The Opt-*In* Revolution: Contraception, Women's Labor Supply and the Gender Gap in Wages

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Abstract:
This paper uses the *NLS* and *CPS* to document the remarkable changes in lifecycle wages for women born from the 1920s to the 1950s. Using birth-cohort by state-of-residence variation in access to "the Pill" by age 21, our results show that women with earlier access to the Pill earned *lower* wages in their twenties as they invested in human capital but 8 percent more than their peers by age fifty. "Opting-*in*" with the Pill accounted for 1/3 of the wage gains between the 1943 and 1951 cohorts and 10 percent of the narrowing of the gender gap over the 1980s.

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I. INTRODUCTION

During the late 1970s and early 1980s, the long-standing gender gap in U.S. wages narrowed rapidly. The median annual earnings of women working full-time, full-year rose from roughly 60 percent of male earnings in 1979 to 69 percent in 1989. Although the gender gap in wages has continued to narrow, it has slowed since the 1980s. With increasing historical perspective, the speed of convergence of the 1980s appears increasingly exceptional.

The correlates of rapid convergence in the 1980s are well documented: the 1980s witnessed a narrowing of differences in measured labor market skills between men and women, especially work experience (O'Neill and Polachek 1993, Wellington 1993). Increases in demand for skill that benefited women relative to men increased the returns to women's investments in market skills (Blau and Kahn 1997, Welch 2000) and provided incentives for women to continue their career investments and remain in the labor market. Widening earnings inequality among women may have also encouraged women to invest in market skills and led the more able women to select into full-time employment (Mulligan and Rubinstein 2008).

To some, it may seem obvious that convergence in the wage gap occurred in the aftermath of the 1970s. The resurgence of the women's movement in the late 1960s and early 1970s changed attitudes and norms about women's employment. The growth in co-education granted women access to new educational opportunities at historically male colleges and universities. New legal protections afforded to women under the 1964 Civil Rights Act (and later federal enforcement) may have reduced overtly-discriminatory hiring and compensation practices. Expecting to remain in the labor-force longer, cohorts born in the 1950s (who came of age in the 1970s) narrowed the gender gap in college going and completion, attained more professional degrees, and entered non-traditionally female occupations (Goldin 2004, 2006). What is less obvious from the aggregate trends or the history is that patterns in women's labor market investments and employment changed noticeably among women born more than a decade earlier.

This article begins by describing the tremendous life-cycle wage gains that began among cohorts born in the late 1930s and early 1940s. Using the *National Longitudinal Surveys of Mature Women* (*NLS-MW*), *Young Women* (*NLS-YW*), and the March *Current Population Surveys* (*CPS*), we show that women born during this period invested more in their careers from early ages and worked substantially more over their lifetimes than their predecessors. The bulk of these women worked in "women's jobs" as secretaries, nurses and teachers, and these cohorts narrowed the wage gap by accumulating greater labor-force experience in these professions. Around age forty, the annual earnings of working women born in the mid-1940s exceeded those of women born a decade earlier by forty percent. This is more than three times the speed of earnings growth relative to the previous decade's cohorts, and it is twice the speed of earnings growth of women born in the 1950s.

This article then quantifies the importance of shifts of women's labor supply induced by the birth control pill, or "the Pill." This focus is both for substantive and practical reasons. The diffusion of the "Pill" over the 1960s makes it a strong candidate for explaining the acceleration in the labor-market investments of cohorts born in the 1940s. And, unlike many other candidate explanations, an established empirical strategy for quantifying the Pill's impact exists (Goldin and Katz 2002, Bailey 2006). Following these studies, we leverage variation in the age of consent during the 1960s and 1970s to estimate the impact of *early access to the Pill* on women's wages—and the mechanisms for these wage gains—across the lifecycle in the *NLS-YW*.

Our central results show that women with access to the Pill before age 21 earned *lower* wages in their twenties but statistically-significant hourly and annual premia of 8 percent in their late forties. These magnitudes imply that *early access to the Pill* accounted for roughly 27 to 37 percent of the annual and 33 to 46 percent of the hourly wage gains for the cohorts of the late 1940s relative to those born a decade earlier. These gains were largest for women with average IQ scores and who attended college. We then link these Pill-induced wage gains directly to measures of women's human capital investments, occupational choices, accumulation of labor-market experience, and marital status (as it relates to nonwage income), employing the decomposition methodology of DiNardo, Fortin and Lemieux (1996) to quantify the contribution of each. Roughly 60 percent of the Pill-induced, log hourly wage premium at the mean can be attributed to increases in women's labor-force experience, and another 33 percent is due to changes in educational attainment and occupational choice. Despite the rapidly rising divorce rate over this period, we find no evidence that the Pill premium was due to changes in current marital status.

As a final exercise, we simulate a counterfactual wage distribution by subtracting out the age-specific gains associated with early access to the Pill in the 1980, 1990, and 2000 censuses. Without early access to the Pill, the magnitudes of our estimates imply that the convergence in the gender gap in annual earnings among 25 to 49 year olds would have been 10 percent smaller in the 1980s and 30 percent smaller in the 1990s. In short, the narrowing of the gender gap in wages in the 1980s and 1990s reflected large labor-supply shifts from women investing and opting *into* paid employment—an Opt-*In* Revolution that began in the 1960s.

II. THE REVOLUTION

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 remained constant at roughly 60 percent (Blau, Ferber and Winkler 2010: figure 51). Beginning in the late 1970s, the gender gap in wages began to narrow. The pace of this narrowing has slowed since the 1980s but narrowing has continued to the present.

Because the 1970s were a remarkable decade for women's rights and challenges to traditional gender roles, the fact that the gender gap in wages began to close in that period may seem like a foregone conclusion. What is less obvious from the period histories is that patterns in women's employment began to change with women born around World War II. Using the rich set of labor-force (labor-force participation, wages, human capital investments) and family outcomes with comparable definitions across years in the 1964 to 2009 *March Current Population Surveys* (*CPS*), we describe the life-cycle evolution of women's compensation and link it to changes in their productive characteristics.[1] We supplement this description with unique features of the *National Longitudinal Surveys of Mature* (*NLS-MW*) and *Young Women* (*NLS-YW*) when this information is unavailable in the *CPS*.

As a starting point, figure 1 plots the evolution of real wage earnings profiles for seven different cohorts of women: women 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). We use these cohort groupings throughout the analysis, so that the *NLS-MW* (sample of 1922 to 1937 cohorts) and the *NLS-YW* (sample of 1943 to 1954 cohorts) can be divided into three roughly equal-sized groups. For most of the outcomes, we use the March *CPS* because we can also present the 1938 to 1942 cohort. Altering these groupings does not change the substantive conclusions of this descriptive exercise. Another important detail is that wage earnings are inflated using the PCE deflator to 2000 dollars throughout the analysis.

Figure 1 plots real annual wages for women with positive earnings (panel A), annual wages including zeros to measure the changes in the average woman's earnings (as opposed to the average working woman's earnings) (panel B), and real hourly wages (for working women) at each age (panel C). For each measure of labor earnings, the series fall into two distinct groups. Women born before the 1940s have relatively similar earnings profiles; they are lower in levels at each age and increase more slowly with age. Beginning with women born in the early 1940s, the earnings profiles are higher at every age and have an increasingly steeper age-gradient.

Among women with any earnings (panel A), the acceleration in the speed of wage earnings growth at age 50 begins with women born in the 1938 to 1942 cohort: the annual incomes of 50-year-olds had increased by 35 percent over the cohort born four years earlier. The acceleration continued for

[1] The trends in the *CPS* are virtually identical to those in the *National Longitudinal Surveys of Mature* (*NLS-MW*) and *Young Women* (*NLS-YW*), but the *CPS* measures are less noisy owing to larger samples.

women born in the mid-1940s, who achieved gains of $6400 per year at age 50 over women born from 1938 to 1942—twice the rate of growth over the previous decade. The change in annual earnings from women born in the 1930s to the 1940s and 1950s is equally dramatic if non-workers are included in the averages (panel B): while earnings among women at age 50 increased by 36 percent between cohorts born from 1922 and 1927 to cohorts born from 1933 to 1937, earnings increased by nearly 50 percent for women born in the mid-1940s over those born a decade earlier. These changes are also reflected in real *hourly* earnings as well (panel C). Women born in the mid-1930s increased their real hourly pay by roughly 1.10 dollars during their early fifties over the cohorts born the decade before (1922 to 1927). But for women born from 1943 to 1946, hourly earnings had increased by 3.1 dollars per hour over cohorts born from 1933 to 1937—almost three times the increase over the previous decade. As we discuss below, these remarkable changes in earnings represent tremendous increases in women's *pre-market* and *post-entry* investments in their jobs.

An important contributor to women's wage growth over this period was increases in labor-market experience (O'Neill and Polacheck 1993, Wellington 1993). As shown in panel A of figure 2, women's labor-force participation increased from roughly 39 percent at age 30 for women born in the 1930s to 55 percent for women born just one decade later; this statistic increased another 14 percentage points over the next decade. Increases in the labor-force participation rate at age 40 were more concentrated among the older cohorts: the rate increased by 14 percentage points between women born in the 1930s and 1940s, but by only 4.5 percentage points between those born in the mid-1940s and 1950s.[2] These increases in labor-force participation translated into considerably more work experience. Using information from the *NLS-YW*, panel B shows that, by age 40, the cumulative hours worked since age 24 among women born in the early 1950s was about 3000 hours—or 1.5 full-time, 50-week years—

[2] The NLS figures correspond closely to the March *CPS* but are slightly larger for labor-force participation. These increases according to the *NLS* are 20 and 10 percentage points, respectively.

greater than women born in the mid-1940s.[3] Panel C shows that increases in women's educational attainment, measured by the highest completed grade, also started to accelerate between cohorts born in the mid-1930s and mid-1940s. Mean attainment at age 30 grew by about a year between cohorts born from 1933 to 1937 to those born from 1943 to 1946. Mean attainment at age 30 continued to increase by nearly half a year between cohorts born in the mid-1940s and early-1950s. These increases appear even more dramatic when compared to the slow-down in educational attainment among men (Goldin, Katz, Kuziemko 2006).

Another important contributor to women's wage growth was their likelihood of working in more prestigious and higher-earning occupations. Panel A of figure 3 shows that 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 it was for cohorts born a decade earlier. These changes continued for the cohorts born in the 1950s but the relative pace of growth slowed. Furthermore, panel B shows that some of these gains were in non-traditionally female occupations, which we define as jobs other than nursing and teaching.

