The Pill and the College Attainment of American Women and Men*

by

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Abstract

This paper considers the educational consequences of the increased ability of young women to delay childbearing as a result of the birth control pill. In order to identify the effects of the pill, I utilize quasi-experimental variation in U.S. state laws governing access to contraception among female adolescents during the 1960s and 1970s. Inference based on this natural experiment indicates that, by lowering the incidence of early fertility, unconstrained access to the pill increased the enrollment rate of college-age women by almost 5 percentage points. Further, early pill access was associated with a rise in the rate of college completion of approximately 0.9 percentage points among women over the age of thirty. Finally, I analyze the educational outcomes of men in relation to the contraceptive laws governing their likely female partners during adolescence. The results for male college completion suggest that the schooling options for men might also have been constrained by undesired early fertility among their female partners.

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> There is only one drug in the world so well known that it's called "the Pill." In 1968 a popular writer ranked the Pill's importance with the discovery of fire and the development of tool-making. Twenty-five years later, the leading British weekly, *The Economist*, listed the Pill as one of the seven wonders of the modern world. For more than forty years, more people have taken it than any other prescribed medicine in the world.
>
> – From the PBS *American Experience* website[1]

1 Introduction

Oral contraception was introduced in the United States in 1960, and within two years 1.2 million prescriptions were sold. The birth control pill had become the leading form of contraception by 1965, and at the end of the decade it was used by more than one out of five married American women of reproductive age (Westoff and Ryder, 1977). By the late 1980s approximately 80 percent of all women in their reproductive years had used oral contraceptives at some point in their lives (Dawson, 1990). The rapid diffusion and wide-spread use of the pill has led many to conclude that its introduction marked the beginning of a "contraceptive revolution."[2] What made the pill so remarkable, and what drove its rapid diffusion, was the degree of autonomy and control it offered women over their reproductive lives, especially with respect to the timing of their fertility.[3] In this paper, I consider the educational consequences of the increased ability of young women to delay childbearing to later stages of the lifecycle as a result of the pill.

Early fertility can alter the set of human capital investment opportunities available to a woman. Child-rearing is time-consuming and is spread out over a number years after a child is born. This implies that a woman's ability to accumulate human capital may be substantially constrained for some time after her first birth. As a result, many women may find it optimal to delay childbearing to later stages of the lifecycle and undertake career investments first.[4] This notion of optimality, of course, relies on perfect

[1] URL: http://www.pbs.org/wgbh/amex/pill/peopleevents/e_usa.html

[2] See, for example Watkins (1998), from whom the leading quotation is largely derived. This idea is also implicit given the findings in – and title of – the Westoff and Ryder (1977) volume.

[3] The impact of the pill on the alterative dimension of fertility, completed family size, may not have been so profound in the era of contraceptive sterilization, which had gained wide-spread acceptance during the 1960s. By 1970, at least one of the partners was sterilized or would "seriously consider it" in more than two thirds of married couples (Presser and Bumpass, 1972).

[4] This is consistent with the theoretical results of Mullin and Wang (2002), who develop a dynamic

control over fertility. As highlighted by Goldin and Katz (2002), in a world of highly imperfect fertility control, young women desiring to invest in their human capital must choose between two second-best scenarios. They must either forgo sexual activity (which presumably enters as an argument into the utility function) or run a substantial risk of an early pregnancy, which could mean delaying or forgoing their human capital investments.

Oral contraceptives should have allowed a sexually active woman a much greater degree of confidence in a decision to delay her fertility. Compared to the previously prevailing reversible methods of contraception, the pill reduced the risk of an unwanted pregnancy by more than five-fold. We would thus expect more women to arrive at a first-best outcome in which they were able to both be sexually active and undertake early career investments with the introduction of the pill. The findings in Goldin and Katz are consistent with such an improvement. However, their focus is on the trade-off between career and marriage, rather than human capital and fertility per se. Further, they limit their empirical sample for the most part to college-graduate women and so, in some sense, women who were likely to have forgone sexual activity without access to the pill. My focus is essentially on women that would be in the other second-best outcome were the pill not available. Specifically, one might imagine a large class of women who would be sexually active despite the less efficacious contraceptive technology. Presumably, such women would have been better able to avoid early fertility upon the introduction of the pill, and so able to attend and complete college.

Inference based on actual use of the pill is likely subject to substantial self-selection biases. As a result, my analysis relies on differences between young women in their access to oral contraceptives. In particular, I utilize quasi-experimental variation in state laws governing the consent for medical and family planning services during the 1960s and 1970s, similar to that was used by Goldin and Katz in their career-and-marriage analysis of the pill.[5] Since the legal changes were largely orthogonal to fertility and education

general equilibrium model of fertility timing and human capital investment. It may also be motivated from the idea that, if learning ability diminishes over the lifecourse (as Weinberg's (2002) results might suggest), human capital investments should be undertaken earlier in life, rather than later.

[5]It should be noted that these laws also applied to other highly effective forms of prescription contraception, such as the intrauterine device (IUD), which was also approved by the FDA in the early 1960s. However, the IUD did not diffuse as rapidly or widely as did the pill (Ryder, 1972). At its peak in the early 1970s it was used by less than 10% of contracepting women. Its use declined substantially thereafter and it is currently used by less than 1% of contracepting women (Hubacher, 2002). Nonetheless, wherever "the pill" is used in this paper, the arguments and discussion naturally extend to the IUD, as

preferences, a differences-in-differences estimation strategy can potentially identify the effects of early access to the pill on the educational attainment of women.

The line of analysis undertaken here relates strongly to Klepinger, Lundberg and Plotnick's (1995 and 1999) work on the negative consequences of teenage childbearing on the accumulated human capital of young adult women. In their empirical work, Klepinger et al. employ a large set of instruments for adolescent fertility. While they include among their instruments state consent laws governing minors' access to contraception, they do not isolate the effects of contraceptive access on the outcomes, which is the goal of this paper. This is endeavour that is likely infeasible in their sample.[6] In fact, the available data make it difficult to conduct two-stage least squares to formally estimate the effects of access to the pill on early fertility and then of an instrumented version of early fertility on human capital investments. Consequently, in my empirical work I estimate the reduced-form influence of early pill access on measures of educational attainment among women. These reduced-form estimates can be compared to the direct effects of the pill on early fertility provided by Bailey (2004), who employs a similar indirect least squares methodology to consider the effects of the pill on female lifecycle labor supply. In addition to assessing the role that the pill played in the outcomes of females, I also take some first strides toward understanding the effects of female contraceptive access on the educational attainment of their male partners. This represents a relatively novel contribution to the literature on the consequences of early fertility.

The syllabus for the remainder of the paper is as follows. Section 2 describes the nature of state laws circumscribing the availability of the pill to young women. In it, I argue that certain changes to these laws during the 1960s and early 1970s constitute a quasi-natural experiment, which can be used to identify a causal effect of the pill. I also recapitulate evidence provided by Bailey on how these legal changes affected early fertility, providing justification for the reduced-form approach undertaken in the empirical sections of the paper. In Section 3, I estimate the impact of access to oral contraceptives on various measures of the early human capital investment of young women. I find that early access to the pill was associated with a substantially higher rate of school

well as more recent forms of efficient medical contraception, such as injectables and implants.

[6] The variation in consent laws during the more recent period they consider is both less sharp and less likely to be orthogonal to state-level preferences than during the timeframe I consider. Further, their relatively small sample size does not allow them to control for unobserved state-level heterogeneity, which might co-vary with the access laws.

enrollment among college-age women, but did not seem to affect their likelihood of labor force participation. Section 4 considers how increased enrollment rates translated into college completion among adult American women, finding persistent differences based on early access to contraception. In Section 5, I consider the effects of the contraceptive access laws with respect to the educational outcomes of men. The evidence presented there suggests that the direct effect of the legal changes was female-specific, but that they had a strong and indirect effect on male educational attainment through their impact on female fertility. Section 6 provides additional discussion and concluding remarks.

2 Identifying the Impact of the Pill

Oral contraceptives dramatically reduced the risk of an unintended pregnancy for sexually active women. According to data from clinical trials submitted to the FDA (U.S. Food and Drug Administration, 1997), the annual contraceptive failure rate for pill users is 1-2%. The next most effective form of temporary contraception available in 1960, the male latex condom, is associated with a failure rate of 11%. Taking the higher failure rate for oral contraceptives, this implies a 5.5% reduction in the likelihood of an unwanted pregnancy for a couple using the pill, as compared to a couple relying on the condoms.[7] Thus, the pill represented a dramatic "technology shock" to the set of fertility control options available to women.

This paper is primarily interested in the effects of this improved means of fertility control on formal human capital investments, i.e. education. Considering the relationship between actual use of the pill and educational outcomes might not be a meaningful endeavor in this regard. Specifically, choices regarding sexual activity and contraceptive use are likely jointly determined with educational aspirations. As a result, any observed correlation between the use of contraceptives and educational attainment might simply reflect unobserved differences between individuals in preferences and abilities. In order to assess the structural impact of the pill, my analysis relies on variation in laws governing access to contraceptives among college-age women. As argued below, the changes to these laws that occurred during the 1960s and 1970s were largely unrelated to preferences over

[7] This likely understates the average contraceptive efficiency gain upon the introduction of the pill, as couples probably also switched from other methods that were even less effective.

fertility and education. Accordingly, they represented a natural experiment that can be used to assess the role that the pill played in educational outcomes.

