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

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May 13, 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

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I. INTRODUCTION

During the 1980s, the long-standing U.S. gender gap in pay narrowed rapidly. The median annual wage and salary earnings of full-time, full-year women workers rose from roughly 60 percent of men's earnings in 1979 to 69 percent a decade later. Not only was this decade 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. 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, professional degree attainment, and employment 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 career investments (Blau and Kahn 1997, Welch 2000). Widening wage inequality among women 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 changes are less clear. Two important but elusive candidate explanations 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. Recent literature also 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 childbearing during a period critical to their career investments, and reduced the cost of increasing their early career investments. 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 reductions in the age of consent from 21 to 18 and, thereby, also reduced the age of legal access to prescription birth control. Subsequent literature links this "earlier 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, among college graduates, occupational upgrading (Goldin and Katz 2002, Bailey 2006, Guldi 2008, Hock 2008, Ananat and Hungerman 2012).

This article examines the role of the Pill in altering women's life-cycle wages and its ultimate implications for the convergence in the gender gap during the 1980s and 1990s. Although previous work implies that the Pill benefitted individual women's careers, changes in the composition of working women and increased labor supply mean that its effect on aggregate women's wages need not be large or even positive. Following earlier work, our empirical strategy leverages state-by-birth-cohort changes in laws reducing the age of consent for medical care and, by extension, access to prescription birth control for unmarried women under age 21. We extend the literature by providing new evidence relating to this empirical strategy's identifying assumptions. Using the *1970 National Fertility Study (NFS)*, we show that early access laws increased 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 and 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 larger effects on work experience accrued to women in the middle of the IQ distribution with some college as did its effects on lifecycle wages. 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.

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[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.

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 1950

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[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.

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[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.

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 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 years crucial to their human capital investment. Changes in *expectations* even affected women who—without the Pill—would not have married or had a child before age 22, as these women did not *know* this would be the case at age 18.

The difficulty of parsing the Pill's effect on women's wages relates to the timing of its appearance. By cause or coincidence, its 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 early access to the Pill, for instance, directly reduced the cost of delaying childbearing and marriage to enter or stay in college.[5] Even among women 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, early access to the Pill may have altered the *expected* returns to early human capital investments. All else equal, the expected lifetime returns to human capital investments would increase if women *anticipated* lower costs of completing them. 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 (2012) 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 framework suggests three (potentially reinforcing) mechanisms linking *ELA* to steeper wage and salary earnings profiles. First, *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." Second, *ELA* may have increased labor-force participation, which should enable women to accumulate more labor-market experience and job- or firm-specific human capital and, thus, have more rapid wage growth. We call this mechanism the "experience mechanism." Third, women with *ELA* may have shared the costs of gaining on-the-job human capital by accepting lower initial wages but then enjoying more rapid wage growth with tenure. We call this channel the "on-the-job-investment mechanism."

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[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 2011). 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).

There are two off-setting forces as well. Because *ELA* could increase labor-force participation for large numbers of women (and, thus, reduce the capital-to-labor-ratio), its effect on any one woman may be larger than its effect on an entire birth cohort, the parameter recovered in the analysis. 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 estimates include this labor-supply channel, 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.

Changes in selection—while endogenous—also complicate the analysis. 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 information 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,

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

where Y is the outcome of interest for individual i, at age a, who was born in year $c = 1943, 1944, \dots,$ 1953 (also referred to as "birth cohort"), and residing in state $s = 1, 2, \dots, 51$ 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-

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[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. β_g will understate the impact of early Pill access if local compliance and enforcement were imperfect (young women drove across state lines to obtain the Pill or doctors prescribed it to underage women).

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 cov(*ELA*,ε| **Z**)=0, where **Z** captures the fixed effects in equation (1).

One reason that cov(*ELA*,ε| **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 respondents' backgrounds at age 14 *before treatment with ELA*, we test this possibility using the following specification,

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[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).

$$(2) \quad X_{ics} = \gamma ELA_{cs} + \sum_s \lambda_s D_s + \sum_c \lambda_c D_c + \varepsilon_{ics},$$

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 useful validity check 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 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

[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.

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| **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 (1) with a rich set of abortion controls:

$$(1') \quad Y_{iacs} = \sum_g \beta_g ELA_{cs} D_{g(a)} + \sum_g \gamma_g EAA_{cs} C50_c D_{g(a)} + \sum_g \theta_g ELA_{cs} EAA_{cs} C50_c D_{g(a)}$$
$$+ \delta LnDist_s C50_c + \sum_g \lambda_g D_{g(a)} + \sum_s \lambda_s D_s + \sum_c \lambda_c D_c + \eta_{iacs},$$

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 (1'): 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 importance 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. Goldin and Katz (2002) use a different legal coding, which means their estimates may not generalize for our analysis. In addition, their single cross-section of data in the *NSYW71* 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 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 were ages 17 to 21 in the 1960s and married by 1970. Using a linear probability model, we estimate equation (2) where *X* is a binary

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[10] Disclosure limitations from the Research Data Center prevent us from reporting effects for *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. The coefficients on the interactions are consistent with the Pill and abortion acting as substitutes, which agrees with Ananat and Hungerman (2012), 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 (1') 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.

dependent variable equal to 1 if a respondent first used the birth control pill before age a, which we estimate separately for a = 18, 19, …, 22. So that we observe each of the birth cohorts at each of these ages, the analysis restricts the sample to the birth cohorts of 1942 (age 18 in 1960) to 1948 (age 22 in 1970). Thus, estimates of γ are based upon the subset of states that transition to *ELA* before 1970 (Georgia, Kentucky, Mississippi, Ohio, and Washington), which are different than the full set used in the remainder of the analysis.

Panel A of table 2 uses these data to show that *ELA* increased Pill use at the appropriate ages. 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. However, women with *ELA* were 17 percentage points more likely to use the Pill before age 19.[13] Pill use before age 21 was 16 percentage points higher among women with *ELA*, a statistically significant 42 percent increase over the national mean. This difference falls to a statistically insignificant 5 percentage points at age 21 (before 22), when women without *ELA* could also obtain the Pill legally.[14] A chi-squared test rejects at the 5-percent level ($\chi^2(4)$=11.03) that the effects of *ELA* on Pill use before ages 19, 20, 21, and 22 are jointly equal to zero.

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 (2) 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

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[13] An astute reader might puzzle over the divergence of the reported standard errors from what might be inferred using the reported mean dependent variable and R^2, especially the small standard error in column 1. The difference in the standard error estimates across columns reflects both the role of the Huber-White correction for heteroskedasticity and for serial correlation (in year-of-birth cohort) within states. In column 1, the standard error increases to 0.042 when calculated under the (false) assumption of homoskedasticity (we are estimating linear probability models). Additionally correcting for serial correlation in birth cohort, the standard error becomes 0.017, which is the standard error reported in column 1. In contrast, the correction for heteroskedasticity plays a less important role in columns 2 through 5, where corrections for serial correlation are more important.
[14] 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.

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).[15]

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 retrospective nature of the sample and stigma about reporting premarital sex and contraceptive use would lead our analysis to understate the true effect of *ELA* on Pill use.[16] Second, the *1970 NFS* does not include unmarried young women. Because women with *ELA* tended to delay marriage (cf. Goldin and Katz 2002, Appendix C) and those who delayed the most (presumably by using the Pill more) are less likely to be sampled by the *1970 NFS*, the effects of *ELA* on Pill use may also be understated. Understating the effects of *ELA* on Pill use for either reason would inflate ATT estimates. Third, the estimates for the subset of states that transition to *ELA* in the *NFS* may not represent the effects for the full set of cohorts (1943 to 1953) considered in the main analysis. Finally, even if the true effect of *ELA* on Pill use lies in our estimated range, dividing other *ELA* effects by this amount yields the ATT only if *ELA* has zero effect on women who did 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 in the marriage or labor market to the Pill's diffusion. For instance, as more women enter the workplace with

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[15] 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.

[16] See also Goldin and Katz (2002, footnote 11, p. 734), who note that the 1970 NFS question about retrospective pill use was only supposed to be asked for women who were married "a month before the date of the question." This provides an additional survey-based reason that Pill use would be underreported for unmarried women in the age range of interest.

ELA, women in these markets who did not use the Pill may benefit from reductions in employers' statistical discrimination. Whereas the *NFS* estimates of *ELA* on the use or oral contraception do not measure the Pill's effects on expectations or demand side responses in general equilibrium, the intention-to-treat estimates in the next sections measure its effects on outcomes through all of these channels.

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 1), a specification with abortion controls (using equation 1'), and the four alternative specifications described above. Throughout the results section, our discussion focuses on the magnitudes of our estimates with abortion controls (1'), but it is important to note that the estimates from each of the other five specifications are generally not statistically different from those in (1'). (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.[17] 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

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[17] 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.[18] 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 earlier discussion 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

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[18] 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.[19] 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.[20]

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[19] 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.

[20] 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.

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 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).[21] 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.

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[21] 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*.

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 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.[22]

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, 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

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[22] 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.

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.

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.[23] Equation (1') 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.[24] 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.

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[23] 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.
[24] 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.

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 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).[25]

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.[26] 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

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[25] 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.

[26] The estimated effects of *ELA* by college attainment in tables 5 (for wages) and 7 (for experience) may be smaller than for affected woman if the marginal women who attended college (because of *ELA*) had higher IQs on average than the women with *ELA* who did not attend college but lower IQs than women who attended college without *ELA*.

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 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.[27] 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 and upper thirds of the IQ distribution and those with some college. Middle IQ women (column 2) with *ELA* had accumulated 3,500 additional hours of work experience by their early forties, a 16-percent increase over the mean. Women in the highest IQ

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[27] 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.

group (column 3) with *ELA* accumulated 2,800 additional hours by their early forties, an 11.5 percent increase over the mean. Echoing the wage results, the effects of *ELA* on labor-force experience are largest for women with some college (column 5).[28]

In summary, the data 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 and into 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 accumulated greater work experience. Women with some college became more likely to work for pay. Interestingly, the Pill's larger effects on work experience accrued to women in the middle of the IQ distribution, not only 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.[29]

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

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[28] 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.

[29] 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.

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 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, 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.[30] 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. 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, which suggests 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 women with cheaper and more effective control over childbearing in late adolescence, they 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

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[30] 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.[31] 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.[32] 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 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

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[31] 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).

