THE OPT-IN REVOLUTION?
CONTRACEPTION AND THE GENDER GAP IN WAGES

Martha J. Bailey, Brad Hershbein, and Amalia R. Miller

March 7, 2012

Abstract:

Decades of research on the U.S. gender gap in wages describes its correlates, but little is known about *why* women changed their career paths in the 1960s and 1970s. This paper explores the role of "the Pill" in altering women's human capital investments and its ultimate implications for life-cycle wages. Using state-by-birth-cohort variation in legal access, we show that younger access to the Pill conferred an 8-percent hourly wage premium by age fifty. Our estimates imply that the Pill can account for 10 percent of the convergence of the gender gap in the 1980s and 30 percent in the 1990s.

JEL: J13, J22, N32

Contact Information: Bailey, Department of Economics, University of Michigan, 611 Tappan Street, Ann Arbor, MI, 48109, Website: www-personal.umich.edu/~baileymj, Email: baileymj@umich.edu; Hershbein, Department of Economics, University of Michigan, 611 Tappan Street, Ann Arbor, MI, 48109, Email: bjhersh@umich.edu; Miller, Department of Economics, University of Virginia, 237 McCormick Road, Charlottesville, VA 22904, Website: http://virginia.edu/economics/miller.htm, Email:armiller@virginia.edu.

Acknowledgements: The research in this paper was conducted while the authors were Special Sworn Status researchers of the U.S. Census Bureau at the Michigan Census Research Data Center. Research results and conclusions expressed are those of the authors and do not necessarily reflect the views of the Census Bureau. This paper has been screened to ensure that no confidential data are revealed. The University of Michigan Population Studies Center provided generous financial support for work in the University of Michigan Research Data Center. We are grateful to Jim Davis, Maggie Levenstein, Stan Sedo, and Clint Carter for extensive support with the preparation and revision of the restricted data proposal and disclosure process and to John Bound, Charlie Brown, John DiNardo, Claudia Goldin, David Lam, Bob Margo, Paul Rhode, Jeff Smith, Gary Solon, and Sarah Turner for helpful comments and suggestions.

I. INTRODUCTION

During the 1980s, the long-standing U.S. gender gap in pay narrowed rapidly. The median annual wage and salary earnings of women working full-time, full-year rose from roughly 60 percent of men's earnings in 1979 to 69 percent a decade later. Not only was this a striking departure from the stability of women's relative pay during the 1970s, but the speed of women's convergence in the 1980s was also faster than during the 1990s and the 2000s.

The correlates of the narrowing of the gender gap in the 1980s are well documented: the decade witnessed a convergence in measured labor market skills between men and women. Expecting to remain in the labor-force longer, women born in the 1950s (who came of age in the 1970s) narrowed the gender gap in college going and completion, attaining professional degrees, and working in non-traditionally female occupations (Goldin 2004, 2006). Increases in demand for skills that benefited women relative to men increased the returns to women's investments in market skills (Blau and Kahn 1997, Welch 2000). Widening wage inequality among women may have also encouraged women to invest in market skills and led more able women to select into full-time employment (Mulligan and Rubinstein 2008). Each of these factors may have contributed to and resulted from the growth in women's work experience (O'Neill and Polachek 1993, Wellington 1993)

The root causes of these tremendous changes are less clear. Two important but elusive candidates include the resurgence of the women's movement in the late 1960s and early 1970s and the new legal protections afforded to women under the 1964 Civil Rights Act (and later federal enforcement) that reduced overtly-discriminatory hiring and compensation practices—both of which should have changed attitudes and norms about women's employment. Recent literature suggests oral contraception, often called "the Pill," as another important candidate. Its diffusion to younger, unmarried women improved their ability to time births, altered their expectations about future childbearing, and reduced the cost of altering career investments to reflect their changed expectations. The timing of its diffusion during the 1960s and 1970s also fits well with the slow growth in women's wages during the 1970s (as younger women invested more in their human capital) and the rapid convergence in the gender gap during the

1980s (when these women enjoyed the returns on their human capital investments and accumulated labor-market experience). To quantify the importance of the Pill, Goldin and Katz (2002) use state-by-birth-cohort changes in the age of consent from 21 to 18 for medical care and, thereby, prescription birth control. Based upon extensions of this empirical strategy, the recent literature links "early access to the Pill" to delays in marriage (among college goers) and motherhood, changes in selection into motherhood, increased educational attainment, labor-force participation, and occupational upgrading among college graduates (Goldin and Katz 2002, Bailey 2006, Guldi 2008, Hock 2008, Ananat and Hungerman forthcoming). Although these studies imply that the Pill benefitted individual women's careers, its effect on aggregate wages need not be large or even positive due to changes in the composition of working women and increased labor supply. No study, however, has considered the impact of these many changes on the gender gap in compensation.

This article examines the role of the Pill in altering women's life-cycle wages and its ultimate implications for convergence in the gender gap during the 1980s and 1990s. Following earlier work, our empirical strategy leverages state-by-birth-cohort changes in laws reducing the age of consent for medical care and access to prescription birth control for unmarried women under age 21. We extend the literature by providing two new tests of this empirical strategy's identifying assumptions. Using the *1970 National Fertility Study*, we show that early access laws doubled Pill use among women between the ages of 18 and 20—precisely the ages affected by access laws—but not beyond age 21, when the laws did not bind. In addition, we test the excludability of Pill access laws (i.e., the assumption that early legal access to the Pill was conditionally, randomly assigned) using the *National Longitudinal Survey of Young Women (NLS-YW).* Among 18 family background characteristics that should not have been affected by these legal changes, early access to the Pill is correlated with only one at the 10 percent level—no more than would be expected by chance.

Using longitudinal wage information from the *NLS-YW*, our main results show that early access to the Pill *lowered* women's wages in their early twenties (corresponding to the 1970s) but raised their wages in their thirties and forties (corresponding to the 1980s and 1990s). By their late forties, women with early access to the Pill earned a statistically-significant hourly premium of 8 percent—enough to account for between a third and half of the total hourly wage gains for these cohorts over their peers born a decade earlier. Consistent with the well-known relationship of women's wage growth to cumulative labor-force experience, our decomposition indicates that almost two thirds of the Pill-induced wage premium at the mean is explained through its effect on women's labor-force experience. Another third of the premium is due to changes in educational attainment and occupational choice.

The *NLS-YW* also sheds light on the mechanisms for these effects. Stratifying our sample by measures of high school "IQ score" reveals that the flexibility conferred by the Pill had no measurable impact on the education or experience of lower IQ women. Both middle and higher IQ women, however, raised their educational attainment in their twenties and, in their thirties, acquired more labor-market experience and increased their representation in non-traditionally female occupations. Interestingly, the Pill's largest effects on work experience accrued to women in the middle of the IQ distribution with some college, not to the high-achieving women who have been the focus of earlier studies. In keeping with this finding, early access to the Pill had the largest impact on the lifecycle wages of women in the middle of the IQ distribution. Thus, the rapid narrowing of the gender gap during the 1980s reflected, in part, a Pill-induced revolution in middle-ability women planning for and opting into paid work.

II. THE REVOLUTION IN WOMEN'S WORK

Aggregate statistics documenting women's wages from the 1950s and 1960s only hint at the tremendous changes in women's earning capacity. Goldin (1990: table 3.1) shows that women's real wages fell relative to men's from the 1950s to the 1960s; from the 1960s through the mid-1970s, the gap in pay remained constant at roughly 60 percent (Blau, Ferber and Winkler 2010: figure 51). Beginning in the 1980s, the gender gap in wages narrowed substantially. Although this narrowing has continued to the present, its pace has slowed since the mid-1990s. To provide context for our cohort-age based investigation, this section uses the 1964 to 2009 March *Current Population Surveys* (*CPS*) to describe by age and cohort the changes in women's wages and labor-force outcomes, what Goldin (2006) dubbed the "quiet revolution."[1] We also present statistics *relative to men* to underscore the convergence in outcomes.

Figure 1 shows the evolution of mean annual wage and salary earnings in 2000 dollars (PCE deflator) for seven different birth cohorts of women relative to men—a measure of the age-specific gender gap for the following cohorts: those born from 1922 to 1927 (called mid-1920s), 1928 to 1932 (early 1930s), 1933 to 1937 (mid-1930s), 1938 to 1942 (early 1940s), 1943 to 1946 (mid-1940s), 1947 to 1950 (late 1940s), and 1951 to 1954 (early 1950s).[2] For cohorts born before the 1940s, the relative wage series have similar age profiles. Beginning with cohorts born in the early 1940s, the gender gap increases less rapidly (i.e., the pay of women relative to men falls less rapidly) in women's twenties and rebounds more quickly after age 30. For 34 year olds, annual incomes increased from 39 percent of similarly aged men for the 1938 to 1942 cohort to 55 percent for cohorts born less than a decade later.

Large changes in relative wage and salary earnings followed dramatic relative increases in women's *pre-market* and *post-entry* career investments. Goldin, Katz, and Kuziemko (2006) show that the share of women (relative to men) attending and completing college accelerated for cohorts born after the mid-1930s. Labor-force participation during the childbearing years grew rapidly as well. At the extensive margin, participation of 30-year-old women born in the mid-1940s increased by 16 percentage points (from a base of 39 percent) over cohorts born a decade earlier. For women born in the early 1950s, this statistic increased another 14 percentage points.[3] Because the labor-force participation of men was stable over this period, these increases imply a narrowing in the cohort-based gender gap in participation, shown as a flattening of the relative labor-force participation series plotted in figure 2A. Women's greater labor-force participation also translated into considerably more work experience (cf. O'Neill and Polachek 1993, Wellington 1993). In the *NLS-YW*, we calculate that women born in the early 1950s

---

[1] We use *CPS* rather than the *NLS*, because the *CPS* contain information on older cohorts and their larger sample sizes make our series less noisy. Data from the *NLS-YW* augment this discussion when informative.
[2] This divides the cohorts of the *National Longitudinal Surveys of Mature and Young Women* into roughly equal-sized groups. Wage and salary earnings in figure 1 exclude farm, business or self-employment income. Our sample excludes those who report zero earnings, but figure 1 makes no further sample restrictions.
[3] Statistics for women alone are computed using the March *CPS,* but *only* statistics relative to men are presented for brevity.

worked 3000 more hours between ages 24 and 40 than did women born in the mid-1940s—an increase of 1.5 full-time, 50-week years.[4]

Changes in the nature of women's work for pay—along with their experience—also coincide with the narrowing of the cohort-based gender gap. The fraction of women working in professional or managerial jobs in their mid-thirties was roughly twice as high for cohorts born in the mid-1940s as for cohorts born a decade earlier. Figure 2B shows that, after accounting for the increase in the share of men working in professional and managerial jobs, women's representation in these fields at age 30 increased by 25 percentage points between the cohorts born in the early and late 1940s and another 24 percentage points for cohorts born in the early 1950s.

Although the remarkable, late-twentieth-century transformation in women's careers is well known, its catalysts are less well understood. Women may have been pulled into the labor force by changes in demand reflecting increasing enforcement of anti-discrimination legislation or skill- (and gender-) biased technological change (Welch 2000, Black and Juhn 2000, Weinberg 2000, Black and Spitz-Oener 2010). At the same time, rapidly changing ideas about women's work and roles in the workplace (Fernandez, Fogli, and Olivetti 2004, Fernandez and Fogli 2009, and Fortin 2009), shifts in divorce rates (Stevenson and Wolfers 2007), and the availability of better colleges and better education at the same colleges (Goldin and Katz 2010) may have increased the supply of women's skills to the market. The next sections describe the potential importance of the Pill for young women's decisions and wages and outline our empirical strategy for quantifying its role within the broader social and economic changes of the last 40 years.

III. WAS THIS AN OPT-*IN* REVOLUTION? THE EXPECTED EFFECTS OF CHANGES IN PILL ACCESS ON WOMEN'S LIFECYCLE WAGES

The diffusion of oral contraception, first released for the regulation of menses in 1957 and approved by the U.S. Food and Drug Administration as a contraceptive in 1960, had an important impact

[4] We cannot compare these estimates with cohorts born earlier than the mid-1940s, as the *Mature Women* were first interviewed when they were between the ages of 30 and 45. Therefore, we are missing information on these older cohorts' labor-force participation at younger ages. For construction of these experience measures, see Appendix A.

on younger women's ability to time births and plan future childbearing. Women born in the early 1940s (who would be young adults in the early 1960s) would have been the first with access to the Pill in late adolescence when they made decisions about family formation, childbearing, and career investments. They would have also been the first to gain autonomy in deciding to use contraception (rather than sharing it with their partners), the first to be able to make decisions about contraception at a time separate from intercourse, and the first to benefit from the reliability and *expectation* of birth predictability the Pill conferred over the entirety of their childbearing years and early careers. Changes in *expectations* are key. Even women who would not have married or had a child before age 22 without the Pill may have altered their career investments as their expectations about future childbearing changed.