Changes in family structure (spousal income) and the average "ability" of working women may have also contributed to women's wage growth. Because spousal income is an important determinant of women's labor supply that is often poorly measured (especially in the *NLS-YW* used later in the analysis), we use marital status as an alternative measure. Panel A of figure 4 plots changes in the

[3] It would be interesting to compare this 18 percent increase in actual work experience to older cohorts, but these experience measures are not available for the *Mature Women*, who were first interviewed between the ages of 30 and 45. Our measure of cumulative work hours is an approximation based on reported hours of work starting in 1967. We compute work hours for each year covered by *NLS-YW* survey questions by taking the product of weeks worked and usual work hours per week. We impute weeks worked for time periods that are not covered by any survey questions using the average share of weeks worked during reported periods. The figure shows cumulative hours since age 24 (the earliest age at which we observe the oldest women) to enable comparisons from a common base. See the data appendix for details.

proportion of the cohort that is currently married by age. The overall downward trend in currently married after age 30 is due to widowing and divorce. As in figures 1 and 2, shifts in marital status show up for cohorts born after 1940. The proportion of women who were married at age 30 began falling rapidly for cohorts born after 1940, and these cohort gaps narrowed but did not disappear as the women aged. At age 30, the difference in marriage rates between women born in the mid-1930s and the mid-1940s was roughly 9 percentage points (87 percent versus 78 percent); by age 40, this difference was 7 percentage points, and by age 50, it was still 3 percentage points. This divergence in marital status may have had an independent effect on women's earnings by increasing their labor supply (reduction in nonwage earnings) and thus labor-force experience and also altering their roles from secondary earners to bread-winners.

Changing "ability" among working women may have also influenced our estimates of women's wage growth. Panel B of figure 4 uses the *NLS-YW* to plot the mean IQ score (as a proxy for cognitive ability) by birth cohort and age.[4] Because IQ is measured only once, the positive age trends in the dashed lines for *all* respondents show that sample attrition affected aptitude measures positively. Changing selectivity into market work is reflected in the changing differences between all respondents (dashed lines) and working women (solid lines). For women aged 22 and older, each cohort shows evidence of positive selection into the labor market, as the solid lines uniformly fall above the dashed lines. For women born in the mid-1940s, positive selection appears larger and relatively constant across age groups. The difference between labor-force participants and all respondents falls in magnitude and with age for women born in the late 1940s. Interestingly, for cohorts born in the early 1950s, the IQs of working women and the average respondent are much closer by age 30, indicating that labor-force participants of these younger cohorts are *less positively selected on IQ* than earlier cohorts. Although

[4] "IQ" information was collected by the Census Bureau in a survey of high schools about *NLS-YW* respondents' most recent intelligence or aptitude tests. The Census Bureau then converted this information into a unified score that is nationally-normed to a mean of 100 and standard deviation of 15. Scores are available for 3,530 respondents, nearly all born before 1953. See data appendix.

these measures of selection cannot be compared to the *Mature Women* (for whom no aptitude data are available), these patterns suggest that the sizable changes in earnings may reflect increases in the measured "ability" of women workers (even though younger workers are *less* selected).

III. WAS THIS AN OPT-*IN* REVOLUTION? EVALUATING THE IMPORTANCE OF LABOR SUPPLY USING ACCESS TO THE PILL

A. Background Literature and Hypothesized Effects of the Pill

Women may have been pulled into the labor force with changes on the demand side 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 forthcoming). At the same time, rapidly changing ideas about women's work and roles in the workplace, shifts in divorce rates, and the availability of better colleges may have increased the supply of women's skills to the market (Fernandez, Fogli, and Olivetti 2004, Fernandez and Fogli 2009, and Fortin 2009). Changes in women's wage earnings and market skills may, therefore, reflect both demand and supply factors.

The diffusion of the birth control pill, first released for the regulation of menses in 1957 and approved by the U.S. Food and Drug Administration as a contraceptive in 1960, may be closely related to shifts in women's labor supply. The timing of the diffusion of the Pill corresponds closely to these shifts: women born in the early 1940s would have been the first with access to the Pill before marriage and within marriage during their twenties. With the Pill, these women gained exclusive control of contraception rather than sharing it with their partners; they were the first to make decisions about contraception at a time separate from intercourse; they were the first to benefit from the improved reliability of the Pill and the predictability it conferred for the entirety of their childbearing years. That is, women with the Pill could *expect* to time births better in order to avoid costly withdrawals from their education or the labor force later on. Not only might the Pill have influenced (1) childbearing and marriage decisions in the shorter-term, but it may have also affected (2) investments in human capital including schooling and occupational training, and (3) investments in market employment. Changes in childbearing and marriage may have also had independent effects on women's market employment by altering the division of labor within the household and, perhaps, by affecting the stability of marriages.

But how large should these effects be on wages? Attributing all of the Revolution to the Pill would almost certainly be an overstatement. However, structural and reduced-form economic models provide limited guidance as to the lifecycle effects of (1) through (3). Dynamic, structural models that jointly estimate human capital and fertility outcomes illustrate the complexity of these interactions, but they do not provide a means for gauging the impact of the Pill.[5] To do so would require estimates of how the Pill affected each of the three types of decisions above and how these investments influence the evolution of wages.

Quasi-experimental, reduced-form approaches that leverage variation in the number of children due to twins or the sex mix of children (Rosenzweig and Wolpin 1980, Bronars and Grogger 1994, Angrist and Evans 1998, Gangadharan and Rosenbloom 1996) likely understate the impact of the Pill. This is, in part, because delaying the initial transition to motherhood may have a larger impact than transitions to higher order births for existing mothers. Perhaps more importantly, the exogenous variation exploited in these papers, like the individual variation used in studies of motherhood delay (Geronimus and Korenman 1992, Hotz, McElroy and Sanders 1997, Klepinger, Lundberg, Plotnick 1999, Miller 2004), largely abstracts away from improvements in birth timing and reductions in uncertainty surrounding completed family size—arguably two of the biggest potential contributions of the Pill to women's career choices.

[5] Dynamic, structural models of labor supply, human capital investment, and home production typically simplify in one of the three dimensions. Keane and Wolpin (1997), for instance, omit the interaction of labor supply and human capital decisions with childbearing, because their focus is on men. Hotz and Miller (1988) focus on the dynamics of fertility choice, but treat women's pre-marriage investments in human capital as well as their wages as exogenous.

B. Early Legal Access to the Pill and the Potential Impact on Labor Supply

Our empirical approach to estimating the impact of the Pill on women's wages via labor supply extends Goldin and Katz (2002) and Bailey (2006). As in these studies, the key independent variable in our analysis is "earlier legal access to the Pill" (*ELA*), which varied by birth cohort and state of residence as laws changed to allow younger women to consent for medical care.[6] Most of these legal changes were due either to judicial expansions in the rights of legal minors or to legislative changes in the definition of legal "minority." Although these changes occurred in different branches and levels of government, they all gave physicians latitude to prescribe oral contraception for young, unmarried women without consulting their parents (Paul, Pilpel, and Wechsler 1974). Variation in *ELA* facilitates comparisons of women born from 1940 to 1956 who typically gained legal access to the Pill by their 18th birthdays to women who gained access at age 21.

This three-year difference in access to the Pill may seem small, but *ELA* affected the cost of delaying childbearing and marriage at a time crucial to career investment. Having access to the Pill, for instance, directly reduced the cost of delaying childbearing to try or stay in college. It may have also affected women's decisions about going to or remaining in college by altering their expectations about finishing. Because obtaining at least some college, and especially finishing, raised the returns to working for pay, women would also be induced into working more at younger ages, which in turn increased labor market experience and amplified the effects of *ELA* on wages over time. Even among women who did not attend college, better fertility control may have allowed women to stay at a job long enough to obtain a promotion or additional training, which should have reinforced the effects described above. For each of these reasons, early access to the Pill could lead to greater labor market investments well past age 21.

[6] Goldin and Katz (2002) use this variation to link the Pill to the age at first marriage and college women's career choices. Bailey (2006) uses a similar empirical strategy to relate the Pill to the age at first birth and women's life-cycle labor-force participation.

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

Finally, early legal access to the Pill might produce general equilibrium effects in marriage and labor markets. In the marriage market, one woman delaying marriage may reduce the costs of other women delaying marriage, thus amplifying the effects of *ELA* within and across cohorts (see Goldin and Katz 2002). In the labor market, there may be positive spillovers, even for women who do not increase their human capital investments, if changes in some women's labor-force attachment cause employers to update their beliefs about women workers favorably. On the other hand, the general equilibrium effects may be negative if the large expansion of female labor supply leads to lower wages, or the increased supply of college-educated workers dampens the acceleration in the returns to skill.

In summary, changes in early human capital investment, through formal schooling or market employment, may lead to temporary or permanent effects on wages. The effects of *ELA* operate both immediately and through the accumulated effects of past decisions. They may also be amplified (or weakened) through multipliers that affect women in the marriage and labor markets.

C. Data and Empirical Strategy

The analysis uses the rich, longitudinal data of the *National Longitudinal Survey of Young Women* (*NLS-YW*). This dataset is ideal, because it contains interviews beginning in 1968 for 5,159 women, ages 14 to 24, with 21 subsequent interviews. Crucial is that the *NLS-YW* sampled women born from 1943 to 1954, cohorts that varied in their legal access to the Pill. Although this dataset is smaller than those used in earlier studies (*CPS* and Census), the restricted version of the *NLS-YW* contains information on the legal state of residence for the respondents in each year they were interviewed. Observing residence at age 21 allows us to infer treatment status with considerably less error.[7]

The *NLS-YW* confers several additional advantages. One is that the *NLS-YW* contains a rich set of pre-treatment outcomes for testing the validity of our empirical strategy; it also contains information on age at first marriage and first birth to examine hypothesized mechanisms linking the Pill to career outcomes. A second advantage is that these data facilitate an analysis of heterogeneity in the impact of the Pill by socio-economic status and "cognitive ability" of the respondent. This allows us to understand the way in which the Pill may have impacted the selection of women into paid work.[8] One final advantage is that the *NLS-YW* provides information on wage earnings in every survey year, as well as information on women's career investments at earlier ages, including educational attainment, job training and certification, and labor-force participation (weeks and hours). With this information, we construct measures of cumulative labor-force experience, which is arguably one of the most important factors increasing women's wages over the 1980s (O'Neill and Polacheck 1993, Wellington 1993).

