pill and abortion impacted men's labor outcomes, showing that there are significant positive impacts of abortion legalization on men's earnings later in life. Third, this paper contributes to the ongoing discussion of the methodologies of analyzing the relative impact of the Pill versus abortion, finding that abortion legalization was the primary policy that impacted men's marital outcomes and labor outcomes.

The results of this paper are consistent with past work on the impacts of these policies on women's marriage and labor market outcomes. Previous work finds that access to abortion and oral contraception delay marriage; it is perhaps then unsurprising to find that men also marry at a later age and marry older women. The value of framing this paper from the perspective of benefits for men is to highlight the fact that these policy changes do not only impact women. The spill-overs

on men are substantive and this study demonstrates that reproductive healthcare policies and their impacts on couples' fertility options have potential benefits for all genders.

Moreover, our findings about the impacts of these policies on men's earnings test previously untested hypotheses about the costs and benefits of delaying fertility for men. A priori, it is possible that delaying family responsibilities may result in lower earnings for men due to less need for income to support a family. We find the opposite, with men whose partners had more control over fertility through contraception and abortion earning more at the age of 30 than those whose partners didn't. Given that we find small effects on educational attainment, this suggests that early fertility disrupts men's human capital formation as a teenager and young adult beyond just changing their educational attainment. Future analyses should explore how men's careers are impacted by early fertility, such as through changing selection of major or occupation or changing the types of jobs they are willing to accept.

Finally, one of the contributions of this paper is reconciling past results with updated legislative coding from Myers (2021). As found in Myers (2018) paper about women's outcomes, abortion policy rather than contraceptive policy seems to be the primary pathway through which the reproductive revolution changed socioeconomic outcomes of men. That said, our paper is able to replicate the magnitude of the effects of contraception on education in Hock (2008), despite the difference in legislative coding and our added controls for abortion policy. However, the effects on men's education are only marginally significant and small.

One limitation of this work is our inability to observe men's retrospective childbearing history. Without this data, we do not know if the small impacts we for educational attainment is due to men's child responsibilities being unchanged by these or if men did have fewer children due to these policies but this did not change their educational attainment. That is, it is possible that in the absence of these policies, men whose partners had a child did not tell the man or the man did not contribute to child raising and therefore did not have their education disrupted by the birth. Conversely, it is possible that the birth of a child did not interrupt a man's educational attainment even if he was involved in the child's life. Both of these scenarios, however, would imply a null effect of women's fertility control improving on men's educational attainment. Data that better measures men's realized fertility would allow researchers explore more in depth how contraceptive and abortion access for women may create spill-over benefits for the partners.

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Table 1. Effects of the Pill and Abortion Roll Out on Men's Age at Marriage

	Married by 20			Married by 25			Married by 30		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Contr. Access, 16	0.0009			0.0030			0.0020		
	(0.0045)			(0.0042)			(0.0020)		
Contr. Access, 18	0.0017			0.0032			0.0003		
	(0.0028)			(0.0032)			(0.0011)		
Abortion Access, 16	0.0059			0.00956**			0.00504**		
	(0.0046)			(0.0032)			(0.0017)		
Abortion Access, 18	-0.0100***			-0.0168***			0.0068**		
	(0.0028)			(0.0040)			(0.0021)		
Lag Contr. Access, 16		0.0010			0.0032			0.0027	
		(0.0042)			(0.0036)			(0.0021)	
Lag Contr. Access, 18		0.0024			0.0034			0.0020	
		(0.0031)			(0.0027)			(0.0016)	
Lag Abortion Access, 16		0.0035			0.0046			0.0050**	
		(0.0043)			(0.0033)			(0.0018)	
Lag Abortion Access, 18		-0.0097**			-0.0143***			0.0052**	
		(0.0029)			(0.0033)			(0.0018)	
Contr., % Treated 15-20			0.00362			0.00846			0.00364
			(0.00775)			(0.00722)			(0.00322)
Abortion, % Treated 15-20			0.00749			0.00949			0.0140**
			(0.0103)			(0.00702)			(0.00471)
Birth State FE	X	X	X	X	X	X	X	X	X
Birth Year FE	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X
N	8437451	8437451	8437451	7117556	7117556	7117556	5812125	5812125	5812125