The difficulty of parsing the Pill's effect on women's wages relates to the timing of its appearance. By cause or coincidence, the Pill's diffusion coincided with important changes in norms and ideas about women's work and the end of the baby boom. Following Goldin and Katz (2002) and Bailey (2006), our empirical strategy makes use of state-level variation *within* birth cohorts in "early legal access to the Pill" (*ELA*), which allowed younger women to consent for medical care. As described in Bailey (2006), most legal changes were due either to judicial expansions in the rights of legal minors or to legislative changes that lowered the age of majority to 18. The timing of changes in *ELA* differed considerably across states (the earliest change was in 1960 and the latest in 1976), but the common feature of these laws is that they gave physicians latitude to prescribe oral contraception to unmarried women under 21 without consulting parents (Paul, Pilpel, and Wechsler 1974, 1976). State-by-birth cohort variation in *ELA*, therefore, facilitates comparisons of labor-force outcomes for women who gained legal access to the Pill earlier (typically at their 18th birthdays) to those who gained access at 21.

This three-year difference in access to the Pill during a formative life stage potentially affected a host of decisions. Having access to the Pill at age 18, for instance, directly reduced the cost of delaying childbearing and marriage to enter or stay in college.[5] Even among those who did not attend college, better fertility control reduced the cost of remaining at a job long enough to obtain a promotion or additional training. In addition to decreasing the costs of investing, access to the Pill at 18 may have altered the *expected* returns to early human capital investments. All else equal, the same early human capital investment would yield larger *expected* lifetime returns if women *anticipated* being in the labor force more or being more successful in achieving their career aspirations. In short, earlier access to the Pill should have both reduced the costs of and increased the expected returns to early career investments—predictions consistent with the empirical literature: Hock (2008) and Ananat and Hungerman (forthcoming) show Pill access affected college enrollment and education; Bailey (2006) shows that it increased women's labor-force attachment; and Goldin and Katz (2002) find that it increased college women's representation in non-traditionally female professions.

This theoretical framework suggests three (potentially reinforcing) mechanisms linking *ELA* to steeper wage and salary earnings profiles. First, *ELA* may have increased labor-force participation, which enabled women to accumulate more labor-market experience and job- or firm-specific capital. Thus, women with *ELA* would experience more rapid wage growth. We call this mechanism the "experience mechanism." Second, women with *ELA* may have shared the costs of gaining on-the-job human capital by accepting lower initial wages but then enjoyed larger wage growth with tenure. We call this channel the "on-the-job-investment mechanism." Third, *ELA* may have increased school enrollment and participation in training programs, which should lower wage earnings at younger ages, and increase them following school exit. We call this channel the "formal human-capital investment mechanism."

Our empirical estimates of the effect of *ELA* on wages should be interpreted cautiously for two reasons. The first relates to the off-setting effects of labor supply. Because *ELA* could increase labor-

[5] A lower risk of childbearing at ages 18 to 19 may have also affected when and whom women married, which could have an independent effect on their careers (Chiappori and Oreffice 2008). Staying in college longer could allow marriage to a more educated man and, therefore, increase a woman's nonwage income and reduce her labor-supply (Ge 2008). On the other hand, staying in college longer should increase a woman's own earnings and, therefore, increase her options outside of marriage. If this leads to greater divorce, women would have lower nonwage incomes and, therefore, tend to work more at older ages (and younger ages, to the extent that women are risk averse and forward looking). For both reasons, marriage delay may improve women's career outcomes independently of fertility delay (Loughran and Zissimopolous 2009, Miller 2011).

force participation for large numbers of women thus reducing the capital-to-labor-ratio, its effect on any one woman may be larger than its effect on an entire birth cohort, which our analysis recovers. As shown in the theory appendix, the magnitude of these supply-side effects depends (among other things) on the degree of substitutability of male and female labor in production. The closer substitutes men and women are in production, the smaller the labor-supply effect and the more likely the overall effect of *ELA* on wages will be positive (due to its effect on human capital accumulation). Our analysis recovers estimates that include this labor-supply effect, so our estimates will tend to understate the effect of the Pill on an individual woman's wages, especially in the shorter-run (at younger ages) before firms adjust their capital stock.

The second reason relates to selection. Because wages are only observed for labor market participants, the observed impact of *ELA* on women's wage growth will be larger than the effect on the average woman if the Pill differentially affects human capital investments and labor supply of higher ability women. If, for instance, early access to the Pill causes higher ability woman to continue in their education and makes them less likely to work in their early twenties, then the *ELA*-induced growth in wages will reflect both the returns to these greater investments and changes in the composition of working women to favor those of higher ability. Our analysis explores these compositional effects explicitly by breaking our sample into three IQ tertiles (based upon a composite developed from high school aptitude tests) and examining the effects of *ELA* for women within each of these tertiles.

IV. DATA AND EMPIRICAL STRATEGY FOR IDENTIFYING THE IMPACT OF THE PILL ON WAGES

Our analysis uses the rich, longitudinal data of the *National Longitudinal Survey of Young Women* (*NLS-YW*), which contains interviews beginning in 1968 for 5,159 women, ages 14 to 24, with 21 subsequent interviews. Crucial is that the *NLS-YW* sampled women born from 1943 to 1954, cohorts that varied in their early legal access to the Pill. Although this dataset is smaller than those used in earlier studies, the restricted version contains information on the legal state of residence for the respondents at age 21. We use residence at age 21 (which should be reported as parents' residence for unmarried, college women) to infer treatment status with considerably less error than previous studies.[6]

The *NLS-YW* confers several additional advantages. It contains a rich set of pre-treatment outcomes for testing the validity of our empirical strategy and also facilitates an analysis of heterogeneity in the impact of the Pill by socio-economic status and high school IQ of the respondent, which allows us to understand the ways in which the Pill influenced the selection of women into paid work.[7] Finally, the *NLS-YW* provides information on women's wage earnings in every survey year as well as their career investments including educational attainment, job training and certification, and labor-force participation (weeks and hours). Repeated reports of women's labor-force participation allows us to construct measures of their cumulative labor-force experience and link the Pill to this important correlate of women's wage gains.

A. Empirical Specification

Our empirical strategy follows the previous literature with several modifications. We estimate the following linear regression models for continuous dependent variables,

$$Y_{iacs} = \sum_g \beta_g ELA_{cs} D_{g(a)} + \sum_g \lambda_g D_{g(a)} + \sum_s \lambda_s D_s + \sum_c \lambda_c D_c + \eta_{iacs}, \tag{8}$$

where Y is the outcome of interest for individual i, at age a, who was born in year c = 1943, 1944, …, 1953 (also referred to as "birth cohort"), and residing in state s = 1, 2, …, 51 at age 21. Fixed effects for state of residence, $\sum_{s=2}^{51} \lambda_s D_s$ where $D_s = 1$ if i resided in state s at age 21, and single year-of-birth cohorts, $\sum_{c=1944}^{1953} \lambda_c D_c$ where $D_c = 1$ if i was born in year c, are included in all specifications. The dummy variables $D_{g(a)}$ are set to 1 if the respondent's age fell into the five-year age group, g (14-19, 20-

[6] Restricting the sample to those with valid date of birth (cohort) and state of residence information reduces the sample to 4354. Both Goldin and Katz (2002) and Bailey (2006) use repeated cross-sections that contain no information on an individual's state of residence at ages 18 to 21. As a result, Goldin and Katz (2002) and Bailey (2006) infer *ELA* based upon the reported birth state or state of residence *at the time of the survey* respectively.

[7] Appendix A describes the survey questions and coding of each variable.

24, …, or 45-49). Standard errors for all models are robust to heteroskedasticity and clustered at the state level.[8]

Early legal access to the pill, ELA_{cs}, is equal to one if a woman born in year c would have had access to oral contraception before age 21 in her state of residence at age 21, and interactions of *ELA* with the age-group dummy variables allow its effect to vary across the lifecycle. Therefore, the key parameters of interest, the β_g terms, measure differences in the outcome of interest in age group g between women with and without early legal access to the Pill. It is worth noting that β_g will understate the impact of early Pill access for three reasons: local compliance and enforcement were imperfect; many young women could not have afforded the Pill even when it was legal; and young women may have driven across state lines to obtain it.

The main modification to Bailey (2006) is that we rely upon a revised legal coding (see Appendix B). This updated legal coding reduces measurement error in ELA and allows the estimation of more precise effects over the lifecycle. Because these laws are not used elsewhere in the literature, the following section establishes their relationship with Pill use and subjects them to validity checks using detailed information on pre-treatment characteristics.

B. Validity of Using ELA to Identify the Impact of the Pill

One important assumption required to obtain consistent estimates of β_g is that *ELA* is uncorrelated with the error term after conditioning on state, age-group and birth-cohort fixed effects, or $\text{cov}(ELA,\varepsilon|\,\boldsymbol{Z})=0$, where $\boldsymbol{Z}$ captures the fixed effects in equation (8).

One reason that $\text{cov}(ELA,\varepsilon|\,\boldsymbol{Z})$ may not be zero is that *ELA* may not be conditionally, randomly assigned at baseline. That is, a systematic correlation between omitted characteristics and *ELA* could drive the relationship between ELA and outcomes. Because the *NLS-YW* contain rich information on

[8] For dichotomous dependent variables, we estimate probits and report average partial effects (APEs). The standard errors are calculated using a non-parametric bootstrap method with states as clusters (1,000 repetitions).

respondents' backgrounds at age 14 *before treatment with ELA*, we test this possibility using the following specification,

$$(9) \quad X_{ics} = \gamma ELA_{cs} + \sum_s \lambda_s D_s + \sum_c \lambda_c D_c + \varepsilon_{ics},$$

where X is a pre-treatment characteristic and other notation remains as previously described. Thus, γ measures the residual correlation between ELA and pre-treatment characteristics that could indicate correlations with other, unobserved characteristics. (This approach is akin to testing for balance in observable characteristics in a controlled experiment.) Failure to reject $\gamma = 0$ is consistent with conditional random assignment of early legal access to the Pill. Although the power of this test is limited by our small sample sizes, it provides a strong validity test of the empirical strategy.

Table 1 reports the results of this exercise for 18 pre-treatment characteristics including a binary variable for whether the respondent's father was born in the U.S.; a binary variable for whether the respondent's father/mother worked for pay or held a professional job when she was 14 (four separate outcomes); an occupational prestige index for the father, conditional on working; a socio-economic status index for the respondent's parents in 1968; a binary variable for whether the respondent resided on a farm or in a rural area at age 14; a binary variable for whether the respondent had access to magazines, newspapers or a library card at age 14 (three separate outcomes); a binary variable for whether the respondent lived in a household with two parents at age 14; the number of siblings a respondent had; the highest grade completed by father/mother by 1968 (two separate outcomes); the number of years of schooling parents wanted the respondent to obtain when she was age 14; the atypicality of the respondent's mother's job (conditional upon mother working; negative numbers represent more atypical outcomes); and the respondent's IQ score in high school (see Appendix A for details). Each column represents a separate, least-squares regression estimate of γ.[9] Consistent with treating ELA as conditionally, randomly assigned, only one of the 18 estimates is statistically significant at the ten percent

[9] Linear probability models are used for binary outcomes to circumvent potential problems with disclosure. The results are robust to using negative binomials and probits where appropriate.

level—no more than expected by chance. It is also reassuring that the pattern of correlations suggests no consistent relationship between *ELA* and the pre-treatment characteristics. For instance, *ELA* is negatively associated with father's employment and with family socio-economic status, but is positively associated with mother's education and professional employment.