[32] 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, but 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.
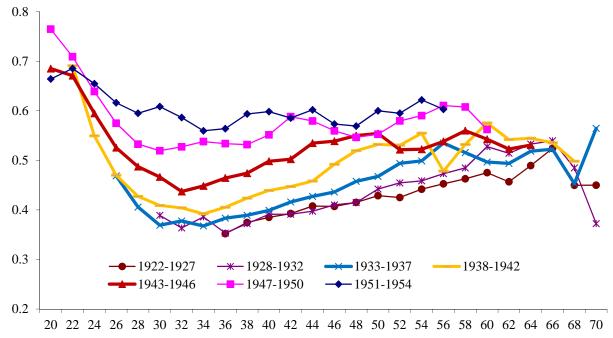
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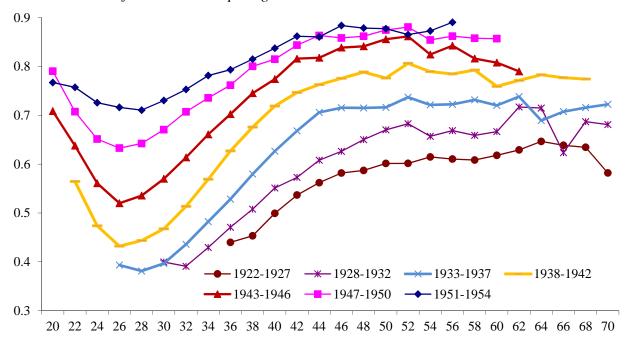
Figure 1. The Evolution of the Real Annual Wage Earnings of Women Relative to Men by Age and Birth Cohort
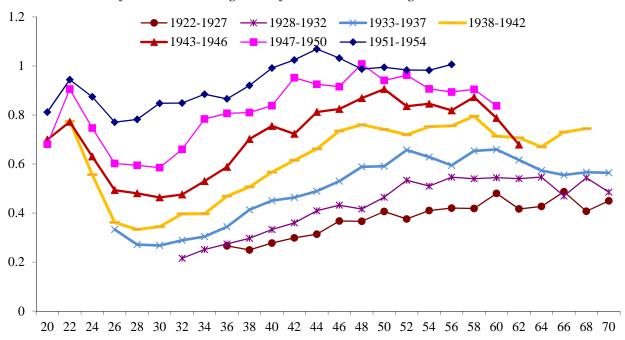
Annual labor earnings include income from all jobs, including self-employment. The series is adjusted for inflation to year 2000 dollars using the personal consumption expenditures deflator (BEA 2009). Data are weighted using *CPS* sample weights and collapsed into two-year age groups. *Source*: 1964-2009 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*.
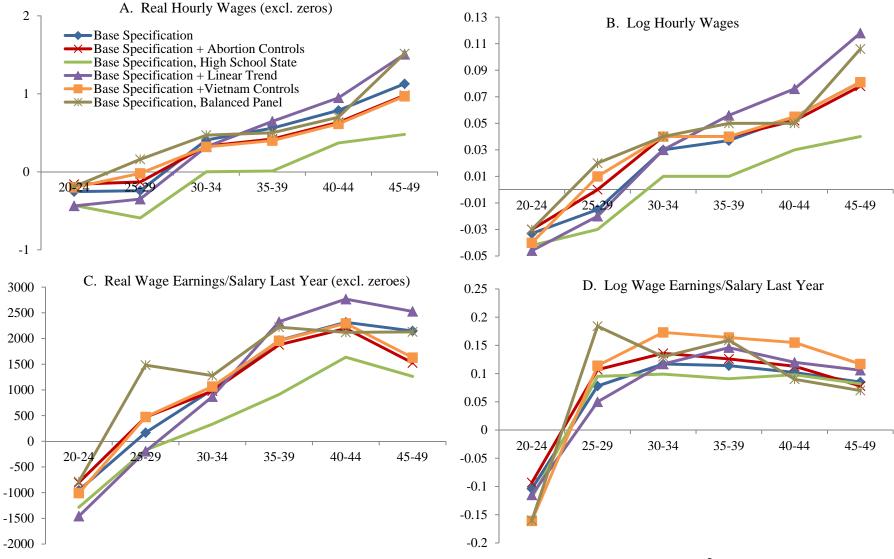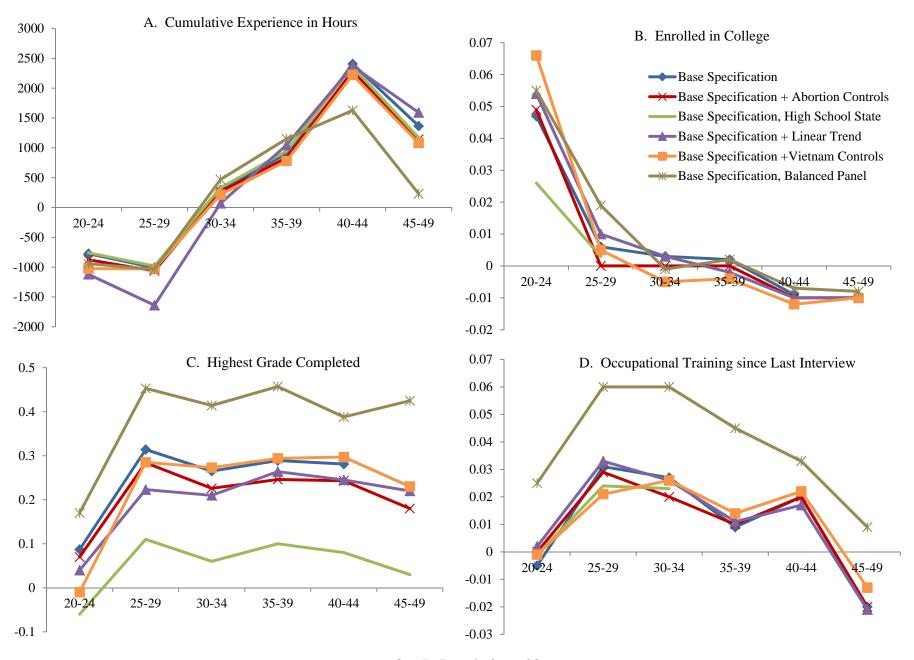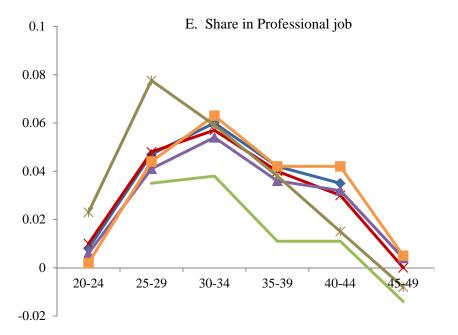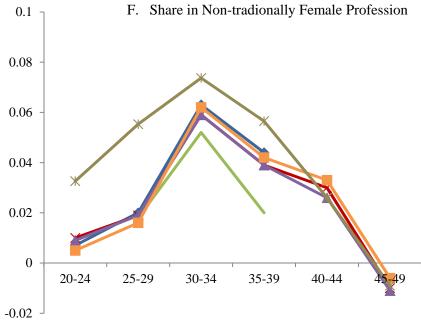
Figure 3. The Effects of Early Access to the Pill on Lifecycle Wage Earnings

A. Real Hourly Wages (excl. zeros)

- Base Specification
- Base Specification + Abortion Controls
- Base Specification, High School State
- Base Specification + Linear Trend
- Base Specification +Vietnam Controls
- Base Specification, Balanced Panel

B. Log Hourly Wages

C. Real Wage Earnings/Salary Last Year (excl. zeroes)

D. Log Wage Earnings/Salary Last Year

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 1), baseline + abortion controls (equation 1') which corresponds to our tables, and four variants of equation (1'): 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 footnote 20 regarding selection problems with this sample). Source: *NLS-YW*.

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

A. Cumulative Experience in Hours

B. Enrolled in College

- Base Specification
- Base Specification + Abortion Controls
- Base Specification, High School State
- Base Specification + Linear Trend
- Base Specification +Vietnam Controls
- Base Specification, Balanced Panel

C. Highest Grade Completed

D. Occupational Training since Last Interview

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E. Share in Professional job

F. Share in Non-tradionally Female Profession

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 Appendix A 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 serial correlation at the state level and are presented in parentheses below each estimate

Table 2. The Impact of *ELA* on Pill Use among Women Married by 1970

	(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 (2), while Panel B presents estimates from equation (2) augmented with a dummy for non-metropolitan area and the interaction of this dummy with *ELA*. Both panels 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 1942) 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 serial correlation 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

	Mean real hourly wages excl. zeros	Real hourly wage (excl. zeros) (1)	Log real hourly wage (2)	*Mean real wages/salary last year excl. zeros*	Wage or salary last year (excl. zeros) (3)	Log real annual wage (4)	*Mean real wages/salary last year incl. zeros*	Wage or salary last year (incl. zeros) (5)
ELA * Ages 20-24	*7.88*	-0.160 (0.315)	-0.025 (0.025)	*9943*	-804 (681)	-0.093* (0.053)	*7661*	-1,187* (625)
ELA * Ages 25-29	*9.60*	-0.126 (0.347)	-0.001 (0.028)	*15610*	469 (741)	0.107** (0.046)	*10911*	202 (721)
ELA * Ages 30-34	*10.62*	0.332 (0.332)	0.036 (0.028)	*18116*	978 (731)	0.136** (0.059)	*12452*	803 (683)
ELA * Ages 35-39	*11.74*	0.420 (0.333)	0.038 (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 (1'). Heteroskedasticity-robust standard errors are corrected for serial correlation 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) Cumulative Experience in Hours	(2) 1= Enrolled in College	(3) Highest Grade Completed	(4) 1=Occupational training since last interview	(5) 1= in Professional Job	(6) 1=in Non-traditional Job
ELA * Age 20-24	-876**	0.049**	0.069	-0.004	0.009	0.007
	(369)	(0.022)	(0.136)	(0.013)	(0.013)	(0.008)
ELA * Age 25-29	-1,062**	0.004	0.284**	0.029***	0.048***	0.019*
	(443)	(0.008)	(0.131)	(0.011)	(0.019)	(0.011)
ELA * Age 30-34	263	0.002	0.226*	0.024	0.057***	0.059***
	(405)	(0.013)	(0.132)	(0.016)	(0.021)	(0.016)
ELA * Age 35-39	836	0.002	0.246*	0.011	0.035	0.039**
	(550)	(0.010)	(0.133)	(0.018)	(0.023)	(0.019)
ELA * Age 40-44	2,282***	-0.007	0.243*	0.019	0.031	0.029
	(784)	(0.010)	(0.129)	(0.022)	(0.027)	(0.020)
ELA * Age 45-49	1,143	-0.007	0.182	-0.016	-0.002	-0.009
	(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 (1'). Heteroskedasticity-robust standard errors are corrected for serial correlation 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

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-0.672	0.579	-0.393	-0.255	-0.730
	(0.634)	(0.623)	(0.444)	(0.294)	(0.529)
ELA * Age 25-29	-0.193	0.982	0.458	-0.105	0.053
	(0.580)	(0.724)	(0.477)	(0.293)	(0.518)
ELA * Age 30-34	-0.956	1.873**	0.722	0.062	0.757
	(0.519)*	(0.759)	(0.669)	(0.306)	(0.583)
ELA * Age 35-39	-0.123	1.888**	0.543	-0.185	1.346**
	(0.654)	(0.794)	(0.577)	(0.410)	(0.662)
ELA * Age 40-44	-0.423	2.216**	0.790	0.553	1.347**
	(0.958)	(0.944)	(0.632)	(0.479)	(0.611)
ELA * Age 45-49	0.722	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

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) Lower third SES distribution	(5) Middle third SES distribution	(6) Upper third SES distribution
ELA * Age 20-24	-0.507**	0.227	0.174	0.216	-0.143	0.202
	(0.205)	(0.217)	(0.185)	(0.141)	(0.218)	(0.316)
ELA * Age 25-29	-0.409*	0.364	0.420**	0.480***	0.021	0.339
	(0.207)	(0.228)	(0.191)	(0.147)	(0.242)	(0.274)
ELA * Age 30-34	-0.431**	0.386*	0.426**	0.410***	0.004	0.284
	(0.206)	(0.224)	(0.197)	(0.161)	(0.246)	(0.288)
ELA * Age 35-39	-0.401**	0.437*	0.505**	0.434***	0.077	0.266
	(0.197)	(0.220)	(0.202)	(0.161)	(0.253)	(0.309)
ELA * Age 40-44	-0.494**	0.455*	0.449**	0.427**	0.079	0.272
	(0.215)	(0.243)	(0.191)	(0.175)	(0.254)	(0.274)
ELA * Age 45-49	-0.377	0.326	0.584***	0.425**	0.025	0.202
	(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

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-874	-319	218	-1,324**	-567
	(1,264)	(880)	(826)	(545)	(736)
ELA * Age 25-29	-1,039	-668	176	-1,516**	-539
	(1,335)	(1,009)	(873)	(609)	(777)
ELA * Age 30-34	-518	1,208	1,416	-488	1,246
	(1,128)	(1,066)	(994)	(470)	(838)
ELA * Age 35-39	-55	1,950	2,212*	-319	2,500***
	(1,377)	(1,322)	(1,102)	(748)	(931)
ELA * Age 40-44	-451	3,467**	2,808**	1,185	3,840***
	(1,977)	(1,524)	(1,265)	(998)	(1,059)
ELA * Age 45-49	-1,484	1,580	2,196	69	2,793**
	(2,669)	(1,950)	(1,340)	(1,352)	(1,361)
Observations	10626	14076	16597	40555	21181
Unique women	788	969	1104	2880	1449
R-squared	0.600	0.642	0.681	0.581	0.703
Mean of DV for 20-24	2577	3167	2811	2867	2442
Mean of DV for 25-29	5383	6330	6543	5540	6765
Mean of DV for 30-34	9795	10994	11655	9932	12401
Mean of DV for 35-39	15017	16078	17502	14876	18434
Mean of DV for 40-44	21308	21924	24420	21088	25434
Mean of DV for 45-49	28093	29602	32319	28124	33224

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

Theory Appendix

We adapt an economic framework from Acemoglu, Autor, and Lyle (2004) to explore the general equilibrium effects of *ELA* on the gender gap in wages. Assume that aggregate output, *Y*, is produced using capital, *K*, and the labor of women, *W*, and men, *M*, using the following constant-returns to scale ($\alpha < 1$) production function,

$$Y = AK^\alpha \{\phi(\theta_w W)^\rho + (1-\phi)(\theta_m M)^\rho\}^{(1-\alpha)/\rho},$$

where *A* is a total factor productivity parameter, ϕ is a share (0 to 1) parameter, and θ_i, with i = *m*, *w* denoting men and women respectively, is a labor-augmenting productivity parameter that varies by gender of the worker. The aggregate elasticity of substitution between women and men in production is $\sigma \equiv 1/(1-\rho)$. In competitive labor markets, women's and men's wages, y_i, are equal to their marginal products,

(1) $\quad y_w = A(1-\alpha)\phi K^\alpha \{\phi(\theta_w W)^\rho + (1-\phi)(\theta_m M)^\rho\}^{\frac{1-\alpha-\rho}{\rho}} \theta_w{}^\rho W^{\rho-1}$ and

(2) $\quad y_m = A(1-\alpha)(1-\phi)K^\alpha \{\phi(\theta_w W)^\rho + (1-\phi)(\theta_m M)^\rho\}^{\frac{1-\alpha-\rho}{\rho}} \theta_m{}^\rho M^{\rho-1}$,

so a measure of the gender gap in wages can be expressed as the ratio of these expressions,

(3) $\quad \dfrac{y_m}{y_w} \equiv \dfrac{(1-\phi)}{\phi} \left(\dfrac{\theta_m}{\theta_w}\right)^\rho \left(\dfrac{W}{M}\right)^{1-\rho}.$ [1]

Our empirical exercise explores how a Pill-induced change in the labor supply of women, *W*, and their productive skills (driven by pre-market investments, investments while in the labor market, and ability), θ_w, affects women's wages across the lifecycle. As a starting point, we motivate this exploration by considering the isolated effects of shifts in either parameter.