2.1 Late-Adolescent Consent Laws and Access to Prescription Contraception: A Natural Experiment

While the pill diffused relatively quickly among American women, there were a number of state-imposed legal barriers that might have dampened the pace of diffusion among young women. In particular, oral contraceptives (unlike prophylactics such as the condom) were initially available only by prescription from a licensed physician. Based on either common law or explicit legislation, unemancipated individuals below the age of majority (historically 21 in most states[8]) were required to obtain parental consent for any medical services. While marriage served to emancipate minors in many states, this was not universally the case. Further, only a minority of women married below the age of 20 in 1960.[9] Thus, when oral contraceptives were introduced in America, parental consent would be required for the typical young woman wanting to "go on the pill" if she were under the age of 21.[10]

State laws governing the access to contraception among young women were loosened substantially over the late 1960s and early 1970s. This was generally accomplished through one of four means. Most commonly, state legislatures either reduced the age of majority or introduced legislation that allowed certain categories of "mature minors" the general capacity to consent to medical care. Additionally, minors were in some instances granted general medical consent through a judicial mature minor ruling, or were able to consent specifically for contraception based on family planning legislation.

Table 1 summarizes the status of state laws affecting single women in late adolescence

[8] The District of Columbia had legal restrictions that evolved in a similar manner to the U.S. States, and so for the purposes of this paper will also be counted as a "state."

[9] Based on data from the U.S. Census (Ruggles et al., 2004), in 1960 approximately 41% of women were married at the age of 19. The rate of marriage was, of course, lower at younger ages.

[10] In a number of states, pregnancy also emanicapted the minor. However, this is precisely the condition that the woman wishes to avoid by seeking contraception. Hence, I omit pregnancy as an emancipator from my discussion, although it plays a role in determining the abortion consent laws utilized as controls in my estimation.

(defined here as ages 18 and 19) between 1960 and 1979.[11] By 1977, all restrictions on the ability of such women to obtain oral contraceptives had been removed. Moreover, changes in access laws came overwhelmingly through reductions in the age of majority and the introduction of mature minor laws. Changes of this sort, as pointed out by Bailey (2004), were not typically undertaken with the explicit intent of granting young women access to contraception. Instead, they were brought about as part of a general movement of empowering young adults, a movement that was at least partially a response to the war in Vietnam. (The bulk of such changes came during a five-year period centered around 1972, the year in which the Twenty-Sixth Amendment was passed.) In fact, family planning legislation served as a basis for consent among late adolescent women in only two states.

Hence, the variation in state laws affecting access to contraceptives among late adolescents can, for the most part, be considered quasi-experimental in nature. Changing social norms (e.g., the feminist movement) and economic conditions may have shaped young women's schooling and fertility choices over the timeframe, increasing their desire to use the pill. But, the ability to obtain the pill unconditionally was granted in different states at different times for reasons largely unrelated to a desire for fertility control. As a result, a differences-in-differences estimation based on late-adolescent consent laws can plausibly identify a structural impact of the pill.

There might be, however, two concerns regarding the validity of these laws as measures of access to the pill. First, there is the question of how relevant parental consent laws were with respect to actual use of oral contraceptives, as well as fertility outcomes. This is addressed in the following subsection, which recapitulates evidence provided in other papers using similar measures of contraceptive access. The second concern regards the fact that in over two-thirds of the states late-adolescent own-consent for contraception was a by-product of laws governing the age of majority. In addition to medical consent, reaching the age of majority granted late adolescents other legal privileges reserved for adults. In particular, full adulthood established the right to contract, which might affect educational opportunities through other mechanisms, for example the ability to borrow.

[11] The legal summary presented in Table 1 differs slightly from what might be gleaned from Goldin and Katz (2002, Table 2). This is partially a consequence of the expanded set of legal souces that form the basis of Table 1, but might also reflect differences in opinion regarding the interpretation of certain laws. See the discussion in Appendix A, which also describes the sources I use to document the evolution of state reproductive laws.

This issue is taken up in Section 5, which considers the impact these consent laws, as well as an alternative contracting-based set of laws, had on the outcomes of men. The results presented there are consistent with the idea that the direct effects of late adolescent consent laws were female-specific, affecting male educational attainment only indirectly through female fertility.

2.2 Relevance of Consent Laws

The absence of an affirmative consent law did not necessarily prevent young women from obtaining the pill. As noted above, married women were considered emancipated in many states and so could consent for contraception. Further, particularly as sexual mores evolved during the 1960s, some young single women might very well be able to obtain their parents' consent in order to go on the pill. And, even without the requisite consent, highly motivated young single women were able to get access to the pill through various means. Noting these concerns, Goldin and Katz (2002) provide a number of pieces of cross-sectional and time-series evidence that suggest that parental consent laws were, indeed, well-correlated with actual use of the pill among adolescents. Nonetheless, the importance of these consent laws with respect to fertility outcomes is ultimately an empirical question.

The liberalization of consent laws should have mattered primarily in terms of the ability of young women to avoid unwanted pregnancies below 21, the age at which most women would typically otherwise gain access to the pill. Defining "early fertility" as having giving birth to a child at or below age 21, Figure 1 plots the rate of early fertility for American women born between 1935 and 1959, based on retrospective fertility histories in the June Current Population Survey (CPS). As is apparent, the rate of early fertility began to fall substantially starting with the 1941 birth cohort – the first to obtain pill access during late adolescence. Further, a steady rate of decline in early fertility is seen for the later birth cohorts that were increasingly likely to be able to consent for contraception by the age of 18.

Even so, there may have been secular factors, e.g. shifts in attitudes and opportunities, that drove this trend. To address this, Bailey (2004) utilizes a differences-in-differences framework to consider the impact of late-adolescent consent laws on early fer-

tility in large sample of American women. Despite a number of data-imposed limitations to the analysis, her findings indicate that access to contraception played a substantial role in the decline of early fertility. Pill access during late adolescence decreased the rate of early fertility by approximately 4 percentage points in her sample, which implies an 11.5% reduction in the likelihood of early childbearing.[12] Thus, there is strong reason to believe that the consent laws detailed above actually mattered a great deal.

3 The Pill and the Enrollment of College-Age Women

Raising a child is a time-intensive undertaking. This was particularly the case during the 1960s and 1970s when there were fewer extra-household institutions in place to help with child-rearing. Consequently, a woman's ability to build human capital might be limited for a number of years after giving birth. In particular, women desiring to pursue higher education, an endeavor that is also quite time-intensive, might be prevented from doing so in the event of an early birth. This implies that the increased ability of women to avoid early childbearing as a result of the pill should be reflected in their enrollment behavior.

Using data from the October CPS, Panel A of Figure 2 plots the rate of school enrollment of 20-22 year-old American women between 1969 and 1979.[13] These women would have turned 18 between 1964 and 1977, the period during which most of the consent laws governing late adolescents were changed. Panel B of Figure 2 depicts the share of these women who would have had pill access during late adolescence. Comparing the two figures one sees that the steepest increase in enrollment seems to have occurred during the period in which the bulk of late adolescents first gained access to the pill.

To assess the causal effects of late-adolescent consent laws on enrollment patterns, I utilize a differences-in-differences (DD) approach. Given the substantive effect of the pill on fertility by the age of 21 described above, I focus on the impact of pill access on

[12] Baily presents two sets of marginal effects from probit estimation: (1) a marginal effect evaluated at the mean of the distribution of explanatory variables, and (2) one computed as the average over the distribution of individual marginal effects. The number quoted from Baily in the text represents the latter form, which is also used as the basis for all of the marginal effects reported in this paper.

[13] To reduce the year-to-year variation the series is a 3-year moving average of enrollment rates spanning 1968, the first year in which data are available, to 1980.

enrollment patterns at the same age. Consider a woman i who lived in state s during late adolescence and turned 21 in year t. Her school enrollment status (E) at age 21 could be specified according to

$$\Pr\left(E_{ist}=1\right)=F\left(\mu_t+\mu_s+\delta' X_i+\beta P_{st}\right), \tag{1}$$

where μ_t and μ_s are year- and state-specific fixed effects, and X_i contains controls for race. State consent laws are captured by P_{st}, which indicates the ability of late adolescents to consent for contraception in state s at the beginning of the year in which the woman turned 18, i.e. year $t-3$. The presence of unobservables poses a problem for estimation of β only inasmuch as they are correlated with the measure of pill access. At the individual level, unobservables should not influence estimation of β, since an individual woman had little control over the access laws in her state.[14]

Despite the discussion in Section 2.1, it is possible that there were unobserved group-level attitudes toward education and fertility, as well as toward the role of women, that affected both outcomes and the structure of access laws. The year fixed effects should account for any secular evolution of attitudes and outcomes in the nation as a whole, while the state fixed effect accounts for any persistent differences among states. However, attitudes might have evolved differentially across states in a manner correlated with the timing of liberalization. Under the assumption that these preferences evolved in a smooth manner, including a state-specific trend, $\gamma_s t$, in (1) should substantially ameliorate this problem.

Fertility and schooling outcomes may also have been affected by other changes to reproductive law during the 1960s and 1970s. A number of states in 1960 had legislation preventing the sale and/or use of contraceptives. These "Comstock laws" were only gradually phased out over the 1960s, and two states maintained prohibitions on the sale of contraceptives to unmarried women of all ages until a 1972 decision by the U.S. Supreme Court. Moreover, by the end of the period in question a number of states introduced laws that also allowed single minors under the age of 18 ("early adolescents") to consent for contraception. Additionally, between 1970 and 1972 abortion on demand

[14] This highlights the importance of using a measure of access in attempting to ascertain the effects of the pill. Actual use of the pill (P_i^*) is likely to be co-determined with fertility and schooling preferences, which implies that if P_i^* were substituted for P_{st} in (1), individual selection on unobservables would bias the estimation of β.

became legal in 7 states, and after the Supreme Court Ruled in *Roe v. Wade* it became legal across the United States in 1973. Finally, conditional on the legality of abortion, there were a variety of consent restrictions governing the minors during this time period. Appendix A describes these laws and their evolution more fully.