Even if *ELA* is conditionally, randomly assigned, another reason that cov(ELA,ε| $\boldsymbol{Z}$) may not be zero is that *ELA* is packaged with other policy changes. Although the history of these legal changes makes this unlikely, one concern is that cohorts with *ELA* were differentially treated with abortion access by chance—a treatment that could have a similar effect. Although data limitations mean that abortion access cannot be measured directly, our analysis accounts for this possibility by augmenting our equation (8) with a rich set of abortion controls:

$$(8') \qquad Y_{iacs} = \sum_g \beta_g ELA_{cs}\, D_{g(a)} + \sum_g \gamma_g EAA_{cs}\, C50_c D_{g(a)} + \sum_g \theta_g ELA_{cs} EAA_{cs}\, C50_c D_{g(a)}$$
$$+\delta LnDist_s C50_c \ + \textstyle\sum_g \lambda_g D_{g(a)} + \sum_s \lambda_s D_s \ + \sum_c \lambda_c D_c \ + \eta_{iacs},$$

where *EAA* represents "early access to abortion" and is equal to 1 if an individual resided (at age 21) in Alaska, California, the District of Columbia, Hawaii, New York or Washington, states that legalized abortion in 1970. *C50* is equal to 1 for birth cohorts born in 1950 or later, because the early legalization of abortion in 1970 could not have affected Pill use or fertility timing among 18 to 20 year olds *before* 1970 (cohorts born before 1950). It is also important to note that any cohort-invariant, state-level differences in access to abortion will be captured in the state effects. The interaction of *EAA* and *C50* with age-group dummies allows the differential evolution of outcomes for state-birth-cohort groups exposed to legal abortion in their state of residence before their 21st birthdays. Separate interactions of *EAA* and *C50* with *ELA* and age-group dummies allow early abortion access and early access to the Pill to be complements or substitutes. Finally, cross-state travel to obtain abortion is accounted for by inclusion of log distance to the nearest large city providing legal abortions to out-of-state residents (Buffalo, New York City, Los Angeles, San Francisco, or the District of Columbia), $LnDist_s$, for cohorts born in 1950 or later (cf. Joyce, Tan and Zhang 2010). Therefore, the key parameters of interest, β_g, measure differences in outcomes in age group *g* between women with and without *ELA* for cohorts that did not have early access to abortion in their home state after adjusting for cohort-level changes in cross-state travel for abortion.[10]

Finally, we test the sensitivity of our results in four alternative specifications of (8'): one with linear, state-specific time trends; another with controls for Vietnam casualties;[11] another using only a balanced sample of individuals (those missing information in any year or attriting are omitted); and another using state where the respondent attended high school to match to *ELA* rather than state of residence at 21.[12]

C. The Relevance of Early Legal Access for Pill Use

Testing the relevance of *ELA* for women's use of the Pill is more difficult, because the *NLS-YW* contains no information on young women's contraceptive decisions. Goldin and Katz (2002) examined this question with a single cross-sectional dataset (1971 *National Study of Young Women*, *NSYW71*) and found that *ELA* increased Pill use among 17 to 19-year-olds by 4 percentage points (40 percent), but it is unclear how this evidence bears upon this analysis for two reasons. One reason is that Goldin and Katz (2002) used a different legal coding, which means their estimates may not generalize to the coding used in this paper. A second and more important reason is that the single cross-section of data in the *NLSY71* cannot be used to estimate the implicit first stage of this analysis, because state and cohort fixed effects cannot be included. Key for our investigation is that *ELA* increased Pill use at ages 18 to 20 *after conditioning on year of birth and state fixed effects.*

The 1970 *National Fertility Survey* (*NFS*), which asked ever-married women to recall Pill use over the decade of the 1960s, allows us to examine this question directly for the subset of women who

---

[10] Disclosure limitations from the Research Data Center prevent us from reporting the estimates on *EAA* and the *ELA-EAA* interactions, although we can summarize these findings generally. We find that early abortion access does have independent effects on many (but not all) of the outcomes we examine, of a comparable magnitude to *ELA*. The coefficients on the interactions are consistent with the Pill and abortion acting as substitutes, which agrees with Ananat and Hungerman (forthcoming), although the estimates are seldom statistically significant. The inclusion of these abortion controls has a negligible effect on the *ELA* point estimates, as can be seen by comparing estimates here to those without abortion controls in Appendix C.

[11] Using data from the National Archives on the Vietnam Conflict, the specification in equation (8') is augmented with controls for state-level casualties. These controls include state-specific annual death rates lagged one, two, and three years; and cohort-specific, state-level death rates within two years of a woman's date of birth.

[12] Due to disclosure requirements on implicit sample sizes, we cannot include all of these controls and restrictions in one specification. More details on each specification can be found in Appendix A.

were ages 18 to 21 before 1970 and women who were married by 1970. We re-estimate equation (9) where X is a binary dependent variable equal to 1 if a respondent first used the birth control pill before age a, where a=18, 19, …, 22. If *ELA* mattered for Pill use at ages 18 to 20, we would expect γ to be positive.

Before presenting the results, several limitations of the data should be noted. First, the sample is restricted to ever-married women. Because women treated with early access to the Pill tended to delay marriage (cf. Goldin and Katz 2002, Appendix C), unmarried young women not in the 1970 *NFS* may have been among those with the strongest response to *ELA*. This would lead our estimates to *under*state the impact of *ELA* on Pill use. Second, the 1970 *NFS* provides information on a smaller set of cohorts and identifying variation than does the *NLS-YW* analysis. In order to estimate γ using a balanced panel, the analysis restricts the sample to the birth cohorts of 1942 (age 18 in 1960) to 1948 (age 22 in 1970), which results in 1,985 observations. Implicitly, this limits the states transitioning to *ELA* to Georgia, Kentucky, Mississippi, Ohio, and Washington. Finally, stigma-induced underreporting of Pill use among young, unmarried women with *ELA* who started systematically earlier would also lead to an understatement of the impact of *ELA* on Pill use.

Despite these limitations, these data provide strong evidence that *ELA* increased Pill use at the appropriate ages. Panel A of table 2 presents separate regressions of equation (9) for first Pill use before a given age. By chance, it appears that women in the five states that transitioned to *ELA* before 1968 were significantly *less* likely to use the Pill before age 18—a bias that works against our finding effects. However, Pill use by age 18 (before age 19) was 17 percentage points higher—an increase of roughly 140 percent over the national mean use at that age. Pill use by age 20 was 16 percentage points higher, an increase of 43 percent over the national mean. These striking differences fall sharply to a statistically-insignificant 5 percentage points at age 21, when women without *ELA* could obtain the Pill legally.[13]

[13] Although omitted here for brevity, we also find that these differences in use translated into meaningful differences in marriage timing (cf. Goldin and Katz 2002) and age at first birth (cf. Bailey 2006, 2009): women with *ELA* delayed marriage by an average of 0.42 years and motherhood by 0.25 years.

Panel B of table 2 explores heterogeneity in this effect by the community size of the primary sampling unit. We implement this by augmenting equation (9) with a dummy variable for non-metropolitan area as well as the interaction of this variable with *ELA*. Not surprisingly the strongest responses to *ELA* occurred in metropolitan areas. Consistent with changes in *ELA* increasing access to the Pill at age 18, use of the Pill in metropolitan areas with *ELA* was 30.4 percentage points higher—2.5 times the national mean in metro areas. This difference was 13.7 percentage points in less populated areas. Use of the Pill before age 21 was 26.9 percentage points, or 77 percent, higher among women with *ELA* in metro areas and 12.7 percentage points, or 31 percent higher, in non-metro areas, and these estimates are virtually unchanged with the inclusion of state linear time trends (see Appendix C). For metro and non-metro areas, the difference in Pill use for women with *ELA* fell to 10 percentage points and 3 percentage points, respectively, by age 22, when early access laws ceased to bind. Stronger results in metropolitan areas are consistent with the difficulty of getting contraceptives anonymously in small towns or rural areas (even when legal).[14]

Although these results provide the best evidence in the literature of the relevance of *ELA*, we caution against using them as a denominator to approximate average treatment effects for Pill *use* on the treated (ATT) for several reasons. First, the sample of married women and stigma about reporting premarital Pill use may lead this analysis to understate the true effect of Pill access on Pill use, which would inflate estimates of the treatment effect on the treated. Second, the external validity of these results is difficult to establish. Not only was the 1970 *NFS* not designed to be representative at the state level, but the estimates for the handful of states that transition to *ELA* (cohorts of 1942 to 1948) during our sample period may not represent the effects for the full set of cohorts (1943 to 1953) considered in the analysis. Finally, even if the effect of *ELA* on Pill use lies in our estimated range of 16 to 19 percentage points, dividing other *ELA* effects by this amount yields the ATT only if *ELA* has zero effect on women who did

[14] Knowing the town doctor—or knowing that your parents did—or potentially being observed by your neighbor entering the local Planned Parenthood may have deterred many young women from seeking a prescription for the Pill—even if it was legal. Moreover, small town physicians may have been less willing to prescribe the Pill to unmarried women even when legal.

not use the Pill. That would not be the case if the *option* to use the Pill affects human capital investment or if there are general equilibrium effects or demand-side responses to Pill diffusion. For instance, as more women enter the workplace with *ELA*, women in these markets who did not use the Pill may benefit from reductions in employers' statistical discrimination. Our intention-to-treat estimates in the following section include these general equilibrium effects, but our estimates of Pill use in the *NFS* do not.

V. RESULTS: HOW THE PILL AFFECTED WOMEN'S LIFECYCLE WAGES

A. The Effect of the Pill on Women's Wages

Figure 3 plots the effect of *ELA* on women's life cycle wage earnings for four dependent variables in each of four panels. The figure includes our baseline specification (using equation 8), a specification with abortion controls (using equation 8'), and the four alternative specifications described above. Throughout the results section, our discussion focuses on the magnitudes of our estimates with abortion controls (8'), but it is important to note that the estimates from each of the other five specifications are generally not statistically different from those in (8'). (See Appendix C for a tabular presentation of estimates for each of these five specifications.)

Across the six specifications, samples (including and excluding nonworking women), and definitions of the dependent variable, figure 3 shows a consistent pattern. Women with *ELA* earned less in terms of hourly and annual wages in their early twenties, but their wage and salary earnings grew more rapidly than their counterparts as they aged.[15] At ages 20 to 24, working women with *ELA* earned 3 percent less in hourly terms (table 3 columns 1 and 2) and 9 percent less on an annual basis (table 3 columns 3 and 4). By their early forties, women with *ELA* earned a statistically significant premium of 5 percent hourly and 11 percent annually. This implies they earned 63 cents more per hour and roughly 2,200 dollars more per year. Notice that the annual amount is substantially larger than the 1,300 dollars implied by the hourly increase for a full-time, full-year worker, which is consistent with *ELA* also

[15] Although the estimates are not statistically different, it is noteworthy that using high school state rather than state at age 21 reduces the effect of ELA on wages. This is the case because we are less likely to have information on high school state for women who left the state for college. (Note that our estimates of college enrollment in table 4 are also much smaller for this sample.) Because women attending out-of-state colleges may have been the most able or ambitious, it makes sense that our wages estimates are slightly smaller when we omit them.

affecting labor-force participation.[16] Column 5 confirms this. Including women who did not work increases the *ELA* annual wage premium to 2,700 dollars per year.

Although previous work links the diffusion of the Pill among younger, unmarried women to increased educational attainment (Hock 2008), women's lifecycle labor-force participation (Bailey 2006), and marital outcomes and occupational upgrading among college graduates (Goldin and Katz 2002), none of these studies explores the implications of these changes for women's wages, which is this paper's objective. The following sections extend the literature by reexamining these mechanisms and explicitly linking them to wages. For thoroughness, we replicate previous findings in the literature for a sample of all women and compare our findings, which are based on different cohorts and measures of *ELA*, to previous estimates. In addition, we add to the literature on the Pill's labor-market effects by examining novel outcomes such as on-the-job training and cumulative labor-market experience (section V.B) and by considering how the Pill changed selection into human capital investments and paid work across ages (section V.C).

B. Mechanisms for the Pill's Effect on Wages

Our theoretical framework provides three potentially reinforcing explanations for *ELA*'s effects on wage profiles. The experience mechanism suggests that the initial *increase in women's labor-force participation* could have depressed wages at younger ages but increased wages later as these women accumulated labor-market experience and/or job/firm-specific capital. The on-the-job training mechanism requires *no initial or longer-run differences in labor-force participation*, but suggests that workers with *ELA* increased their on-the-job human capital investments, which would also result in steeper wage earnings profiles. The formal human capital investment mechanism is consistent with women *reducing their initial labor-force participation* as they invested in their education or training and then reaping the returns to these early investments when they returned to the labor market, which would also result in

[16] The annualized value of the hourly premium may also differ from the annual wages because the compensation information represents different pay periods. Hourly wages are from the most recent job, whereas annual wage and salary earnings reflect earnings in the previous calendar year from 1968 to 1993 and in the previous 12 months after 1994.

steeper wage earnings profiles. Each of these explanations likely operated to some degree in practice, so our exploration of the Pill's labor-force participation effects here aims to shed light on the predominant mechanism for its observed wage effects. Importantly, each of these explanations postulates *different* labor-force participation and human capital investment patterns.

As a starting point, we examine the effect of *ELA* on women's labor market participation at the extensive (1=in the labor force) and intensive margins (using "usual weekly hours" for working women) and find that women with *ELA* participated *less* in their early twenties and *more* in their late twenties and thirties.[17] These differences in labor-force participation resulted in different cumulative experience profiles as shown in figure 4A and column 1 of table 4, which define women's cumulative work experience as weeks worked multiplied by usual weekly hours summed across survey waves (see Appendix A for more details). The results show that women with *ELA* had worked 18 percent fewer hours by their late twenties but erased this deficit during their thirties. By their early forties, women with *ELA* had amassed the equivalent of 1.15 years more of full-time, full-year work (2,300 more hours)—an increase of over 10 percent relative to their same-aged peers without *ELA*, and about 30 percent larger than the increase found by O'Neill and Polachek (1993) between cohorts born in the mid-1930s and those born a decade later.[18]

This pattern of reduced labor-force participation is the reverse of the labor-supply shift needed to decrease wages at younger ages. Similarly, the on-the-job training channel is also inconsistent with early

[17] These findings are consistent with Bailey's (2006) results using repeated cross-sections from the March *CPS*, but the magnitudes in the *NLS-YW* are larger than in the *CPS* but less precisely estimated owing to significantly smaller sample sizes. These differences in magnitude are expected because Bailey's (2006) use of current state of residence (rather than residence at age 21) should attenuate her results. For brevity, we omit estimates for labor-force participation from this paper and compare our *NLS-YW* estimates to Bailey (2006) in this footnote. At ages 25 to 34, women with *ELA* were roughly 3.8 percentage points, or 6 percent, more likely to work for pay in the *NLS-YW*; Bailey reports an almost identical estimate (3.9 percentage points for women ages 26 to 30) but her estimate is smaller at 1.6 percentage points for women ages 31 to 35. The *NLS-YW* also shows a larger effect in the late thirties than the *CPS*, although the *NLS-YW* estimate is statistically insignificant. The effect of *ELA* on hours worked (excluding zeros) in the *NLS-YW* is not as comparable, because it asks usual hours worked whereas the *CPS* asks the number of hours worked in the *CPS* reference week. The effects at older ages are larger for usual hours worked in the *NLS-YW*, where women 30 to 34 years old worked one additional hour per week on average, 2.5 percent more than their counterparts without *ELA*; 35 to 44 year olds worked 1.3 to 1.7 additional hours, or 3.5 to 4.8 percent more. Full results are available upon request.

