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Legal Access to Reproductive Control Technology, Women's Education, and Earnings Approaching Retirement†

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The landscape for reproductive health care in the United States has undergone massive changes in recent years. In 2017, the set of employers and insurers who are exempt from the Affordable Care Act's contraceptive coverage mandate was broadened to include those with moral objections. In 2019, Title X rules were changed to deny funding to family-planning providers that refer patients for abortion, which could restrict women's access to both contraception and abortion care. At the same time, several states, including Delaware, Massachusetts, South Carolina, and Washington, have launched major initiatives to expand access to the full range of contraceptives, including intrauterine devices and implants, which can be difficult for some women to obtain because of costs and a lack of trained providers. A variety of state restrictions have made it harder for women to access abortion, including restrictions that have caused abortion clinics to close. Telemedicine for consultation and/or medication abortion has expanded access in some states. Questions about the economic effects often come up when the desirability of such policies is discussed. The economic effects are relevant to considering the merits of subsidizing access and to considering the costs imposed by regulations that limit access.

What do historical changes in contraception and abortion access tell us about the long-run effects of such changes? In this study, we investigate this question by using data from the Health and Retirement Study (HRS) and an identification strategy that leverages variation in exposure to legal changes in access across cohorts born in the same states during the 1960s and 1970s. We follow the methodology of Bailey, Hershbein, and Miller (2012; hereafter BHM), which uses the National Longitudinal Survey of Young Women and documents significant increases in contraception use at age 18–20 associated with unmarried women's ability to consent for contraception at such ages. The authors also document increased educational attainment and increased earnings in women's thirties and forties associated with this confidential access to contraception. Our analysis revisits the effects on education and earnings. We also investigate the sensitivity of the estimated effects to the legal coding and control variables used in Myers (2017), which studies of the effects on fertility and marriage.

The results for educational attainment align with prior work but are not statistically significant. The results for earnings indicate increases in the probability of working in a Social Security (SS)-covered job in women's twenties and thirties associated with early access to contraception and abortion, but we find no evidence of positive effects on women's earnings in their fifties.

I. Data and Methodology

Our analyses use restricted-use data from the HRS, a longitudinal survey of Americans over age 50 and their spouses. The study interviews approximately 20,000 respondents every two years on subjects like employment, health care, housing, assets, pensions, and disability. We use restricted-use data from the HRS that include individuals' earnings histories from

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1951 to 2013 based on information provided by the Social Security Administration. The HRS has collected information on six groups of birth cohorts across multiple survey waves since it began conducting surveys in 1992.

Our analysis of educational outcomes follows the approach used in Goldin and Katz (2002), Bailey (2006, 2009), Guldi (2008), Hock (2008), and Myers (2017), which analyze the effects of legal access to contraception and abortion on women's marital and fertility outcomes by using within-state-across-cohort variation. Following Myers (2017), our analysis of education focuses on women born from 1935 to 1958 and considers two measures of access to each reproductive control method (contraception and abortion): (i) the method being legal and young unmarried women being able to provide legal consent ("pill consent," or *PiCon*; "abortion consent," or *AbCon*) and (ii) the method being legal but young unmarried women *not* being able to provide legal consent ("pill legal," or *PiLeg*; "abortion legal," or *AbLeg*). We measure a woman's exposure to legal access on the basis of the legal circumstances in her state of residence between the ages of 18 and 20, allowing variables to range from zero to one for the proportion of years of legal access during these years. We infer a woman's state of residence at these ages on the basis of her state of residence at age ten for the vast majority of women for whom this is available and on the basis of state of birth for the remainder. Our regression model, identical to that in Myers (2017), is as follows:

$$(1)\quad Ed_{ics} = PiLeg_{cs}\gamma + PiCon_{cs}\beta + AbLeg_{cs}\theta + AbCon_{cs}\delta + \eta_c + \varphi_s + \mathbf{X}_{ics}\boldsymbol{\lambda} + \varepsilon_{ics},$$

where Ed_{ics} measures the educational attainment for woman i born in cohort c who lived in state s as a youth, the legal access measures are as defined above, η_c are cohort fixed effects, φ_s are state fixed effects, and $\mathbf{X}_{ics}$ includes a rich set of additional controls including state–linear cohort trends.[1] In constructing standard error estimates, we allow the error term ε_{iys} to be correlated across cohorts from the same state. In addition to reporting estimates based on Myers's legal coding, we also report estimates that use BHM's legal coding for contraception access.[2]