Failing to account for the additional liberalizations to reproductive law might affect the estimation of β inasmuch as they represent observable differences in attitudes and policies among states. Since their timing is correlated with the introduction of late-adolescent contraceptive consent laws, omitting them might bias estimation of β if they have a similar structural impact on outcomes. Alternatively, since the temporal correlation is not perfect, omitting measures of the other liberalizations might lead to an attenuated estimate of β, as the year fixed effects and state trends would account for more of the secular increase in enrollment. To account for these effects, I create a vector R_{st} that includes:

1. An indicator of the ability of early adolescents to consent for contraception in state s in the beginning of year $t-5$ (i.e., the year in which woman i turned 16)
2. Indicators for legalized contraception in state s at the beginning of years $t-3$ and $t-5$,
3. An indicator of the ability of early adolescents to consent for abortion in year $t-5$,
4. An indicator of the ability of late adolescents to consent for abortion in year $t-3$, and
5. Indicators for legalized abortion in state s in at the beginning of years t, $t-3$ and $t-5$.[15]

The other changes to reproductive law might, in principle, offer an additional source of variation to assess the impact of fertility control on the human capital investments of young women. However, it is more difficult to justify them as quasi-experimental in nature. The Comstock laws, which only applied in 12 states in 1960, were clearly

[15] Note that R_{st} includes a measure of legalized abortion in the woman's 21st year (year t). A comparable measure for prescription contraception is excluded, since pill access in a woman's 21st year should have little effect on her ability to avoid births for much of the same year. The empirical results presented below are virtually identical to those obtained when such a measure is included.

repealed with the intent of making contraception more widely available. Out of the 26 states that introduced laws granting single early adolescents the ability to consent to the pill, 17 of them did so by means of contraceptive-specific legislation. With regards to abortion, 3 out of the 7 early legalization were based on repeals of earlier restrictive statutes. Finally, abortion consent laws were very specific to abortion and largely reflected deeply-held differences in attitudes between states. Therefore, it seems less likely that the variation in these other dimensions of reproductive law was uncorrelated with state-level preferences. Further, the primary focus of this paper is on assessing the impacts of the pill through the natural experiment in late-adolescent consent laws. Thus, while I include R in most specifications to alleviate any structural or attenuation bias otherwise induced, I suppress the coefficients in order to focus on the more "trustworthy" coefficient, β.

3.1 Estimation: The Effects of Late Adolescent Contraceptive Consent on Female Enrollment at Age 21

The October Current Population Survey (CPS) is the only large-scale source of data on enrollment the time in which most of the variation in pill access laws occurred, and hence will be used in implementing DD estimation. Due to the nature of the geographic information available in the CPS, two small changes must be made to the empirical specification, which is premised on knowledge of a woman's state of residence during late adolescence. First, since the CPS only provides information on the current residence of respondents a woman's residence at age 21 (r) must be used as a proxy for her residence at age 18 (s). This substitution might well serve to attenuate the estimate of β.[16] Second, during the early 1970s a number of states were grouped together by the CPS in various ways such that it is only possible to identify 21 consistent geographic units during the time-frame in question. Some of these "state-groups" consist of a single state, while others comprise between two and eight states. Using state-groups as the base geographic unit necessitates forming measures of reproductive law at a higher level of aggregation.

[16] Using r instead of s could also, in principle, lead to bias in the estimation on β if women's migration decisions during late adolescence are based in part on consent laws. However, it seems unlikely that contraceptive access played a large role in migration behavior, especially given that most states changed their consent laws over a relatively short time-span.

In particular for each state r in g, I construct

$$P_{gt} = \frac{1}{N_{gt}^{21}} \sum_{r \in g} N_{rt}^{21} P_{rt} \,, \tag{2}$$

where N_{rt}^{21} is the number of 21 year-olds in state in state r in year t and P_{rt} indicates the abilities of late adolescents to consent for contraception in state r in year $t-2$.[17] To the extent that the distribution of women in the CPS state-groups is representative of the underlying population distribution, P_{gt} should provide a reasonable, although noisy, measure of pill access for the average 21 year-old woman in state-group g in year t.

Modifying (1) to reflect this aggregation and including the additional controls described above, the final estimating equation is

$$\Pr\left(E_{igt} = 1\right) = F\left(\mu_t + \mu_g + \gamma_g t + \delta' X_i + \phi' R_{gt} + \beta P_{gt}\right) \,, \tag{3}$$

where R_{gt} is constructed in a similar manner to P_{gt} from the underlying state laws. Based on this specification, Table 2 presents the results of logistic estimation of the likelihood of enrollment for a sample of 21-year old women drawn from the October CPS between 1968 and 1979.[18] Ignoring for the moment variation in other forms of reproductive law and the state trends, column 1 presents a base estimate of the impact of contraceptive consent laws on enrollment. Despite the noise induced by aggregating consent laws to the state-group level, pill access had a statistically significant (at the 5.3% level) and positive association with the likelihood of enrollment.[19] The marginal effect of pill access

[17] The population weights N_{rt}^{21} are based on estimates from the U.S. Census Bureau of the resident population of 21 year-old females in each state from 1970–1980. Values for 1968 and 1969 are based on a linear backwards extrapolation of the 1970 and 1971 estimates. While the Census Bureau does not differentiate by gender in the estimates it provides, the distribution of females across states should follow the distribution of the total population rather closely.

[18] These bounds are chosen due to two data limitations. First, enrollment data from the CPS is only available starting in 1968. Second, including abortion consent laws implies the upper bound – see the discussion in Appendix A. Additionally, out of the original 14,951 observations, 43 women were recorded as being not enrolled even though their "major activity last week" was attending school. Further, 416 women were coded as "unable to work" or "other, including retired." These observations were dropped for the purposes of estimation, although their inclusion did not substantially alter the results.

[19] As Bertrand, Duflo and Mullainathan (2002) point out, serial correlation poses a substantial problem for DD estimation. To account for this, throughout the empirical analysis standard errors are based on one of their suggested solutions: clustering on the state-group (or state) level. Further, as noted above, all marginal effects reported in this paper are computed as the average across the distribution of individual marginal effects.

is roughly 2.6 percentage points, which corresponds to an increase in the enrollment rate of almost 10%.

In order to control for any pre-existing tendencies in enrollment patterns that may be correlated with the timing of the introduction of access laws, state-group specific trends are added to the specification, and the results are reported in column 2 of Table 2. Including state-group trends does not attenuate the coefficient on pill access. Rather, the point estimate and significance of β rises, possibly because intertemporal variation in outcomes within states is better accounted for with their inclusion.

Finally, I consider adding the measure of the other forms of liberalization to reproductive law, R, discussed earlier. As seen in column 3 of Table 2, including both state-group trends and R further increases the magnitude of the coefficient on pill access. In this final specification, the marginal effect of late-adolescent consent to contraception is 4.7 percentage points, corresponding to an increase of over 17% in the rate of enrollment. The estimated marginal effect estimated effect is statistically indistinguishable from Bailey's (2004) estimate of the effect of pill access on the likelihood of childbearing by the same age, which supports the role of early fertility as the proximate mechanism linking contraceptive access to schooling outcomes.

3.2 Schooling Versus Work

The empirical relationship established between school enrollment and contraceptive access is consistent with the idea that the pill allowed women to delay childbearing in order to invest in their education. However, early work experience might constitute an alternative means by which women build their skill set in order to augment their future wages. Klepinger, Lundberg and Plotnick (1999; KLP) establish that adolescent fertility had negative and significant consequences on both the schooling and early work experience of young women during the 1980s. Hence, it seems plausible that late-adolescent pill access during the period under consideration could have led to increases in both forms of human capital.

To explore this possibility, I consider the same sample of women used in the analysis of enrollment rates. In the CPS, respondents are coded according to their major activity

last week, which is based on their primary use of time. From this I construct the tripartite variable M_i, which indicates whether a woman's major activity was staying home, participating in the labor market, or attending school. A small but substantial number of women were enrolled even though their major activity was not attending school. As such, M_i might capture the deeper human capital investments of women, as well as their alternative uses of time.

The effects of late-adolescent consent to contraception are considered on the relative likelihoods of the mutually exclusive outcomes in M_i based on a multinomial logistic framework. The specification parallels that used in the analysis of enrollment, i.e. equation (3). Table 3 reports the estimated effects of the pill on the relative likelihoods of school and work, with staying home as the base category. According to the coefficient estimate presented in column 1, late-adolescent pill access seems to have a significant and positive effect on the relative likelihood of being at school. Based on the point-estimate, the marginal effect of pill access on the likelihood of school attendance, conditional on being either at home or at school, is 9.4 percentage points, representing 14% of the women not in the labor force.

By contrast, the estimate of the effect of pill access on the relative likelihood of participating in the labor force is entirely insignificant, as seen in column 2 of Table 3. Using a similar contemporaneous measure of labor force participation, Bailey (2004) considers the impact of late-adolescent contraceptive access on the labor supply of women at various points over the lifecycle. Her findings parallel the result described here: in Bailey's full sample late-adolescent pill access had no significant impact on the labor supply of 21-25 year-old women.[20] These results stand somewhat in contrast to the findings in KLP. This could reflect the fact that KLP construct a cumulative measure of early work experience, while the measure used both here and by Bailey is labor force participation measured in a single year. That is, schooling decisions tend to be more "sticky," while year-to-year variation in labor force participation within a state-group might swamp any effect that access to the pill might have. Late-adolescent pill access laws might also have affected women along a different margin than did the alternative

[20] When Bailey limits the sample college-graduate women, however, she finds a statistically significant and negative effect of the pill on the likelihood of labor force participation of 21-25 year-old women. This might be taken as evidence that, in addition to any direct effects due to early fertility, the increased certainty over fertility outcomes that the pill provided altered the educational aspirations of young women.

set of instruments for teenage fertility employed by KLP. Nonetheless, it seems that late-adolescent pill access had a greater impact on the ability of women to invest in their formal human capital than it did on their labor force participation in the sample considered here.