[18] The comparison with O'Neill and Polachek is approximate, both because they analyze slightly different groups of women and because their measure of labor market experience is different. In particular, they count years in which at least 26 weeks were worked as a full year of experience; changes at the extensive margin or changes on the intensive margin that do not cross the 26-week threshold are thus missed by their measure.

career dips in labor supply: if fewer women are working for pay, more cannot be accumulating on-the-job training at these ages. The Pill-induced accumulation of experience is most consistent with the formal human-capital investment channel, which postulates that *ELA* women used the Pill to make more investments in formal schooling and training early in their careers and enjoyed the returns on these investments in terms of steeper wage profiles, which also encouraged greater labor-force attachment, as they aged.

Panels B through F of figure 4 examine *ELA*'s effect on these more formal human capital investments including women's college enrollment, years of education, occupational training, and professional occupations for the six specifications; table 4 presents estimates in tabular form. The results provide a rich picture of Pill-induced changes in women's career investments. College enrollment was 4.9 percentage points, or 20 percent, higher for women with *ELA* in their early twenties but not at later ages (table 4 column 2; figure 4B).[19] Their advantage in grades completed (table 4 column 3; figure 4C) peaks in their late twenties, at a little more than one quarter of a year and erodes a bit as women without *ELA* returned to school in their thirties. A difference of one quarter of a year of schooling, however, persists through the early forties. In addition to completing more formal education in their early twenties, women with *ELA* were 15 percent more likely to report occupational training (table 4 column 4, figure 4D) in their late twenties. Although reports of occupational training remain modestly elevated for *ELA* women at older ages, the estimates are not statistically different from zero.

Women's greater human capital investments also appear in their occupational choices, which capture both observed (more formal education) as well as unobserved career investments (such as more career commitment or effort) (see Appendix A for more information on occupational coding). With *ELA*, women were 17 to 30 percent (4 to 6 percentage points) more likely to be working in a professional or

[19] Estimates are 30 percent larger than our baseline estimate (0.066 for a 27 percent increase) when we include controls for Vietnam mobilization. Estimates are 50 percent smaller (0.026 for an 11 percent increase) when we use high school state. Using high school state reduces our estimates because we are less likely to have information on high school state for women who went out of state to college. Thus, our sample of women for whom we have high school state disproportionately drops out-of-state college enrollees. These estimates are larger than reported in Hock's (2008) working paper. Using the October *CPS*, he finds—using a different measure of *ELA*—that college enrollment was roughly 2.5 percentage points higher among 21 and 22 year olds with ELA.

managerial job during their late twenties and thirties, respectively (table 4 column 5, figure 4E). Half of this increase in the late twenties, and all of it during the thirties, was due to entry into non-traditionally female professional occupations—professions other than nursing or teaching (table 4 column 6, figure 4F). It is also interesting that differences in professional work erode with age, as female professionals with *ELA* retire.[20]

Together, more investments in formal human capital and greater labor-market attachment contributed to women's steeper age-earnings profiles. But given *ELA*'s reduction in labor-supply during women's early twenties, the *decrease* in working women's wages at those ages remains an open question. It is also unclear to what extent changes in the composition of women investing in their human capital and working for pay drive the increase in women's wages at older ages. We address both questions in the next section.

C. Heterogeneous Effects of the Pill and the Role of Workforce Composition in Wage Growth

In addition to shifting women's investments in their human capital, early access to the Pill may have shifted *which women* pursued an education, went to graduate or professional school, and got promoted. If higher ability women disproportionately used the Pill to make career investments with the expectation of working longer, and thus were initially more likely to be out of the labor force, then women working during their early twenties may have been negatively selected. As higher-ability women entered the work force in their later twenties after having made their career investments, their greater skills (unobserved and observed) would lead their earnings profiles to be steeper than those of less skilled women. Moreover, less skilled women may have seen their earnings fall as their more skilled counterparts began working. In short, access to the Pill may have altered selection into the labor market at younger ages, which could help explain the effect of the Pill on age-earnings profiles shown in figure 3.

[20] Our estimates are larger than those found in Goldin and Katz (2002, Table 5), who use a sample of U.S. born college graduate women ages 30 to 49 and find that the Pill increased the share in professional occupations, excluding teachers and nurses, by 0.4 percentage point (3 percent). One reason for the difference may be that their estimate includes women in their forties, where we find smaller effects.

To examine the importance of selection, we use a composite of respondents' performances on aptitude tests from their high school transcripts, which was reported to the *NLS-YW* in 1968 and called an "IQ score" in the documentation. IQ is available for only two-thirds of the sample, so we divide respondents into IQ tertiles (low, middle, and high) to maintain samples sizes large enough for disclosure.[21] Equation (8') is then estimated for each of the IQ tertiles separately. We also examine heterogeneous effects of *ELA* by educational attainment (any versus no college) and, for education outcomes, family background (socio-economic status tertiles of families when the respondent was 14). Whereas IQ tertile measured in high school is not affected by *ELA* directly (cf. table 1), educational attainment is (table 4). The latter breakdown should be viewed as a description to help us explore how different groups of women differentially benefitted from early access to the Pill.

Table 5 begins this analysis by examining the effect of ELA on women's hourly wages by IQ tertile and college attainment.[22] Whereas *ELA* reduces or has no significant effect on earnings for the lowest IQ tertile (column 1), it increases them in the middle and upper third of the IQ distribution (columns 2 and 3) for women aged 30 to 49. Almost all of the wage gains accrued to women in the middle of the IQ distribution, where the effects are largest both absolutely and relatively. For this group, women with *ELA* enjoyed greater hourly wages throughout their twenties and the premium grew to a statistically-significant 20 percent at ages 30 to 49.

It is worth noting that the estimates in this table are from a more flexible version of the regression model that allows the state, cohort and age group fixed effects to vary by IQ group. The fact that *ELA* had an effect *within* the middle IQ group suggests that the labor market gains described previously are not the sole result of shifts in the composition of the workforce. Furthermore, if the wage effects of *ELA* were driven by changing selection into the labor market by women with different ability levels, we would

[21] Griliches, Hall and Hausman (1978) point out that these IQ composite scores are missing "almost at random" in the *National Longitudinal Survey of Young Men*, which is also the case in the *NLS-YW*. See Appendix A for details on the composite score.
[22] We note that the results in table 5 are from samples that included observations with zero earnings, unlike table 3, which included only observations with positive earnings. This change was unfortunately necessary for disclosure reasons but does not affect the patterns we observe.

expect the overall wage effects from models without IQ controls to be substantially larger than those from table 5's models that stratify by IQ tertile. Instead, table 3 and table 5 imply similar average estimates (compare the *ELA* estimates averaged across the three IQ tertiles in table 5 to the overall population estimates in table 3).[23]

The fact that the wage effects are strongest for women attending some college suggests that one mechanism for these middle-IQ women was college enrollment. Although *ELA* conferred little if any wage premium for women without college (column 4), women with some college (column 5) experienced lower wages in their early twenties (perhaps as they worked at temporary jobs) but a 12-percent wage premium in their late thirties.[24] The effects for the highest IQ group are considerably smaller and not statistically significant at any age below 44, which suggests these women may have already been taking advantage of their educational and career opportunities without *ELA*. In contrast to these positive effects, the lowest IQ women with *ELA* suffered a statistically significant wage reduction of roughly 15 percent in their early thirties. Although this negative effect is consistent with the Pill increasing crowding in jobs where lower IQ women were working or decreasing the relative skills of lower IQ women, the estimate is not robust to the inclusion of state linear time trends (appendix table C5B). The lack of wage benefits for lower IQ women may be related to the limited returns to human capital investments in low-skilled jobs or the absence altogether of these women's investments in their human capital, which we examine next.

The next set of tables explores how the Pill affected human capital investments and paid work by IQ and childhood SES. The estimates in table 6, which uses highest grade completed as a dependent variable, are roughly consistent with the pattern of *ELA*'s effects on wages. *ELA*'s effects on education

---

[23] There are two other reasons why the averages of the estimates in table 5 might differ from those in table 3: the smaller sample in table 5 (excluding women with missing IQ information) and the different outcome variable (including women with zero earnings). We further confirmed that the averages of the ability-group specific *ELA* estimates are also similar to the overall estimates when the samples both include women with zero earnings: the former tend to be smaller at younger ages but larger for women in their forties.

[24] The estimated effects of *ELA* by college attainment in Tables 5 (for wages) and 7 (for experience) may be downward biased because of compositional effects. If the marginal women who attended college because of *ELA* were on average higher ability than the women with *ELA* and no college, but of lower average ability than the women who attended college even without *ELA*, the estimated effects of *ELA* on average wages and experience for each group will appear lower than the actual impact on individuals in either group.

are large and positive in the middle of the IQ distribution and negative for the lowest IQ group. (These negative effects may reflect higher IQ women crowding out lower IQ women in colleges.) Unlike the wage estimates, however, *ELA*'s effects on education are also large and statistically significant for the highest IQ tertile. By age forty, *ELA*'s effects for the middle and upper IQ groups translate into a 0.4 to 0.5 year schooling advantage. The right side of the table shows that *ELA*'s effects are largest for women from the *lowest* SES households (columns 4 through 6). Women with *ELA* from the most disadvantaged backgrounds attained roughly *half of a year more education* than their peers (column 4). This is a large effect, amounting to roughly one third of the difference in grades completed between women in the low and middle SES groups.[25] Although our data do not reveal whether these effects arise at the stage of high school completion, college admission, or class standing and persistence, it is clear that higher IQ women with access to the Pill—especially those from disadvantaged households—were more likely to continue their educations. Thus, *ELA* shifted women's educational attainment into more of a meritocracy.

Is the heterogeneity in the Pill's effects by IQ apparent for labor-force attachment as well? Table 7 uses cumulative labor-force experience to examine this question. As with education, the effect of *ELA* on labor-force experience is largest for women in the middle third of the IQ distribution and with some college. Middle IQ women (column 2) with *ELA* had accumulated 2,200 to 4,800 additional hours of work experience by their early thirties to late forties. Women in the highest IQ group (column 3) with *ELA* also participated more, but these effects on experience are smaller and less precise. Echoing the wage results, the effects of *ELA* on labor-force experience are largest for women with some college (column 5).[26]

---

[25] The effect of *ELA* on college enrollment among 20 to 24 year olds for the lowest IQ group was 0.9 percentage points (s.e. 3.6, mean 12 percent); it was 3.9 (s.e. 3.5, mean 19 percent) and 5.9 percentage points (s.e. 2.7, mean 37 percent) for the middle and upper IQ groups, respectively. The effect of *ELA* on college enrollment among 20 to 24 year olds for the lowest SES group was 11.3 percentage points (s.e. 3.8 percentage points), an implied increase of 108 percent (of the mean of 10.5 percent). It was 3.9 percentage points (s.e. 4.1, mean 21 percent) and 2.1 percentage points (s.e. 3.0, mean 36 percent), respectively, for the middle and upper SES groups.

[26] We also directly estimated the effect of *ELA* by IQ tertile and college attendance on labor force participation. The heterogeneity in effects is similar: women in the middle IQ tertile in their late twenties and early thirties show the largest increases in participation. Higher IQ women also show increased participation at these ages, but the estimates are smaller and less precise. Women with some college show significant participation responses to ELA as well, with significantly lower rates in their early twenties, followed by significantly higher rates over the next decade.