The empirical strategy used in the analysis follows the spirit of Bailey (2006) with several modifications. As in Bailey (2006), we estimate linear regression models for continuous dependent variables that take the following form,

[7] When we restrict the sample to those with valid date of birth (cohort) and state of residence information, our sample falls to 4354. This is particularly important in both Goldin and Katz (2002) and Bailey (2006), as the data were repeated cross-sections which contained 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 respectively *at the time of the survey*. This introduced considerable measurement error in the *ELA* variable that attenuated the estimates—especially at older ages.

[8] A description of the survey questions and more information on the coding of each variable used in the analysis can be found in the data appendix.

(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, …, 1953 (also referred to as "birth cohort"), and residing in state *s* = 1, 2, …, 51 at age 21.[9] 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 $D_{g(a)}$=1 are set to 1 if the respondent's age fell into the five-year age group, *g* (14-19, 20-24, …, or 45-49). Standard errors for all models are robust to heteroskedasticity and clustered at the state level.[10]

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. The interactions of *ELA* with the age-group dummy variables allow its effect to vary across the lifecycle. Therefore, the key parameters of interest, the β_g terms, measure differences in the outcome of interest in age group *g* between women with and without early legal access to the Pill. It is also worth noting that β_g will understate the impact of the Pill for three reasons: local compliance and enforcement were imperfect; many young women could not have afforded the Pill even when it was legal; and young women may have driven across state lines to obtain it.

[9] The *NLS-YW* collected information on women born from 1943 to 1953, so the analysis is limited to these cohorts. Bailey (2006) exploits variation in *ELA* for women born from 1940 to 1956.

[10] When the dependent variable is dichotomous, we estimate probits and report average partial effects (APEs), APE(g) $= \frac{1}{N}\sum_{i=1}^{N} \Phi\left(\hat{\beta}_g + \hat{\lambda}_g + \sum_s \hat{\lambda}_s D_s + \sum_c \hat{\lambda}_c D_c\right) - \Phi\left(\hat{\lambda}_g + \sum_s \hat{\lambda}_s D_s + \sum_c \hat{\lambda}_c D_c\right)$. The standard errors for the APEs are calculated using a non-parametric bootstrap method with states as clusters (1000 repetitions).

The main modification to the empirical strategy in Bailey (2006) is that we rely upon a revised legal coding (Bailey and Guldi 2009).[11] This updated legal coding reduces measurement error in our key independent variable 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 marriage and motherhood timing (tests of their relevance) and subjects them to validity checks using detailed information on pre-treatment characteristics.

IV. Testing the Relevance and Validity of using ELA to Identify the Impact of the Pill

One important assumption required to obtain consistent estimates of β is that *ELA* is uncorrelated with the error term after conditioning on state, age and birth-cohort fixed effects. Unlike previous studies, the *NLS-YW* contain rich pre-treatment characteristics, X_{ics}, that allow us to test this assumption. Using pre-treatment characteristics as dependent variables, we estimate the following specification:

$$X_{ics} = \gamma ELA_{cs} + \sum_s \lambda_s D_s + \sum_c \lambda_c D_c + \varepsilon_{ics}. \tag{2}$$

Thus, γ measures correlations between *ELA* and observable 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 random assignment of individuals to treatment status (early legal access to the Pill). Although the power of this test is limited by our small sample sizes, it is the strongest test of the validity of the empirical strategy permitted by the *NLS-YW*.

Table 1 reports the results of this exercise for the following outcome variables described in more detail in the data appendix: 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

[11] This paper reconciles and tests alternative legal coding in Goldin and Katz (2002), Bailey (2006), Guldi (2008), and Hock (2008).

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 in 1968; 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 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 1968. Each column represents a separate, least-squares regression estimate of the partial correlation of γ.[12] Consistent with treating *ELA* as a quasi-experiment, only one of the 18 point estimates is statistically significant at the ten percent level. It is also reassuring that the pattern of correlations suggests no consistent relationship between *ELA* and the pre-treatment characteristics. *ELA* is negatively associated with father's employment and with family socio-economic status, but is positively associated with mother's education and professional employment.[13]

Testing the relevance of *ELA* for women's use of the Pill is more difficult, because the *NLS-YW* contain no information on young women's contraceptive decisions. For this reason, quantitative

[12] 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.

[13] These estimates do not appear to reflect systematic non-response by *ELA* status. We test this by estimating equation (2) with a binary dependent variable of whether a woman was missing information for each of the variables in table 1. Three variables have marginally significant (at the 10 percent level) partial correlations: whether the father held a professional job, the father's occupational prestige score, and the number of siblings. The first two of these indicators are highly correlated, as they are both created using information on the occupation of the father. The fraction of women who did not report their number of siblings is less than 1 percent, so any partial correlation is unlikely to play a meaningful role in our estimates.

evidence of associated delays in first marriage and first birth is used. The idea is that marriage and childbearing are the mechanisms through which the Pill affected women's labor supply. Consistent with these laws affecting contraceptive access, table 2 provides evidence that earlier access to the Pill is positively and significantly related to marital and fertility delay. Column 1 presents least-squares estimates using the specification in equation 2 using the age at first marriage (panel A) and age at first birth (panel B) as the outcome variables. Columns 2 to 6 present mean marginal effects for a probit specification for a sequence of binary dependent variables: age at first marriage/birth before age 19 (column 2), age at first marriage/birth before age 20 (column 3), etc. Although *ELA* was not significantly related to pre-treatment variables in table 1, panel A shows large and statistically significant reductions in the number of marriages before age 22. Column 2 shows a 6.4 percentage point, or 24 percent reduction, in the number of women getting married before age 19; column 3 a 5.9 percentage point, or 15 percent, reduction in the likelihood of marriage before age 20; column 4 a 2 percentage point, or 4 percent, reduction in the likelihood of marriage before age 21; and column 5 a 1.8 percentage point, or 3 percent, reduction in the likelihood of marriage before age 22. By age 23 (columns 6 and 7), the difference in marriage timing by *ELA* status is economically and statistically insignificant. In summary, these delays show that the mean age of first marriage was .42 years higher among women with *ELA* (column 1).

Differences in the timing of motherhood are also evident, although less pronounced, in panel B. One reason for this lack of precision may be that fertility histories are difficult to construct in the *NLS-YW*.[14] As in panel A, the effects of *ELA* on the age-specific hazard of a first birth are most concentrated among women aged 19 and 20. Column 2 shows a 2.3 percentage point, or 11 percent reduction, in first

[14] There is little we can do about this problem. The first fertility questions were asked in 1973 about all children born up to that point, and follow-up fertility history questions were asked in 1978, 1983, 1985, 1987, 1988, 1991 about children born since the last fertility interview. Consequently, complete histories are missing for women who miss the 1973 interview (about 500 women), and the missing data problem grows as women miss the later fertility interviews.

births before age 19; column 3 shows a 3.4 percentage point, or 12 percent, reduction in the likelihood of a birth before age 20; and column 4 shows a 2.2 percentage point, or 6 percent, reduction in the likelihood of a birth before age 21. These effects are slightly more persistent than the marriage estimates. Column 6 registers a 2.1 percentage point, or 4 percent, reduction in the likelihood of childbirth before age 23. Column 1 summarizes these results showing that women with *ELA* had their first birth approximately a quarter of a year later.

Thus, laws affecting the legal age of consent for birth control are closely linked to the age of first marriage and the age of first birth. These changes may have affected women's initial human capital investments through the mechanisms of marriage and motherhood timing or through their expectations and planning of their careers after age 21. Moreover, changes in laws affecting the legal age of consent for birth control appear uncorrelated with a wide number of pre-treatment characteristics, which supports the credibility of this paper's empirical strategy.

V. EVIDENCE OF PILL-INDUCED OPTING-*IN*

A. Did the Pill Increase Women's Wage Earnings?

Table 3 begins by presenting the effect of *ELA* on women's wage earnings. Equation (1) is estimated using least squares for four dependent variables, which capture different dimensions of changes in women's wage earnings.[15] Columns 1 and 2 present the estimates for real hourly wages rate among working women and columns 3 to 5 present estimates for wage or salary earnings in the previous year. The regressions are estimated in levels and logs, and heterskedasticity-robust standard errors, clustered at the state level, are reported beneath each estimate. The results show that women with *ELA* experienced more sharply increasing hourly and annual wages after the mid-twenties. Although working women with *ELA* earned 3 percent less in hourly terms (columns 1 and 2) and 10 percent less

[15] Two definitional changes occur in 1995. From 1995 through 2003, the hourly rate of pay variable is asked for the first (main) job, and annual wage and salary earnings are for the previous 12 months rather than the previous calendar year. We have verified that that the reported results hold with the inclusion of post-1994, wave-specific dummies.

on an annual basis (columns 3 and 4) at ages 20 to 24, they earned a statistically-significant, hourly premium of 6 percent and an annual premium of 10 percent by ages 40 to 44. This translates into an 80 cent hourly premium and, roughly, a 2,300 dollar annual premium. This annual amount is substantially larger than the 1600 dollars that the hourly increase would imply for a full-time full-year worker, suggesting that *ELA* also increased the participation at the intensive margin, through the number of weeks worked per year and hours worked per week.[16] Column 5 shows that including women who did not work as zeros increases the *ELA* annual earnings premium to roughly 2,800 dollars per year. The change in the premium from columns (3) to (5) for women in their early twenties and in their forties suggests that differences in women's labor-force involvement are an important determinant of these wage differentials. The larger estimated effect of *ELA* among working women in their thirties is consistent with *ELA decreasing* labor force participation rates during the early twenties and increasing them later in life.

But differences in work intensity do not appear to be the only reason for earnings differences. Columns 1 and 2 show that hourly earnings for women were higher in their forties but lower in their twenties. These patterns are consistent with *ELA* inducing different investments in human capital through formal or informal channels such as labor-market experience, on-the-job training or certifications, and educational investment in the form of more years of schooling or different choices for educational specialization. This is consistent with the importance of greater college and nontraditionally-female, professional schooling (Goldin and Katz 2002) [17]

[16] The annualized value of the hourly premium can differ from the annual wage and salary earnings, 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.