Our analysis of women's economic outcomes across the life cycle follows BHM. This methodology also leverages variation in access across cohorts of women from the same state but focuses on variation in young women's ability to provide consent to access contraception and extends the model to assess the effects on women's outcomes that are measured at different ages. Specifically, we estimate

$$(2)\quad Y_{iacs} = \sum_g \beta_g PiCon_{cs} D_{g(a)} + \sum_g \gamma_g EAA_{cs} C50_c D_{g(a)} + \sum_g \theta_g PiCon_{cs} EAA_{cs} C50_c D_{g(a)} + \delta \ln Dist_s C50_c + \lambda_{g(a)} + \phi_s + \psi_c + \epsilon_{iacs},$$

where g corresponds to five-year age groups (20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, and 55+), $D_{g(a)}$ is an indicator of whether an observation is in age group g based on its corresponding age a, EAA_{cs} is an indicator for early legal access to abortion (defined as residing in an early-legalizing state[3] before age 21), $Dist_s$ is the distance to the nearest large city providing legal abortions to out-of-state residents (Buffalo, New York City, San Francisco, or the District of Columbia), $C50_c$ is an indicator for being born in 1950 or later and thus potentially being affected by abortion legalization before age 21 for women residing in early-legalizing states, and the other variables are defined as in

[1] The additional control variables include race, ethnicity, the interaction of "early pill legal" and "abortion legal," and the interaction of "early pill legal" and "early abortion legal." They also include exposure (measured as the fraction of years from age 18 to 20) to state abortion reforms, which were enacted in 13 states prior to *Roe v. Wade* and permitted abortion under limited circumstances; state policy permitting no-fault divorces; state equal pay law prior to the enactment of federal legislation in 1963; and state fair employment practices act prohibiting racial discrimination in hiring, discharge, and compensation.

[2] BHM's coding is based on Bailey et al. (2011).

[3] Early-legalizing states are states that legalized in 1969–1971: Alaska, California, the District of Columbia, Hawaii, New York, and Washington.

equation (1). For this analysis, we follow BHM by considering women born no later than 1954.[4]

Two notable differences between the models characterized by equation (1) and equation (2) are that the latter model (i) does not distinguish between legal access to abortion and minors' ability to consent for abortion and (ii) does not consider the degree to which there may be effects of legal access when these women are themselves older. We intend to examine these possibilities in future work. In this study, we replicate BHM, extend the analysis to consider effects at older ages, and examine the sensitivity of the estimates to using legal coding and additional control variables based on Myers (2017).[5] When we do so, we measure early abortion access when women were age 18–20 on the basis of whether unmarried women of such ages could consent to abortion according to Myers's coding.

II. Results

A. *Educational Attainment*

Table 1 reports our estimated effects on years of education (up to 17) based on equation (1). Consistent with estimates reported in BHM, and previously in Goldin and Katz (2002) and Hock (2008), our estimates suggest that both legal access and being able to consent for contraception from age 18 to 20 are associated with increased levels of education. With that said, we note that these estimates are only marginally statistically significant when we use BHM's coding of legal access to contraception (column 1) and that the estimates are somewhat smaller and are not statistically significant when we use Myers's coding (column 2). Our analysis of black women also suggests positive effects of greater legal access to reproductive control technology, and to legal access to abortion in particular (columns 3 and 4).