4 The Pill and Female College Completion

The results presented so far suggest that access to the pill enabled more women to attend college. This effect presumably operates through an increased ability to avoid unwanted pregnancies during late adolescence. That is, women that bore children due to their reliance on less efficient contraception in the absence of the pill would have to devote a substantial portion of their time to child-rearing and, as a result, not be able to go to college. However, the findings in Hotz and Miller (1994) suggest that the time intensity of childrearing by mothers declines as their children grow older. So, while there might be differences in college attainment based on pill access at younger ages, the differential might dissipate as women grow older. In this section, I extend the basic DD framework to account for this possibility, finding a substantial increase in college completion at the age of 30 based on late-adolescent access to the pill. Further, the evidence of a catch-up dynamic at higher ages is less robust, indicating long-term consequences of the pill for female college completion.

4.1 An Expanded DD Framework

To estimate of the educational effects of the pill over the lifecycle, the DD estimation framework needs to be modified slightly to account for heterogeneity in age. As a starting point, consider a general DD framework that estimates the effects of pill access on some measure of educational attainment H observed at a some uniform age $\hat{a}$ (above 21). For the purposes of exposition, suppose, for the moment that H is continuous. For a woman i who turned 18 in state s in year t and is observed at age $\hat{a}$, the standard DD specification would be

$$H_{ist\hat{a}} = \mu_t + \mu_s + \gamma_s t + \delta' X_i + \phi' R_{st}^{\hat{a}} + \beta P_{st} \,, \tag{4}$$

where the explanatory variables are equivalent to those used in (3). The exception is $R^{\hat{a}}$, which expands R to include measures of reproductive laws (e.g. legalization of abortion) for ages above 21.[21]

In specifying the rate of increase of H for ages above $\hat{a}$, I allow for non-parametric fixed effects, λ_{ta}, that capture any innate tendencies of women in each birth-year cohort t to increase their education as they grow older. In principle these non-parametric controls should also be allowed to vary by state. However, similar to the problem in standard DD modelling, including the full set of controls completely identifies variation between birth-year cohorts within a state. Further, even including a smaller set of state-specific non-parametric age fixed effects is not computationally feasible, given the large set of ages and states used in the empirical analysis below. Consequently, I assume that the manner in which within-state growth in H deviates from the age-path set by the common non-parametric fixed effects λ_{ta} can be adequately captured by a linear trend in age. In addition, I allow the parameters of the age function themselves trend over time.[22] Thus, growth in H for ages above $\hat{a}$ as a function of these basic controls can be specified as

$$\left.\frac{dH_{ista}}{da}\right|_{a>\hat{a}} = \lambda_{ta} + \lambda_s + \rho_s t, \tag{5}$$

where λ_{ta} is as described above, λ_s accounts for state-specific age growth reflecting idea that in some states women may be more likely to defer schooling to older ages, and ρ_s accounts for the rate at which educational attainment increases with age might be trending within a state.

In addition to the controls in (5), the rate at which educational attainment rises with age might vary systematically across racial groups in a manner not well-captured by the common non-parametric fixed effects λ_{ta}. Further, reproductive laws may affect the likelihood of returning to school due through their impact on lifecycle fertility. For simplicity, these effects are assumed to be linear, so that the fully specified equation for

[21] The exact measures of adult reproductive law used for empirical purposes are described in Section 4.2 below.

[22] Specifying the state-specific growth of H as a quadratic in age does not alter the empirical results to any meaninful degree.

the rate of increase of H at ages greater than $\hat{a}$ is

$$\left.\frac{dH_{ista}}{da}\right|_{a>\hat{a}} = \lambda_{ta} + \lambda_s + \rho_s t + \sigma X_i + \omega' \tilde{R}_{st} + \psi P_{st} \,,$$

where $\tilde{R}$ expands $R^{\hat{a}}$ further to include reproductive laws governing women at ages above $\hat{a}$. Putting this together with (4), we have

$$\begin{aligned} H_{ista} &= \mu_{ta} + \mu_s + \gamma_s t + \delta' X_i + \phi' R^{\hat{a}}_{st} + \beta P_{st} \\ &\quad + \left(\lambda_s + \rho_s t + \sigma' X_i + \omega' \tilde{R}_{st} + \psi P_{st}\right)(a - \hat{a}) \,. \end{aligned} \tag{6}$$

In this set-up, β captures the effect of the pill on H at age $\hat{a}$, while ψ reflects the differential in the rate of increase of H in age based on early access to oral contraceptives. Since early fertility is thought to prevent women from attending school at younger ages, β should continue to be positive. A negative ψ would reflect the return of such women to school as their children grow up.

4.2 Estimation: The Effects of Late Adolescent Pill Access on Adult Female College Completion

While the CPS could be again used to consider the effects of the pill on adult women, the nature of the geographic data it contains become more problematic in such an analysis. As discussed above, only the current state of residence of the respondents are reported. Americans are highly mobile, especially those with higher completed education, which implies that the correlation between the current residence of adult women and their residence during late adolescence substantially diminishes with age. This means that using the geographic measures in the CPS should attenuate any estimates of the effects of late-adolescent contraceptive access on completed schooling. This should particularly be the case for women at older ages, making it more difficult to assess the relationship between adolescent pill access and the age-pattern of educational attainment. As a result, I estimate (6) based on data from the Census Public Use Microsamples (PUMS), in which respondents are coded according to their state of birth. The birth state of a woman (b) should correlate rather well with her state of residence at age 18 (s).[23] In fact, according

[23] In addition to its fixity over the lifecourse, state of birth has an additional advantage over state of current residence: geographic selection on unobservables, which might bias the estimation of the pill

to data from the 1970 Census, over three fourths of late adolescent women still lived in their state of birth.

From the 5-percent-count PUMS samples of the 1990 and 2000 Census, I draw data on women born in America between 1939 and 1959.[24] The lower bound is chosen so that the earliest birth cohort would reach the age of 21 in 1960, the year in which the pill was approved by the FDA. By minimizing inter-cohort variation in terms of the availability of the pill at age 21, this should allow access during adolescence to be more relevant. The upper bound is chosen due to the sample restriction that results from including abortion consent laws in the specification (see Appendix A). Each birth cohort is observed twice, once in 1990 and again in 2000. In the earlier census year, this implies that ages range between 30 and 50; adding 10 to each gives the range of ages in 2000.[25]

Due to the large sample size involved and the numerous controls required by (6), estimation using the full sample of women is very computationally costly. As a result, I aggregate the data and undertake my analysis using variable means for each state-of-birth/year-of-birth/age cell.[26] The dependent variable of interest is the proportion of women with a bachelors degree, which I denote by C_{bta}. Since this is a continuous measure, a linear specification that parallels (6) is

$$\begin{aligned} C_{bta} &= \mu_{ta} + \mu_b + \gamma_b t + \delta' \tilde{X}_{bt} + \phi' R^{\hat{a}}_{bt} + \beta P_{bt} \\ &\quad + \left(\lambda_b + \rho_b t + \sigma' \tilde{X}_{bt} + \omega' \tilde{R}_{bt} + \psi P_{bt}\right)(a - 30) \ , \end{aligned} \tag{7}$$

where $\tilde{X}_{bt}$ denotes the racial and ethnic composition of state/year birth cohort bt.[27] While $\tilde{X}_{bt}$ includes the same measures of racial status as used in the CPS estimation (African-American and "other non-white", with white as the omitted category), it has

acess laws, is avoided.

[24] The samples I use are drawn from the Integrated Public Use Microdata Series (Ruggles et al., 2004).

[25] In the Census, respondents' ages are given as of April of the survey year. Thus, women aged 50 in the 1990 Census reached that age primarily in 1989. This implies that these women were mostly born in 1939, which corresponds to the lower bound on birth years. Similarly women at the age of 30 at the 1990 Census were primarily born in 1959, the upper bound.

[26] The means are computed using the weights provided by the Census in order to make them representative of the underlying population.

[27] Analyzing the log-odds ratio for state-of-birth/year-of-birth/age cell using logistic regression yields results that are both qualitatively and quantitatively similar to those obtained using a regression with simple proportions. The latter results are presented here for ease of exposition.

been expanded to include proportion of Hispanics in cohort bt.[28] The vector $R^{\hat{a}}_{bt}$ includes the measures of reproductive laws detailed in Section 3 above, but has been expanded to include:

1. An indicator of the legality of contraception in state b at the beginning of years t and $t+4$, i.e. the years in which the woman turned 21 and 24, respectively.

2. Indicators for legalized abortion in state b at the beginning of year $t+4$.

In principle, it is possible to add reproductive laws at still higher ages to $\tilde{R}$. However, there is so little variation in such measures that they are essentially collinear with the other explanatory variables. Consequently $\tilde{R}$ is identical to $R^{\hat{a}}$ for the purposes of estimation.

Column 1 of Table 4 presents the results of regression based on (7) for American women born between 1939 and 1959.[29] Focusing first on β, the estimated coefficient is positive and highly significant. Further, the point estimate indicates that women with late-adolescent pill access are approximately 0.89 percentage points more likely to have obtained a bachelors degree by the age of 30, which represents an increase in the rate college completion of just over 4%. For posterity, column 2 of Table 4 replicates the analysis using a younger sample of women equivalent to that used in the analysis of enrollments in Section 3.1. The point estimate of β rises slightly, but is statistically identical to that reported for the full sample. This suggests that the effect of pill access during late adolescence was relatively stable across birth cohorts.

The estimated effects of pill access on the enrollment of women at the age of 21 reported earlier are substantially higher than the estimates of college completion by age 30 reported here. This might indicate that a number of women without late-adolescent pill access were able to return to school before the age of 30. Indeed, the point-estimates

[28] As constructed, the vector of racial measures used in the Census PUMS estimation is constant within a state/year birth cohort. Ideally, it would also vary between the cohort observed in the 1990 Census and that observed in the 2000 Census, since there might be compositional differences in the samples. However, the coding of race changed between the two surveys, making it difficult to construct a consistent set of racial measures. In practice I use the means from the 1990 Census as the measure of racial characteristics for the state/year birth cohort at each of the two survey dates. Given the relatively large (1 in 20) sample coverage, this should provide a reasonable proxy for the racial composition of an equivalent cohort ten years later.