First, consider the impact of a Pill-induced increase in women's labor supply on women's wages in this framework represented in the following elasticity,

(4) $\quad \dfrac{\partial ln y_w}{\partial ln W}\big|_{M,K} = -s\alpha - \dfrac{(1-s)}{\sigma},$

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[1] Note that Blau and Kahn (2004) use this expression as the basis for deriving their empirical analysis of the gender gap in wages.

where the share of wages paid to women, $s \equiv \frac{y_w W}{y_w W + y_m M}$, is assumed to be less than 1.[2] Notice that the

sign and the magnitude of the Pill's effects on women's wages through changes in their labor supply

depend upon women's share of labor costs, s, the elasticity of substitution between women and men in

production, σ, and the elasticity of production with respect to changes in capital, α. In this framework, a

Pill-induced increase in women's labor supply will tend to reduce their wages. Moreover, the reduction in

women's wages, both in absolute terms and relative to men's wages, will be larger if their labor is less of

a substitute for men's labor.

Next consider the impact of a Pill-induced change in women's productivity, θ_w, which could

reflect changes in women's career investments (schooling, occupational training, etc.), their innate,

market productivity (often called ability), or a combination of both:

(5) $\quad \frac{\partial ln y_w}{\partial ln \theta_w}|_{M,K} = 1 + \frac{\partial ln y_w}{\partial ln W}|_{M,K} = 1 - s\alpha - \frac{(1-s)}{\sigma}.$

The response of women's wages depends upon the combination of a positive impact on productivity

offset by the increase in effective labor supply. If women and men are sufficiently substitutable, then the

labor supply effect is less than 1, and an increase in productivity will increase women's wages.[3]

Moreover, the positive effect of a Pill-induced increase in women's wages will be greater the greater their

substitutability with men.

Finally, consider the impact of a Pill-induced increase in women's productivity, θ_w, and labor

supply, W, on the gender gap in wages:

(6) $\quad \frac{\partial ln(y_m/y_w)}{\partial ln W}|_{M,K} = 1 - \rho = \frac{1}{\sigma}$ and

(7) $\quad \frac{\partial ln(y_m/y_w)}{\partial ln \theta_w}|_{M,K} = -\rho = \frac{1-\sigma}{\sigma}.$

As long as women and men are not perfect substitutes, Pill-induced increases in women's labor supply

increase the gender wage gap; however, if the elasticity of substitution between men and women is

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[2] As noted in Acemoglu, Autor, and Lyle (2004), this elasticity is best thought of as a "short-run" elasticity where capital and men's labor supply are held fixed.

[3] $1 - s\alpha - \frac{(1-s)}{\sigma} > 0$ $\;iff\; \rho > \frac{s}{1-s}(-1 + \alpha).$

greater than 1,[4] a Pill-induced increase in women's market productivity will decrease the gender gap in wages. The magnitudes of these effects depend upon the substitutability of women and men in production.

Importantly, Pill-induced changes in the gender gap potentially reflect changes in both women's wages (as shown in equations 4 and 5) *and* men's wages (equations omitted for brevity)—a fact implying that men provide a poor falsification test for the empirical exercise pursued in this paper. This is also true if, in addition to the labor market interactions modeled here, men interact with women through the marriage market. We investigate the relationship of *ELA* on men using the restricted March *CPS* data containing the full set of state identifiers to assign *ELA* by state of current residence. Our analysis shows no systematic relationship of *ELA* on men's annual earnings or wage rates. These results are reported in Appendix C.

This simple, static framework illustrates the Pill's complex and potentially countervailing effects on women's aggregate wages. Even abstracting from longer-term adjustment and household bargaining, the Pill's effects on wages depend upon unobserved changes in selection (part of θ_w in our framework) as well as upon the sign and magnitudes of unobserved theoretical parameters. These effects become considerably more complicated over the longer-term if men's labor supply or human capital changes or firms adjust physical capital, K, in response. Although we omit the dynamic extension of this model for brevity, the Pill's overall effect may evolve over the lifecycle as its productivity effects accumulate (e.g., with greater experience or schooling) and affect dynamic sorting or selection into employment or occupations.

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[4] Blau and Kahn (2004) review the estimates of σ in the literature and report a range from 2 to 2.4. Acemoglu, Autor and Lyle (2004) put this parameter around 3 for their investigation of the 1940s.

Appendix A. Data and Specifications

This appendix summarizes the creation of the variables used in the analysis as well as the construction of the alternative specifications used for figures 3 and 4. The independent variables, including the key *ELA* measure, are described first, followed by the sequence of dependent or outcome variables. (The dependent variables are available in every wave of the survey unless otherwise stated.) Finally, each alternative specification is discussed.

Age and year of birth

Determining the age of the respondents at each survey is crucial, both in identifying early legal access, which is age dependent, and because the effects of early legal access are likely to vary over the lifecycle. Both age at time of interview and date of birth (month and year) are asked in various waves of the survey; however, they are not always consistent. Date of birth was asked in 1968, 1977, 1978, 1982, 1988 and 1991 and confirmed or corrected in 1995, 1997, 1999, 2001, and 2003. Of the 5,159 women in the sample, 94 (1.8 percent) had conflicting birth date reports, and another 818 (15.9 percent) had only a single report. For the conflicting cases, all available data were used to check birth reports, but, in most cases, the modal reported year and month of birth was used.[1] From the date of birth information, age at the end of each survey year (not at the time of interview) was constructed for consistency between early and later waves.[2]

State of residence

The geocoded version of the *NLS-YW*, available at Census Research Data Centers, contains the state of residence of each respondent for each wave of the survey. Using respondents' age information and variables pertaining to mover status in the public-use data, one can construct variables for the state of residence at key ages (such as 18, 19, 20, and 21) for most but not all respondents. In some cases, women exit the sample before they reach the key ages; in others, women in the older cohorts who move

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[1] The exact code is available from the authors upon request.
[2] The early waves sampled respondents in the early months of the year but later waves sampled respondents in later months.

frequently during the key ages are not observed until they are older. Nonetheless, for each of the key ages (18 through 21), between 80 and 90 percent of the respondents were successfully matched to a state of residence.

Early Legal Access to the Pill (ELA)

By researching state laws, the authors compiled a list of the years in which each state legally allowed unmarried women (of age 20 or younger) to have access to the birth control pill (see Appendix B: Legal Variables). Using the restricted version of the *NLS-YW*, state of residence at each survey is observed and the respondents' state of residence at age 21 is used to generate the *ELA* variable. A respondent's *ELA* status was coded 1 if her year of birth plus 20 was greater than or equal to the year in which her residence state at age 21 first allowed legal access. State of residence at age 21 rather than age 20 was used because it was identifiable for more women (4,419 versus 4,398) and the correlation between the two was high (r = 0.94).

Early Abortion Access (EAA)

Five states (Alaska, California, Hawaii, Washington, and New York) and the District of Columbia legalized abortion in 1970, three years before *Roe v. Wade*. We code a respondent as having EAA if she lived in one of the above areas at age 21 *and* was born in 1950 or later; these are the cohorts of women who had legal abortion access in their states of residence before the age of 21. To address the possibility that women crossed state lines to obtain an abortion, we also constructed a measure of the distance in miles between each state's population centroid in 1970 and the closest major location providing abortions in the pre-*Roe* period (District of Columbia, Los Angeles, San Francisco, Buffalo, and New York City. This distance was then transformed into its natural logarithm.

Age at first marriage

Although age at first marriage is directly asked in 1968, this is useful only for women who had been married prior to the first interview. To determine marital ages for the rest of the sample, three additional sources are used: (a) marital histories, (b) changes in current marital status, and (c) timing of changes in marital status. Marital history questions are asked in 1978, 1983, 1997, 1999, 2001, and 2003.

In 1978 and 1983, the questions ask about up to the three most recent marriages (including the current one); in the latter years, only the date of the most recent marriage is asked. Current marital status is asked in every survey year. Changes in marital status are reported in 1969 and 1970 and every survey year from 1985 onwards. We observe no first marriage date for 809 women. This outcome is used only in Appendix C: Additional Estimates and Sensitivity Checks.

Wages and salary earnings

Hourly rates of pay for the current or most recent job (measured in cents) and annual wage and salary earnings from the previous calendar year are available for years 1968 through 1993. For 1995 through 2003, the hourly rate of pay variable is for the first (main) job, and annual wage and salary earnings are for the previous 12 months rather than the previous calendar year. Information on wages and salary earnings excludes farm, business, or self-employment income. Each of the wage, earnings, and income variables is converted from nominal to 2000 dollars using the PCE deflator and then converted to natural logarithms. Although there is no effective top code to hourly wages, annual earnings are subject to censoring from above, with the top code varying across years. (Generally, fewer than 2 percent of women have top-coded earnings in any year.) In the analysis, hourly wage outliers (less than 2 or more than 100 real dollars) are excluded.

Cumulative experience

We measure cumulative work hours at the start of each calendar year as the sum of hours of work reported since 1967. We approximate hours of work with the product of usual weekly hours and our best estimate for the number of weeks worked each year.

We rely on three sets of questions to compute number of weeks worked. In 1968, 1969, 1975, 1977, 1980, 1982, 1985 and 1987, respondents were asked to report the number of weeks they worked in the previous calendar year. In 1970, 1971, 1972, 1973, 1978, 1983, 1988, 1991 and 1993, the survey asked the number of weeks worked since the last eligible interview, regardless of whether or not that interview took place. In 1970, 1971, 1972, 1973, 1995, 1997, 1999, 2001 and 2003, they survey asked weeks worked since the last actual interview. We combine these measures as available, being careful to

avoid double-counting. (This procedure is complicated and idiosyncratic to each survey wave; the code used is available upon request.)

Despite our best efforts, we note that it is not possible to create a truly comprehensive measure of weeks worked for several reasons. First, there are some gaps in coverage for which no weeks worked questions were asked: The initial shift from calendar year to survey period leads to a small time period (generally under 6 weeks) for which we have no measure of weeks worked. The size of this coverage gap increases over time. For example, we miss nine to eleven months between the 1973 interview and January 1, 1974, and the entire calendar year of 1975. Second, item non-response for a question regarding weeks worked poses a significant problem because cumulative experience is dependent on all past responses. It is only possible to recover cumulative experience for women who miss an interview and are subsequently re-interviewed *if* the later interview asks about weeks worked since the last actual interview.

Our main measures address these concerns with additional sample restrictions or assumptions. We address the coverage issue by rescaling the experience measure to a base of full coverage. We effectively assume that the fraction of weeks *observed* working is the same as the fraction of weeks *elapsed* spent working; that is, we scale the cumulative weeks worked measure by the ratio of total weeks elapsed to total weeks for which there is coverage. For the second problem, we exclude women once they have an episode of an item non-response for the weeks worked question. For the third problem, we restrict estimation to women who have a valid weeks report in every survey wave (no missed interviews and no item non-response). None of these alternate measures, whether used individually or all together, changes the qualitative pattern of results we find of *ELA* on cumulative experience. The numbers and estimates reported in table 4 apply the first and second measures but exclude the third in the interest of maintaining a larger sample size.

College enrollment

Using questions that asked about current enrollment in an academic program of study, as well as the highest grade completed, a respondent was coded as enrolled in college (a binary variable) if she was enrolled and the highest grade completed was at least 12. As a result, "college enrollment" includes all

forms of academic post-secondary education but excludes vocational/occupational training. Note that women who did not graduate from high school are excluded (coded as missing).