In summary, the data provide strong support that the Pill influenced *which women* invested in their careers and shifted into paid work. Given the lack of labor-supply or schooling gains for low IQ women, the Pill appears to have induced positive selection into higher education as well as the labor market. This analysis also shows different responses to early access to the Pill across IQ tertiles. While lower IQ women with *ELA* did not gain ground in terms of education or experience, both middle and higher IQ women raised their educational attainment and those with some college became more likely to work for pay. Interestingly, the Pill's largest effects on work experience accrued to women in the middle of the IQ distribution, not to the high achievers who have been the focus of other studies. Thus, our findings highlight the different ways in which women across the IQ distribution used the flexibility conferred by early access to the Pill to opt into paid work.[27]

VI. DECOMPOSING PILL-INDUCED WAGE GAINS

To quantify the contribution of each of these different human capital investments to the estimated Pill premium in wages, we decompose women's *ELA*-induced log hourly wage premium in their late forties into five components: formal education, on-the-job training, cumulative experience, occupational choice, and changes in marital status (that affect wages through the income of a spouse). We present results using the standard Blinder-Oaxaca decomposition at the mean (Blinder 1973; Oaxaca 1973) and the recentered influence function procedure (RIF) proposed in Firpo, Fortin, and Lemieux (2009), which generalizes Blinder-Oaxaca to other quantiles. This approach has the advantage of not being sensitive to the decomposition order and permits a richer characterization of the importance of Pill-induced changes in productive characteristics at different points in the skill distribution. To implement both procedures, we

[27] Another potential mechanism for the Pill's wage effects is its interaction with the marriage market and the size of spousal earnings. To investigate this "marriage-market channel," appendix table C4 in online appendix C examines the relationship of *ELA* with both the likelihood of never having married (panel A) and the likelihood of having divorced (panel B) by IQ group and college attendance. In almost all cases, we cannot reject that the likelihood of having married is unrelated to *ELA*. In contrast, divorce rates were significantly higher for women with *ELA* in the lower IQ groups and among women without any college. Women in the lowest third of the IQ distribution with *ELA* were almost twice as likely to divorce (9.7 percentage points) by their late twenties (panel B, column 1). Similarly, *ELA* women with no college were almost 34 percent (4.4 percentage points) more likely to divorce. However, these effects are for the wrong groups of women to be driving the wage effects. Although they are strong for women in the middle of the IQ distribution, they appear for those without any college—not the middle IQ women who pursued college. In short, little evidence points to divorce and the absence of a second earner as the explanation for the wage effects.

restrict the estimation sample to the last available wage observation for each woman in the 45 to 49 age group and use women without ELA as the reference group.

Table 8 quantifies how much of the difference in the log hourly wage premium of women with *ELA* at various points along the wage distribution can be explained (in an accounting sense) by each of the characteristics. Panel A reports the Blinder-Oaxaca decompositions at the mean and shows that cumulative experience accounts for just under two-thirds of the Pill premium. Education and occupation each account for another sixth of the gap, with both job training and marriage having negligible effects. Together, these five factors explain over 90 percent of the *ELA* wage premium at the mean.

What do our estimates imply about the returns to education and experience for women? Women with *ELA* obtained 0.18 years more schooling by their late forties (table 4, col. 3), which increased their wages by 0.015 log-points (table 8, panel A), for an implied return of 0.083 (=0.015/0.18). If we also attribute the entire 0.014 log-point increase in wages (table 8, panel A) from occupational upgrading to schooling, the total return to women's schooling would be 0.161 (=0.029/0.18). These estimates are both within a plausible range of Heckman, Lochner and Todd's (2006) 0.128 estimate of the returns to education for white men in 1990 (p. 326). For the same group, Heckman, Lochner and Todd estimate coefficients on experience and experience squared of 0.1301 and −0.0023, respectively (Ibid). Applying these returns to experience to our estimates indicates that, from an initial experience level of 15 years, that 0.57 years more experience (table 4, col. 1) would increase women's log-wages by 0.034 ($0.1301*0.57 -0.0023*(15.57^2-15^2)$). Our decomposition attributes more than that, 0.056 log points, to the 0.57 years of additional experience, which is also reasonable if the returns to women's experience are higher than the returns for men or level off less quickly (cf. Weinberger and Kuhn 2010).

The results of the RIF procedure, shown in panel B, are consistent with the Oaxaca-Blinder decompositions, with experience accounting for the largest share of the premium, followed by education and occupation.[28] The relative roles of experience and education-occupation, however, vary at different points in the wage distribution. Consistent with table 5's result that the largest wage effects occur for women in the middle of the IQ distribution, panel B shows that the total log-wage differential associated with *ELA* varies non-monotonically across the distribution and is largest (0.106) at the median. Furthermore, education and occupation explain relatively more of the wage gap (and cumulative experience relatively less) higher in the wage distribution, which accords with the results from table 7 showing stronger cumulative experience in the middle rather than highest IQ group. At the 25th percentile the five components explain nearly all of the wage gap while at the median they explain about 85 percent of the gap; at the 75th percentile, they actually over-explain the gap suggesting they may be offset by other factors near the top of the wage distribution.

VII. THE "OPT-*IN*" REVOLUTION

In 2003, Lisa Belkin's *New York Times Magazine* article, "The Opt-Out Revolution," reopened the debate about the reasons for persistent differences in women's and men's labor market outcomes. In particular, she argued that the women who might have been the professional equals of men *chose not to be*—these women "opted out" to raise their children. Shang and Weinberg (2009) find some evidence that college graduate women have begun to have more children, but these changes seem small relative to the Opt-*In* Revolution that began 50 years ago.

This paper quantifies the role of the Pill in catalyzing this revolution. As the Pill provided younger women the *expectation* of greater control over childbearing, women invested more in their human capital and careers. Most affected were women in the middle of the IQ distribution and with some college, who experienced remarkable wage gains over their lifetimes. To put our results into perspective, the Pill-induced effects on wages amount to roughly one-third of the total wage gains for women in their

---

[28] The decomposition results are also similar if we use the semi-parametric approach of DiNardo, Fortin, and Lemieux (1996) to re-weight the characteristics of women without *ELA* to resemble those of women with *ELA* at different points in the distribution.

forties born from the mid-1940s to early 1950s.[29] Our decomposition shows that almost two thirds of these Pill induced gains (at the mean) can be attributed to increasing labor-market experience and another third is due to greater educational attainment and occupational upgrading.

What do our estimates imply about the importance of the Pill in narrowing the gender gap from 1980 to 2000? To answer this, we simulate a counterfactual hourly wage distribution from the 1980, 1990, and 2000 population censuses by removing age-specific estimates of early legal access to the Pill from the earnings of cohorts born after 1940 (table 3, column 2) and compute the actual hourly wage distribution for men and women in 1980, 1990 and 2000.[30] From 1980 to 1990, the actual gender gap in real hourly wages for 25 to 49 year olds closed by 0.126 log points, and the simulated gender gap closed by 0.113 log points. From 1990 to 2000, the actual gender gap in real hourly wages closed by 0.074 log points, and the simulated gender gap closed by 0.051 log points. Our main estimates, therefore, imply that 10 percent of the narrowing in the gender gap during the 1980s and 31 percent during the 1990s can be attributed to early access to the Pill. While improvements in contraception play an important role in increasing women's earnings, our results also implicitly highlight the importance of other factors. The unexplained component of cross-cohort changes due to, for example, shifts in the demand for women's labor (e.g., anti-discrimination legislation and enforcement or changes in preferences) as well as shifts in the quality of women's education remain substantial.

Did the Pill unleash the Opt-*In* Revolution? Our results provide no conclusive answer. They may understate the Pill's broader influence because our empirical strategy does not allow us to explore the

---

[29] This estimate is obtained by comparing the coefficients for *ELA**40-44 and *ELA**45-49 in table 3 to the total change in wage rates for women in their 40s between the 1943-46 and the 1951-1954 cohorts in the *NLS-YW.* Weinberger and Kuhn (2010) distinguish between changing "levels," the starting wage at labor-force entry, and "slopes," the growth in wages after entry, and argue that changes in "slopes" can account for one third of the narrowing in the gender gap over the last 40 years—a number they argue provides a reasonable upper bound for the importance of all post-schooling investments. Our measures of career investment combine both pre-market investments (e.g., college and occupational choice, which should shift levels) and post-market investments (e.g., labor market experience and on-the-job training, which should shift slopes).

[30] Real hourly wage is total wage and salary earnings of last year divided by the product of weeks worked last year and usual hours worked per week and divided by the PCE deflator to get year 2000 dollars. The estimates use IPUMS person weights and exclude real hourly wage outliers of less than $2 or more than $200. The sample contains native-born women ages 25 to 49 whose wages were not imputed and who were not self-employed. The simulated log hourly earnings values are adjusted by subtracting the estimates in column 2 of table 3 for women who were born in or after 1940 and born in a state where they would have had early access to the Pill.

effect of changes in access to the Pill beyond age 20 and fails to capture the potentially large social multiplier effects. For instance, the Pill's availability likely altered norms and expectations about marriage and childbearing and firms' decisions to hire and promote women—even among cohorts without legal access to the Pill. Thus, the effects of the Pill may be larger than we find, though it is not clear how much larger. Even these conservative estimates, however, suggest that the Pill's power to transform childbearing from probabilistic to planned shifted women's career decisions and compensation for decades to come.

VIII. References

Acemoglu, Daron, David H. Autor, and David Lyle. (2004). "Women, War and Wages: The Effect of Female Labor Supply on the Wage Structure at Mid-Century." *Journal of Political Economy* 112(3): 497-551.

Ananat, Elizabeth Oltmans and Daniel M. Hungerman. Forthcoming. "The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics." *Review of Economics and Statistics*.

Bailey, Martha J. (2006). "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." *Quarterly Journal of Economics* 121(1): 289-320.

Belkin, Lisa. (2003). "The Opt-Out Revolution," *New York Times Magazine*, Sunday, Oct. 26.

Black, Sandra E. and Chinhui Juhn. (2000). "The Rise of Female Professionals: Are Women Responding to Skill Demand" *American Economic Review* 90(2): 450-455.

Black, Sandra E. and Alexandra Spitz-Oener. (2010). "Explaining Women's Success: Technological Change and the Skill Content of Women's Work." *Review of Economics and Statistics* 92(1): 187-194.

Blau, Francine D. and Marianne A. Ferber and Anne E. Winkler. (2010). *The Economics of Women, Men and Work*. (Prentice Hall).

Blau, Francine D. and Lawrence M. Kahn. (1997). "Swimming Upstream: Trends in the Gender Wage Differential in 1980s." *Journal of Labor Economics* 15(1): 1-42.

Blau, Francine D. and Lawrence M. Kahn. (2004). "The U.S. Gender Pay Gap in the 1990s: Slowing Convergence." *NBER Working Paper 10853.*

Blinder, Alan S. 1973. "Wage Discrimination: Reduced Form and Structural Variables." *Journal of Human Resources* 8: 436-455.

Bureau of Economic Analysis (BEA) (2009). National Income Product Accounts, Table 1.1.4: Personal Consumption Expenditures Price Index. Available at: http://www.bea.gov/national/nipaweb/SelectTable.asp?Selected=Y, accessed 8/17/2009.

Chiappori, Pierre-Andre, and Sonia Oreffice. (2008). "Birth Control and Female Empowerment: An Equilibrium Analysis." *Journal of Political Economy* 116(1): 113-140.

DiNardo, John, Nicole M. Fortin and Thomas Lemieux. (1996). "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach." *Econometrica* 64(5): 1001-44.

Duncan, O. (1961). "A Socioeconomic Index for All Occupations." *Occupations and Social Status*.

Fernandez, Raquel and Alessandra Fogli. (2009). "Culture: An Empirical Investigation of Beliefs, Work, and Fertility." *American Economic Journal: Macroeconomics* 1 (1): 146-177.

Fernandez, Raquel, Alessandra Fogli, and Claudia Olivetti. (2004). "Mothers and Sons: Preference Formation and Female Labor Force Dynamics." *Quarterly Journal of Economics* 119 (4): 1249-99.

Firpo, Segio, Nicole M. Fortin and Thomas Lemieux. (2009). "Unconditional Quantile Regressions." *Econometrica* 77 (3): 953-973.

Fortin, Nicole. (2009). “Gender Role Attitudes and Women’s Labor Market Participation: Opting-Out, AIDS, and the Persistent Appeal of Housewifery.” University of British Columbia Working Paper, http://www.econ.ubc.ca/nfortin/Fortin_Gender.pdf, accessed 9/1/2009.

Goldin, Claudia. (1990). *Understanding the Gender Gap: An Economic History of American Women.* (New York: Oxford University Press).

Goldin, Claudia. (2004). “The Long Road to the Fast Track: Career and Family.” *Annals of the American Academy of Political and Social Science* 596(1): 20-35.

Goldin, Claudia. (2006). “The Quiet Revolution that Transformed Women's Employment, Education, and Family.” *American Economic Review* 96(2): 1-21.

Goldin, Claudia and Lawrence Katz. (2002). “The Power of the Pill: Oral Contraceptives and Women’s Career and Marriage Decisions.” *Journal of Political Economy* 110(4): 730-770.

Goldin, Claudia and Lawrence Katz. (2010). “Putting the Co in Education: Timing, Reasons, and Consequences of College Coeducation from 1835 to the Present.” *NBER Working Paper 16281*.

Goldin, Claudia, Lawrence Katz and Ilyana Kuziemko. (2006). “The Homecoming of American College Women: The Reversal of the College Gender Gap.” *Journal of Economic Perspectives* 20(4): 133-156.

Griliches, Zvi, Bronwyn H. Hall, and Jerry A. Hausman. (1978). “Missing Data and Self-Selection in Large Panels.” *Annales de l’inséé* 30/31: 137-176.

Heckman, James J., Lance J. Lochner and Petra Todd (2006). “Earnings Functions, Rates of Return and Treatment Effects: The Mincer Equation and Beyond.” In eds. Eric A. Hanushek and Finis Welsh. *Handbook of the Economics of Education, vol. 1* (Elsevier: Amsterdam): 307-358.

Loughran, David S. and Julie M. Zissimopoulous (2009). “Why Wait? The Effect of Marriage and Childbearing on the Wages of Men and Women.” *Journal of Human Resources* 44 (2): 326-49.