[29] In addition to clustering on state of birth, standard errors for estimation based on (7) are weighted to account for heteroskedasticity in the precision of the state-of-birth/year-of-birth/age cohort means.

of ψ reported in Table 4 are negative, which lends credence to the idea of a "catch-up" dynamic. However, the estimated coefficients are quite insignificant, which implies that this effect is not very strong at the older ages considered here.[30] Even taking the point estimates seriously, women without early pill access would not catch up to women with early access until their late 50s. Thus, it seems that for women over the age of 30, the differences in college completion due to early pill access were quite persistent.

5 Bringing the Men Back In

The evidence relating contraceptive consent laws to female outcomes indicates that, by decreasing early fertility, the pill enabled more women to invest in their higher education. However, early female fertility might also have consequences for their male partners: men usually contribute money, and very often contribute a substantial amount of time, to raising their children. Since college is financially costly and time-intensive, the ability of a young male to invest in higher education might also be constrained after his partner gives birth to a child. Thus, through its effects on female fertility, the pill may have indirectly affected the educational outcomes of males.

As noted by Goldscheider and Kaufman (1996), there is a paucity of data that can be used to analyze the connection between men and fertility.[31] In fact, all of the data in which male outcomes can be linked to female fertility come from surveys of married couples. The consent laws used to identify the impact of the pill in this paper primarily applied to contraceptive access among young *single* women.[32] This limits the value of late-adolescent contraceptive consent laws in assessing the impact of pill-induced reductions in childbearing among young married woman on the outcomes of their husbands.[33] Even so, late adolescent contraceptive consent laws might have implications for the sexual

[30] Even when using the "less conservative" standard errors clustered on state-of-birth/year-of-birth cells, the p-value of the estimate of ψ is 0.23. Higher-order polynomials in age did not remedy the lack of differential effect of late-adolescenct pill access among these older women.

[31] The heading of this section is drawn from the title of the Goldscheider and Kaufman paper.

[32] In the early 1970s, over 70% of states followed a doctrine of marital emancipation (Paul, Pilpel, and Wechsler, 1974).

[33] There is little reason to believe that most marital formation and fertility is the result of the shotgun effect described below. As a result, data for married couples as a whole are unlikely to substantially reflect differences in early pregnancies due to access to contraception. However, considering the consequences of pill access in a dataset in which shotgun marriages are identifiable might prove to be a fruitful endeavor for future research.

partners of young unmarried women. In particular, a pregnancy to a young unmarried couple may induce male commitment to the mother and child. This could arise through a "shotgun marriage" effect, which might be the product of an endogenous household formation decision or a socially-imposed norm.[34] Additionally, a male may provide for his child even without marrying the mother; this might also arise endogenously or be the product of legally-imposed child support obligations.

While there are no data linking single males to their actual female partners during adolescence, the analysis undertaken in Section 5.2 below provides evidence that male educational outcomes are affected by the contraceptive consent laws governing their *likely* partners. First, however, I rule out a direct effect of contraceptive consent laws on male educational outcomes. This further validates the use of consent laws as proxies for female access to contraception and indicates that their effect on males is indirect, operating through early female fertility.

5.1 Consent for Contraception or the Ability to Contract?

As discussed in Section 2, the ability of late-adolescents to consent for contraception (P) was obtained primarily through reductions in the age of majority. Thus, P might in actuality be measuring other benefits conferred upon reaching full adulthood, rather than access to the pill per se. In particular, adults are empowered to sign contracts. As a result, a reduction in the age of majority should have allowed late-adolescents to take out educational loans in their own right, which alone might induce a higher rate of college completion and attendance. If this is the case, then the empirical approach undertaken in this paper might not be legitimately measuring the impact of oral contraceptives on educational outcomes.

Supposing that P is more properly a measure of the ability to contract than of the access of young women to contraception, it should also directly affect the educational outcomes of males. Considering this possibility, the Census PUMS estimation of BA completion in (7) is replicated for males and the results are presented in column 1 of Table 5. The estimates of both β and ψ are small and entirely insignificant, which

[34] Akerloff, Yellen and Katz (1996) provide evidence of a strong shotgun marriage norm during the 1960s. While they attribute part of the subsequent erosion in shotgun marriage to the increased availability of contraception and abortion, the decline was neither immediate nor complete.

indicates that the laws encoded in P did not have a substantial direct effect on male college completion.

However, this result might obtain even if the true mechanism driving the rise in education was contracting capacity, due to the fact that P is not a perfect measure of the ability to contract for males. In a number of states the age of majority was historically different for males than for females. Further, part of the variation in P is driven by changes in laws governing medical and contraceptive consent. To account for this, I construct a pure measure, A, of the ability of late-adolescent males to sign contracts as adults, and re-estimate (7) for males with A_{st} substituted in place of P_{st}.[35] As seen in column 2 of Table 5, the estimated coefficients on the ability of late-adolescent male to contract are insignificant and, in fact, have the "wrong" sign. Further, in results not reported here, the direct effects of both P and A on male enrollment status are also insignificant. Thus, while the capacity to contract might truly have had an effect on educational outcomes, this is not reflected in the data.

5.2 The Pill, Female Fertility, and Male College Completion

The direct impact of contraceptive laws on college attendance and completion seems to be female-specific, which corroborates the notion that they affected female outcomes through the proximate mechanism of late-adolescent fertility. The lack of effect on the outcomes of men might seem puzzling, given the discussion above regarding the potential consequences of female fertility for male educational attainment. This puzzle can be relatively easily resolved upon recalling the tendency of couples to form in which the male is older than the female.[36] That is, males should not be affected by the contraceptive access of women their own age, but rather by that of their slightly younger partners.

All of the available survey data only provide information on a man's current spouse, who might be an entirely different woman than his partner during late adolescence. Further, they provide no information on previous marriages or on unions other than marriage. Utilizing a sample of men and their actual spouses should serve to attenuate

[35] In this specification R is excluded, since the null hypothesis is essentially that reproductive laws had no effect on educational outcomes. Including R, however does not alter the insignificance of the estimated effects of contracting ability.

[36] Evidence for this phenomenon is presented below.

estimation of the effects of contraceptive consent laws on male outcomes. But, given the endogeneity of partnering decisions, various selectivity biases might also arise. The most problematic of these is a result of the well-established tendency for couples to sort on education: women who were able to complete college as a result of the pill may simply have married more educated men.[37] Hence, a positive partial correlation between an adult male's educational attainment and his wife's late-adolescent access to contraception could result solely as a result of the process of assortative mate selection later in life.

To avoid these biases, I analyze the likelihood of college completion for a male in relation to the contraceptive consent laws governing a pool of his *potential* partners during his college years.[38] In particular, I use the consent laws from the man's state of birth that would have been applicable to the laws governing slightly younger females in the same state.[39] Empirically, this amounts to re-estimating (7) for males using leaded values of the measures of reproductive laws governing females: $P_{b,t+g}$ and $\tilde{R}_{b,t+g}$ are substituted in place of P_{bt} and $\tilde{R}_{bt}$, where g measures the gap in age between males and their potential partners. Note that this implies shifting the span of male birth cohorts according to g. For example, when g equals one I use males born between 1938 and 1958, since the underlying laws are applicable to females born between 1939 and 1959. As discussed above, the bounds on female birth cohorts are necessary to properly estimate the effects of the late adolescent contraceptive consent laws, while controlling for changes to reproductive law. Incorporating these two changes, the estimating equation becomes

$$\begin{aligned} C_{bta} &= \mu_{ta} + \mu_b + \gamma_b t + \lambda_b + \delta' \tilde{X}_{bt} + \beta P_{b,t+g} + \phi' \tilde{R}_{b,t+g} \\ &\quad + \left(\lambda_b + \rho_b t + \sigma' \tilde{X}_{bt} + \psi P_{b,t+g} + \omega' \tilde{R}_{b,t+g}\right) [a - (30+g)] \ . \end{aligned} \tag{8}$$

Table 6 presents the results of estimating (8) using reproductive laws led by one, two, and three years. The estimates of β are positive and significant in columns 1 and 2, which correspond to leads of the same magnitude. While the sign remains positive, the estimate

[37] Mare (1991) provides a thorough review of asssortative matching on education in the context of American marriage patterns.

[38] As in Section 5.1, all of the findings on male college completion here also extend to the enrollment of college-age males. However, I do not report the latter set of results since they do not provide much additional insight to those presented here.

[39] As with females, a large majority of males tend to spend their college years in the state in which they were born, which again justifies the coding of laws according to birth state. Any migration only serves to attenuate, rather than bias, the estimated coefficients.

of β in column 3 just fails a test of significance at conventional levels (with a p-value of 0.107). Increasing the gap to more then three years further reduces the magnitude and significance of the estimated coefficients. The point estimates reported across the three columns imply an effect of the leaded laws on the likelihood of male BA attainment that is between 0.66 and 0.83 percentage points. This corresponds to a 2-3% increase in the rate of college completion. As with females, there is little evidence of a catch-up dynamic: in none of the three columns is the estimate of ψ even close to statistically significant. Thus, the increases in male college completion that are associated with the laws governing the contraceptive access of younger females in their birth state seem to be more or less permanent.

I also consider the relationship between female BA attainment and $P_{b,t+g}$, which serves as an additional robustness check regarding the relationship between female responses and the status of laws. If the legal changes were indicative of a shift in underlying preferences over fertility and education, then leaded values of the laws should have some predictive power in explaining female outcomes. The results, however, indicate that this is not the case. Leading the laws by even one year results in coefficient estimates that, while of the right sign, are insignificant at conventional levels.[40] This adds to the case made in Section 2.1 that the variation in late-adolescent consent laws was quasi-experimental in nature, rather than a reflection of differentially evolving group-level preferences.