Note. This table reports results of a regression of an indicator for being married by a given age (20, 25,30) on an indicator for whether a 16-year old (18-year old) had confidential access to contraception or abortion in the man's birth state when the man was 16 (18) in panel A or when the man was 18 (20) in panel B. Panel C measures the treatment as the percent of years aged 15-20 men's same-age partners had access to contraception/abortion. All regressions include birth state fixed effects, birth year fixed effects, dummies for being white, Black, or Hispanic, and dummies for Census year. Standard errors are clustered at the birth state level. Significance reported with stars: + p<0.10, * p<0.05, ** p<0.01, and *** p<0.01.

Table 2. Effects of the Pill and Abortion Roll Out on Men's Spousal Qualities

	Spouse Age			Age Gap			Wife More Educated		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Contr. Access, 16	0.0711*			-0.0709*			-0.0010		
	(0.0288)			(0.0288)			(0.0035)		
Contr. Access, 18	0.0152			-0.0150			0.00182		
	(0.0140)			(0.0140)			(0.0019)		
Abortion Access, 16	0.0276			-0.0272			0.0100**		
	(0.0366)			(0.0365)			(0.0035)		
Abortion Access, 18	0.0574***			-0.0568***			0.00158		
	(0.0112)			(0.0111)			(0.0025)		
Lag Contr. Access, 16		0.0810**			-0.0812**			-0.0004	
		(0.0279)			(0.0279)			(0.0040)	
Lag Contr. Access, 18		0.0152			-0.0148			0.0027	
		(0.0151)			(0.0151)			(0.0024)	
Lag Abortion Access, 16		0.0054			-0.0045			0.0084*	
		(0.0315)			(0.0315)			(0.0033)	
Lag Abortion Access, 18		0.0440*			-0.0435*			-0.0014	
		(0.0206)			(0.0209)			(0.0021)	
Contr., % Treated 15-20			0.117*			-0.117*			0.0006
			(0.0452)			(0.0451)			(0.0055)
Abortion, % Treated 15-20			0.0796			-0.0783			0.0206**
			(0.0725)			(0.0723)			(0.0073)
Birth State FE	X	X	X	X	X	X	X	X	X
Birth Year FE	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X
N	5419553	5419553	5419553	5419553	5419553	5419553	5251266	5251266	5251266

Note. This table reports results of a regression of spouse quality (age of spouse, age gap with spouse, indicator for spouse being more educated) on an indicator for whether a 16-year old (18-year old) had confidential access to contraception or abortion in the man's birth state when the man was 16 (18) in panel A or when the man was 18 (20) in panel B. Panel C measures the treatment as the percent of years aged 15-20 men's same-age partners had access to contraception/abortion. All regressions include birth state fixed effects, birth year fixed effects, dummies for being white, Black, or Hispanic, and dummies for Census year. Standard errors are clustered at the birth state level. Significance reported with stars: + p<0.10, * p<0.05, ** p<0.01, and *** p<0.01.

Table 3. Effects of the Pill and Abortion Roll Out on Men's Education and Earnings Outcomes

	College Degree			Number of Years of Education			Log Earnings		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Contr. Access, 16	0.0080+			0.0727			0.0189		
	(0.0045)			(0.0531)			(0.0332)		
Contr. Access, 18	0.0058			0.0478			0.0339		
	(0.0051)			(0.0526)			(0.0338)		
Abortion Access, 16	-0.0025			-0.0670+			0.0688*		
	(0.0035)			(0.0357)			(0.0294)		
Abortion Access, 18	-0.0001			-0.0142			0.0003		
	(0.0062)			(0.0516)			(0.0664)		
Lag Contr. Access, 16		0.0080			0.0740			0.0434	
		(0.0049)			(0.0599)			(0.0325)	
Lag Contr. Access, 18		0.0029			0.0093			0.0511	
		(0.0064)			(0.0684)			(0.0383)	
Lag Abortion Access, 16		-0.0016			-0.0727			0.0710*	
		(0.0046)			(0.0467)			(0.0333)	
Lag Abortion Access, 18		0.0051			0.0487+			0.0362	
		(0.0058)			(0.0270)			(0.110)	
Contr., % Treated 15-20			0.0155+			0.142			0.0577
			(0.0086)			(0.104)			(0.0481)
Abortion, % Treated 15-20			-0.0047			-0.154+			0.168*
			(0.0084)			(0.0838)			(0.0686)
Birth State FE	X	X	X	X	X	X	X	X	X
Birth Year FE	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X
N	1568458	1568458	1568458	1568458	1568458	1568458	1568458	1568458	1568458