[17] This is the typical prediction for investment in general human capital, when workers purchase their training with lower wages. If some of the training is firm-specific or otherwise not transferable across

Section B below explores changes in women's human capital investments. Section C explores two other potential mechanisms for these patterns. The first mechanism is that the Pill reduced marriage stability. For instance, women with greater human capital investments early on may have greater bargaining power and better outside options at later ages. Increases in wage rates (and work intensity) at later ages are consistent with a reduction in nonwage income due to divorce. The second mechanism is through the Pill changing the composition of the labor force—the unobserved characteristics of working women.[18]

B. Did the Pill Increase Wages through Greater Human Capital Investments?

The results in this section show that formal and informal educational investments were higher among women with early access to the Pill. Table 4a presents three measures of formal career investments. College enrollment was 4.7 percentage points, or 20 percent, higher for women with *ELA* in their early twenties but not at later ages (column 1). The advantage in grades completed (column 2) peaks among women with *ELA* in their late twenties, at one third of a year. This difference erodes a bit as women without *ELA* returned to school in their thirties. A difference of one quarter of a year, however, persists until age 50. Increases in occupational training (column 3) complemented investments in general human capital after the early twenties. Occupational training among women not in a formal degree program was roughly 3 percentage points, or about 15 percent, higher among women with *ELA* at ages 25 to 34.[19]

Table 4b also shows that *ELA* is associated with different occupational choice. The four columns represent binary variables for four different occupational groupings (described in the data

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jobs, wage growth will be lower and employers will pay for training. By increasing women's labor force attachment, *ELA* may increase employers' willingness to provide training.

[18] Mulligan and Rubinstein (2008) argue that selection into full-time full-year work for women changed from negative in the 1970s to positive in the 1990s.

[19] This variable is coded as a one for respondents who reported any on-the-job training or other occupational or vocational training since the last interview. See data appendix.

appendix). Women with *ELA* were 4 to 6 percentage points (17 to 30 percent) more likely to be working in a professional or managerial job during their late twenties and throughout their thirties. The estimates in column 2 show that half of this increase in the late twenties, and all of it during the thirties, was due to gains in working in non-traditionally female professional occupations—jobs other than nurse or teacher. The last two columns of the table show that *ELA* also reduced the likelihood that a woman would work in a clerical or sales job, especially as a teenager or during her forties, without substantially affecting the likelihood of working in other types of jobs.[20]

A second channel through which *ELA* may influence wages is through the informal acquisition of human capital from greater accumulation of labor-market experience. Table 5 examines the impact of *ELA* on the accumulation of work experience using three measures of participation: current labor market participation at the extensive margin (1=in the labor force, column 1),[21] at the intensive margin using "usual weekly hours" (column 2 excludes those not working), and cumulative labor market experience (column 3, the sum of weeks worked multiplied by usual weekly hours across survey waves). Consistent with younger women with *ELA* investing more in formal schooling, *ELA* is associated with a 4.6 percentage point, or 11 percent, reduction in work at the extensive margin and a 4 hour, or 17 percent, reduction in usual hours worked among teens. By contrast, for women in their late 20s and 30s with *ELA*, early access to the Pill is associated with greater labor-force participation and hours worked at these ages. This increase is consistent with *ELA* increasing the opportunity cost of remaining at home for older women. The effect of *ELA* on the extensive margin (column 1) is positive

[20] Using the Duncan index of occupational prestige as a continuous measure of occupational choice can capture additional variation both within and between occupational categories. The point estimates using this measure as a dependent variable are consistent with those shown in the table.

[21] Because this variable is based on whether the respondent reported working during the month the survey wave was conducted, which varies across respondent and survey waves, we also ran a specification for column 1 with month fixed effects (not shown) as a robustness check. This specification did not affect the estimates.

as well as economically and statistically significant—a 4 percentage point, or 6 percent, increase at ages 25 to 34. Similarly, the effect of *ELA* on the usual hours worked among working women (column 2) is also positive and economically and statistically significant for women older than 24. 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. These increases at the extensive and intensive margins translate into substantially greater cumulative experience by women's thirties (column 3). Owing to their greater formal human capital investments, women with *ELA* had worked 17 percent fewer hours than their peers by their late twenties. Women with *ELA* erased this disadvantage and accumulated 3 percent more hours of experience by their early thirties. As their cumulative hours of experience grew faster through their thirties, women with *ELA* amassed more than 10 percent more hours than their counterparts.

C. Did the Pill Increase Wages by Changing Selection on Ability and Non-wage Income?

One interpretation of the results in the previous two sections is that the technology of the Pill altered the costs and benefits associated with different life decisions and enabled women to improve their wages later in life. By increasing the expected benefits and reducing the expected costs of deferring childbearing and marriage, *ELA* facilitated greater formal and informal career investments. In this view, Pill-induced improvements in motherhood timing enabled better coordination of family and work and allowed women to "opt in" to more remunerative careers.

But the results lend themselves to alternative interpretations as well. First, *ELA* may have affected the ability composition of working women. If women with higher ability disproportionately took advantage of the Pill, then greater educational achievement and labor-market experience may be complemented by higher returns to these characteristics. Second, *ELA* could have affected women's bargaining power before marriage and within marriage by reducing early childbearing and increasing their human capital investments. If increases in women's options outside of marriage increased the likelihood of remaining single or divorcing, then women with *ELA* would have been more likely to be single earners. As a result, differences in their spouses' non-wage incomes may independently link *ELA* to women's differential labor-force attachment and earnings growth. These explanations are not mutually exclusive, but they have different implications for understanding how the Opt-*In* Revolution influenced women's well-being.

We explore both mechanisms using respondents' performances on IQ tests from their high schools and reported to the *NLS-YW* in 1968 (available for two-thirds of our sample) and information on their marital status at each interview. To maintain samples sizes large enough to permit disclosure of the estimates, respondents are divided into tertiles: low IQ, middle IQ and high IQ.[22] In separate exercises, we also examine heterogeneous effects of *ELA* by educational attainment and family background. For attainment, we split the sample into women with some college experience and those with none. We use three groups for family background of the respondent: those with low, middle and high socio-economic status parents. Due to space constraints, we only report results for a parsimonious set of specifications and report others in footnotes.

Table 6 breaks down the effects of *ELA* on real wages by both IQ and education group. Using the same dependent variable and specification as column 1 of table 3, the effects of *ELA* are negative (though generally statistically insignificant) for the low IQ group (column 1) but positive in the middle and upper third of the IQ distribution (columns 2 and 3) for women aged 30 to 49. This absence of wage effects for the group of lower IQ women may reflect their lower returns to investing in their careers and, therefore, lower value from using the Pill to delay motherhood. Interestingly, the effects of *ELA* are largest, absolutely and proportionally, for women in the middle of the IQ distribution. For this group, women with *ELA* had hourly wages that were over 20 percent higher from 30 to 44. These effects are also strongest for women attending some college. Although *ELA* conferred a wage premium among women in their forties for women with (column 4) and without college (column 5), the wage

[22] See data appendix. Our balancing tests in table 1 show that *ELA* does not predict measured IQ.

premium appears earlier (early thirties) and is larger, at roughly 10 percent, among women with some college.[23]

Table 7 shows that the effects of *ELA* on educational attainment are largest in the middle and upper end of the IQ distribution but also present among women from the most disadvantaged backgrounds. Using the same dependent variable and specification as column 2 of table 4, the effects of *ELA* are negative for the lowest IQ group (column 1) but positive and statistically significant in the middle and upper thirds of the IQ distribution (columns 2 and 3) for women ages 25 to 44. By age forty, the effects are similar in magnitude and translate into 0.4 to 0.5 year schooling advantage in both the middle and upper IQ groups, but into 0.4 *fewer* years of schooling for lower ability women. On the other hand, 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.[24]

Table 8 shows that the effects of *ELA* on labor-force participation are largest for women in the middle third of the IQ distribution and with some college. Women in the middle ability group (column 2) increased their labor-force participation by 9 to 10 percentage points during their late twenties and early thirties.[25] This shift on the extensive margin may have contributed to the accumulated experience

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[23] We also find the largest employment and wage effects for women in the highest SES group. Women in the lowest SES group showed the next largest gains in wages.

[24] The effect of *ELA* on college enrollment among 20 to24 year olds for the lowest IQ group was 1.4 percentage points (s.e. 6.0, mean 12 percent) and 5.4 (s.e. 4.5, mean 19 percent) and 5.6 percentage points (s.e. 2.9, mean 37 percent) for the middle and upper IQ groups, respectively. The effect of *ELA* on college enrollment among 20 to24 year olds for the lowest SES group was 16 percentage points (s.e. 4.5 percentage points), an implied increase of 160 percent. It was 5.1 (s.e. 4.6, mean 21 percent) and 2 percentage points (s.e. 2.8, mean 36 percent) for the middle and upper SES groups.

[25] In unreported results, we also find that *ELA* significantly increased usual weekly hours for high IQ women in their mid-twenties to thirties.

of middle-ability women and, ultimately, their wages.[26] Similar to the patterns for wages, these effects are concentrated among women with some college (column 5) but negligible in magnitude among women without college (column 4).

In addition to human capital explanations, *ELA* could have affected women's wages through marriage. Changes in when a woman first married (table 2) may have affected whom she married and the stability of that union. If women with *ELA* were differentially likely to be married or to divorce, their labor-force participation rates may have been affected and, through this channel, their wage growth. Conversely, their labor-force participation may have affected their marriage prospects and divorce rates.

Table 9 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. Consistent with *ELA* interacting with women's careers through marriage, column 1 shows that women in the lowest IQ group with *ELA* were more likely to have been married than women in that IQ group without *ELA*, whereas women in the middle and upper third of the IQ distribution with *ELA* were slightly more likely to have remained single. Moreover, women with *ELA* in the lower end and middle of the IQ distribution are 10.2 percentage points, or 97 percent, and 8.7 percentage points, or 72 percent, more likely to have ever been divorced by their late twenties (panel B, columns 1 and 2). The same patterns hold by college attendance. The main effects of *ELA* on marriage and divorce propensities are concentrated among women *without* any college (columns 4). These less educated women were more likely to have been married for all ages past 20 but were more likely to divorce in their late twenties.

[26] For high ability women, the impact of *ELA* on usual weekly hours is negative and significant after age 44. The increase in usual weekly hours associated with *ELA* is larger (positive and significant) between ages 20 to 44. The point estimates for low ability women are negative and statistically insignificant between ages 20 to 44.

Women with some college and ELA were slightly less likely to have married and are only slightly more likely to divorce, but neither estimate is statistically significant.[27]

VI. DECOMPOSING PILL-INDUCED WAGE GAINS

This section decomposes women's *ELA*-induced wage gains presented in table 3 into five components: formal education, on-the-job training, cumulative experience, occupational choice, and changes in marital status that presumably affected non-wage income. The analysis focuses on the gains in log hourly wages as measured in women's late forties near the peak of women's lifecycle earnings and the period when the effects of *ELA* have had the most time to accumulate. Using the semi-parametric re-weighting approach of DiNardo, Fortin, and Lemieux (1996), we re-weight the characteristics of the women untreated with *ELA* to resemble the characteristics of those with *ELA* at different points in the distribution. In the graphical and numerical decomposition results presented below, we use the Epanechnikov kernel function and the bandwidth selector of Sheather and Jones (1991). We also restrict the estimation to a single observation per woman, using only the last available wage observation for each woman in the 45 to 49 age group.