Miller, Amalia (2011). “The Effects of Motherhood Delay on Career Path.” *Journal of Population Economics* 24(3): 1071-1100.

Mulligan, Casey and Yona Rubinstein (2008). “Selection, Investment and Women’s Relative Wages over Time.” *Quarterly Journal of Economics* 123(3): 1061-1110.

Oaxaca, Ronald. 1973. “Male-Female Wage Differentials in Urban Labor Markets.” *International Economic Review* 14: 693-709.

O’Neill, June and Solomon Polachek (1993). “Why the Gender Gap in Wages Narrowed in the 1980s.” *Journal of Labor Economics* 11(1): 205-228.

Paul, Eve, Harriet Pilpel, and Nancy Wechsler (1974). “Pregnancy, Teenagers and the Law, 1974.” *Family Planning Perspectives* 6(3): 142-147.

Paul, Eve, Harriet Pilpel, and Nancy Wechsler (1976). “Pregnancy, Teenagers and the Law, 1976.” *Family Planning Perspectives* 8(1): 16-21.

Shang, Qingyan and Bruce Weinberg. (2009). “Opting for Families: Recent Trends in the Fertility of Highly Educated Women.” Mimeo, November.

Stevenson, Betsey and Justin Wolfers. (2007). “Marriage and Divorce: Changes and their Driving Forces,” *Journal of Economic Perspectives* 21(2):27-52.

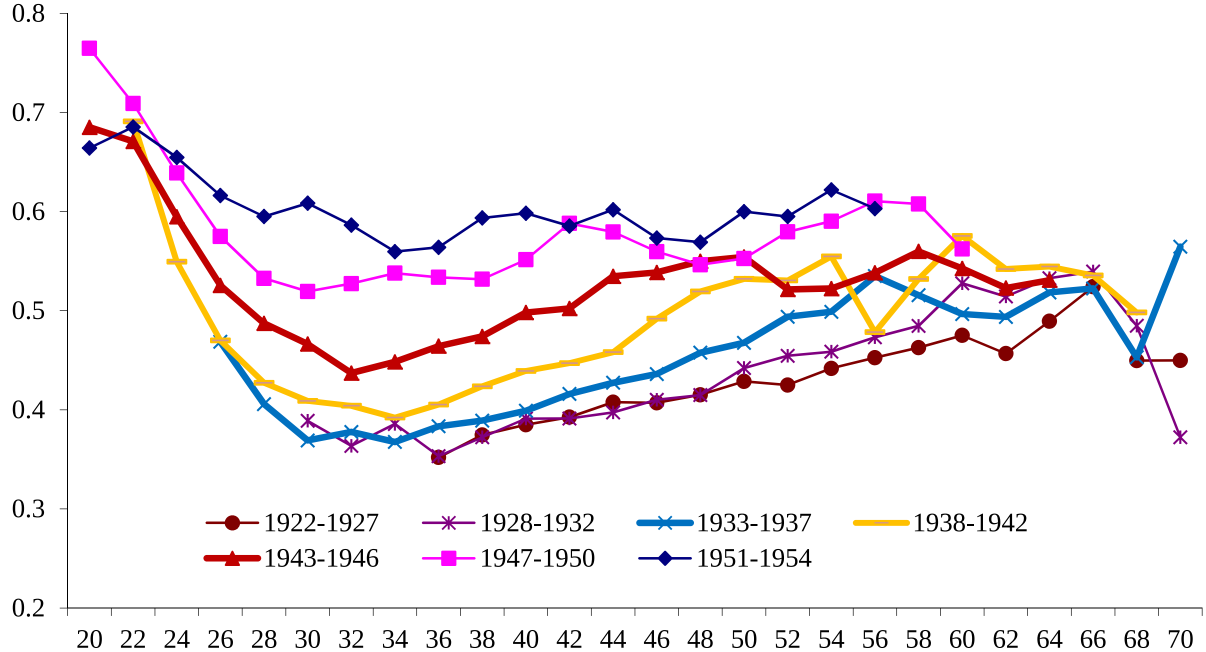
Weinberg, Bruce. (2000). “Computer Use and the Demand for Female Workers.” *Industrial and Labor Relations Review* 53(2): 290-308.

Weinberger, Catherine and Peter Kuhn. (2010). “Changing Levels or Changing Slopes? The Narrowing of the U.S. Gender Earnings Gap, 1959-1999.” Industrial and Labor Relations Review 63 (3): 384-406.

Welch, Finis. (2000). “Growth in Women’s Relative Wages and in Inequality among Men: One Phenomenon or Two?” *American Economic Review Papers and Proceedings* 90: 444–449.

Wellington, Allison. (1993). “Changes in the Male/Female Wage Gap, 1976-85.” *Journal of Human Resources* 28(2): 383-411.

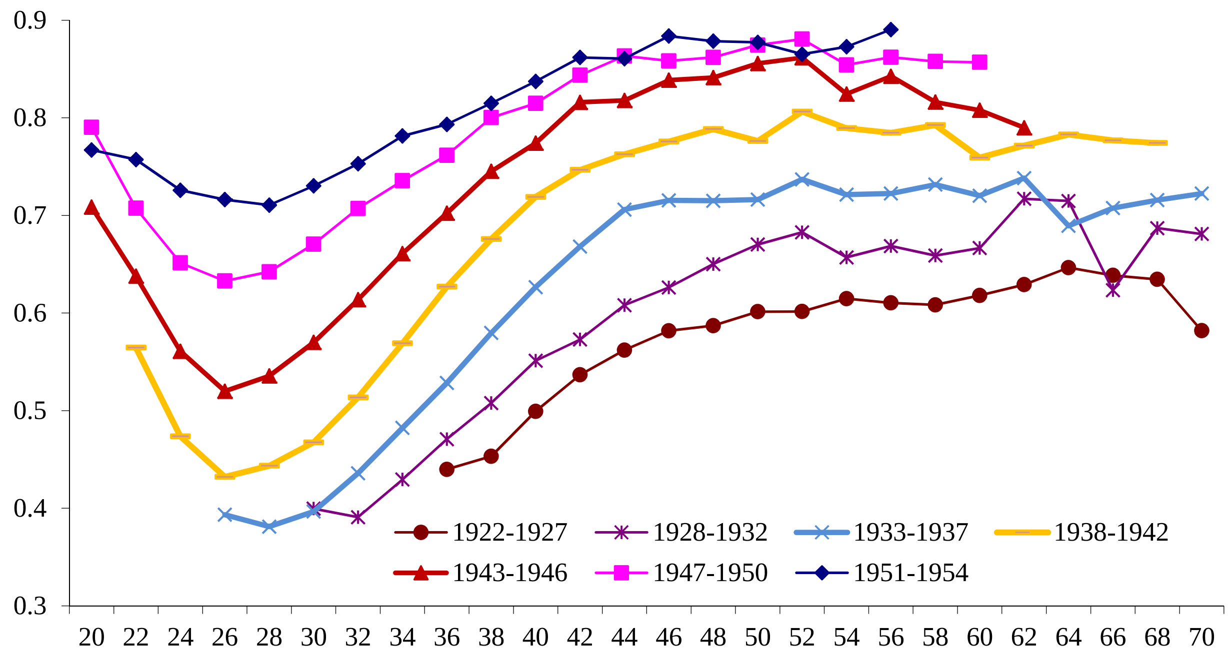
Figure 1. The Evolution of the Real Annual Wage Earnings of Women Relative to Men by Age and Birth Cohort



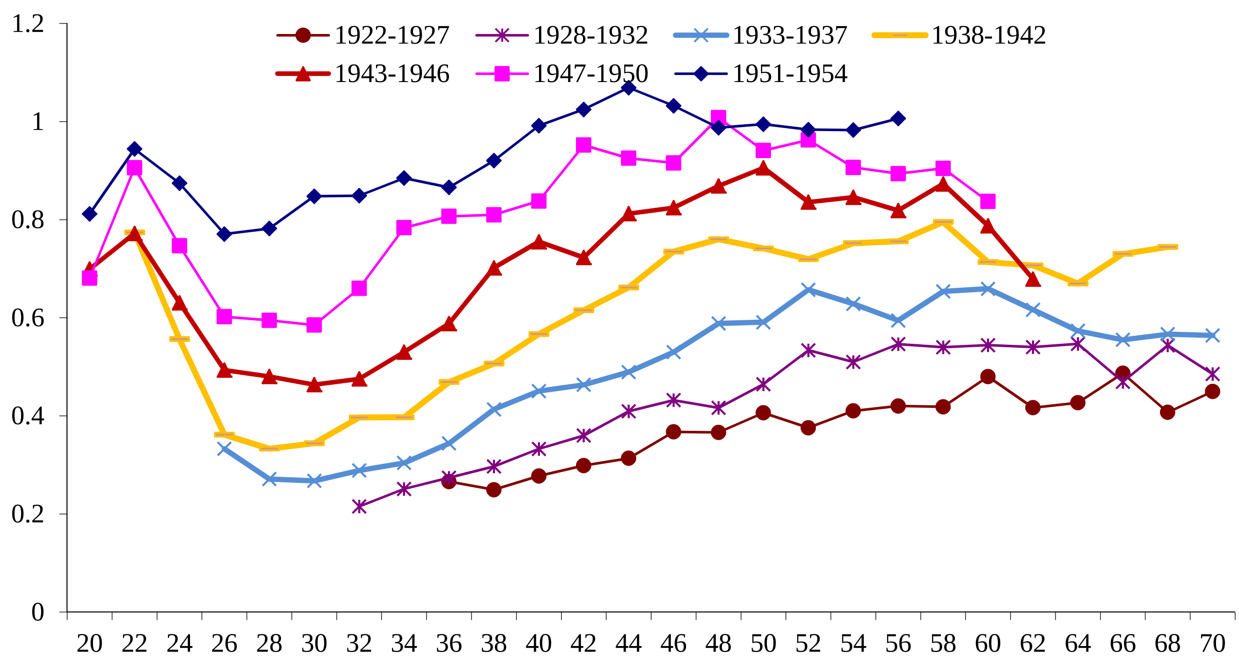
Annual labor earnings include income from all jobs, including self-employment. The series is adjusted for inflation to year 2000 dollars using the personal consumption expenditures deflator (BEA 2009). Data are weighted using *CPS* sample weights and collapsed into two-year age groups. *Source*: 1964-2009 March *CPS*.