Highest grade completed

The basis of these variables is the set of revised highest grade completed questions. Although the "revised" set has supposedly been cleaned and corrected of errors found in the original highest grade completed questions, an inspection revealed that several problems remained, and these were often some form of non-monotonic progression. Five hundred thirteen women (10.0 percent) had at least one discrepancy, but in most cases these were minor, such as a jump up or down of one grade in a single survey wave before returning to trend. The "revised" variables were cleaned further of likely misreports using responses from previous and later years. Specifically, "jump" deviations that last only a single wave (in some cases, two waves) are smoothed by replacing these values with those that occur both before and after the deviation. For example, a woman whose highest reported grade is 12 in 1975 and 1977, 10 in 1978, and 12 in 1980 and 1982, would have the 1978 value recoded to 12. This procedure leaves 205 women (4.0 percent) with a non-correctable discrepancy, such as multiple, non-monotonic jumps; these respondents are flagged and excluded from the analysis. Including these women alters the results very little.

Labor-force participation

Labor-force participation (LFP) is based on the employment status recode (1968 through 1993) or monthly labor recode (1995 through 2003) variables. The LFP dummy variable takes the value of 1 if the respondent is employed at the time of the survey (whether at work or not) or unemployed, and 0 otherwise. Note that choice of specific activities in the survey for non-labor-force participants changed between 1993 and 1995, when the *NLS-YW* adopted the new *CPS* definitions. Results using this measure are reported in footnote 27.

Usual weekly hours

These variables are based on a question asking about the usual hours worked per week at the respondent's job. For most years, the job is defined to be either the one currently held or the job most

recently held since the last interview; however, in 1970, 1971, 1972, 1973, 1978, and 1983, the question

pertains to the current job only. In these cases, another question specifically referring to the usual hours

worked at the most recent job is used to supplement the current job question to maintain comparability:

Respondents with missing values for the current job only question are replaced with the usual hours

worked from the most recent job question. Finally, because responses in some years are top-coded at 99

hours while some are not, values above 99 are recoded to exactly 99. This affects no more than 1 to 3

women in any year and has a negligible impact on the estimates.

Occupational training

Although the *NLS-YW* asks several questions throughout the survey waves about occupational

training, the questions are not completely consistent across waves. In 1968 and again from 1980 through

2003, the survey asked whether respondents had undergone (a) any on-the-job training since the last

interview, and (b) any other occupational or vocational training. From 1969 to 1978, however, these two

different types of training were co-mingled in a single training question. For consistency, both training

types are combined into a single (binary) indicator that captures whether the respondent underwent any

form of vocational or occupational training, on-the-job or otherwise, since the last interview. The

estimation sample for training includes only respondents who were not currently attending an academic

program, because training questions were asked only of respondents not enrolled in an academic program

until 1975.

Occupation

For each wave of the survey, there is a variable containing the 3-digit Census code of the

respondent's current or most recent job. Through 1993 the variable is for current or most recent job; for

1995 through 2003, when the new (circa 1994) *CPS* definitions were used, the variable for job 1 (the main

job) is used. Unfortunately, a consistent coding is not available in the data. The coding at the beginning of

the survey is based on the 1960 scheme, and it is available through 1993. Coding based on the 1980

scheme begins in 1980 and runs through 1999; the 1990 scheme runs from 1993 through 2001; and the

2000 scheme runs from 1995 through 2003. Thus, there is significant overlap for several years. In the

interest of creating a longer series, we aggregate the different coding schemes by collapsing the 3-digit

job codes into four groups that can be made consistent over the entire time period. We use a coding

scheme as soon as it becomes available, so we use the 1960 scheme for data years 1968 through 1978, the

1980 scheme for years 1980 through 1991, the 1990 scheme in 1993, and the 2000 scheme for years 1995

through 2003. The four groups are: all professional and managerial jobs, non-traditionally female

professional and managerial jobs, clerical and sales jobs, and all other jobs. "All professional and

managerial jobs" generally includes any 3-digit code that falls under the "professional, technical and

kindred workers" or "managers, officials, and proprietors except farm" categories (or their equivalent)

from any of the coding schemes. "Non-traditionally female professional and managerial jobs" is a subset

of the first category that excludes the traditionally female occupations of nurses and elementary,

secondary, and not elsewhere classified (n.e.c.). teachers. "Clerical and sales jobs" includes 3-digit codes

listed under the clerical or sales categories, and "all other jobs" includes all 3-digit codes not in one the

previous groups, including craftspeople, operatives, agricultural workers, and service jobs. The complete

list of 3-digit Census job codes to our four groups by coding scheme is available by request. For the

analysis in table 4, a woman must be currently employed to be counted in one of the four job groups; if

she reported a 3-digit code in the survey but also reports not being currently employed, we code her as a

zero in all four job categories.

IQ and Childhood Family Socioeconomic Status

The 1968 wave of the *NLS-YW* included a questionnaire for the high schools of the respondents,

which in addition to asking about school characteristics also asked for the most recent intelligence or

aptitude test of the respondent. Scores were reported for 3,530 of the respondents (though almost none for

respondents born in 1953). See Griliches, Hall and Hausman (1978) for an assessment of whether scores

are missing at random in the *National Longitudinal Survey of Young Men*. The agency that processed the

NLS-YW, the Center for Human Resource Research (CHRR), converted these scores from various tests

composites to a unified "IQ score" based on a normally-distributed national population with mean 100

and standard deviation 15. (More information on this procedure can be found at

.) Based on this distribution and the unified score, a respondent

was also classified into an IQ quantile and stanine. Using information from the initial survey wave on

father's occupation and education, mother's education, eldest sibling's education, and availability of

reading material at home, CHRR also constructed a summary family socioeconomic status variable to

follow a normal distribution with mean 100 and standard deviation 30. Our analysis breaks these

measures into tertiles.

Attrition

In most cases, the empirical analysis has made no attempt to restrict the sample to non-attriters.

The decision to exploit every person-year observation was made in order to maximize sample size. One of

our sensitivity checks, reported in figures 3 and 4 and in tables in appendix C, shows that findings based

upon a balanced panel of individuals are very similar to those reported in the paper. In addition,

regressions, available upon request, show no correlation between each year's interview status and *ELA*.

Variables Used in Table 1 Balancing Tests

(1) **Father worked for pay:** binary variable equal to one if a respondent's father worked for pay when

respondent was 14. About 93 percent of the sample had a father working for pay at age 14. (Note:

This is *not* conditional on having a father in the HH).

(2) **Father held professional job:** binary variable equal to one if a respondent's father had a

"professional" job when respondent was 14. "Professional" has the same coding as in the main

results, based on 1960 occupational definitions. About 20 percent of the sample had a father

working in a professional job. (Note: This is conditional on having had a father working at age 14).

(3) **Mother worked for pay:** binary variable equal to one if a respondent's mother worked for pay

when respondent was 14. This was *not* asked of respondents who lived with their mother as the sole

parent. About 39 percent of the effective sample had a mother working for pay at age 14. (Note:

This *is conditional* on having a father (or other male adult) in the HH).

(4) **Mother held professional job:** binary variable equal to one if a respondent's mother had a "professional" job when respondent was 14. "Professional" has the same coding as in the main results, based on 1960 occupational definitions. About 13 percent of the sample had a mother working in a professional job. (Note: This is conditional on having had a mother working at age 14).

(5) **Duncan index of household head**: Duncan index socioeconomic job score of head of household when respondent was age 14, as created by CHRR in the data. Values are conditional on the head (not necessarily father) working when respondent was 14. (The scale runs from 3 to 97).

(6) **Socio-economic status**: socioeconomic index of respondent's parents in 1968, as provided in the data. Based on father's occupation and education, mother's education, eldest sibling's education, and availability of reading material at home. By construction, SES ~ N(100,900).

(7) **Magazines in home:** binary variable equal to one if a respondent had magazines available at home when she was age 14. About 64 percent of the sample did.

(8) **Newspapers in home:** binary variable equal to one if a respondent had newspapers available at home when she was age 14. About 83 percent of the sample did.

(9) **Respondent held library card:** binary variable equal to one if a respondent had a library card when she was age 14. About 70 percent of the sample did.

(10) **Two-parent household:** binary variable equal to one if a respondent lived in a household with two parents (including step-parents) at age 14. About 80 percent of the sample lived with two parents at age 14.

(11) **Number of siblings**: number of siblings of respondent in 1968 (not necessarily in the household); we can't reliably determine whether this includes step- and half-siblings.

(12) **Father born in U.S.**: binary variable equal to one if a respondent's father was born in U.S./Canada. About 96 percent of sample had father born in U.S./Canada.

(13) Highest grade completed by father: highest grade completed by father, in 1968. Conditional on having a father in household. Item non-response is relatively high; *ELA*, however, is uncorrelated with whether father's HGC is observed.

(14) Highest grade completed by mother: highest grade completed by mother, in 1968. Conditional on having a mother in household. Item non-response is relatively high; *ELA*, however, is uncorrelated with whether mother's HGC is observed.

(15) Parents' education goals for respondent: number of years of schooling respondent's parents want respondent to obtain, when respondent was 14.

(16) Atypicality index of mother's job: atypicality index of respondent's mother's job when respondent was 14, conditional on respondent's mother working then. Atypicality index is the female percentage of an occupation minus the percent of the experienced civilian labor force that was female in 1970; negative numbers indicate more atypical occupations.

(17) Respondent's IQ score: continuous IQ score of respondent. Reference distribution is independent national norm, not empirical sample. Only two-thirds of the entire sample had an IQ or achievement test administered; while these two-thirds were slightly above national norms, the presence of an IQ score is uncorrelated with *ELA*.

(18) Rural residence: binary variable equal to one if a respondent resided on a farm/ranch or in another rural area at age 14. About 26 percent of the sample lived in a rural area at age 14.

Alternative Specifications

Figures 3 and 4 include six specifications: one following equation (1) called our baseline specification, one following equation (1') that augments our baseline specification with abortion controls, and four alternative specifications of (1') described below. Estimates from equation (1') are presented as the main tables of the paper. Tabular presentations from all other specifications can be found in Appendix C.

Linear state-specific time trends: The specification in equation (1') is augmented with the interactions of each state of residence dummy with the year of observation.

Vietnam casualties: Using data from the National Archives on the Vietnam Conflict (http://www.archives.gov/research/military/vietnam-war/electronic-records.html), the specification in equation (1') 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.

Balanced panel: The specification in equation (1') is estimated on a sample that is restricted to women who are interviewed in every survey wave from 1968 through 2003 and successfully answer all relevant questions (no item non-response).

High school state: This specification uses state of residence during high school (rather than at age 21) for all state-based variables. Like state of residence at age 21, this variable is created using each wave's state of residence, move histories, and tenure at current residence. Because older cohorts are father removed from high school age, they are less likely to be successfully matched, particularly if they moved frequently. (While this problem exists for state of residence at age 21, it is more pronounced for high school state.)

Appendix B. Legal Coding

The coding used in this paper relies upon the updated coding of Bailey and Guldi (2009) and differs from the coding used in Bailey (2006) for 15 states. These differences in coding reflect two main changes: (1) Non-specific female age of majority statutes are not treated as emancipation for the purpose of consenting for medical care unless this is specifically noted in the statute. As a result, the coding changes in 4 states. (2) Statutes were interpreted incorrectly, enforcement was ambiguous, or earlier statutes, policy changes or attorney general decisions were found. These changes affected coding in 11 states; in six of these cases, the date of legal change shifts by only one or two years. These legal changes are summarized in Table 1, and then the explanation of each of the changes is discussed in detail, including legal citations by state.

Table 1
Dates of Legal Change Granting Early Access to the Pill

State	Bailey (2006)	Bailey and Guldi (2009)	Different?	Reason for recoding?
Alabama	1971	1971		
Alaska	1960	1960		
Arizona	1972	1972		
Arkansas	1960	1973	X	FAOM->AOM
California	1972	1972		
Colorado	1971	1971		
Connecticut	1972	1972		
Delaware	1972	1972		
District of Columbia	1971	1971		
Florida	1974	1974		
Georgia	1968	1968		
Hawaii	1970	1972	X	TFP->AOM
Idaho	1963	1972	X	FAOM->AOM
Illinois	1973*	1969		
Indiana	1973	1973		
Iowa	1973	1972	X	Earlier AOM
Kansas	1970	1970		
Kentucky	1968	1965/1968?	X	Ambiguous interpretation
Louisiana	1972	1972		
Maine	1971	1969	X	Earlier AOM
Maryland	1967	1971	X	TFP->MM
Massachusetts	1974	1974		
Michigan	1972	1972		
Minnesota	1973	1972	X	Earlier AGD
Mississippi	1966	1966		

Missouri	1976	1973	X	Earlier AGD
Montana	1971	1971		
Nebraska	1972	1972		
Nevada	1969	1973	X	FAOM->AOM
New Hampshire	1971	1971		
New Jersey	1973	1973		
New Mexico	1971	1971		
New York	1971	1971		
North Carolina	1971	1971		
North Dakota	1972	1972		
Ohio	1965	1960		MM
Oklahoma	1966	1972	X	FP->AOM
Oregon	1971	1971		
Pennsylvania	1971	1970	X	Earlier MM
Rhode island	1972	1972		
South Carolina	1972	1972		
South Dakota	1972	1972		
Tennessee	1971	1971		
Texas	1974	1974		
Utah	1962	1975	X	FAOM->AOM
Vermont	1972	1972		
Virginia	1971	1971		
Washington	1971	1968	X	AOM->FP
West Virginia	1972	1972		
Wisconsin	1973	1972	X	Earlier AOM
Wyoming	1969	1969		
Differences in coding			15	

Legal change is coded as the earliest date, at which an unmarried, childless women under age 21 could legally consent for medical treatment without parental or spousal consent. A full legal appendix and scans of statutes are available from Bailey and Guldi (2009). FAOM->AOM: lower female age of majority changed to the legal majority for men and women for all purposes. FP->AOM: family planning law changed to age of majority law; AOM->FP indicates the reverse. TFP->AOM/MM: erroneously coded treatment for pregnancy statute changed to be the date for the change in legal age of majority/mature minor doctrine. Earlier AGD/AOM/MM indicates that an earlier attorney general decision/age of majority/mature minor doctrine was located. *Illinois is a typo in the published version of Bailey (2006) that the author did not catch before publication. The correct coding and the coding used in her analysis is 1969. See notes below for more details.