Figure 5a presents sequential re-weightings of the log hourly wage distributions of women aged 45 to 49. The first graph, in the upper left, shows kernel density estimates of women with (dashed line) and without (solid line) *ELA*. As shown in column 2 of table 3, women with *ELA* earned more on average than those without; the dashed line lies to the right of the solid line. The next five plots, reading across then down, show how the wage distribution of women without *ELA* changes as the characteristics of women without *ELA* are re-weighted to resemble those with *ELA*. The first adjusts the education distribution alone. The adjusted density shows slightly less mass in the middle third of the distribution and slightly more in the upper third, with almost no difference in the bottom third. Next, the distribution of job-training is adjusted together with the education distribution. This incremental change shifts the

[27] Among all women, those with *ELA* were slightly less likely to have ever married and had a 3.7 percentage point, or 35 percent, increased likelihood of having been divorced by their late twenties.

wage distribution slightly *leftward*. Adding an adjustment for cumulative experience produces a large rightward shift in the non-*ELA* wage distribution and results in significantly less mass between log hourly wages of 1.7 to 2.4 (about $5.50 to $11) and much more between log hourly wages of 2.5 to 3.4 (about $12 to $30). The marginal impact of adjusting for occupational classification and then marital status in the next two plots is negligible: the first of these has a very small positive impact at the middle of the distribution, while the second has no visible effect.

The two plots in the third row of figure 5a summarize the total effects of the adjustments. In the first, the actual density estimate of the wage distribution of women without *ELA* is plotted alongside the counterfactual density estimate with all five adjustments. The second plots the fully adjusted counterfactual density for non-*ELA* women with the actual density for women with *ELA*. The difference in the two densities indicates the presence of factors beyond the ones considered as channels for the *ELA* wage premium. Because the order of adjustments matters in apportioning the relative importance of correlated explanatory factors, figure 5b repeats the decomposition exercise in reverse. Adjusting for current marital status and occupational choice earlier in the sequences has no effect on the role attributed to marital status, but it does increase the role attributed to occupation choice. In fact, occupation now almost completely offsets the role of education due to the high correlation between the two measures. Labor-force experience, however, remains equally important.

Table 10 quantifies how much of the difference in log hourly wages between women with and without *ELA* can be explained by each of the factors at the mean and 25^{th}, 50^{th}, and 75^{th} percentiles of the distribution. The first panel corresponds to figure 5a and shows that education accounts for about 31 percent of the wage differential at the mean, and that experience accounts for nearly 62 percent of the *ELA* wage premium. Occupational choice, job-training and marriage play a much smaller role. What was less apparent in the figures, however, are the relative magnitudes of the factors at different points in the distribution. The marginal impact of adjusting for education at the median or 75^{th} percentile accounts for approximately 43 percent of the *ELA* wage differential, but it accounts for only a quarter of the gap at the 25th percentile. In contrast, work experience is considerably more important lower in the wage distribution (82 percent of the gap at the 25th percentile) than it is higher in the distribution (56 percent of the variation at the 75th percentile). The roles of occupational choice and marital status, while still small, also tend to increase farther up the wage distribution. The last column in the first panel shows the unexplained difference, the gaps between the density estimates in the last plot of figure 5a. At the points chosen, the five factors account for most of the *ELA* premium: 11 percent of the differential is unexplained on average, with even less unaccounted for at the chosen quantiles. At the 75th percentile, the factors actually over-explain the *ELA* wage differential by about 4 percent.

The lower panel of the table is the analogue to figure 5b and show similar results for accumulated experience. At the mean, cumulative experience accounts for roughly 60 percent of the ELA wage premium, 73 percent at the 25th percentile and 52 percent at the 75th percentile. Reversing the order of the decomposition, however, ascribes more weight to occupation: roughly 28 percent of the ELA wage premium at the mean is accounted for by adjusting for the occupational distribution, 24 percent at the 25th percentile, and 48 percent at the 75th percentile.

Overall, differences in labor market experience can account for 60 percent of the *ELA* hourly wage premium of women in their late 40s, and either education or occupational differences, which are highly correlated, can account for another 30 percent of the premium. Experience differences matter more, and educational/occupational differences matter less, lower in the wage distribution while the opposite is true higher in the distribution. Differences in job training episodes play a small role, but in the opposite direction, possibly owing to negative selection into training. Marital status has a negligible effect on the *ELA* premium, perhaps because there is little difference in current marital status between women with and without *ELA* in their late 40s.

VII. THE "OPT-*IN*" REVOLUTION

Lisa Belkin's 2003 New York Times Magazine article, "The Opt-Out Revolution," reopened questions 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 recently begun to have more children, but these changes seem small relative to the Revolution that began 50 years ago.

When the Pill provided greater choice over childbearing so as not to preclude career investments, our estimates show that women opted to delay fertility and marriage and invest more in their labor market skills. Our rough counterfactual estimates suggest that Pill-induced changes in career investments account for roughly 32 percent of the wage gains between women born in the mid-1940s and early 1950s in their forties—90 percent of which can be attributed to increasing labor-market experience, greater educational attainment and different occupational choices.[28] While improvements in birth control technology play an important role in increasing women's wage earnings, our results also point to the importance of other factors such as changes in the demand for women's labor (e.g. anti-discrimination legislation and enforcement or changes in preferences) as well as shifts in their training (e.g. the rise of co-education).

What do our estimates imply about the importance of Pill-induced investments for the narrowing of the gender gap from 1980 to 2000? To assess the implications of our estimates, we simulate a

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[28] This is a rough estimate obtained from 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 forties between the 1943-46 and the 1951-1954 cohorts in the *NLS-YW*. Weinberger and Kuhn (2008) 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) and post-market investments (e.g. labor market experience and on-the-job training), and this counterfactual is not taken over the past forty years. It is reassuring that the Pill's combined influence on both pre-market and post-entry investments is less than one-third of the wage gains to women in their forties.

counterfactual hourly wage distribution in 1980, 1990 and 2000 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 computing the actual hourly wage distribution for men and women in 1980, 1990 and 2000.[29] From 1980 to 1990, the actual gender gap in real hourly wages for 25 to 49 year olds closed by 12.6 percentage points, and the simulated gender gap closed by 11.3 percentage points. From 1990 to 2000, the actual gender gap in real hourly wages closed by 7.4 percentage points, and the simulated gender gap closed by 5.1 percentage points. The estimates in column 2 of table 3, therefore, imply that 1.3 percentage points, or 10.3 percent, of the 12.6 percentage point change in the gender gap during the 1980s and 2.3 percentage points, or 31 percent, of the 7.4 percentage point change in the gender gap over the 1990s, can be attributed to early access to the Pill. The effect of Pill-induced labor supply shifts on the gender gap was not trivial, but it was not overwhelming either.

Did the Pill unleash the Revolution? Our results provide no conclusive answer to this question. They are large enough to explain roughly one third of the wage gains by age 40, but they may understate the Pill's broader influence for several reasons. Using variation in *early access* to the pill does not allow for the effects of access to the Pill beyond age 20 to be estimated, nor does the empirical strategy capture the potentially large social multiplier effects: the Pill may have impacted norms and expectations about marriage and childbearing as well as decisions to hire and promote women. The effects of the Pill may be larger than we claim, but it is not clear how much larger. The bottom line is that even these conservative estimates suggest that the Pill's power to transform childbearing from

[29] 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.

probabilistic into a planned and purposeful practice shifted women's career decisions and compensation for decades to come.