Thus, late adolescent consent laws seem to affect female educational attainment solely in an "age-appropriate" manner, and have no such direct effect on men. Male educational attainment is only affected by the consent laws when they are applied to reflect a gap in age between males and the females directly affected by the laws. The significance and magnitude of the estimate for males is highest when the late-adolescent contraceptive access are applied to males with a two year lead, which gels rather nicely with two pieces of evidence on the typical age gap between partners. First, according to data maintained by the U.S. Census Bureau, the male-female difference in the age at first marriage averaged approximately 2.3 years over the second half of the twentieth century, and remained relatively stable over that timespan. Second, Moore and Driscoll (1997) consider data from the 1995 National Survey of Family Growth to examine the age

[40] Since the coefficient estimates of β and ψ are insignificant across the board when using $P_{b,t+g}$ (and, of course, including $\tilde{R}_{b,t+g}$ and the other controls) for any value of g, I do not report them in table format.

difference between teenage women and their first sexual partners. They find that over half of women in their late teenage years initiated sexual activity with a male partner who was no more than two years older.[41]

Taken together, the following results strongly suggest that reductions in the early fertility of females, which resulted from better access to the pill, led to increases in male educational attainment: (1) late-adolescent contraceptive consent laws affected female fertility during late adolescence, as well as their college completion and attendance; (2) the laws have no direct, age-appropriate effect on male educational attainment; and (3) when a gap in age is allowed between the males and the females to which the laws directly applied, the maximal effect of the laws on males occurs when the gap is set to a value that resembles the "typical" male-female age difference in a young couple. The relationship between male educational attainment and the contraceptive access of their potential partners is established only indirectly. Based on the discussion at the start of this section, a mechanism that connects them is that "unwanted" (in the sense of being mis-timed) early female fertility constrained the educational investment ability of their male partners. That is, men had to work to support their children (and possibly their partners), which limited their schooling opportunities. Alternative explanations are, of course, possible. For example the increase in female educational attainment due to improved contraceptive access might have had positive social-network effects on that of their potential partners. Regardless of the proximate mechanism, the connection between female contraceptive access and male schooling represents a relatively novel contribution to the understanding of the effects of early fertility.

6 Conclusion

The leading quotation for this paper is largely derived from the introduction of Watkins (1998) who goes on to say that "[t]he image of the oral contraceptive as revolutionary persists in popular culture, yet the nature of the changes it supposedly brought about has not been fully investigated" (p. 1). This endeavor has only recently been taken up by economists, starting with Goldin and Katz (2002), who build a case for the "power of

[41] While the women considered in this paper were born two decades earlier than those considered by Moore and Driscoll, a similar age pattern of sexual activity might be expected to prevail among them, particularly given how stable the median age gap in first marriage was during the interim.

the pill" in terms of its effects on the career and marriage decisions of college-graduate women. The findings I present here suggest that the pill also had powerful consequences for investments in higher education among both women and men.

My results rely primarily on the relationships between measures of educational attainment and late-adolescent medical and contraceptive consent laws during the 1960s and 1970s. Goldin and Katz have empirically related female use of contraceptives to consent laws, and in this paper I show that their direct association with educational attainment is female-specific. Further, the nature of the legal changes, as well as empirical evidence that I provide, indicates that they were largely unrelated to shifts in preferences or any pre-existing tendencies in outcomes. As a result, the variation in late-adolescent consent laws is strongly justified as a natural experiment in access to contraceptives. In the analysis of educational outcomes, the local average treatment effect identified is presumably that of oral contraceptives on women who a) desired to use the pill to delay their fertility and invest in their human capital; and b) were unable to obtain the pill with a parental consent law in place. Laws requiring parental consent were not necessarily binding for all single minors and would not be applicable to late-adolescents who were married. Consequently, the effects of the pill are likely to be understated, relative to the total benefit it provided, when consent laws are used to provide identification.[42]

Despite the potentially limited subpopulation to which the consent laws applied, their effects turned out to be substantial even in the larger group of women in which their effects are considered. Based on my point estimates, unrestricted access to contraceptives during late-adolescence implied an increase of 17% in the (total) average enrollment rate of 21-year-old women and a 4% increase in average female BA completion by the age of 30. The marginal effect of late-adolescent contraceptive consent on a woman's likelihood of enrollment was equivalent to the difference in rates between a state just above the mean (e.g. Ohio) and one at the bottom of the enrollment distribution in 1970 (e.g. Kentucky or Tennessee). Further, out of the approximately 43 million women represented in the analysis of college completion, almost four hundred thousand more of them were able to finish a BA by the age of 30 as a result of unrestrictive contraceptive consent law. When taken in combination with the findings by Bailey (2004) linking late-adolescent

[42] The imprecise geographic measures used in assigning consent laws further attenuates any estimated effects of contraceptive access.

contraceptive access to increases in the labor force participation of adult women, this implies considerable female labor-market gains as a result of access to oral contraceptives.

The pattern of evidence I present also indicates that the introduction of the pill proved beneficial for male educational attainment. The likelihood of a male completing college rose with better access to oral contraceptives among the slightly younger women constituting a pool of his potential partners during the sample period considered. Further, the increase in male college completion that resulted from an unrestrictive consent law was only slightly smaller than that for the corresponding women directly affected by the law.

Due to the nature of the data used in this paper, all of the major empirical results relied on reduced-form relationships between educational outcomes and contraceptive consent laws, rather than a two-stage procedure tracing the effects of the consent laws through early fertility. However, the magnitude of my estimated marginal effect of an unrestrictive late-adolescent consent law on enrollment at age 21 is comparable to Bailey's estimate of the effect an unrestrictive law on likelihood of fertility by the same age. This indirect least squares comparison substantiates realized fertility as being the likely proximate mechanism between access to oral contraceptives and educational attainment.

The behavioral changes witnessed in response to better availability of contraception indicated a latent desire among many women to avoid early fertility and invest in education. To be sure, quite a number of other factors may have shaped the underlying preferences and returns, for example the feminist movement and a changing economy. However, by substantially reducing the likelihood of an unwanted pregnancy, the pill acted as a catalyst in allowing women to implement a more optimal fertility plan. My findings support previous research that has shown the negative effects of undesired adolescent fertility on the educational attainment young women (see, e.g., Klepinger, Lundberg and Plotnick, 1995). By contrast, the consequences of early fertility for men are rather less well understood in the economics literature. The results I present here strongly suggest that unintended adolescent female childbearing was detrimental to male educational attainment. The indirect manner in which these results were established is largely due to the fact that, as Goldscheider and Kaufman (1996) point out, there is a scarcity of data in which men can be linked directly to their family histories. Better data need to

be collected, and more work needs to be done, to bring men back into the analysis of fertility.

Appendix A – Additional Detail and Discussion of State Laws

Sources

The following secondary sources were used in order to determine the access among minors' to contraception: Pilpel and Wechsler (1969 and 1971); U.S. Department of Health and Welfare (1974, 1978); Paul, Pilpel and Wechsler (1974, 1976); Paul and Pilpel (1979). For the most part, each of these gives snapshot of laws at the time of writing. In many instances there is no indication of the date as to when the governing jurisprudence came into effect. Additionally, the secondary sources do not always reflect the existence and content of any previous or superceded law. Some of the gaps and missing histories were filled in using the annotated codes available on-line at lexis-nexis. For the remainder, librarians at numerous state law libraries were very helpful in providing the text of session laws and historical sections of code. These librarians were also helpful in interpreting certain ambiguities in jurisprudence, as was Elizabeth Nash at the Allan Guttmacher Institute. In addition to the secondary sources listed above, my understanding of abortion laws is based on the following secondary sources: Merz, Jackson and Klerman (1995); Merz, Klerman and Jackson (1996); Bitler and Zavodny (2002); Donohue and Levitt (2002). Taken together, the secondary sources proved sufficient for coding the abortion access variables used in this paper.

Comstock Laws

A few states repealed or revised the interpretation of their legislation prohibiting the sale of contraceptives in the early 1960s: Illinois in 1961; and Indiana and New Jersey in 1963. In a 1965 decision (*Griswold v. Connecticut* [381 US 479 (1965)]), the U.S. Supreme Court struck down the ban on the *use* of contraceptives in Connecticut – the only state with such a law in place. While this did not necessarily apply to the sale of contraceptives, the remaining states with prohibitions on contraceptive sales took the cue from *Griswold* and revised or repealed their laws by the end of the decade: Louisiana, Minnesota Nebraska, New York and Ohio in 1965; Missouri in 1967; Wyoming in 1969; and Mississippi in 1970. Nonetheless, since the *Griswold* decision was largely based on a doctrine of marital privacy, two states (Massachusetts and Wisconsin) continued to maintain prohibitions on the sale of contraceptives to non-married individuals. These laws were not invalidated until 1972 when the U.S. Supreme Court decision ruled in *Eisenstadt v. Baird* [405 US 438 (1972)].

Contraceptive Access Among Late Adolescents

Table 1 gives the legal history of the contraceptive laws affecting late adolescents. As noted in the main text, the legal history given in there differs from what might be gleaned from Goldin and Katz (2002, Table 2). This largely reflects differences of opinion regarding the coding of certain types of state laws. In particular, Goldin and Katz count certain unrestrictive contraceptive consent laws specific to state health and welfare (H-W) departments as amounting to access to the pill. However, these types of law do not seem as far-reaching as, say, the introduction of a mature minor law, as they affect a relatively small and potentially non-representative group of women. Additionally, more detailed information on H-W law is available in the sources listed above for certain large, "over-researched" states (e.g. New York and California). As a result, a measure of access with H-W laws based solely on those sources would be in some sense unbalanced, which might bias estimation. Constructing a balanced measure of contraceptive access that included H-W laws is, in principle, possible. However historical records of H-W policies are sparse for many states and not as readily obtained as legislative histories. Because of this limitation, and the fact that they might non-representative in their impact, H-W laws are not included in my measure of access to contraception.