Arkansas

Bailey (2006) coded the 1948 Arkansas statute that stipulated that females over 18 were of the age of

majority [AR Code §9-25-101 (1987), AR Stat. Ann. §57-103 (1947)], but it is unclear that this law treated

women as legal adults except for marriage. Effective July, 1973, Arkansas passed a law allowing pregnant

minors of any age to consent to medical care other than abortion (Merz et al. 1995: footnote 150; Acts 1973, No.

32, §1, p.1028). The law provided that *any* female could consent to medical treatment or procedures "for herself

when in given [sic.] connection with pregnancy or childbirth, except the unnatural interruption of a pregnancy"

[AR R.S. §82-363 (1976)]. The statute goes on to grant the power of consent to "any unemancipated minor of

sufficient intelligence to understand and appreciate the consequences of the proposed surgical or medical

treatment or procedures" [ibid.]. Bailey and Guldi (2009), therefore, code a mature minor doctrine as of 1973.

Hawaii

Bailey (2006) erroneously codes a "treatment for pregnancy" statute as a mature minor doctrine: "The

consent to the provision of medical care and services by public and private hospitals or public and private

clinics, or the performance of medical care and services by a physician licensed to practice medicine, when

executed by a female minor who is or professes to be pregnant" [HI Rev. Stat. §577A-2 (1999), L. 1968, c. 58].

Under this law, *only* minors professing to be pregnant or having a venereal disease could consent to "medical

care," defined as "the diagnosis, examination and administration of medication in the treatment of venereal

diseases and pregnancy" [L. 1968, c. 58, §4]. This law did not permit non-pregnant teens to be treated or

prescribed contraception legally. Bailey and Guldi (2009) code the legal change in the age of majority, effective

March 28, 1972, which lowered the age of majority to 18.

Idaho

Bailey (2006) codes a female age of majority statute [ID Code Ann. §31-101 (1932)], but it is unclear

whether consent to contraception would have been covered under this statute. Bailey and Guldi (2009) found a

1972 amendment that equalized the ages of majority for males and females at 18 and extended this majority for

all purposes [ID Code §32-101 (1983); am. 1972, ch. 117, §1, p. 233].

Iowa

Bailey (2006) codes the change in the legal age of majority to 18 in 1973. Bailey and Guldi (2009) located

and code an earlier change in the legal age of majority from 21 to 19 in 1972 [IA Code Ann. §599.1 (1954),

Acts 1972 (64 G.A.) ch. 1027, §49; Acts 1973 (65 G.A.) ch. 140, §49].

Kentucky

Bailey and Guldi (2009) codes a law, effective January 1, 1965, that lowered the legal age of majority "for

all purposes" in Kentucky to 18 [KY R.S. §2.015 (1967), enacted Acts 1964, ch. 21, § 1].[1] Because this Council

of State Governments publication in 1973 noted that this 1965 had law prompted "a good deal of confusion

[about the exact privileges granted to those 18 and older] and four years later [a] clarifying statute was passed"

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[1] Merz et al. cites 1972 KY Acts ch. 98, effective July 26, 1972, as lowering the age of majority from 21 to 18. This citation, however, is in error. The referenced statute is a law "relating to the powers and duties of fiscal courts to control wild animals that carry diseases transmissible to man and domestic animals." We believe this citation to be incorrect; we have verification that the age of majority did, in fact, change in 1964, effective January 1, 1965, with the clarification added in 1968 (see text).

[1972: pp.12-3], Bailey (2006) codes the 1968 amendment to the age of majority statute that included the clause "all other statutes to the contrary notwithstanding" [KY Acts ch. 100, §1, approved March 25, 1968] that clarified the interpretation of the statute.

Maine

Bailey (2006) codes a change in the legal age of majority passed in 1971 which lowered the legal age of majority to 18 [1 M.R.S.A. §73 (1979); 1969, c. 433 §8; 1971 c. 598, §8]. Bailey and Guldi (2009) located an earlier statutory change in the age of majority, effective October 1, 1969, which lowered the legal age of majority in Maine from 21 to 20.[2]

Maryland

Bailey (2006) erroneously codes a "treatment for pregnancy" statute based upon Merz et al. (1995: footnote 388), which notes that minors could consent to medical treatment for "alcohol and drug abuse, venereal diseases, pregnancy, contraception other than sterilization, and in cases of rape or sexual abuse" since June 1, 1967. However, the specific language relating to contraception was not added until 1971. The original statute, effective June 1, 1967, restricted the law to "apply … to minors who profess to be in need of hospital or clinical care or services or medical or surgical care or services to be provided by a physician licensed to practice medicine, whether because of suspected pregnancy or venereal disease, regardless of whether such professed suspicions of pregnancy or venereal disease are, or are not subsequently substantiated on a medical basis" [MD Laws 1967 ch. 468]. Therefore, Bailey and Guldi (2009) code the 1971 revision to the 1967 statute that eliminated the restriction to pregnant minors or minors suspected to be pregnant.

Minnesota

Bailey (2006) codes the change in the age of majority to 18 effective June 1, 1973 [Minn. Stat. § 518.54(2) (1990)]. One year prior to the change in the age of majority, on May 27, 1971, a series of statutes concerning the consent to medical care of minors became effective. One section provides for an extension of the rights of emancipated minors [MN Stat. Ann. §144.341 (1989); see also CA Civil Code §34.6 (1982)]. Although

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[2] Merz et al. only states that the general age of majority has been 18 since 1971[ME RSA tit. 1, §72.1]; the text does not mention what the age changed from to become 18. The statutory change, lowering the age of majority from 20 to 18, is cited as 1971, c. 598, §8; however, this was during a special session of the 1971 legislature, and the Acts were not effective until June 9, 1972. Even though the law was passed in 1971, it did not become effective until 1972. Therefore, we do not see any conflict with Merz; we simply provide more precise detail of the changes.

ambiguous in their applicability to consent for birth control, a 1972 Attorney General decision interpreted these statutes as "not making it a crime for physicians to furnish birth control devices to minors" [From LexisNexis Academic: Minn. Stat. §§144.341-144.347, 617.251 (1971), No. 494-b-39, 1972 Minn. AG LEXIS 35]. The interpretation of these statutes remained in dispute for some time; they were again challenged in *Maley v. Planned Parenthood of Minnesota, Inc.* Cir. Case No. 37769 (Minn. Dist. Ct., Third Jud. Dist., Jan. 5, 1976). In this case, six couples filed a class action lawsuit, seeking to prevent Planned Parenthood from providing contraceptive services to unemancipated minors without parental consent (Paul, Pilpel and Wechsler, 1974; http://www.popline.org/docs/730457). However, the Minnesota District Court upheld the constitutionality of sections 144.343 and 144.344, writing that "under these sections Planned Parenthood could provide minors with contraceptive information and services without parental consent, unless a parent specifically notifies Planned Parenthood that he/she does not wish his/her child to receive such services" (DHEW 1978, p.244).[3] This decision, therefore, reinforced the attorney general's broad interpretation of the statute. Legally, Planned Parenthood could provide contraceptives to unmarried minors as long as they had not been explicitly informed by parents. Bailey and Guldi (2009), therefore, revise the coding to reflect the 1972 attorney general decision.

Missouri

Bailey (2006) coded the *Planned Parenthood of Central Missouri v. Danforth* decision [428 U.S. 52 (1976)], in which the Supreme Court ruled that the state could not prohibit minors from obtaining abortions and, by extension, contraception. Bailey and Guldi (2009) located an earlier Attorney General decision issued in March of 1973 stating that "no law prohibits physicians from prescribing contraceptives to minors who do not have parental consent or who have not been emancipated by marriage or other means" [DHEW 1978, p. 253, citing Op. Atty. Gen. 3/9/1973].

Nevada

Bailey (2006) codes a 1969 lower female age of majority statute, but this statute was in effect since at least 1930 and applied only to women's ability to enter into contracts [NV C.L. §300 (1930); NV R.S. §129.010 (1963); see also DHEW 1974, p. 236]. Bailey and Guldi (2009) code a 1973 amendment to the age of majority

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[3] Though the final *Maley* ruling was not issued until 1976, according to Paul, Pilpel and Wechsler (1974), the district court came to the same conclusion during a preliminary stage of the case in 1973.

statute which equalized the ages of majority for males and females at 18 [N.R.S. §129.010 (2003); 1973, p. 1578].

Ohio

Ohio courts adopted a mature minor doctrine as early as 1956. The *Lacey v. Laird* [166 Ohio St. 12, 139 N.E. 2d 25 (1956)] opinion states,

> *A charge that this 18-year-old plaintiff [who had nose surgery when she was 18 without her parents' consent] could not consent to what the jury could have found was only a simple operation, would seem inconsistent with the conclusion of our General Assembly, that any female child of 16 can prevent the taking of liberties with her person from being raped merely by consenting thereto at the time such liberties are taken....My conclusion is that performance of a surgical operation upon an 18-year-old girl with her consent will ordinarily not amount to an assault and battery for which damages may be recoverable even though the consent of such girl's parents or guardian has not been secured [139 N.E. 2d at 34].*

Legal interpretations held that minors could consent to minor surgery and general medical care under this decision (DHEW 1974: 265), but Ohio also had an anti-obscenity statute. Ohio's statute originally passed in 1885 and banned the dissemination of information and supplies relating to contraception. The words "for the prevention of conception" were removed from Ohio's statute in 1965, so Bailey (2006) coded 1965 as the earliest date that an unmarried minor could obtain the Pill legally. However, Ohio's statute went on to note that "nothing in this section [about contraception and obscenity] or the next two sections shall be construed to affect teaching in regularly chartered medical colleges, or the publication of standard medical books, or the practice of regular practitioners of medicine, or druggists in their legitimate business" [OH R.S. §7027 (1896)] [April 30, 1885: 82 v. 184]. It is not clear how to interpret this physician and pharmacist exceptions, which makes it unclear whether to code Ohio as 1960, when the Pill was introduced (this assumes that the obscenity statute was not binding for physicians), or 1965, when the law was amended to omit language about contraception (this assumes the obscenity statute was binding for physicians).

Oklahoma

Bailey (2006) coded a family planning statute [OK Stat. Ann. Tit. 63 Ch. 32, §§2071-5 (1984)]. Although no explicit eligibility requirements are stated in the statutes, the Department of Health Education and Welfare (DHEW) contacted the state about their policy and reported that, "[a]ll categories of adults apparently are eligible for family planning services; no exclusions were noted in the CFPPD survey and none appear in the written policies. According to the Division of Maternal and Child Health's *Guidelines for Family Planning*

Programs, 'minors may be accepted for services if: 1) ever married or ever pregnant; 2) bearing acceptable proof of impending marriage; 3) accompanied by parent or guardian requesting services; 4) referred by a recognized agency, a doctor, a nurse, or a clergyman…[However,] contraceptive advice may be given in *all* cases where the 'health needs of the patient make it advisable…'" (1974, p.271). Because these policies only allow legal minors who are pregnant to obtain contraceptive *advice*, Bailey and Guldi (2009) code the change in the legal age of majority which was amended and effective in August 1, 1972, which equalized the ages of majority for men and women at 18 [OK Stat. Ann. Tit. 15 §13 (1972); L. 1972, c. 221, §1].

Pennsylvania

Bailey (2006) coded a mature minor doctrine effective in 1971, but Bailey and Guldi (2009) located an earlier mature minor statute, enacted on February 13, 1970 and effective in April 1970, that allowed any minor 18 or over to consent to medical care: "Any minor who is eighteen years of age or older… may give effective consent to medical, dental and health services for himself or herself, and the consent of no other person shall be necessary" [PA Stat. tit. 35, §10101 (1977)].