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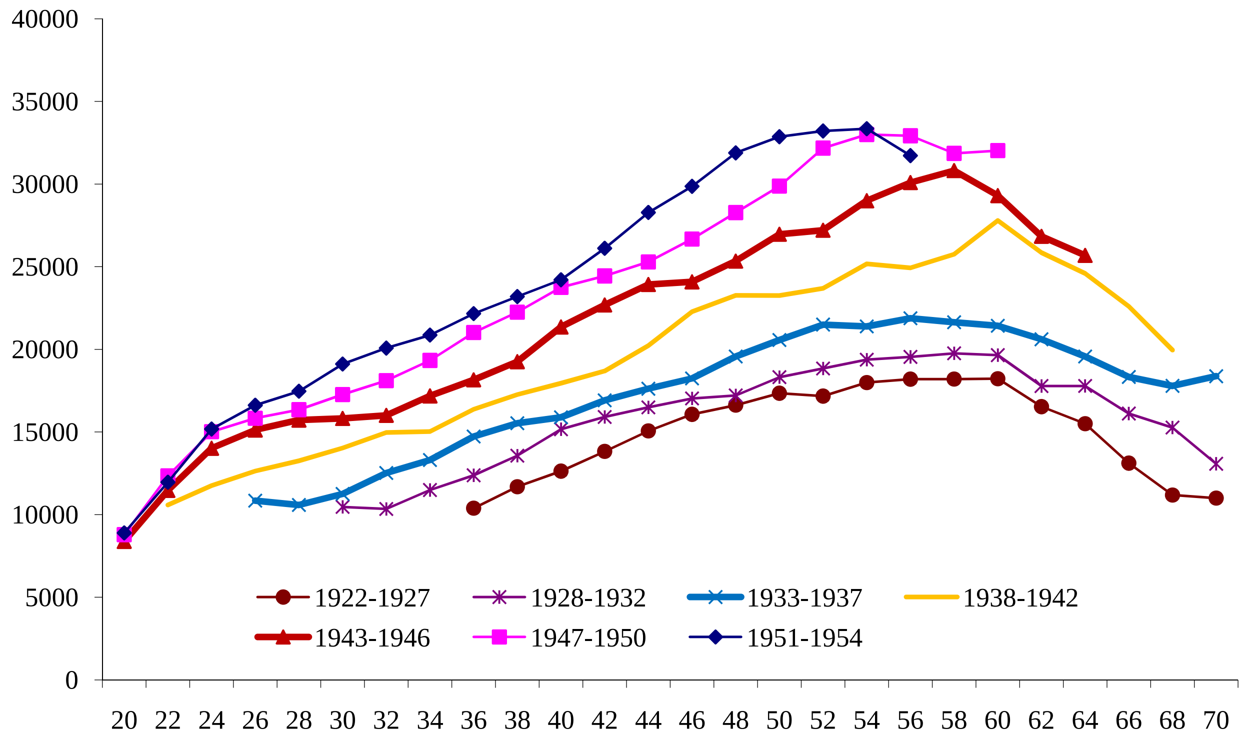
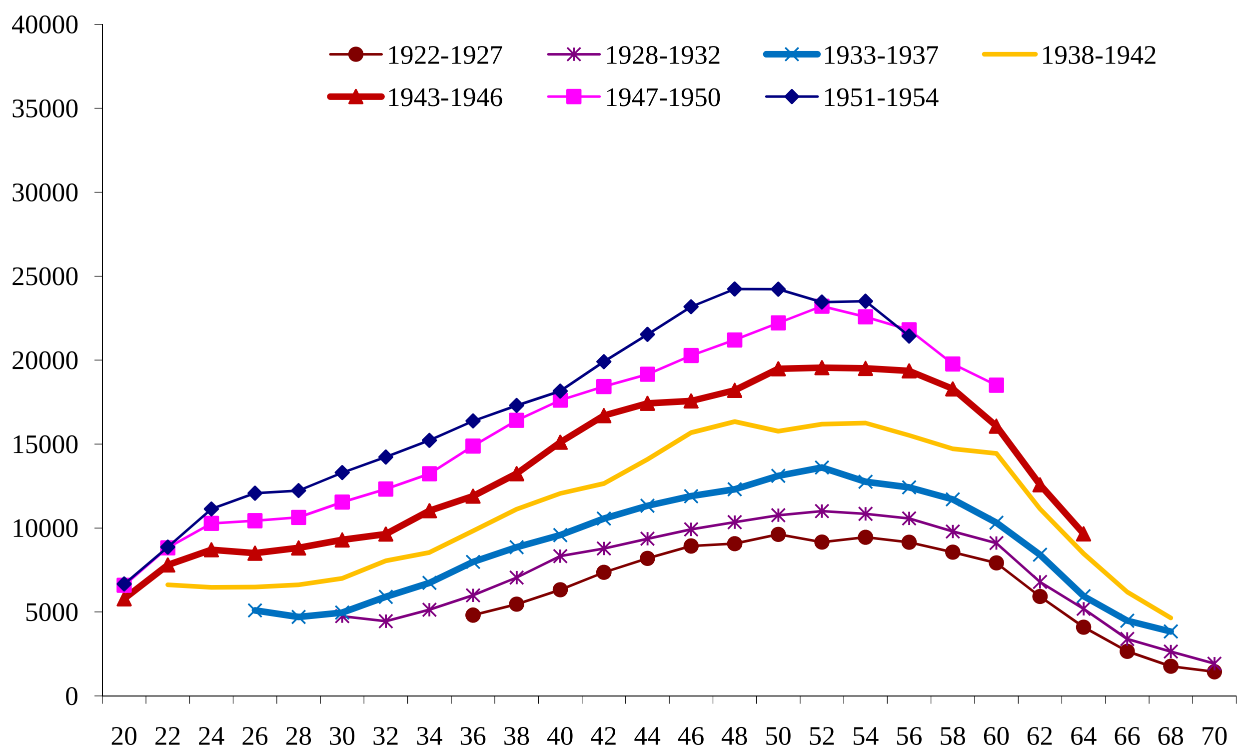
Figure 1. The Evolution of Women's Wages by Age and Birth Cohort

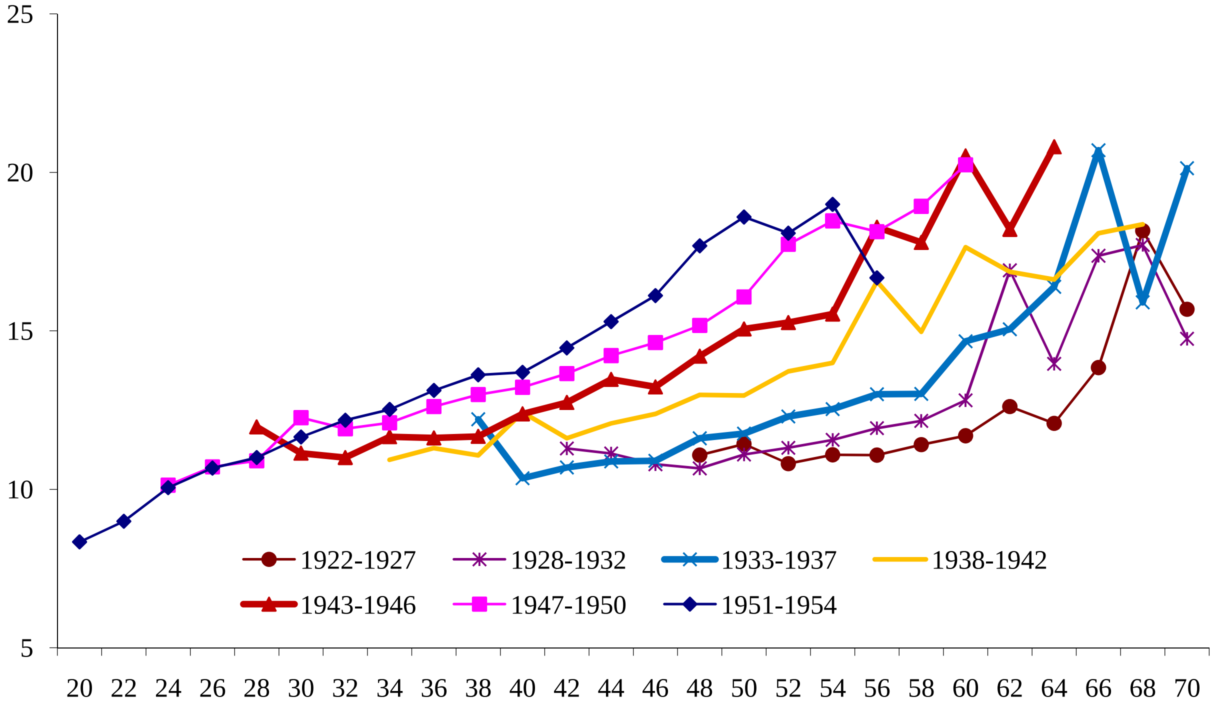
A. Real Annual Labor Earnings for Women with Positive Earnings



B. Real Annual Labor Earnings including Zero Earnings



C. Real Hourly Wages (in 2000 Dollars) for Women with Positive Earnings

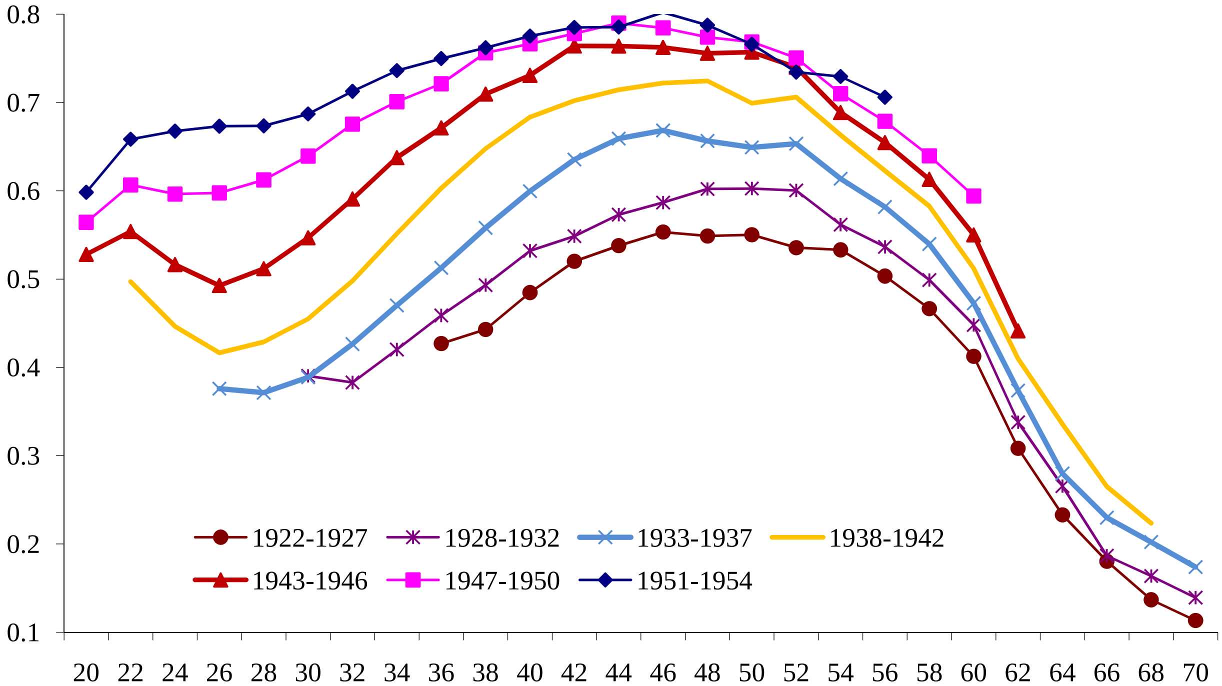


The hourly wage is computed by dividing annual earnings by the product of weeks worked last year and usual hours worked per week. Annual labor earnings include income from all jobs, including self-employment. Each series is adjusted for inflation to year 2000 dollars using the personal consumption expenditures deflator. Data are weighted using *CPS* sample weights and collapsed into two-year age groups.