Figure 2. The Evolution of Human Capital Investments by Age and Birth Cohort

A. Share of Women Participating in the Labor Force Relative to Men

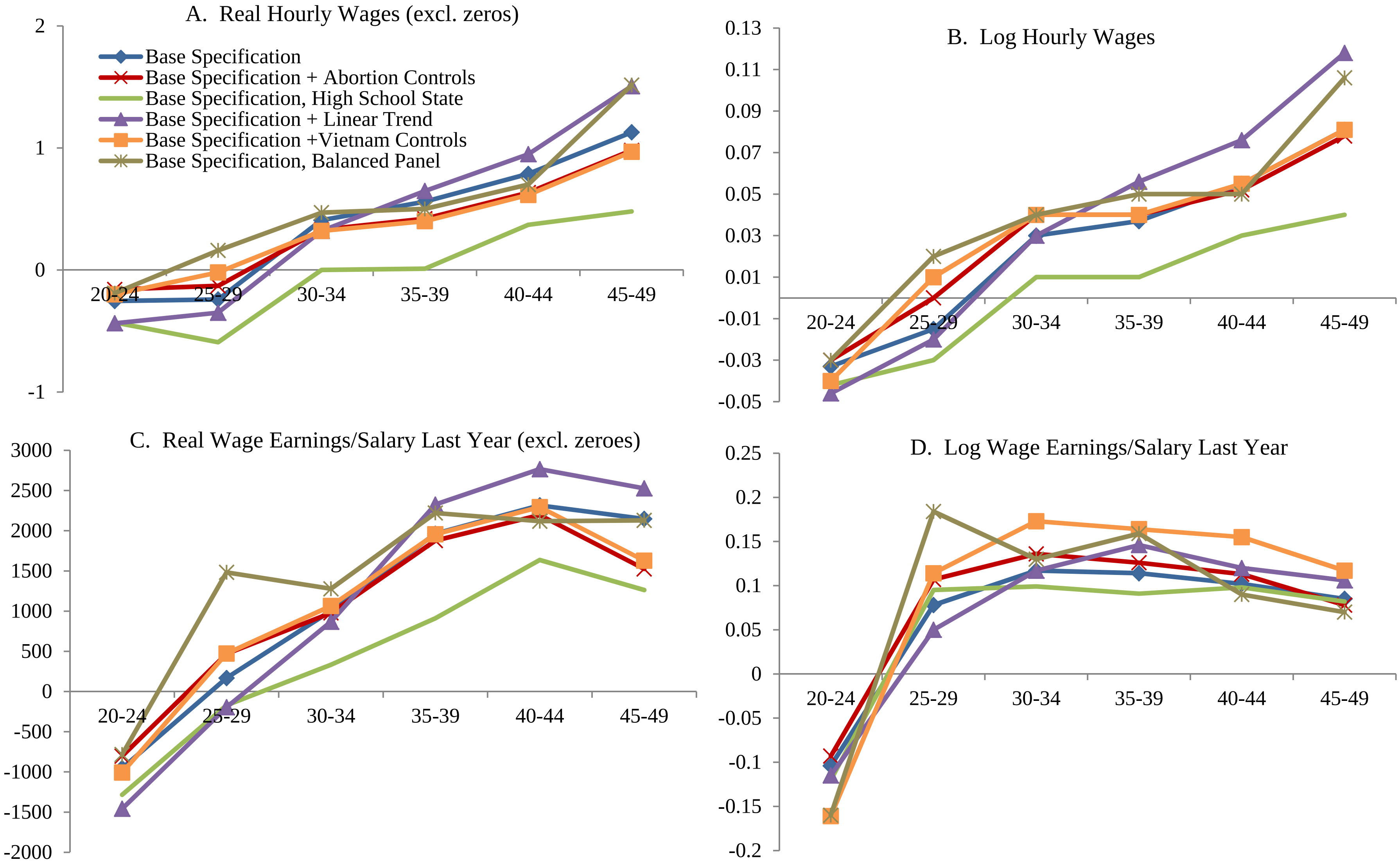


B. Share of Women Working in Professional and Managerial Jobs Relative to Men



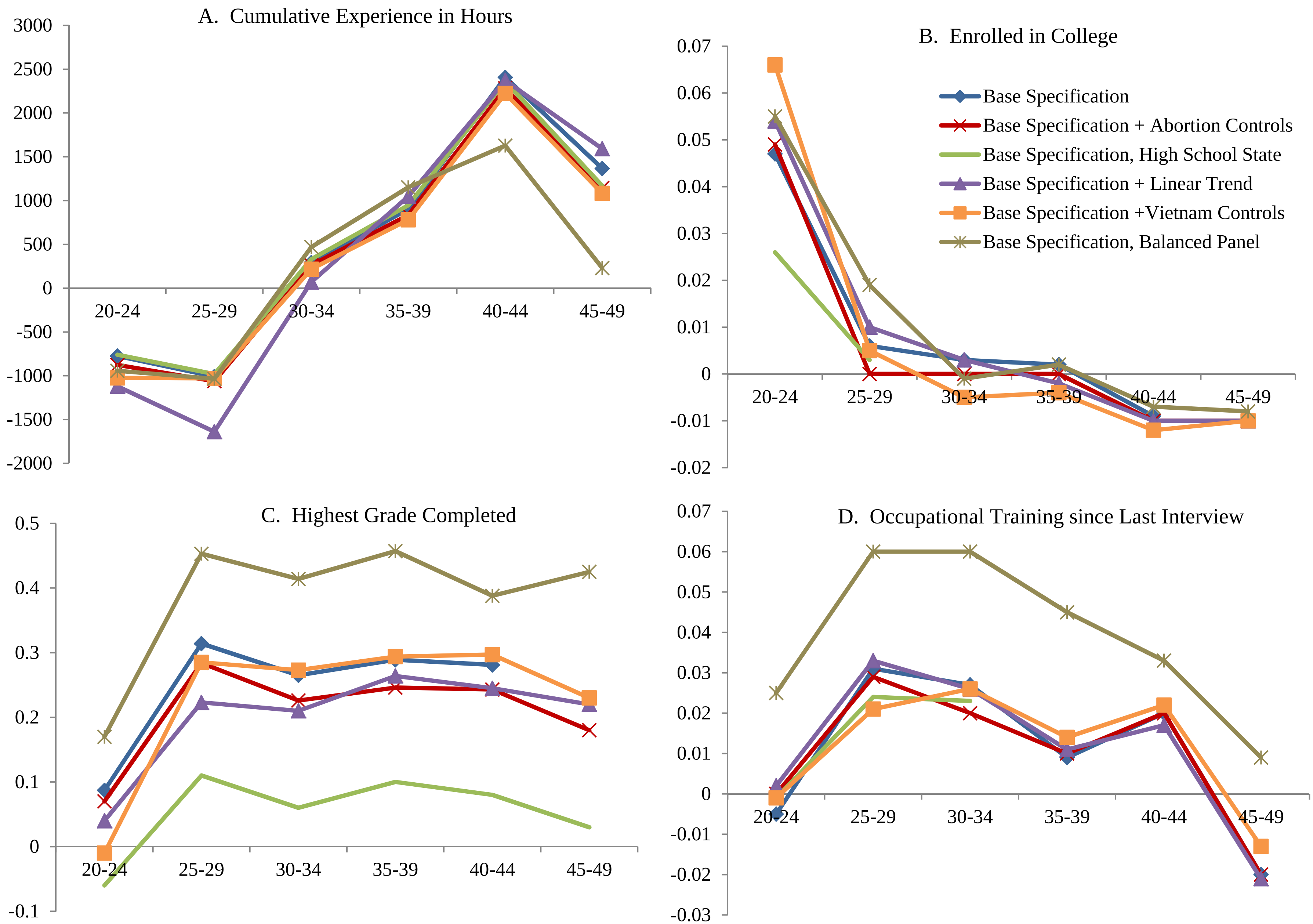
Share participating in the labor force is constructed from a binary variable indicating whether the respondent was employed or looking for a job at the time of the survey. Job groups are coded using the 3-digit Census occupational codes in the *CPS*. Women are counted in a job category only if they are employed at the time of the survey. Data are weighted using *CPS* sample weights and collapsed into two-year age groups. Source: 1964-2009 March *CPS.*

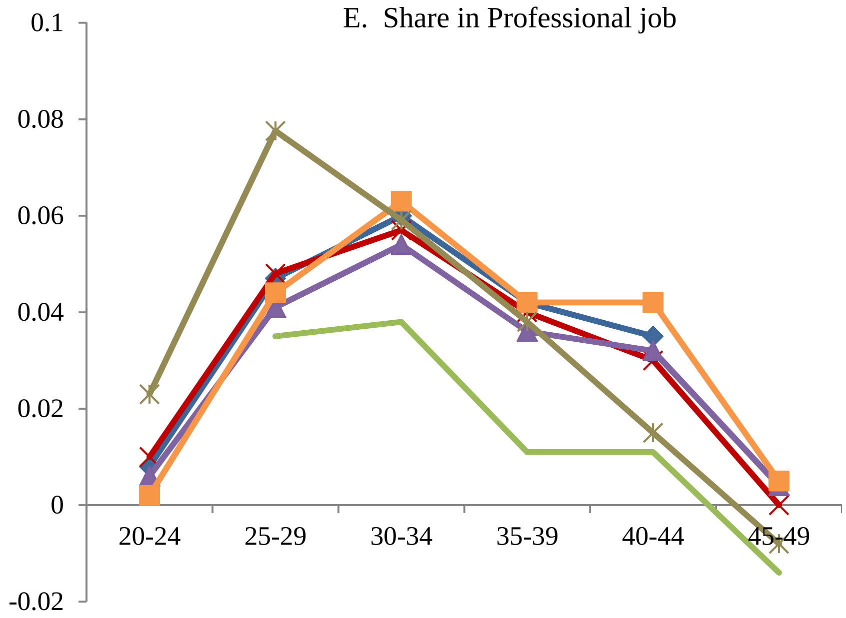
Figure 3. The Effects of Early Access to the Pill on Lifecycle Wage Earnings

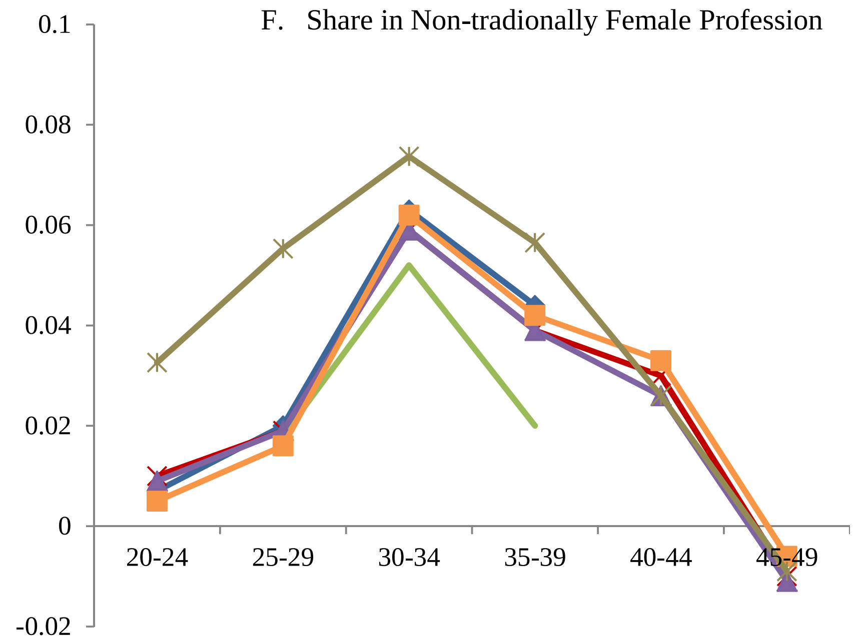


Wage earnings are in 2000 dollars using the personal consumption expenditures deflator (BEA 2009). Each panel plots β_g from six different regressions: baseline specification (equation 8), baseline + abortion controls (equation 8') which corresponds to our tables, and four variants of equation (8'): one with linear, state-specific time trends; another including controls for Vietnam casualties; another using only a balanced sample of respondents (those missing information in any year or attriting are omitted); and another using state where the respondent attended high school to match to *ELA* (see footnotes 20 and 24 regarding selection problems with this sample). Source: NLS-YW.

Figure 4. The Effects of Early Access to the Pill on Lifecycle Human Capital Investments







See notes to figure 3.

Table 1. Relationship of ELA to Pre-Treatment Respondent Characteristics

	Father worked for pay	Father held professional job	Mother worked for pay	Mother held professional job	Duncan index of occupation of head	Family socio-economic status in 1968
ELA	-0.020	0.023	0.003	0.046	0.692	-0.288
	(0.012)	(0.029)	(0.029)	(0.029)	(1.617)	(1.664)
Observations	4352	3930	3754	1426	3930	4100
R-squared	0.01	0.04	0.03	0.05	0.07	0.14
Mean of D.V.	0.929	0.195	0.387	0.126	31.625	99.917
	Magazines available	Newspapers available	Respondent held library card	Lived in two-parent household	Number of siblings in 1968	Father born in U.S
ELA	-0.017	-0.019	-0.012	-0.016	-0.138	-0.017
	(0.029)	(0.022)	(0.033)	(0.025)	(0.194)	(0.012)
Observations	4341	4345	4346	4354	4323	4353
R-squared	0.07	0.09	0.13	0.03	0.07	0.05
Mean of D.V.	0.637	0.833	0.695	0.816	3.586	0.959
	Highest grade completed by father in 1968	Highest grade completed by mother in 1968	Parents' desired education for respondent	Index of atypicality of mother's job	Respondent's IQ score in 1968 (age-adjusted)	Rural residence
ELA	0.065	0.101	-0.105	0.033	1.189	0.027
	(0.241)	(0.210)	(0.179)	(2.490)	(1.430)	(0.030)
Observations	3228	3893	3907	1786	2879	4348
R-squared	0.12	0.09	0.02	0.05	0.08	0.09
Mean of D.V.	10.044	10.313	13.337	29.909	102.091	0.256

See data appendix for more information on survey questions and variable coding. Characteristics are measured at age 14, unless otherwise indicated. Each of the separate regressions also includes a set of state of residence and birth cohort fixed effects. Heteroskedasticity-robust standard errors are corrected for clustering at the state level and are presented in parentheses below each estimate

Table 2. The Impact of ELA on Pill Use among Ever Married Women

	(1)	(2)	(3)	(4)	(5)
	1=Used Pill before age 18	1= Used Pill before age 19	1= Used Pill before age 20	1= Used Pill before age 21	1= Used Pill before age 22
Mean of DV	*0.034*	*0.119*	*0.226*	*0.369*	*0.506*
Panel A: Pill Use					
ELA	-0.056	0.171	0.188	0.158	0.050
	(0.017)	(0.204)	(0.142)	(0.084)	(0.040)
R-squared	0.048	0.105	0.124	0.136	0.127
Panel B. Pill Use Heterogeneity					
ELA	-0.052	0.304	0.254	0.269	0.105
	(0.020)	(0.168)	(0.117)	(0.088)	(0.061)
ELA x Non-metro area	-0.004	-0.167	-0.084	-0.142	-0.073
	(0.014)	(0.060)	(0.057)	(0.067)	(0.055)
R-squared	0.049	0.108	0.125	0.137	0.128
Observations	1985	1985	1985	1985	1985
Fixed effects	S, Y	S, Y	S, Y	S, Y	S, Y

Panel A presents the estimates of equation 9, while Panel B presents estimates from equation 9 augmented with a dummy for non-metropolitan area and the interaction of this dummy with *ELA*. Both panels use are estimated with a linear probability model on the 1942 to 1948 birth cohorts from the 1970 National Fertility Survey, which sampled ever married women. These cohorts are chosen so that the youngest women (born in 1948) were at least 22 in 1970 and that the oldest women (born in 1940) would have varied in their legal access to the Pill by age 21. All regressions include state fixed effects (S) and cohort fixed effects (Y). Heteroskedasticity-robust standard errors are corrected for clustering at the state level and are presented in parentheses below each estimate.