Utah

Bailey (2006) coded the lower age of female majority, but this statute's application was unclear with respect to medical care. Policy documents indicate there was considerable ambiguity regarding whether physicians could prescribe birth control to unmarried women under age 21. On July 21, 1971, the Attorney General advised "*not* to provide family planning information or services to minors without parental consent 'until such time as the state legislature may adopt appropriate legislation.'…In support of this view the Attorney General cites the common law requirement of parental consent in the absence of an emergency, plus the expression of legislative intent inferred from the statute dealing with prophylactics…" (DHEW 1974: 300 citing Op. Atty. Gen. No. 71-017, July 21 1971). Bailey and Guldi (2009), therefore, code the amendment to this statute in 1975 to make both men and women legal adults at the age of 18 for all purposes [L. 1975, ch. 39, §1, approved March 24, 1975].

Washington

Bailey (2006) codes the legal age of majority "for all purposes" which changed from 21 to 18 in 1971. Bailey and Guldi (2009) located an earlier policy change and code 1968, because a Washington Board of Health Policy directed that all persons were eligible for family planning without parental consent, including never-

pregnant, never-married minors [WAC248-128-001 for Board of Health policy adopted August 3, 1967, codified July 1, 1968].

Wisconsin

Bailey (2006) erroneously coded the date of 1973 as the year the legal change in age of majority to 18 became effective [WI Laws 1971, ch. 213; see also DHEW (1978: 363)]. In fact, this statute became effective in March 23, 1972. Bailey and Guldi (2009), therefore, code 1972.

Appendix C. Additional Estimates and Sensitivity Checks

Table C1. The Impact of ELA on Pill Use among Ever Married Women, with State Linear Time Trends

	(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	-0.072	0.381	0.204	0.210	0.133
	(0.030)	(0.167)	(0.209)	(0.106)	(0.046)
R-squared	0.070	0.138	0.141	0.156	0.142
Panel B. Pill Use Heterogeneity					
ELA	-0.067	0.443	0.246	0.292	0.169
	(0.030)	(0.142)	(0.185)	(0.114)	(0.065)
ELA x Non-metro area	-0.006	-0.102	-0.072	-0.141	-0.070
	(0.014)	(0.055)	(0.067)	(0.071)	(0.059)
R-squared	0.070	0.139	0.142	0.157	0.143
Observations	1985	1985	1985	1985	1985
Fixed effects	S, Y	S, Y	S, Y	S, Y	S, Y
State linear time trends	Yes	Yes	Yes	Yes	Yes

See notes to table 2.

Table C2. The Impact of ELA on Pill Use among Ever Married Women, with Region by Year of Birth Fixed Effects

	(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	-0.067	0.141	0.140	0.132	0.049
	(0.017)	(0.208)	(0.140)	(0.107)	(0.045)
R-squared	0.053	0.115	0.134	0.142	0.136
Panel B. Pill Use Heterogeneity					
ELA	-0.055	0.284	0.211	0.255	0.123
	(0.021)	(0.171)	(0.117)	(0.096)	(0.056)
ELA x Non-metro area	-0.013	-0.180	-0.091	-0.158	-0.096
	(0.014)	(0.057)	(0.056)	(0.068)	(0.055)
R-squared	0.053	0.118	0.135	0.144	0.137
Observations	1985	1985	1985	1985	1985
Fixed effects	S, RxY	S, RxY	S, RxY	S, RxY	S, RxY

See notes to table 2.

Table C3. The Impact of ELA on the Timing of First Marriage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age at first marriage	1=Married before 19	1=Married before 20	1=Married before 21	1=Married before 22	1=Married before 23	1=Married before 24
Mean of DV	*21.2*	*0.270*	*0.396*	*0.505*	*0.597*	*0.671*	*0.721*
ELA	0.427	-0.064	-0.059	-0.020	-0.018	-0.004	-0.007
	(0.270)	(0.022)	(0.023)	(0.024)	(0.029)	(0.031)	(0.033)
Observations	3786	4210	4204	4200	4200	4200	4200
(Pseudo) R-squared	0.04	0.04	0.03	0.03	0.03	0.03	0.02
Fixed effects	S, Y	S, Y	S, Y	S, Y	S, Y	S, Y	S, Y

This table presents estimates of ELA on age of first marriage among those ever married (column 1) and binary indicators for whether the respondent was married before age a, for a=19,…, 24. The table uses the 1943 to 1953 birth cohorts from the NLS-YW. The sample in columns 2 through 7 includes women who never get married and the estimates represent average partial effects from a probit. Changes in sample size across columns 2 through 7 are due to dropping of observations that do not contribute to the likelihood. The R-squareds for columns (2) through (7) are pseudo (McFadden's) R^2. 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 C4. Heterogeneity in the Impact of Early Access to the Pill on Marriage and Divorce Propensities

	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	A. Never Been Married					B. Ever Been Divorced				
	Lower third of IC dist.	Middle third of IC dist.	Upper third of IC dist.	No College	Some College	Lower third of IC dist.	Middle third of IC dist.	Upper third of IC dist.	No College	Some College
ELA * Age 20-24	-0.120	0.020	0.060	-0.020	0.010	0.010	0.010	ND	0.000	-0.010
	(0.079)	(0.066)	(0.079)	(0.029)	(0.052)	(0.027)	(0.015)		(0.008)	(0.005)
ELA * Age 25-29	-0.030	0.020	0.030	-0.010	0.030	0.097*	0.085**	0.020	0.044*	0.020
	(0.060)	(0.045)	(0.059)	(0.021)	(0.0501	(0.057)	(0.035)	(0.031)	(0.024)	(0.018)
ELA * Age 30-34	-0.040	0.030	0.030	-0.010	0.030	0.070	0.070	0.020	0.020	0.020
	(0.050)	(0.033)	(0.047)	(0.020)	(0.035)	(0.066)	(0.047)	(0.039)	(0.030)	(0.025)
ELA * Age 35-39	-0.030	0.030	0.020	-0.020	0.020	0.030	0.070	0.020	0.000	0.010
	(0.044)	(0.029)	(0.044)	(0.017)	(0.031)	(0.074)	(0.051)	(0.019)	(0.030)	(0.032)
ELA * Age 40-44	-0.050	0.020	0.040	-0.023*	0.010	0.010	0.020	0.020	-0.030	0.010
	(0.044)	(0.026)	(0.045)	(0.015)	(0.028)	(0.078)	(0.055)	(0.047)	(0.032)	(0.034)
ELA * Age 45-49	-0.050	0.010	0.030	-0.030	0.010	0.010	0.030	0.000	-0.040	0.010
	(0.039)	(0.025)	(0.047)	(0.017)	(0.031)	(0.079)	(0.032)	(0.047)	(0.033)	(0.037)
Observations	12605	16698	20330	48548	26371	13540	18284	21575	54006	26439
Unique women	788	972	1112	2898	1456	776	966	1109	2895	1450
Pseudo R2	0.23	0.33	0.32	0.24	0.35	0.22	0.21	0.19	0.19	0.18
Mean of DV for 20-24	0.459	0.415	0.510	0.347	0.665	0.029	0.030	0.027	0.039	0.013
Mean of DV for 25-29	0.223	0.145	0.187	0.159	0.276	0.106	0.121	0.092	0.127	0.065
Mean of DV for 30-34	0.156	0.080	0.114	0.119	0.165	0.205	0.209	0.180	0.224	0.148
Mean of DV for 35-39	0.131	0.064	0.087	0.104	0.116	0.303	0.287	0.256	0.301	0.226
Mean of DV for 40-44	0.129	0.062	0.083	0.098	0.110	0.381	0.358	0.319	0.373	0.288
Mean of DV for 45-49	0.120	0.057	0.086	0.091	0.107	0.466	0.422	0.368	0.441	0.345

This tables presents mean marginal effects of equation (1') from a probit. Each column presents estimates from a separate regression on the indicated groups. "ND" indicates that disclosure requirements were not met for this estimate. All other notes are as in table 4.

Table C3A. Wages Rates and Annual Income: No Abortion Controls

		(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*	-0.254	-0.033	*9943*	-954	-0.104**	*7660*	-1,300**
		(0.283)	(0.023)		(638)	(0.048)		(572)
ELA * Ages 25-29	*9.60*	-0.242	-0.015	*15610*	168	0.078*	*10911*	-151
		(0.319)	(0.026)		(724)	(0.047)		(663)
ELA * Ages 30-34	*10.62*	0.408	0.030	*18116*	1,004	0.117**	*12452*	722
		(0.313)	(0.025)		(679)	(0.051)		(639)
ELA * Ages 35-39	*11.74*	0.560	0.037	*21174*	1,963***	0.114**	*15442*	1,472**
		(0.334)	(0.024)		(749)	(0.046)		(722)
ELA * Ages 40-44	*12.84*	0.787*	0.055**	*24493*	2,315***	0.102**	*19184*	2,845***
		(0.306)	(0.022)		(878)	(0.045)		(838)
ELA * Ages 45-49	*14.29*	1.128**	0.081**	*28148*	2,148***	0.085*	*25238*	3,986***
		(0.461)	(0.031)		(862)	(0.048)		(1,000)
Observations		46388	46388		51277	51277		68169
Unique women		4210	4210		4245	4245		4351
R-squared		0.21	0.26		0.01	0.10		0.01

See table 3. The estimates here do NOT include controls for abortion.

Table C4A. Human Capital Accumulation and Occupational Upgrading: No Abortion Controls

	(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	-774**	0.047**	0.087	-0.005	0.008	0.007
	(359)	(0.021)	(0.133)	(0.012)	(0.014)	(0.007)
ELA * Age 25-29	-1,010**	0.006	0.314**	0.031***	0.047**	0.020*
	(431)	(0.008)	(0.129)	(0.011)	(0.020)	(0.011)
ELA * Age 30-34	293	0.003	0.265**	0.027*	0.060***	0.063***
	(408)	(0.012)	(0.130)	(0.016)	(0.022)	(0.016)
ELA * Age 35-39	902	0.002	0.289**	0.009	0.042*	0.044**
	(560)	(0.010)	(0.128)	(0.016)	(0.025)	(0.020)
ELA * Age 40-44	2,407***	-0.009	0.281**	0.020	0.035	0.030
	(767)	(0.009)	(0.133)	(0.020)	(0.029)	(0.020)
ELA * Age 45-49	1,366	-0.010	0.232	-0.020	0.000	-0.010
	(987)	(0.007)	(0.143)	(0.019)	(0.023)	(0.017)
Observations	61736	57373	78809	63013	73737	73737
Unique women	4329	3702	4354	4323	4354	4354
(Pseudo) R-squared	0.62	0.14	0.14	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

See table 4. The estimates here do NOT include controls for abortion.

Table C5A. Heterogeneity in the Growth of Real Hourly Wages: No Abortion Controls

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-0.616	0.428	-0.500	-0.223	-0.850*
	(0.560)	(0.576)	(0.447)	(0.265)	(0.489)
ELA * Age 25-29	-0.305	0.710	-0.039	-0.235	-0.200
	(0.553)	(0.696)	(0.504)	(0.271)	(0.457)
ELA * Age 30-34	-1.010*	1.505*	0.645	-0.029	0.821
	(0.533)	(0.794)	(0.654)	(0.269)	(0.639)
ELA * Age 35-39	-0.338	1.743**	0.757	-0.178	1.484**
	(0.647)	(0.750)	(0.672)	(0.377)	(0.699)
ELA * Age 40-44	-0.555	2.267**	0.753	0.625	1.433**
	(0.885)	(0.916)	(0.641)	(0.460)	(0.569)
ELA * Age 45-49	0.730	2.433**	2.371***	0.929*	2.645***
	(1.031)	(0.928)	(0.902)	(0.485)	(0.797)
Observations	10468	14165	16788	40229	21785
Unique women	793	975	1112	2895	1456
R-squared	0.17	0.20	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

See table 5. The estimates here do NOT include controls for abortion.