Further discrepancies between my coding of legal histories and that in Goldin and Katz might have arisen due to additional differences in opinion on the interpretation of certain laws. However, there do seem to be a number of errors in their coding that may be a function of the smaller set of source materials that they utilize – primarily the secondary sources listed above. Thus, to the extent that the information I present in Table 1 is based on an expanded set of sources, it can be considered as an update to the summary presented in Goldin and Katz.

Contraceptive Access Among Early Adolescents

By 1980, the following 17 states changed their laws to allow contraception by women younger than 18 through family planning legislation: Illinois in 1969; Colorado, Maryland, and Tennessee in 1971; Florida, Georgia, Kentucky, and Virginia in 1972; Maine in 1973; Alaska, the District of Columbia, Delaware, Idaho, and Montana in 1974; California in 1975; North Carolina in 1977; and Hawaii in 1979. In the following 7 states, access to contraception was allowed under a legislative or judicial mature minor doc-

trine: Kansas and Mississippi[43] in 1970; Alabama, New Hampshire and Oregon in 1971; South Carolina in 1972; and Arkansas in 1973. A Minnesota court ruled in 1976 that a pregnancy consent clause for minors applied also to contraception. Finally, the Arizona attorney general ruled in 1977 that physicians and family planning agencies would not be held liable for providing contraception to minors without parental consent.

Legalized Abortion

While abortion had been technically legal in a number of states during the 1960s, it was limited to save the life or health of the woman. The legislatures of Alaska, New York, and Hawaii repealed their restrictive abortion laws in 1970. Additionally, a referendum in late 1970 legalized abortion on demand in Washington. While California's abortion law was not completely rewritten until 1972, it is generally agreed that abortion on demand became legal after the Supreme Court of California held the state's restrictive abortion law unconstitutional in late 1969. Further, courts overturned restrictive statutes in Vermont and New Jersey in 1972. In 1973, the U.S. Supreme Court's decision in *Roe v. Wade* [93 S. Ct. 1409 (1973)] and *Doe v. Bolton* [93 S. Ct. 739 (1973)] resulting in the legalization of abortion on demand in the remaining states.

Abortion Consent[44]

At the time at which abortion became legal in most states, women in their late adolescence were considered adults for the purposes of medical care, based on either the age of majority or a mature minor law, as described above. By 1977 this was the case in every state.[45] Additionally, in a number of states women under the age of 18 could consent to abortion based on a mature minor doctrine or legislation giving pregnant minors consent to medical care.

However, in the years following *Roe* quite a few states passed abortion-specific laws with the explicit intent of preventing minors under the age of 18 from obtaining abortion without the involvement of their parents. These often served to reinforce the lack of

[43] The mature minor doctrine in Mississippi actually came at an earlier date. However, it became effective in terms of contraceptive access only after the 1970 repeal of Mississippi's Comstock law.

[44] For the sake of brevity, a full detailing of the evolution of abortion consent laws is not presented here, as it was a somewhat convoluted process in certain states. However, the coding of the abortion access variables used in this paper reflects my best understanding of the law as gleaned from the sources indicated above. Full documentation is available upon request.

[45] The two states in which late adolescents had first obtained contraception based on family planning law (the District of Columbia and Florida) had additionally lowered their age of majority by 1976.

statutory (or common-law) capacity to consent, but in a number of instances nullified the ability of otherwise-emancipated early adolescent. The status of such laws became unclear after a July 1976 Supreme Court decision, which found Missouri's abortion-specific parental consent requirement unconstitutional in *Planned Parenthood of Central Missouri v. Danforth* [96 S. Ct. 2831 (1976)]. In its ruling the court emphasized that this ruling was not intended to imply that "every minor, regardless of age or maturity may give effective consent for the termination of her pregnancy" (p. 73). The Court gradually clarified its position in a series of rulings in the years following the *Danforth* decision. In the meantime states introduced various forms of abortion-specific involvement laws, some of which withstood legal challenges and others which did not. Thus, starting in late 1976 it becomes somewhat difficult to determine the ability of a minor under the age of 18. This results in the sample restrictions employed in the empirical analysis.

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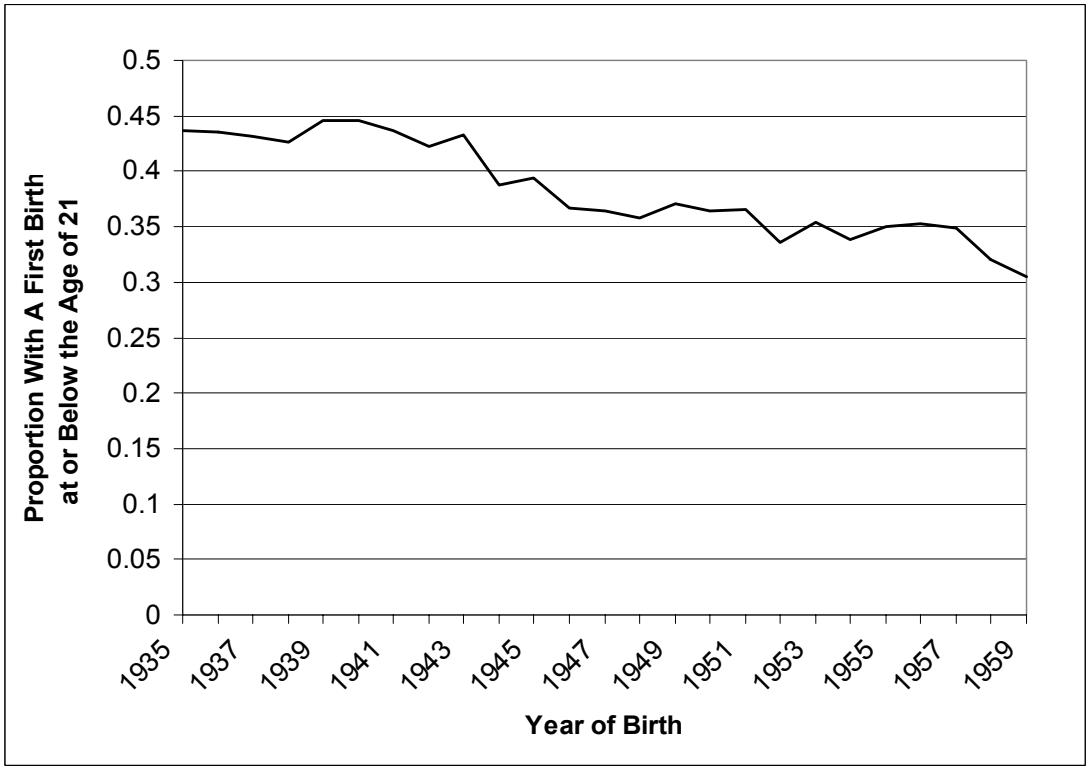
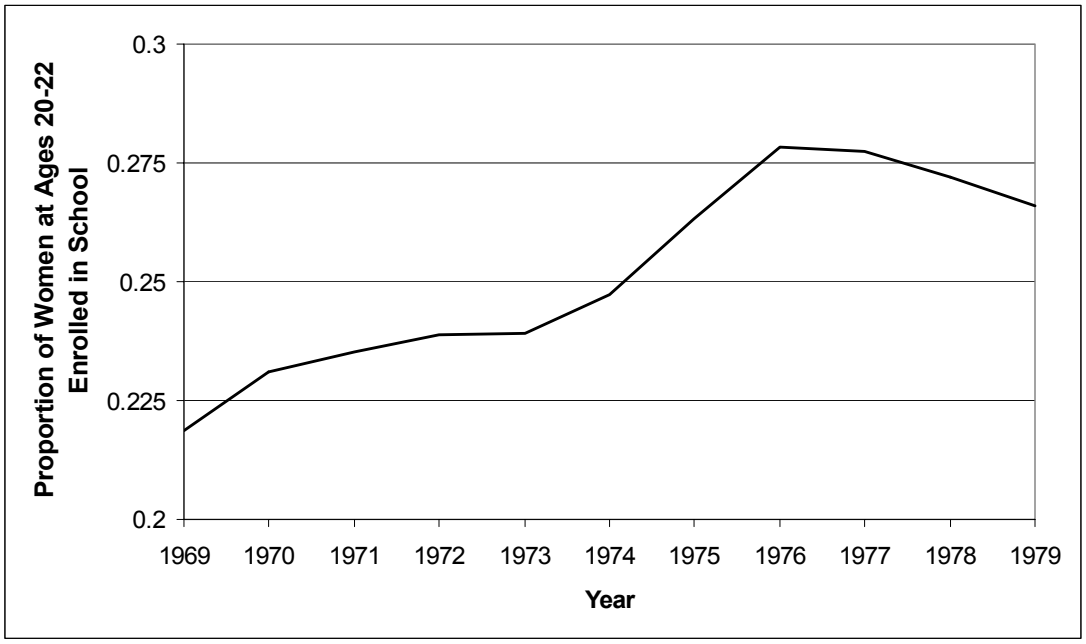
Figure 1: Early Births Among American Women Born 1935-1959