Note. This table reports results of a regression of spouse quality (college degree, years of education, log earnings from wages and salary) on an indicator for whether a 16-year old (18-year old) had confidential access to contraception or abortion in the man's birth state when the man was 16 (18) in panel A or when the man was 18 (20) in panel B. Panel C measures the treatment as the percent of years aged 15-20 men's same-age partners had access to contraception/ abortion. Sample restricted to men ages 30 to 35 at time of the Census. All regressions include birth state fixed effects, birth year fixed effects, dummies for being white, Black, or Hispanic, and dummies for Census year. Standard errors are clustered at the birth state level. Significance reported with stars: + p<0.10, * p<0.05, ** p<0.01, and *** p<0.01.

Table 4. Robustness to Different Measures of Contraceptive Access

		(1) Married by 20	(2) Married by 25	(3) Married by 30	(4) Spouse Age	(5) Age Gap	(6) Wife More Educated	(7) College Degree	(8) Years of Ed	(9) Log Earnings
Panel A: Myers Measures	Contr. Access, 18	0.00170	0.00326	0.000310	0.0163	-0.0160	0.00180	0.00473	0.0382	0.0314
		(0.00285)	(0.00326)	(0.00117)	(0.0134)	(0.0134)	(0.00189)	(0.00485)	(0.0493)	(0.0367)
	Abortion Access, 16	0.00612	0.0102**	0.00546**	0.0429	-0.0424	0.00981*	-0.00122	-0.0554	0.0717*
		(0.00471)	(0.00353)	(0.00200)	(0.0366)	(0.0366)	(0.00373)	(0.00400)	(0.0396)	(0.0318)
	Abortion Access, 168	-0.0102**	-0.0174***	0.00649***	0.0459***	-0.0453***	0.00174	0.00235	0.00829	0.00613
		(0.00306)	(0.00449)	(0.00173)	(0.0124)	(0.0123)	(0.00241)	(0.00615)	(0.0435)	(0.0660)
Panel B: Bailey et al. Measures	Contr. Access, 18	0.00139	0.00230	0.000390	0.00599	-0.00581	-0.000158	0.00616	0.0231	0.0765*
		(0.00186)	(0.00294)	(0.00106)	(0.0106)	(0.0106)	(0.00239)	(0.00713)	(0.0408)	(0.0342)
	Abortion Access, 16	0.00614	0.0102**	0.00547**	0.0430	-0.0426	0.00981*	-0.00119	-0.0551	0.0718*
		(0.00470)	(0.00355)	(0.00200)	(0.0369)	(0.0369)	(0.00377)	(0.00402)	(0.0399)	(0.0313)
	Abortion Access, 18	-0.0101**	-0.0171***	0.00649***	0.0482***	-0.0476***	0.00215	0.00192	0.0186	-0.0152
		(0.00294)	(0.00440)	(0.00177)	(0.0120)	(0.0119)	(0.00237)	(0.00635)	(0.0401)	(0.0755)
	Birth State FE	X	X	X	X	X	X	X	X	X
	Birth Year FE	X	X	X	X	X	X	X	X	X
	Controls	X	X	X	X	X	X	X	X	X
	N	8437451	7117556	5812125	5419553	5419553	5251266	1568458	1568458	1568458

Note. This table replicates the analyses in Tables 1, 2, and 3 using alternative measures of contraceptive access. Panel A uses the same measures are previously but omits access to contraception at age 16. Panel B uses a measure of access for those under age 21 coded in Bailey, Guldi, Davido, and Buzuvis (2011). All regressions include birth state fixed effects, birth year fixed effects, dummies for being white, Black, or Hispanic, and dummies for Census year. Standard errors are clustered at the birth state level. Significance reported with stars: + p<0.10, * p<0.05, ** p<0.01, and *** p<0.01.