Table C6A. Heterogeneity in Highest Grade Completed: No Abortion Controls

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) Lower third SES distribution	(5) Middle third SES distribution	(6) Upper third SES distribution
ELA * Age 20-24	-0.507**	0.230	0.096	0.321**	-0.174	0.148
	(0.216)	(0.198)	(0.185)	(0.142)	(0.190)	(0.295)
ELA * Age 25-29	-0.412*	0.360*	0.343*	0.585***	-0.006	0.297
	(0.224)	(0.211)	(0.190)	(0.152)	(0.225)	(0.255)
ELA * Age 30-34	-0.436*	0.387*	0.369*	0.514***	-0.017	0.258
	(0.225)	(0.205)	(0.191)	(0.162)	(0.234)	(0.267)
ELA * Age 35-39	-0.410*	0.447**	0.446**	0.532***	0.043	0.277
	(0.221)	(0.203)	(0.192)	(0.164)	(0.241)	(0.276)
ELA * Age 40-44	-0.515**	0.472**	0.401**	0.560**	0.022	0.257
	(0.236)	(0.223)	(0.198)	(0.186)	(0.236)	(0.258)
ELA * Age 45-49	-0.401	0.359	0.531**	0.529***	-0.060	0.259
	(0.258)	(0.225)	(0.205)	(0.193)	(0.246)	(0.263)
Observations	13538	17550	20982	25101	24538	24798
Unique women	793	975	1112	1392	1366	1342
R-squared	0.18	0.19	0.23	0.11	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

See table 6. The estimates here do NOT include controls for abortion.

Table C7A. Heterogeneity in Cumulative Experience: No Abortion Controls

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-1,125	486	-628	-713	-1,211**
	(1,279)	(997)	(684)	(512)	(598)
ELA * Age 25-29	-1,366	275	-688	-822	-1,129*
	(1,270)	(1,069)	(667)	(548)	(628)
ELA * Age 30-34	-739	2,247*	430	185	754
	(1,138)	(1,184)	(768)	(501)	(737)
ELA * Age 35-39	-218	3,118**	1,226	509	2,005**
	(1,336)	(1,352)	(851)	(729)	(892)
ELA * Age 40-44	-203	5,216***	1,767*	2,289***	3,088***
	(1,630)	(1,767)	(983)	(879)	(1,048)
ELA * Age 45-49	-881	4,147*	1,180	1,637	2,446*
	(2,109)	(2,327)	(1,267)	(1,076)	(1,330)
Observations	10778	14061	16995	40836	21942
Unique women	804	987	1133	2960	1487
R-Squared	0.61	0.63	0.68	0.58	0.70
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	27954	33133

See table 7. The estimates here do NOT include controls for abortion.

Appendix Table C3B. Wages Rates and Annual Income: State Linear Time Trends

	Mean real hourly wages excl. zeros	(1) Real hourly wage (excl. zeros)	(2) Log real hourly wage	*Mean real wages/salary last year excl. zeros*	(3) Wage or salary last year (excl. zeros)	(4) Log real annual wage	*Mean real wages/salary last year incl. zeros*	(5) Wage or salary last year (incl. zeros)
ELA * Ages 20-24	*7.88*	-0.437*	-0.046**	*9943*	-1,458***	-0.115***	*7660*	-1,919***
		(0.253)	(0.021)		(462)	(0.043)		(426)
ELA * Ages 25-29	*9.60*	-0.350	-0.020	*15610*	-195	0.050	*10911*	-415
		(0.324)	(0.027)		(647)	(0.047)		(624)
ELA * Ages 30-34	*10.62*	0.320	0.030	*18116*	868	0.117**	*12452*	797
		(0.334)	(0.028)		(728)	(0.059)		(704)
ELA * Ages 35-39	*11.74*	0.647*	0.056**	*21174*	2,326***	0.146***	*15442*	2,046***
		(0.366)	(0.028)		(803)	(0.048)		(792)
ELA * Ages 40-44	*12.84*	0.949***	0.076***	*24493*	2,766***	0.120***	*19184*	3,568***
		(0.351)	(0.025)		(954)	(0.044)		(867)
ELA * Ages 45-49	*14.29*	1.506***	0.118***	*28148*	2,527***	0.106***	*25238*	4,720***
		(0.487)	(0.034)		(916)	(0.041)		(865)
Observations		46388	46388		51277	51277		68169
Unique women		4210	4210		4245	4245		4351
R-squared		0.23	0.28		NA	NA		NA

See table 3. The estimates here include state linear time trends.

Table C4B. Human Capital Accumulation and Occupational Upgrading: State Linear Time Trends

	(1) Cumulative Experience in Hours	(2) 1= Enrolled in College	(3) Highest Grade Completed	(4) 1=Occup. training since last interview	(5) 1= in Professional Job	(6) 1=in Non-traditional Job
ELA * Age 20-24	-1,120***	0.054**	0.040	0.002	0.006	0.009
	(298)	(0.024)	(0.130)	(0.014)	(0.012)	(0.008)
ELA * Age 25-29	-1,639***	0.010	0.223*	0.033***	0.041**	0.019*
	(359)	(0.009)	(0.131)	(0.011)	(0.019)	(0.011)
ELA * Age 30-34	70	0.003	0.210	0.026	0.054***	0.059***
	(407)	(0.013)	(0.132)	(0.016)	(0.021)	(0.016)
ELA * Age 35-39	1,048*	-0.002	0.264*	0.011	0.036*	0.039**
	(545)	(0.010)	(0.135)	(0.019)	(0.023)	(0.019)
ELA * Age 40-44	2,370***	-0.010	0.245*	0.017	0.032	0.026
	(763)	(0.009)	(0.138)	(0.022)	(0.027)	(0.021)
ELA * Age 45-49	1,592*	-0.010	0.220	-0.021	0.004	-0.011
	(928)	(0.007)	(0.150)	(0.021)	(0.022)	(0.019)
Observations	61736	57373	78809	63013	73737	73737
Unique women	4329	3702	4354	4323	4354	4354
(Pseudo) R-squared	0.63	0.16	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

See table 4. The estimates here include state linear time trends.

Table C5B. Heterogeneity in the Growth of Real Hourly Wages: State Linear Time Trends

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-1.283**	0.380	-0.610	-0.621**	-0.907*
	(0.631)	(0.590)	(0.491)	(0.266)	(0.493)
ELA * Age 25-29	-0.530	0.780	0.240	-0.350	-0.190
	(0.558)	(0.716)	(0.491)	(0.257)	(0.510)
ELA * Age 30-34	-0.800	1.868**	0.700	0.100	0.760
	(0.558)	(0.761)	(0.687)	(0.303)	(0.625)
ELA * Age 35-39	0.450	2.040**	0.740	0.120	1.610**
	(0.745)	(0.810)	(0.623)	(0.409)	(0.735)
ELA * Age 40-44	0.520	2.477**	1.080	1.042**	1.691**
	(1.108)	(0.935)	(0.704)	(0.470)	(0.710)
ELA * Age 45-49	2.121*	2.625**	3.507***	1.524***	3.184***
	(1.204)	(0.980)	(1.067)	(0.445)	(0.891)
Observations	10468	14165	16788	40229	21785
Unique women	793	975	1112	2895	1456
R-squared	0.20	0.22	0.25	0.19	0.28
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

See table 5. The estimates here include state linear time trends.

Table C6B. Heterogeneity in Highest Grade Completed: State Linear Time Trends

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) Lower third SES distribution	(5) Middle third SES distribution	(6) Upper third SES distribution
ELA * Age 20-24	-0.562***	0.170	0.140	0.240	-0.190	0.190
	(0.195)	(0.197)	(0.179)	(0.167)	(0.203)	(0.273)
ELA * Age 25-29	-0.483**	0.300	0.365*	0.456***	-0.040	0.270
	(0.208)	(0.214)	(0.191)	(0.161)	(0.235)	(0.267)
ELA * Age 30-34	-0.442**	0.370	0.416**	0.396**	-0.020	0.250
	(0.208)	(0.220)	(0.197)	(0.159)	(0.245)	(0.288)
ELA * Age 35-39	-0.370*	0.461**	0.525**	0.431***	0.100	0.270
	(0.195)	(0.224)	(0.207)	(0.152)	(0.258)	(0.320)
ELA * Age 40-44	-0.435*	0.508**	0.464**	0.394**	0.080	0.250
	(0.221)	(0.250)	(0.203)	(0.164)	(0.265)	(0.302)
ELA * Age 45-49	-0.280	0.420	0.627***	0.405**	0.070	0.200
	(0.239)	(0.257)	(0.223)	(0.170)	(0.286)	(0.335)
Observations	13538	17550	20982	25101	24538	24798
Unique women	793	975	1112	1392	1366	1342
R-squared	0.20	0.21	0.24	0.13	0.20	0.27
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

See table 6. The estimates here include state linear time trends.

Table C7B. Heterogeneity in Cumulative Experience: State Linear Time Trends

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-1,319	-128	310	-1,141***	-575
	(870)	(662)	(557)	(390)	(558)
ELA * Age 25-29	-1,943	-399	-525	-1,589***	-1,185*
	(1,199)	(913)	(625)	(495)	(663)
ELA * Age 30-34	-819	2,140*	518	175	487
	(1,400)	(1,212)	(795)	(508)	(747)
ELA * Age 35-39	171	3,392**	1,161	532	1,604*
	(1,901)	(1,449)	(974)	(741)	(873)
ELA * Age 40-44	461	5,449***	1,382	2,205**	2,006*
	(2,464)	(1,852)	(1,216)	(871)	(1,041)
ELA * Age 45-49	224	4,728*	841	1,976*	1,344
	(2,953)	(2,566)	(1,498)	(1,092)	(1,349)
Observations	10778	14061	16995	40836	21942
Unique women	804	987	1133	2960	1487
R-squared	0.65	0.66	0.70	0.60	0.72
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	27954	33133

See table 7. The estimates here include state linear time trends.

Table C3C. Wages Rates and Annual Income: Vietnam Controls

	Mean real hourly wages excl. zeros	(1) Real hourly wage (excl. zeros)	(2) Log real hourly wage	*Mean real wages/salary last year excl. zeros*	(3) Wage or salary last year (excl. zeros)	(4) Log real annual wage	*Mean real wages/salary last year incl. zeros*	(5) Wage or salary last year (incl. zeros)
ELA * Ages 20-24	*7.88*	-0.200 (0.321)	-0.040 (0.026)	*9943*	-1,008 (724)	-0.161** (0.065)	*7660*	-1,046 (638)
ELA * Ages 25-29	*9.60*	-0.020 (0.340)	0.010 (0.027)	*15610*	473 (747)	0.114** (0.045)	*10911*	331 (719)
ELA * Ages 30-34	*10.62*	0.320 (0.340)	0.040 (0.029)	*18116*	1065 (717)	0.173*** (0.058)	*12452*	696 (669)
ELA * Ages 35-39	*11.74*	0.400 (0.342)	0.040 (0.028)	*21174*	1,957** (767)	0.164*** (0.053)	*15442*	1,333* (742)
ELA * Ages 40-44	*12.84*	0.615* (0.348)	0.055** (0.026)	*24493*	2,294** (952)	0.155*** (0.048)	*19184*	2,543*** (914)
ELA * Ages 45-49	*14.29*	0.969** (0.458)	0.081** (0.033)	*28148*	1,628** (804)	0.117** (0.048)	*25238*	3,260*** (943)
Observations		46388	46388		51277	51277		68169
Unique women		4210	4210		4245	4245		4351
R-squared		0.22	0.27		NA	NA		NA

See table 3. The estimates here include controls for Vietnam: 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.

Table C4C. Human Capital Accumulation and Occupational Upgrading: Vietnam Controls

	(1) Cumulative Experience in Hours	(2) 1= Enrolled in College	(3) Highest Grade Completed	(4) 1=Occupational training since last interview	(5) 1= in Professional Job	(6) 1=in Non-traditional Job
ELA * Age 20-24	-524*	0.066***	-0.010	-0.001	0.002	0.005
	(278)	(0.021)	(0.148)	(0.012)	(0.014)	(0.008)
ELA * Age 25-29	-1,025**	0.005	0.285**	0.021**	0.044**	0.016
	(406)	(0.008)	(0.136)	(0.010)	(0.019)	(0.011)
ELA * Age 30-34	-1,030**	-0.005	0.273**	0.026	0.063***	0.062***
	(463)	(0.013)	(0.135)	(0.016)	(0.020)	(0.015)
ELA * Age 35-39	216	-0.004	0.294**	0.014	0.042**	0.042**
	(398)	(0.011)	(0.134)	(0.017)	(0.021)	(0.018)
ELA * Age 40-44	781	-0.012	0.297**	0.022	0.042	0.033
	(561)	(0.010)	(0.131)	(0.020)	(0.025)	(0.020)
ELA * Age 45-49	2,222***	-0.010	0.230	-0.013	0.005	-0.006
	(792)	(0.006)	(0.147)	(0.019)	(0.019)	(0.017)
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

See table 4. The estimates here include controls for Vietnam: 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.