Table 1—Effects of the Pill and Abortion on Years of Education

	Full sample		Blacks	
Contraception coding:	BHM (2012) (1)	Myers (2017) (2)	BHM (2012) (3)	Myers (2017) (4)
Pill consent	0.3677	0.2030	0.6627	0.3537
	(0.2157)	(0.1782)	(0.4279)	(0.5379)
Pill legal	0.2488	0.2282	0.0722	0.0288
	(0.1384)	(0.1548)	(0.3240)	(0.4031)
Abortion consent	−0.3104	−0.3837	0.7801	0.6052
	(0.3482)	(0.3180)	(0.5444)	(0.5454)
Abortion legal	−0.2276	−0.2724	1.4631	1.3402
	(0.2665)	(0.2704)	(0.3490)	(0.3440)
Observations	9,390	9,390	2,095	2,095

Notes: The table reports coefficients; standard errors robust to heteroskedasticity and clustered at the state level are in parentheses. The dependent variable is years of education up to a maximum of 17. Pill (abortion) consent measures the proportion of years from age 18 to 20 in which the pill (abortion) was legally available and allowed minors to legally consent for it. Pill (abortion) legal measures the proportion of years from age 18 to 20 in which the pill (abortion) was legally available but unmarried minors of these ages could not consent. See the text, including footnote 1, for additional details on the models.

B. *Earnings*

We examine earnings by using two types of data available in the HRS: earnings based on SS records and earnings based on HRS surveys. The former has the advantage of a large sample size covering a very broad set of age groups; however, it will vastly understate earnings for women working in jobs that are not covered by SS. For this reason, we use this measure simply to evaluate whether a woman had any earnings in an SS-covered job in a given year, which is measured without error.[6] In 1981, 90 percent

[4] BHM is restricted to using data from the 1943–1954 cohorts because those are the cohorts covered by the National Longitudinal Survey of Young Women, which was first conducted in 1968 and focused on 5,159 women age 14 to 24 at the time. The results reported in the tables in this paper are based on an expanded set of cohorts, 1930–1954. These results are consistent with our analysis of the 1943–1954 cohorts, which produce estimates that are slightly smaller in magnitude but with much larger standard errors.

[5] These additional control variables include indicators for the race and ethnicity of the respondent, state–linear cohort trends, and measures of the fraction of years of exposure (from age 18 to 20) to state abortion reforms and consent to state abortion reforms (enacted in 13 states prior to *Roe v. Wade* and permitted abortion under limited circumstances), state policy permitting no-fault divorces, state equal pay law prior to the enactment of federal legislation in 1963, and state fair employment practices act prohibiting racial discrimination in hiring, discharge, and compensation.

[6] If we instead evaluated earnings levels based on this measure, it could cause us to understate the economic benefits of legal access to reproductive control technology if

(98 million) of all wage and salary workers and 62 percent (13 million) of workers in the public sector were covered under SS (Nelson 1985). We use the HRS's survey-based measure of earnings to evaluate women's earnings levels in their fifties.[7]

Table 2 reports the estimated effects on whether a woman is working in an SS-covered job. Column 1 shows the results following BHM's methodology, and column 2 shows the results using Myers's coding and the additional control variables described in footnote 5. As a whole, these estimates indicate that early legal access to contraception increased women's probability of working in an SS-covered job, particularly in their late twenties and early thirties. While any such effects may reflect increased labor force participation, they could also arise from substitution from SS-uncovered jobs to SS-covered jobs.

The results also indicate that gaining early legal access to abortion is similarly associated with an increased probability of working in an SS-covered job. The estimates again suggest effects for women in their twenties and early thirties. As discussed above, an important caveat to these results is that the estimates could be picking up long-run effects of the conditions when a woman was 18–20 or the effects of having access at older ages.[8]

Table 3 shows estimates focusing on the log of women's hourly wages.[9] As a whole, the estimated effects on this outcome indicate no statistically significant effects on women's earnings in their fifties. These results are not inconsistent with BHM, which finds positive effects of early access to the pill when women were in their thirties and forties. We also do not