Table 3. The Impact of Early Access to the Pill on Wages and Annual Incomes

		(1)	(2)		(3)	(4)		(5)
	Mean real hourly wages excl. zeros	Real hourly wage (excl. zeros)	Log real hourly wage	*Mean real wages/salary last year excl. zeros*	Wage or salary last year (excl. zeros)	Log real annual wage	*Mean real wages/salary last year incl. zeros*	Wage or salary last year (incl. zeros)
ELA * Ages 20-24	*7.88*	-0.160 (0.315)	-0.030 (0.025)	*9943*	-804 (681)	-0.093* (0.053)	*7661*	-1,187* (625)
ELA * Ages 25-29	*9.60*	-0.130 (0.347)	0.000 (0.028)	*15610*	469 (741)	0.107** (0.046)	*10911*	202 (721)
ELA * Ages 30-34	*10.62*	0.330 (0.332)	0.040 (0.028)	*18116*	978 (731)	0.136** (0.059)	*12452*	803 (683)
ELA * Ages 35-39	*11.74*	0.420 (0.333)	0.040 (0.027)	*21173*	1,878** (749)	0.126** (0.050)	*15442*	1,449* (744)
ELA * Ages 40-44	*12.84*	0.634* (0.334)	0.052** (0.024)	*24493*	2,196** (919)	0.113** (0.045)	*19184*	2,674*** (892)
ELA * Ages 45-49	*14.29*	0.980** (0.448)	0.078** (0.031)	*28148*	1,526* (781)	0.078* (0.047)	*25238*	3,376*** (919)
Fixed effects		Y, S, A	Y, S, A		Y, S, A	Y, S, A		Y, S, A
Observations		46388	46388		51277	51277		68169
Unique women		4210	4210		4245	4245		4351
R-squared		0.22	0.27		0.01	0.10		0.01

* significant at 10%; ** significant at 5%; *** significant at 1%

Wages are adjusted to 2000 dollars using the PCE deflator (BEA 2009). All regressions include state fixed effects (S); cohort fixed effects (Y); age group fixed effects (A); controls for abortion access; and abortion access controls interacted with ELA as described in equation (8'). Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate.

Table 4. The Impact of Early Access to the Pill on Human Capital Accumulation and Occupational Upgrading

	(1)	(2)	(3)	(4)	(5)	(6)
	Cumulative Experience in Hours	1= Enrolled in College	Highest Grade Completed	1=Occupational training since last interview	1= in Professional Job	1=in Non-traditional Job
ELA * Age 20-24	-876**	0.049**	0.070	0.000	0.010	0.010
	(369)	(0.022)	(0.136)	(0.013)	(0.013)	(0.008)
ELA * Age 25-29	-1,062**	0.000	0.284**	0.029***	0.048***	0.019*
	(443)	(0.008)	(0.131)	(0.011)	(0.019)	(0.011)
ELA * Age 30-34	263	0.000	0.226*	0.020	0.057***	0.059***
	(405)	(0.013)	(0.132)	(0.016)	(0.021)	(0.016)
ELA * Age 35-39	836	0.000	0.246*	0.010	0.040	0.039**
	(550)	(0.010)	(0.133)	(0.018)	(0.023)	(0.019)
ELA * Age 40-44	2,282***	-0.010	0.243*	0.020	0.030	0.030
	(784)	(0.010)	(0.129)	(0.022)	(0.027)	(0.020)
ELA * Age 45-49	1,143	-0.010	0.180	-0.020	0.000	-0.010
	(988)	(0.007)	(0.145)	(0.020)	(0.021)	(0.018)
Fixed Effects	Y,S,A	Y,S,A	Y,S,A	Y,S,A	Y,S,A	Y,S,A
Observations	61736	57373	78809	63013	73737	73737
Unique women	4329	3702	4354	4323	4354	4354
(Pseudo) R-squared	0.62	0.15	0.15	0.03	0.07	0.09
Mean of DV for 20-24	2723	0.241	12.09	0.203	0.086	0.044
Mean of DV for 25-29	5929	0.077	12.52	0.188	0.163	0.080
Mean of DV for 30-34	10758	0.072	12.85	0.245	0.199	0.137
Mean of DV for 35-39	16098	0.065	12.99	0.285	0.242	0.202
Mean of DV for 40-44	22609	0.049	13.13	0.310	0.249	0.225
Mean of DV for 45-49	30010	0.029	13.28	0.324	0.242	0.218

* significant at 10%; ** significant at 5%; *** significant at 1%

Columns (2) and (4)-(6) report average marginal effects from probit specifications; columns (1) and (3) report coefficients from OLS regressions. All regressions include state fixed effects (S); cohort fixed effects (Y); age group fixed effects (A); controls for abortion access; and abortion access controls interacted with ELA as described in equation (8’). Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate.

Table 5. Heterogeneity in the Impact of Early Access to the Pill on Real Hourly Wages

	(1)	(2)	(3)	(4)	(5)
Sample	Lower third of IQ distribution	Middle third of IQ distribution	Upper third of IQ distribution	No College	Some College
ELA * Age 20-24	-0.670	0.580	-0.390	-0.260	-0.730
	(0.634)	(0.623)	(0.444)	(0.294)	(0.529)
ELA * Age 25-29	-0.190	0.980	0.460	-0.110	0.050
	(0.580)	(0.724)	(0.477)	(0.293)	(0.518)
ELA * Age 30-34	-0.956	1.873**	0.720	0.060	0.760
	(0.519)*	(0.759)	(0.669)	(0.306)	(0.583)
ELA * Age 35-39	-0.120	1.888**	0.540	-0.190	1.346**
	(0.654)	(0.794)	(0.577)	(0.410)	(0.662)
ELA * Age 40-44	-0.420	2.216**	0.790	0.550	1.347**
	(0.958)	(0.944)	(0.632)	(0.479)	(0.611)
ELA * Age 45-49	0.720	2.302**	3.046***	0.797*	2.677***
	(1.043)	(0.939)	(1.010)	(0.470)	(0.907)
Observations	10468	14165	16788	40229	21785
Unique women	793	975	1112	2895	1456
R-squared	0.18	0.21	0.23	0.17	0.26
Mean of DV for 20-24	5.59	6.49	7.18	5.49	7.21
Mean of DV for 25-29	5.89	6.79	8.69	5.52	9.51
Mean of DV for 30-34	6.59	7.19	8.94	6.18	9.74
Mean of DV for 35-39	7.44	8.40	10.79	7.16	11.42
Mean of DV for 40-44	8.34	9.89	12.79	8.34	13.63
Mean of DV for 45-49	10.02	12.59	16.04	10.33	16.76

This table uses a specification similar to column (1) of table 3. Each column presents estimates from a separate regression. Unlike table 3, this table *includes* zero wages in the left-hand-side variable. We cannot report results excluding the zeros among the separate groups for disclosure reasons, but they follow a pattern similar to that shown above. Columns (1) to (3) break women into thirds of the IQ distribution, and columns (4) and (5) divide women into no college and some college. All other notes are as in table 3.

Table 6. Heterogeneity in the Impact of Early Access to the Pill on Highest Grade Completed

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Lower third of IQ distribution	Middle third of IQ distribution	Upper third of IQ distribution	Lower third SES distribution	Middle third SES distribution	Upper third SES distribution
ELA * Age 20-24	-0.507**	0.240	0.170	0.220	-0.140	0.200
	(0.205)	(0.198)	(0.185)	(0.141)	(0.218)	(0.316)
ELA * Age 25-29	-0.409*	0.360	0.420**	0.480***	0.020	0.340
	(0.207)	(0.228)	(0.191)	(0.147)	(0.242)	(0.274)
ELA * Age 30-34	-0.431**	0.386*	0.426**	0.410***	0.000	0.280
	(0.206)	(0.224)	(0.197)	(0.161)	(0.246)	(0.288)
ELA * Age 35-39	-0.401**	0.437*	0.505**	0.434***	0.080	0.270
	(0.197)	(0.220)	(0.202)	(0.161)	(0.253)	(0.309)
ELA * Age 40-44	-0.494**	0.455*	0.449**	0.427**	0.080	0.270
	(0.215)	(0.243)	(0.191)	(0.175)	(0.254)	(0.274)
ELA * Age 45-49	-0.380	0.330	0.584***	0.425**	0.030	0.200
	(0.239)	(0.243)	(0.207)	(0.190)	(0.267)	(0.296)
Observations	13538	17550	20982	25101	24538	24798
Unique women	793	975	1112	1392	1366	1342
R-squared	0.19	0.19	0.23	0.12	0.19	0.26
Mean of DV for 20-24	11.87	12.40	13.30	10.98	12.26	13.22
Mean of DV for 25-29	12.05	12.74	14.08	11.21	12.66	14.01
Mean of DV for 30-34	12.28	13.02	14.39	11.53	12.94	14.35
Mean of DV for 35-39	12.35	13.16	14.58	11.63	13.07	14.52
Mean of DV for 40-44	12.45	13.27	14.72	11.72	13.26	14.64
Mean of DV for 45-49	12.55	13.45	14.87	11.86	13.39	14.77

This table uses the specification in column (3) of table 4. Each column presents estimates from a separate regression. Columns (1) to (3) break women into thirds of the IQ distribution, and columns (4) to (6) divide the sample into thirds of the distribution of family background characteristics. SES is available for more women than IQ score, so the sample sizes in columns (4)-(6) are larger. All other notes are as in table 4.

Table 7. Heterogeneity in the Impact of Early Access to the Pill on Cumulative Experience

	(1)	(2)	(3)	(4)	(5)
Sample	Lower third of IQ distribution	Middle third of IQ distribution	Upper third of IQ distribution	No College	Some College
ELA * Age 20-24	-1,083	409	-397	-871*	-1,056*
	(1,299)	(964)	(720)	(499)	(593)
ELA * Age 25-29	-1,253	278	-389	-928*	-920
	(1,295)	(1,043)	(695)	(552)	(615)
ELA * Age 30-34	-688	2,214*	654	45	862
	(1,145)	(1,150)	(802)	(450)	(722)
ELA * Age 35-39	-153	3,015**	1,377	346	2,045**
	(1,371)	(1,313)	(872)	(693)	(871)
ELA * Age 40-44	40	4,778***	1,853*	2,095**	3,001***
	(1,761)	(1,701)	(983)	(861)	(1,026)
ELA * Age 45-49	-600	3,701*	1,379	1,492	2,344*
	(2,251)	(2,242)	(1,228)	(1,075)	(1,331)
Observations	12469	16531	20181	47925	26150
Unique women	790	975	1112	2898	1456
R-squared	0.610	0.637	0.679	0.582	0.703
Mean of DV for 20-24	2533	3152	2793	2833	2432
Mean of DV for 25-29	5160	6103	6340	5382	6516
Mean of DV for 30-34	9558	10755	11432	9755	12104
Mean of DV for 35-39	14822	15936	17151	14662	18106
Mean of DV for 40-44	20975	21570	23838	20752	25111
Mean of DV for 45-49	27775	29652	31933	27964	33133

This table uses the specification in column (1) of table 4. Each column presents estimates from a separate regression. Columns (1) to (3) break women into thirds of the IQ distribution, and columns (4) and (5) divide women into no college and some college. All other notes are as in table 4.

Table 8. Decomposition of the Impact of Early Access to the Pill on Log Hourly Wages

Statistic	Total Difference	Effect of Education	Effect of Job Training	Effect of Experience	Effect of Occupation	Effect of Marriage	Unexplained Difference
<u>Panel A: Oaxaca-Blinder Decomposition</u>							
Mean	0.088	0.015	-0.003	0.056	0.014	0.000	0.006
		(17.0)	(-3.4)	(63.6)	(15.9)	(0.0)	(6.8)
<u>Panel B: Recentered Influence Function Decomposition</u>							
10^{th} percentile	0.077	0.003	-0.001	0.053	-0.003	-0.001	0.026
		(3.9)	(-1.3)	(68.8)	(-3.9)	(-1.3)	(35.1)
25th percentile	0.077	0.005	-0.004	0.066	0.007	0.001	0.003
		(6.5)	(-5.2)	(85.7)	(9.1)	(1.3)	(2.6)
50^{th} percentile	0.106	0.014	-0.005	0.072	0.013	-0.003	0.015
		(13.2)	(-4.7)	(67.9)	(12.3)	(-2.8)	(14.2)
75^{th} percentile	0.073	0.017	-0.004	0.074	0.028	0.000	-0.042
		(23.3)	(-5.5)	(101.4)	(38.4)	(0.0)	(-57.5)
90^{th} percentile	0.104	0.023	0.000	0.040	0.012	-0.001	0.028
		(22.1)	(0.0)	(38.5)	(11.5)	(-1.0)	(26.9)

The numbers represent the difference in log hourly wages at different points in the distribution between women (aged 45 to 49) with and without *ELA* after adjusting for the specified factors using the indicated decomposition (reference group is those without ELA). Share of total difference are presented in parentheses. The unexplained difference is the residual not accounted for by the five factors. The total difference at the mean (0.088) differs slightly from the estimate reported in table 3 (0.078) because the numbers here are based on a single observation per woman.