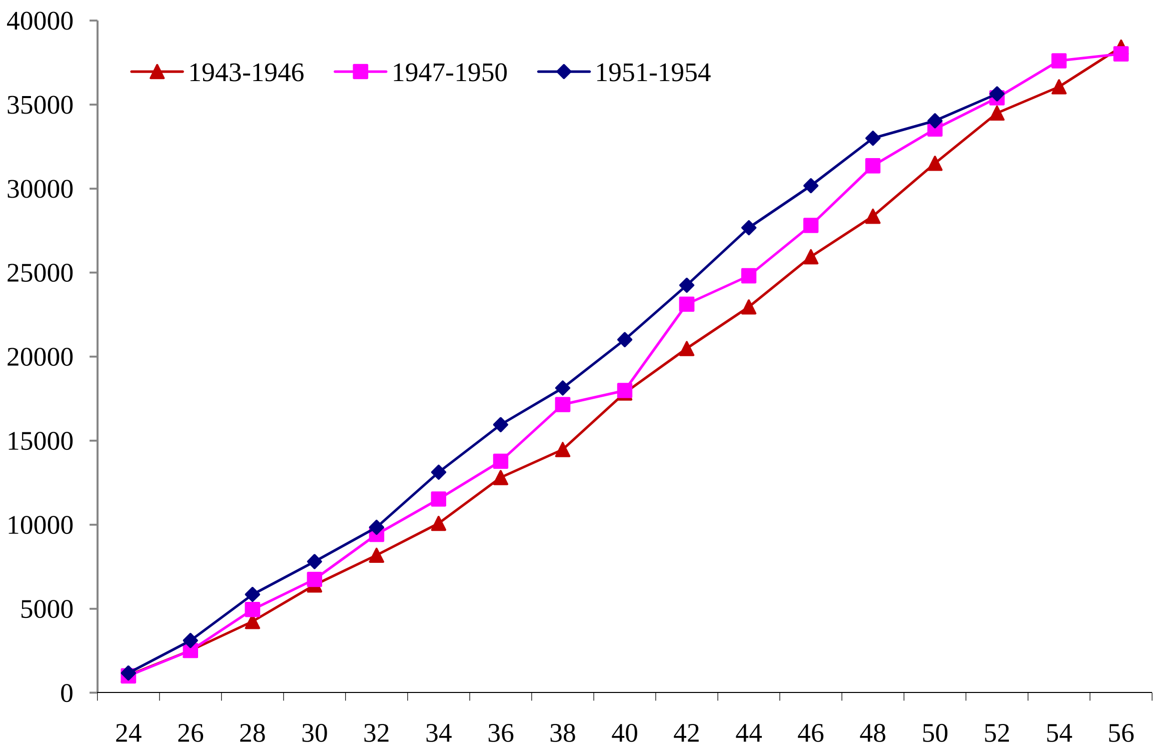
Sources: 1964-2009 March *Current Population Surveys*.

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

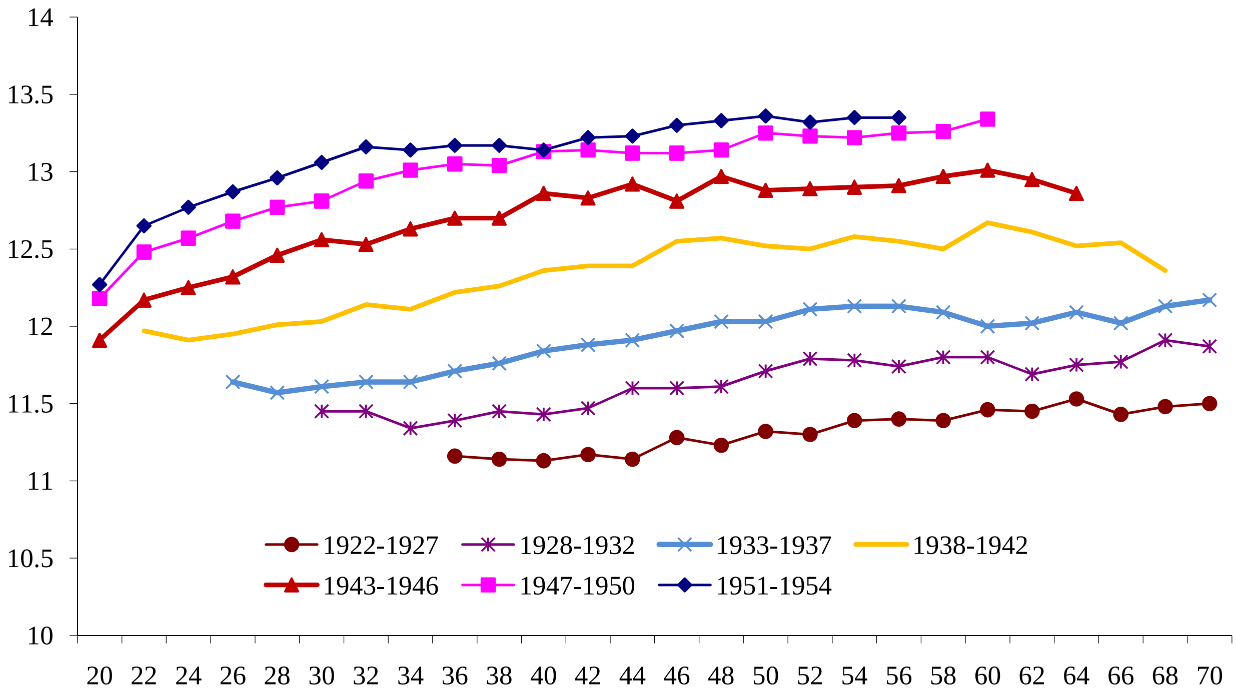
A. Labor Force Participation



B. Cumulative Experience in Terms of Hours Worked Since Age 24 in the *NLS-YW*



C. Highest Grade Completed

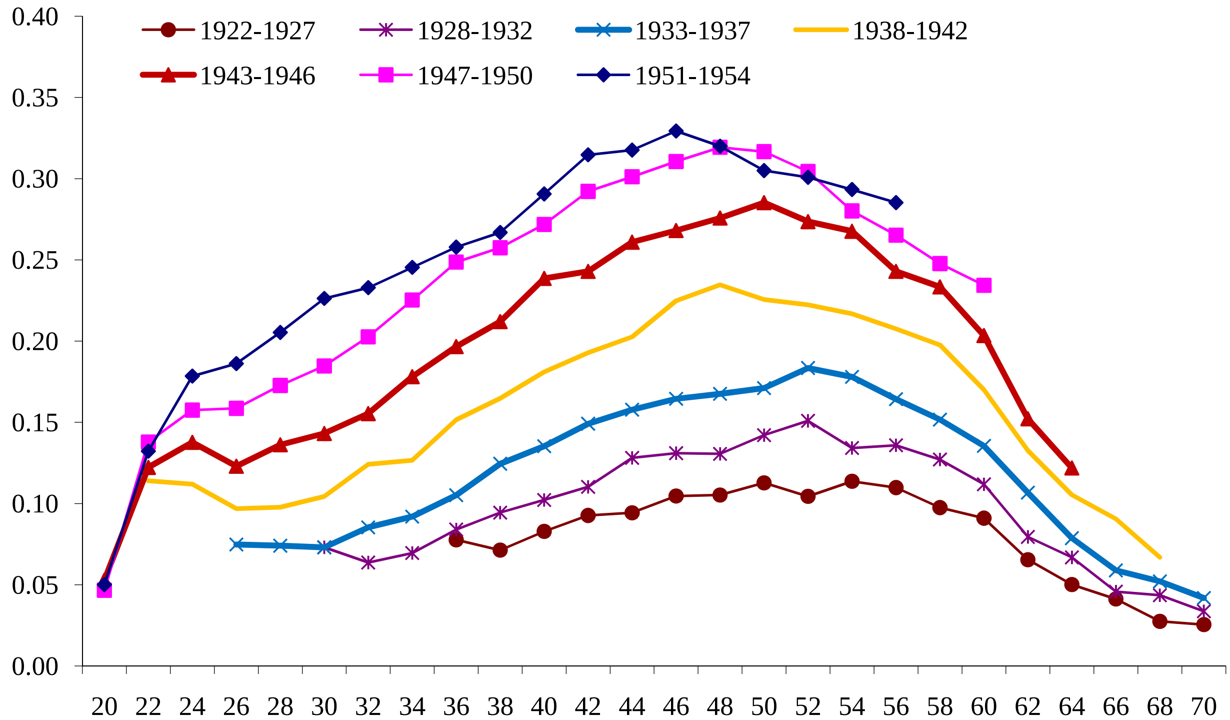


Labor force participation is a binary variable indicating whether the respondent was employed or looking for a job at the time of the survey. Highest grade completed is at the time of the survey, and the first year of college is counted as 13, the second year as 14, etc. Cumulative experience is available only in the *NLS-YW* and is defined here as the total number of hours worked since the age of 24; for the construction of this variable, see the data appendix. Data are weighted using appropriate *CPS* or *NLS* sample weights and collapsed into two-year age groups.

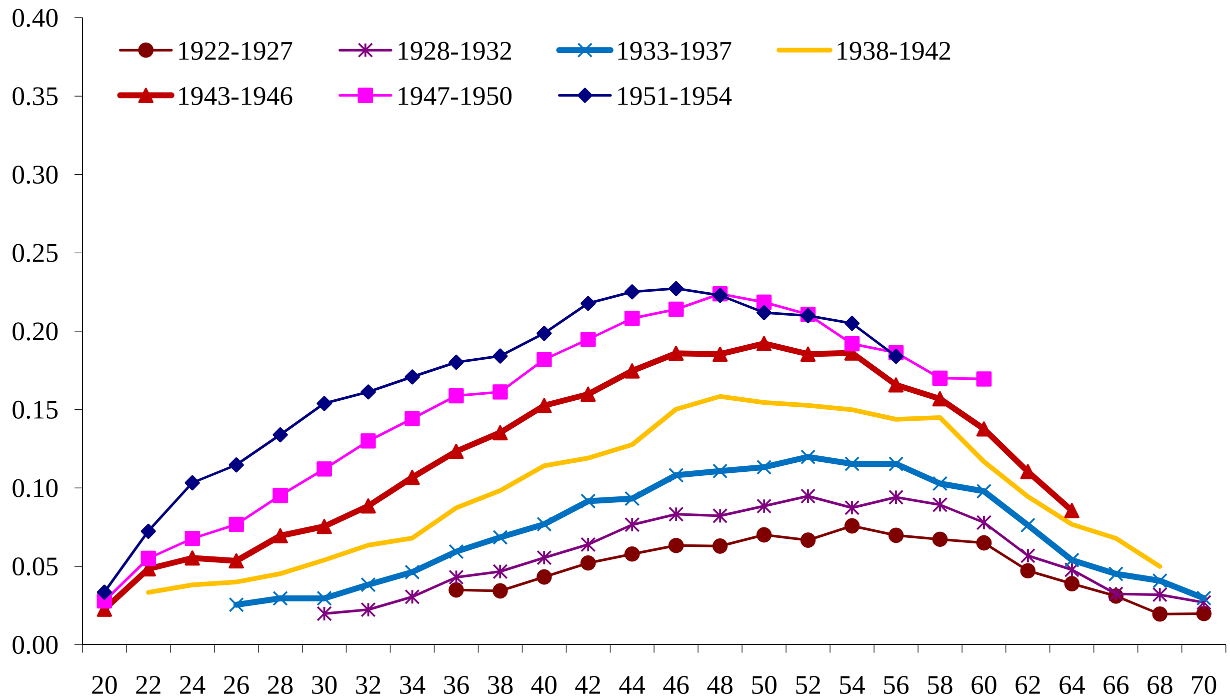
Sources: Panels A and C use the 1964-2009 March *Current Population Survey*. Panel B uses the *National Longitudinal Survey of Young Women*.