Table 2—Effects of the Pill and Abortion on Working in an SS-Covered Job

Legal coding:	BHM (2012) (1)	Myers (2017) (2)
Pill consent × age 20–24	0.037	0.025
	(0.018)	(0.017)
Pill consent × age 25–29	0.076	0.055
	(0.019)	(0.024)
Pill consent × age 30–34	0.044	0.054
	(0.019)	(0.018)
Pill consent × age 35–39	0.017	0.027
	(0.015)	(0.019)
Pill consent × age 40–44	0.011	0.011
	(0.018)	(0.020)
Pill consent × age 45–49	−0.009	−0.003
	(0.020)	(0.017)
Pill consent × age 50–54	−0.043	−0.022
	(0.020)	(0.024)
Pill consent × age 55+	0.042	0.065
	(0.022)	(0.022)
EAA × age 20–24	0.053	0.042
	(0.018)	(0.017)
EAA × age 25–29	0.138	0.070
	(0.026)	(0.040)
EAA × age 30–34	0.056	0.049
	(0.033)	(0.036)
EAA × age 35–39	0.015	0.021
	(0.017)	(0.016)
EAA × age 40–44	−0.044	−0.022
	(0.043)	(0.027)
EAA × age 45–49	−0.098	−0.061
	(0.020)	(0.018)
EAA × age 50–54	−0.045	−0.036
	(0.036)	(0.023)
EAA × age 55+	0.044	0.079
	(0.068)	(0.052)
Observations	305,877	305,877

Notes: The table reports coefficients as well as standard errors robust to heteroskedasticity and clustered at the state level in parentheses. The sample includes 7,608 unique women. The dependent variable is an indicator variable that takes a value of one if the respondent showed zero earnings in the SS information. This information comes from the Social Security Administration supplement to the HRS. "Pill consent" is equal to one if a woman could legally consent for contraception before age 21 in her state of residence as a youth. EAA represents early access to abortion—in column 1, it is equal to one if a woman lived in an early-legalizing state before age 21, and in column 2, it is equal to one if a woman could legally consent to having an abortion before age 21. See the text, including footnote 5, for additional details on the models.

such access led women into higher-paying jobs that are not covered by SS.

[7] The analysis includes younger women, but we report estimates only for women in their fifties because younger women are included in the HRS only if they are married to someone who is older than 50.

[8] Estimated effects of both contraception access and abortion access are slightly smaller in magnitude, with much larger standard errors, if we instead analyze the 1944–1954 cohorts (like BHM) instead of the 1930–1954 cohorts.

[9] The HRS allows respondents to report their earnings in any interval they desire, including their hourly wage. For women reporting their earnings in some other interval, the HRS calculates their hourly wage on the basis of their responses to questions about their normal hours worked per week and normal weeks worked per year.

Table 3—Effects of the Pill and Abortion on the log of Real Hourly Wage of the Previous Year

Legal coding:	BHM (2012) (1)	Myers (2017) (2)
Pill consent × age 50–54	0.018	0.014
	(0.049)	(0.049)
Pill consent × age 55+	−0.029	−0.032
	(0.034)	(0.041)
EAA × age 50–54	−0.0056	−0.031
	(0.083)	(0.105)
EAA × age 55+	−0.077	−0.146
	(0.094)	(0.066)
Observations	24,907	24,907

Notes: The table reports coefficients as well as standard errors robust to heteroskedasticity and clustered at the state level in parentheses. The sample includes 6,533 unique women. The dependent variable is the log of the real hourly wage (year 2000 dollars) of the previous year. Observations with zero wages are excluded from these estimations. Pill consent is equal to one if a woman could legally consent for contraception before age 21 in her state of residence as a youth. EAA represents early access to abortion—in column 1, it is equal to one if a woman lived in an early-legalizing state before age 21, and in column 2, it is equal to one if a woman could legally consent to having an abortion before age 21. See the text, including footnote 5, for additional details on the models.

find evidence of statistically significant positive effects if we evaluate hourly wages (not taking the logarithm), hourly wages excluding zeroes, or weekly wages (taking the logarithm or not, excluding zeroes or not) or if we restrict the sample to the 1943–1954 cohorts (as in BHM).

III. Conclusion

Given major gaps in access to contraception and abortion care, understanding the economic effects of such access will likely continue to be relevant to policy. In this paper, we build on the knowledge base by evaluating how changes in access resulting from policy changes in the 1960s and 1970s affected educational attainment and women's very-long-run earnings. We hope that future work will go deeper in assessing the robustness of these results.

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