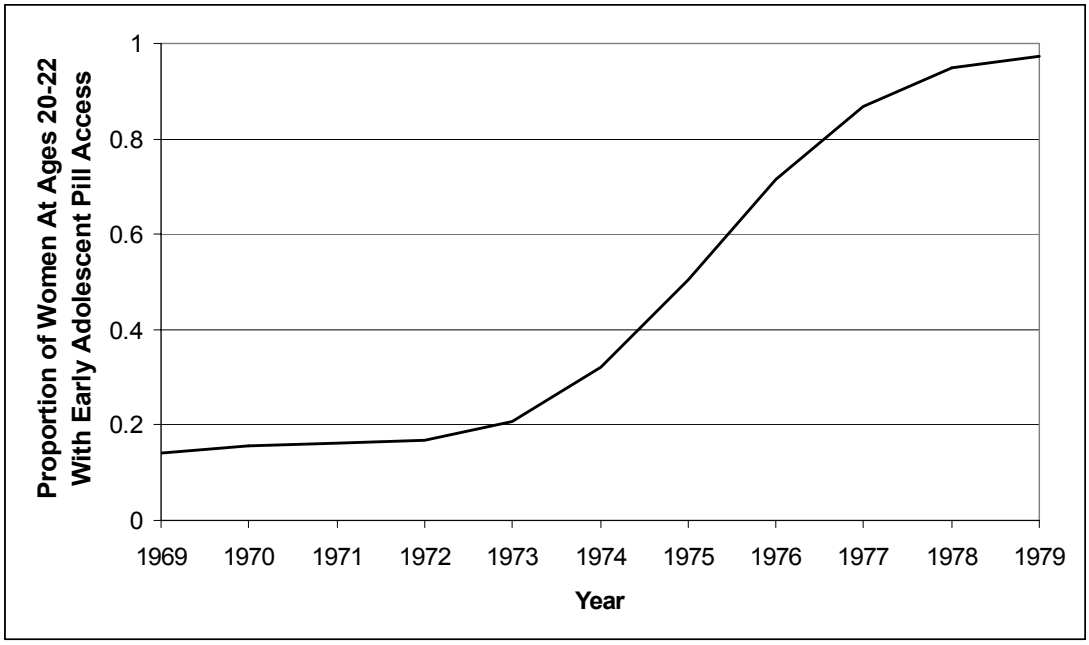
Source – June CPS 1980, 1985, 1990, 1995

Figure 2: School Enrollment Rates and Late-Adolescent Pill Access, 1969-1979
Panel A: Enrollment of Women at Ages of 20-22



Source – October CPS 1968-1980
Notes – The plotted series represents a 3-year moving average of attendance rates centered on the year indicated in the figure.

Panel B: Consent Laws



Sources – October CPS 1968-1980; Population Estimates from the U.S. Census; Contraceptive Laws as Described in Appendix A.
Notes – The plotted series represents a 3-year moving average of enrollment rates centered on the year indicated in the figure and is constructed as indicated in Section 3.1.

Table 1
Access to Contraception Among Single Women in Late Adolescence, 1960-79

State	Age of Majority for Females, 1960	Access Among Single Women at Ages 18-19	
		Year First Obtained	Type of Access (If Not Based on Age of Majority)
Alabama	21	1971	Mature Minor Legislation
Alaska	19	1960	
Arizona	21	1972	
Arkansas	18	1960	
California	21	1972	
Colorado	21	1971	Mature Minor Legislation
Connecticut	21	1971	Mature Minor Legislation
Delaware	21	1972	
DC	21	1974	Family Planning Legislation
Florida	21	1972	Family Planning Legislation
Georgia	21	1971	Mature Minor Legislation
Hawaii	20	1975	
Idaho	18	1960	
Illinois	18	1961*	
Indiana	21	1973	
Iowa	20	1973	
Kansas	21	1970	Judicial Mature Minor Ruling
Kentucky	21	1965	
Louisiana	21	1972	
Maine	21	1972	
Maryland	21	1971	Mature Minor Legislation
Massachusetts	21	1974	
Michigan	21	1972	
Minnesota	21	1974	
Mississippi	21	1970*	Mature Minor Legislation
Missouri	21	1977	
Montana	18	1960	

Table 1, continued

State	Age of Majority for Females, 1960	Access Among Single Women at Ages 18-19	
		Year First Obtained	Type of Access (If Not Based on Age of Majority)
Nebraska	21	1972	
Nevada	18	1960	
New Hampshire	21	1971	Mature Minor Legislation
New Jersey	21	1973	
New Mexico	21	1971	
New York	21	1971	Mature Minor Legislation
North Carolina	21	1971	
North Dakota	18	1960	
Ohio	21	1965*	Judicial Mature Minor Ruling
Oklahoma	18	1960	
Oregon	21	1971	Mature Minor Legislation
Pennsylvania	21	1971	Mature Minor Legislation
Rhode Island	21	1972	
South Carolina	21	1972	Mature Minor Legislation
South Dakota	21	1972	
Tennessee	21	1971	
Texas	21	1973	
Utah	18	1960	
Vermont	21	1972	
Virginia	21	1972	
Washington	21	1970	
West Virginia	21	1972	
Wisconsin	21	1972	
Wyoming	21	1973	

Source – See Appendix A

Notes – An asterisk indicates that a Comstock law was in effect until the indicated year. For additional notes, see Appendix A.

Table 2
Logistic Estimation of the Effects of Late-Adolescent Pill Access
on the School Enrollment of 21 year-old Women, 1968-79

	(1)	(2)	(3)
Unrestrictive Late-Adolescent Consent Law	0.1353*	0.2052***	0.2397**
	(0.0700)	(0.0709)	(0.1127)
Race Indicators	yes	yes	yes
State-group and Year fixed Effects	yes	yes	yes
State-group trend		yes	yes
Additional Reproductive Laws			yes
Log likelihood	-8299.90	-8282.95	-8277.28
Number of Observations		14492	
Implied Marginal Effect	0.0263	0.0400	0.0468
Mean Rate of Enrollment		0.2653	

Source – October CPS, 1968-1979
Notes – Late-adolescent consent laws are coded as described in the text. Standard errors are presented in parenthesis and are clustered on the state-group. The two racial indicators are for African-Americans and other non-white. Further, *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. Finally, the marginal effect of an unrestrictive consent law is computed as the average of individual marginal effects over the sample distribution.

Table 3
Multinomial Logistic Estimation of the Effects of Late-Adolescent Pill Access on the Relative Likelihoods of School and Work for 21-year-old Women, 1968-79

	(1)	(2)
Relative Likelihood	School vs. Home	Work vs. Home
Unrestrictive Late-Adolescent Consent Law	0.3887**	-0.0012
	(0.1834)	(0.1426)
Race Indicators	yes	yes
State-group and Year fixed Effects	yes	yes
State-group trend	yes	yes
Additional Reproductive Laws	yes	yes
Log likelihood	-14596.71	
Number of Observations	14492	
Outcome	School	Work
Proportion of Sample in Outcome	0.1974	0.5086

Source – October CPS, 1968-1979

Notes – Late-adolescent consent laws are coded as described in the text. Standard errors are presented in parenthesis and are clustered on the state-group. The two racial indicators are for African-Americans and other non-white. Further, *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level. The coefficient estimates the log-odds of being at school or in the labor force. Staying home is the omitted category.

Table 4
Effects of Late-Adolescent Pill Access on the Rate
of College Completion of American Women

	(1)	(2)
Years of Birth	1939-1959	1948-1959
Unrestrictive Late-Adolescent Consent Law	0.0089***	0.0102***
	(0.0030)	(0.0035)
(Unrestrictive Late-Adolescent Consent Law)	-0.0002	-0.0004
x (Age-30)	(0.0002)	(0.0003)
R-squared	0.9557	0.9618
Number of Observations	2142	1224
Mean College Completion	0.2333	0.2503

Source – Ruggles et al., 2004
Notes – Coefficients are estimated using state-of-birth/year-of-birth/age means as the unit of observation. Late-adolescent consent laws are coded as described in the text. Additional controls included in the specification are as described in the text – see the discussion in Section 4.1. The standard errors presented in parenthesis are weighted to account for heteroskedasticity in the precision of the cell mean and clustered on state of birth. Finally, *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 5
Direct Effects of Late-Adolescent Contraceptive Consent and Contracting Ability on the Rate College Completion of American Men Born 1939-1959

	(1)	(2)
Law Type	Contraceptive Consent	Contracting Ability
Unrestrictive Late Adolescent Law	0.0018	-0.0030
	(0.0030)	(0.0388)
(Unrestrictive Late-Adolescent Law) x (Age-30)	-0.0001	0.0005
	(0.0002)	(0.0003)
R-squared	0.9644	0.9634
Number of Observations	2142	
Mean College Completion	0.2758	

Source – Ruggles et al., 2004
Notes – Coefficients are estimated using the means of state-of-birth/year-of-birth/age cells as the unit of observation. Late-adolescent consent laws are coded as described in the text. Additional controls included in Column 1 are as described in the text – see the discussion in Section 4.1. Column 2 includes the same controls, but drops the measures of liberalization to reproductive law. The standard errors presented in parenthesis are weighted to account for heteroskedasticity in the precision of the cell mean and clustered on state of birth. Finally, *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 6
Effects of Leaded Late-Adolescent Contraceptive Consent
on the Rate of College Completion of American Men

	(1)	(2)	(3)
Length of Lead	1 year	2 years	3 years
Unrestrictive Late-Adolescent Consent Law	0.0071*	0.0083**	0.0066
	(0.0037)	(0.0042)	(0.0040)
(Unrestrictive Late-Adolescent Consent Law)	-0.0002	-0.0003	-0.0002
× (Age - Minimum)	(0.0003)	(0.0003)	(0.0003)
R-squared	0.9635	0.9631	0.9637
Number of Observations		2142	
Male Years of Birth	1938 - 1958	1937 - 1957	1936 - 1956
Minimum Age	31	32	33
Mean College Completion	0.2761	0.2759	0.2757

Source – Ruggles et al., 2004
Notes – Coefficients are estimated using the means of state-of-birth/year-of-birth/age cells as the unit of observation. Late-adolescent consent laws are coded as described in the text. Additional controls included in the specification are as described in the text – see the discussion in Section 4.1. The standard errors presented in parenthesis are weighted to account for heteroskedasticity in the precision of the cell mean and clustered on state of birth. Finally, *** denotes significance at the 1% level, ** at the 5% level, and * at the 10% level.