Table C5C. Heterogeneity in the Growth of Real Hourly Wages: Vietnam Controls

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-0.560	0.550	-0.460	-0.120	-0.850
	(0.652)	(0.585)	(0.443)	(0.305)	(0.535)
ELA * Age 25-29	-0.050	0.980	0.560	0.020	0.160
	(0.583)	(0.689)	(0.493)	(0.299)	(0.520)
ELA * Age 30-34	-0.985*	1.634**	0.760	0.010	0.760
	(0.531)	(0.768)	(0.695)	(0.313)	(0.592)
ELA * Age 35-39	-0.170	1.628**	0.580	-0.240	1.337*
	(0.648)	(0.803)	(0.615)	(0.410)	(0.672)
ELA * Age 40-44	-0.49	1.995**	0.830	0.490	1.345**
	(0.971)	(0.935)	(0.672)	(0.487)	(0.629)
ELA * Age 45-49	0.680	2.072**	3.101***	0.740	2.685***
	(1.047)	(0.927)	(1.058)	(0.475)	(0.903)
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

See table 5. The estimates here include controls for Vietnam: 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.

Table C6C. Heterogeneity in Highest Grade Completed: Vietnam Controls

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) Lower third SES distribution	(5) Middle third SES distribution	(6) Upper third SES distribution
ELA * Age 20-24	-0.487**	0.140	0.040	0.210	-0.290	0.080
	(0.213)	(0.221)	(0.205)	(0.147)	(0.216)	(0.340)
ELA * Age 25-29	-0.378*	0.290	0.369*	0.512***	-0.020	0.350
	(0.211)	(0.229)	(0.206)	(0.134)	(0.232)	(0.281)
ELA * Age 30-34	-0.396*	0.350	0.445**	0.470***	0.010	0.370
	(0.203)	(0.228)	(0.206)	(0.157)	(0.239)	(0.284)
ELA * Age 35-39	-0.364*	0.408*	0.524**	0.497***	0.080	0.350
	(0.197)	(0.221)	(0.206)	(0.158)	(0.247)	(0.300)
ELA * Age 40-44	-0.459**	0.425*	0.482**	0.495***	0.100	0.370
	(0.216)	(0.244)	(0.194)	(0.174)	(0.248)	(0.267)
ELA * Age 45-49	-0.330	0.300	0.611***	0.489***	0.030	0.290
	(0.242)	(0.244)	(0.207)	(0.180)	(0.261)	(0.291)
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

See table 6. The estimates here include controls for Vietnam: 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.

Table C7C. Heterogeneity in Cumulative Experience: Vietnam Controls

Sample	(1) Lower third of IQ distribution	(2) Middle third of IQ distribution	(3) Upper third of IQ distribution	(4) No College	(5) Some College
ELA * Age 20-24	-1,041	28	-808	-904	-1,458***
	(1,408)	(1,000)	(694)	(565)	(549)
ELA * Age 25-29	-1,127	174	-493	-766	-1,013*
	(1,446)	(1,049)	(677)	(580)	(609)
ELA * Age 30-34	-639	2,013*	804	101	1,076
	(1,187)	(1,152)	(841)	(471)	(772)
ELA * Age 35-39	-89	2,810**	1,542*	395	2,288**
	(1,358)	(1,392)	(920)	(715)	(922)
ELA * Age 40-44	67	4,599**	2,059**	2,143**	3,273***
	(1,746)	(1,787)	(1,018)	(874)	(1,073)
ELA * Age 45-49	-629	3,547	1,581	1,539	2,594*
	(2,214)	(2,307)	(1,247)	(1,094)	(1,355)
Observations	10778	14061	16995	40836	21942
Unique women	804	987	1133	2960	1487
R-Squared	0.61	0.64	0.68	0.58	0.70
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	27954	33133

See table 7. The estimates here include controls for Vietnam: 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.

Table C3D. Wages Rates and Annual Income: Balanced Panel

	Mean real hourly wages excl. zeros	(1) Real hourly wage (excl. zeros)	(2) Log real hourly wage	Mean real wages/salary last year excl. zeros	(3) Wage or salary last year (excl. zeros)	(4) Log real annual wage	Mean real wages/salary last year incl. zeros	(5) Wage or salary last year (incl. zeros)
ELA * Ages 20-24	8.04	-0.190 (0.531)	-0.030 (0.046)	10135	-784 (1,104)	-0.160** (0.078)	7952	-1,044 (996)
ELA * Ages 25-29	9.94	0.160 (0.562)	0.020 (0.044)	16436	1,483 (1,244)	0.184** (0.075)	11966	881 (1,121)
ELA * Ages 30-34	11.07	0.470 (0.558)	0.040 (0.047)	18840	1,278 (1,223)	0.130 (0.084)	13343	1434 (1,176)
ELA * Ages 35-39	12.22	0.500 (0.570)	0.050 (0.046)	21466	2,221* (1,349)	0.159** (0.075)	16136	1422 (1,199)
ELA * Ages 40-44	13.32	0.700 (0.579)	0.050 (0.043)	24965	2,119* (1,265)	0.090 (0.074)	20249	2,249* (1,207)
ELA * Ages 45-49	14.76	1.515** (0.706)	0.106** (0.050)	28809	2,129 (1,469)	0.070 (0.080)	26277	3,670*** (1,329)
Observations		20863	20863		23914	23914		30399
Unique women		1474	1474		1482	1482		1498
R-squared		0.23	0.28		NA	NA		NA

See table 3. The estimates here are based on a balanced panel (respondent must have relevant information in every survey wave).

Table C4D. Human Capital Accumulation and Occupational Upgrading: Balanced Panel

	(1) Cumulative Experience in Hours	(2) 1= Enrolled in College	(3) Highest Grade Completed	(4) 1=Occupational training since last interview	(5) 1= in Professional Job	(6) 1=in Non- traditional Job
ELA * Age 20-24	-515	0.055	0.170	0.025	0.023	0.033***
	(541)	(0.039)	(0.188)	(0.021)	(0.017)	(0.012)
ELA * Age 25-29	-943	0.019	0.453**	0.060**	0.078**	0.055***
	(592)	(0.015)	(0.200)	(0.024)	(0.031)	(0.019)
ELA * Age 30-34	-1034	-0.001	0.414**	0.060**	0.059*	0.074***
	(624)	(0.019)	(0.204)	(0.028)	(0.035)	(0.026)
ELA * Age 35-39	469	0.002	0.457**	0.045*	0.038	0.057**
	(593)	(0.013)	(0.205)	(0.026)	(0.034)	(0.027)
ELA * Age 40-44	1152	-0.007	0.388*	0.033	0.015	0.026
	(729)	(0.011)	(0.213)	(0.031)	(0.035)	(0.026)
ELA * Age 45-49	1,628*	-0.008	0.425**	0.009	-0.008	-0.009
	(920)	(0.008)	(0.194)	(0.031)	(0.032)	(0.027)
Observations	27209	26678	32955	28470	32770	32770
Unique women	1488	1340	1498	1498	1498	1498
(Pseudo) R-squared	0.62	0.17	0.18	0.03	0.07	0.09
Mean of DV for 14-19	1488	0.55	10.24	0.207	0.011	0.008
Mean of DV for 20-24	518	0.274	12.43	0.210	0.114	0.055
Mean of DV for 25-29	2747	0.087	12.93	0.208	0.203	0.091
Mean of DV for 30-34	6197	0.076	13.24	0.273	0.231	0.151
Mean of DV for 35-39	11300	0.070	13.37	0.313	0.271	0.220
Mean of DV for 40-44	17058	0.049	13.47	0.333	0.284	0.255
Mean of DV for 45-49	24322	0.028	13.60	0.349	0.265	0.238

See table 4. The estimates here are based on a balanced panel (respondent must have relevant information in every survey wave).

Table C3E. Wages Rates and Annual Income: High School State

	Mean real hourly wages excl. zeros	(1) Real hourly wage (excl. zeros)	(2) Log real hourly wage	Mean real wages/salary last year excl. zeros	(3) Wage or salary last year (excl. zeros)	(4) Log real annual wage	Mean real wages/salary last year incl. zeros	(5) Wage or salary last year (incl. zeros)
ELA * Ages 20-24	7.87	-0.430 (0.297)	-0.042* (0.023)	9938	-1,285** (614)	-0.120** (0.056)	7685	-1,489*** (552)
ELA * Ages 25-29	9.62	-0.592** (0.293)	-0.030 (0.024)	15615	-170 (627)	0.095** (0.042)	10899	-138 (557)
ELA * Ages 30-34	10.67	0.000 (0.269)	0.010 (0.023)	18266	333 (599)	0.099* (0.057)	12552	457 (564)
ELA * Ages 35-39	11.83	0.010 (0.280)	0.010 (0.021)	21258	910 (662)	0.091* (0.048)	15548	782 (659)
ELA * Ages 40-44	12.93	0.370 (0.246)	0.030 (0.018)	24558	1,636** (734)	0.098** (0.044)	19451	2,160** (862)
ELA * Ages 45-49	14.40	0.480 (0.398)	0.040 (0.029)	28389	1262 (794)	0.082* (0.044)	25615	3,194*** (867)
Observations		46671	46671		51718	51718		68723
Unique women		4367	4367		4427	4427		4577
R-squared		0.22	0.28		NA	NA		NA

See table 3. The estimates here use state of residence during high school (rather than at age 21) to identify ELA.

Table C4E. Human Capital Accumulation and Occupational Upgrading: High School State

	(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	-759**	0.026	-0.060	-0.001	ND	ND
	(299)	(0.019)	(0.110)	(0.013)		
ELA * Age 25-29	-980***	0.003	0.110	0.024*	0.035**	0.018*
	(346)	(0.008)	(0.109)	(0.013)	(0.017)	(0.010)
ELA * Age 30-34	331	ND	0.060	0.023	0.038*	0.052***
	(329)		(0.109)	(0.017)	(0.020)	(0.014)
ELA * Age 35-39	946*	0.002	0.100	ND	0.011	0.020
	(532)	(0.009)	(0.107)		(0.019)	(0.015)
ELA * Age 40-44	2,361***	ND	0.080	ND	0.011	ND
	(752)		(0.110)		(0.023)	
ELA * Age 45-49	1168	ND	0.030	ND	-0.014	-0.018
	(905)		(0.116)		(0.018)	(0.016)
Observations	57844	57881	79446	62932	7475	74275
Unique women	4048	3823	4582	4446	4582	4582
(Pseudo) R-squared	0.62	0.15	0.16	0.03	0.07	0.09
Mean of DV for 14-19	505	0.49	10.14	0.22	0.01	0.01
Mean of DV for 20-24	2751	0.25	12.14	0.21	0.09	0.05
Mean of DV for 25-29	6023	0.08	12.61	0.19	0.17	0.08
Mean of DV for 30-34	10898	0.07	12.92	0.25	0.20	0.14
Mean of DV for 35-39	16270	0.06	13.07	0.29	0.24	0.20
Mean of DV for 40-44	22851	0.05	13.21	0.31	0.26	0.23
Mean of DV for 45-49	30210	0.03	13.35	0.32	0.25	0.22

See table 4. The estimates here use state of residence during high school (rather than at age 21) to identify ELA. "ND" indicates estimate did not meet requirements for disclosure.

Table C3F Wages Rates and Annual Income for Men, CPS

	Mean real hourly wages excl. zeros	(1) Real hourly wage (excl. zeros)	(2) Log real hourly wage	*Mean real wages/salary last year excl. zeros*	(3) Wage or salary last year (excl. zeros)	(4) Log real annual wage	*Mean real wages/salary last year incl. zeros*	(5) Wage or salary last year (incl. zeros)
ELA * Ages 20-24	*12.73*	-0.06 (0.31)	-0.02 (0.019)	*16521*	-62 (823)	-0.05 (0.033)	*15014*	-491 (932)
ELA * Ages 25-29	*14.94*	0.14 (0.24)	-0.00 (0.012)	*28651*	-46 (730)	-0.03 (0.026)	*26252*	-447 (836)
ELA * Ages 30-34	*16.63*	0.12 (0.25)	0.00 (0.023)	*34737*	92 (654)	-0.02 (0.017)	*30817*	-408 (750)
ELA * Ages 35-39	*18.48*	0.05 (0.26)	0.00 (0.021)	*40257*	-42 (593)	0.00 (0.016)	*34762*	-271 (591)
ELA * Ages 40-44	*19.39*	-0.16 (0.22)	0.00 (0.018)	*43819*	-287 (679)	0.01 (0.017)	*37055*	-420 (672)
ELA * Ages 45-49	*20.31*	0.28 (0.19)	0.02** (0.009)	*49596*	363 (710)	0.024* (0.014)	*41106*	622 (763)
Observations		368,358	368,358		396,624	396,624		471,527
R-squared		0.09	0.10		0.12	0.15		0.06

See table 3. The estimates here are for men of the same birth cohorts as the *NLS-YW*, using the 1968 through 2003 waves of the March CPS with restricted exact state identifiers. (Public use versions of the March CPS contain state groups for some states from 1968 to 1976.) The specification used is a variant of (1') that also includes year of observation fixed effects. Hourly wages are constructed by dividing wage earnings of the previous year by the product of weeks worked last year and hours worked last week.