Figure 3. The Evolution of Occupational Choice by Age and Birth Cohort

A. Women Working in Professional and Managerial Jobs



B. Women Working in Non-Traditionally Female Professional and Managerial Jobs

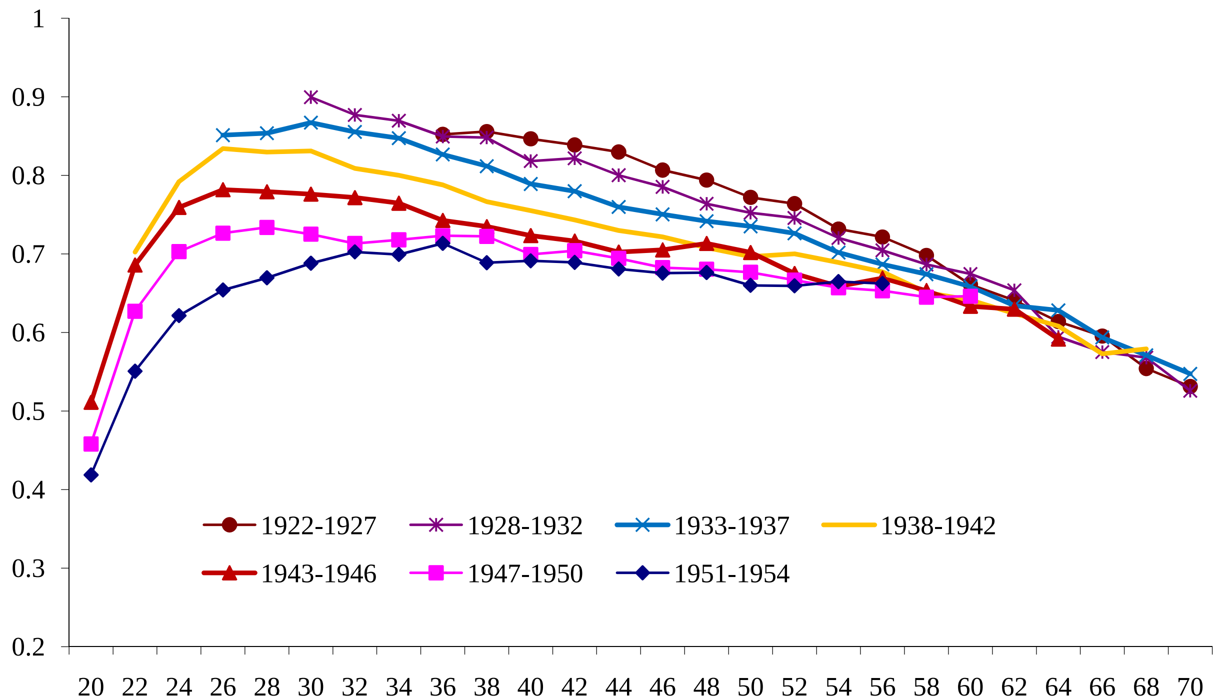


Job groups are coded using the 3-digit Census occupational codes in the *CPS*. Women are counted in a job category only if they are employed at the time of the survey. "Traditionally female" professional and managerial jobs include nurses and non-post-secondary teachers. Data are weighted using *CPS* sample weights and collapsed into two-year age groups.

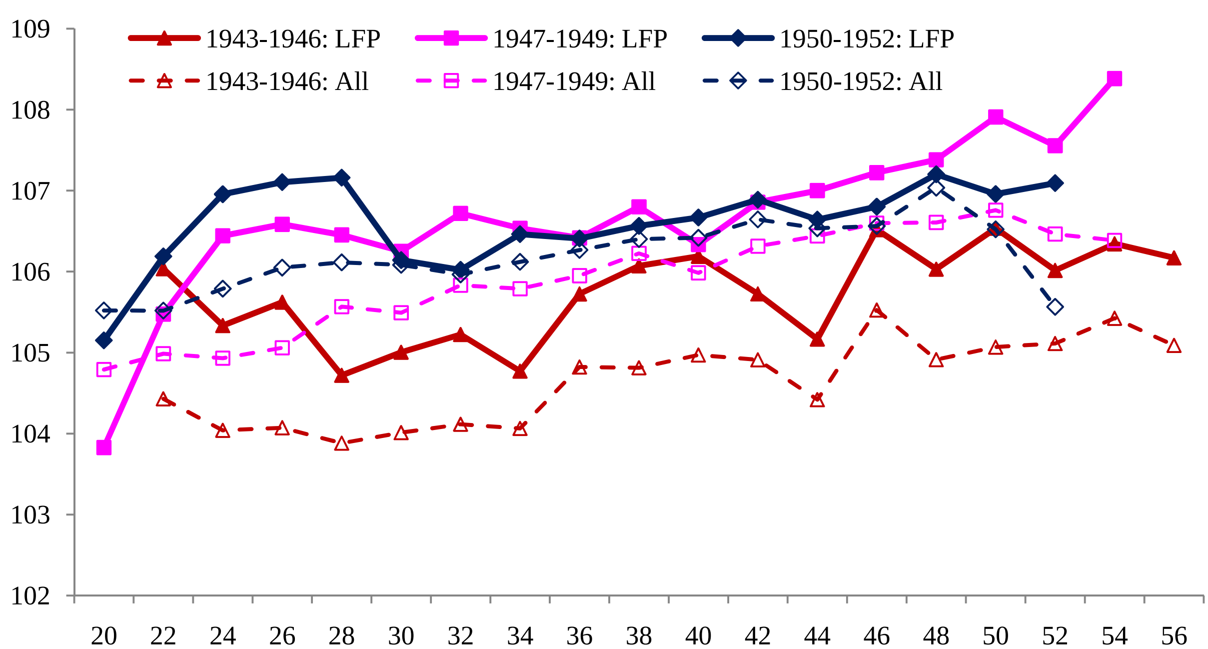
Source: See figure 1.

Figure 4. The Evolution of Spousal Income and the Selection on Cognitive Ability by Age and Birth Cohort

A. Currently Married (Indicator of Non-Wage Income)



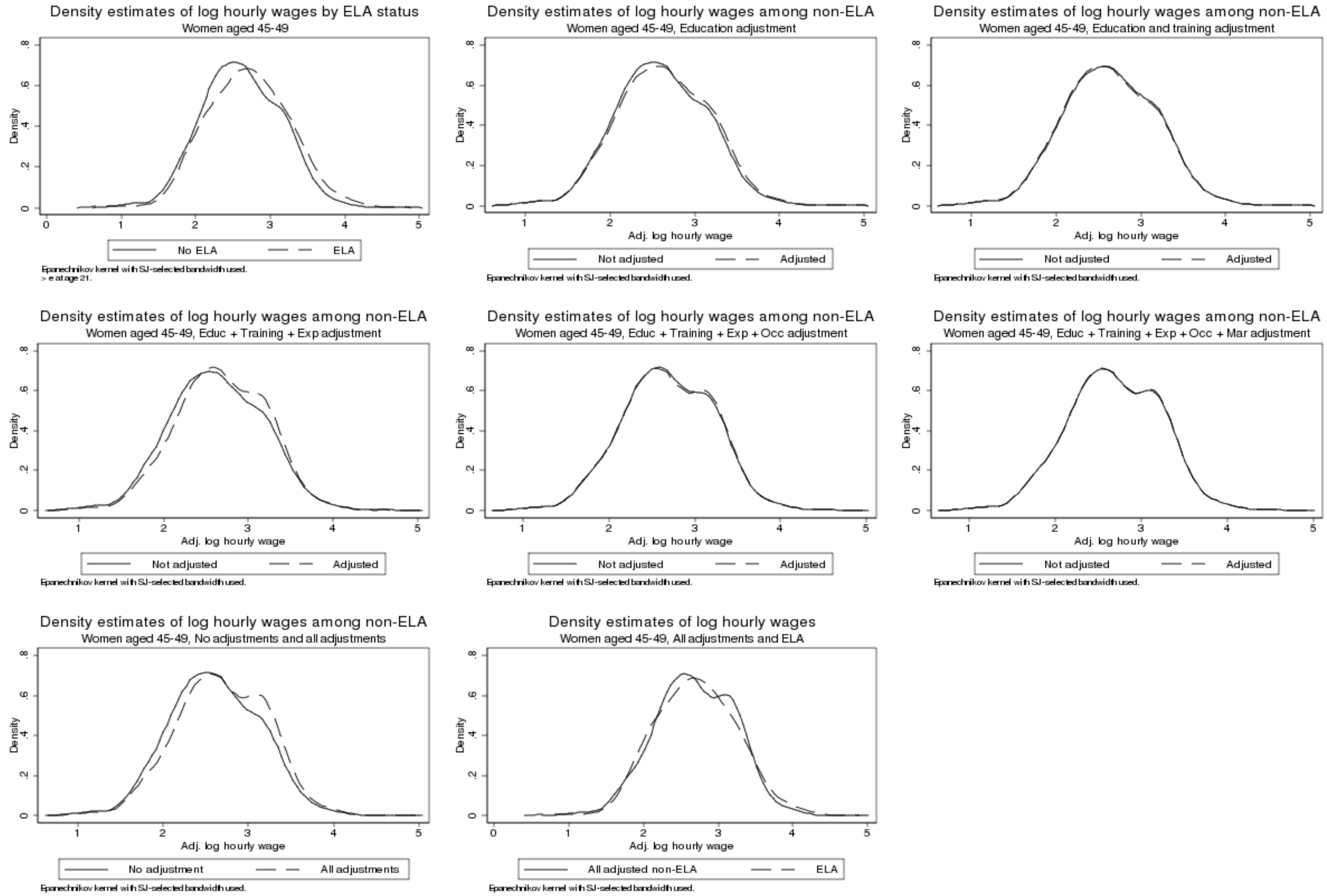
B. IQ Score Among Labor-Force Participants (LFP) and All *NLS-YW* Respondents



Currently married is a binary variable indicating whether the respondent is married (spouse present or not) at the time of the survey. The IQ score is a composite measure of aptitude, scaled within the *NLS-YW* to have a normal distribution with population mean of 100 and population standard deviation of 15. IQ scores are generally unavailable for respondents born in 1953; for this reason, we change the cohort ranges slightly in panel B. The lighter, dashed lines show average IQ for all respondents in a cohort for comparison. For more information on this variable, see the data appendix. Data are weighted using appropriate *CPS* or *NLS* sample weights and collapsed into two-year age groups.

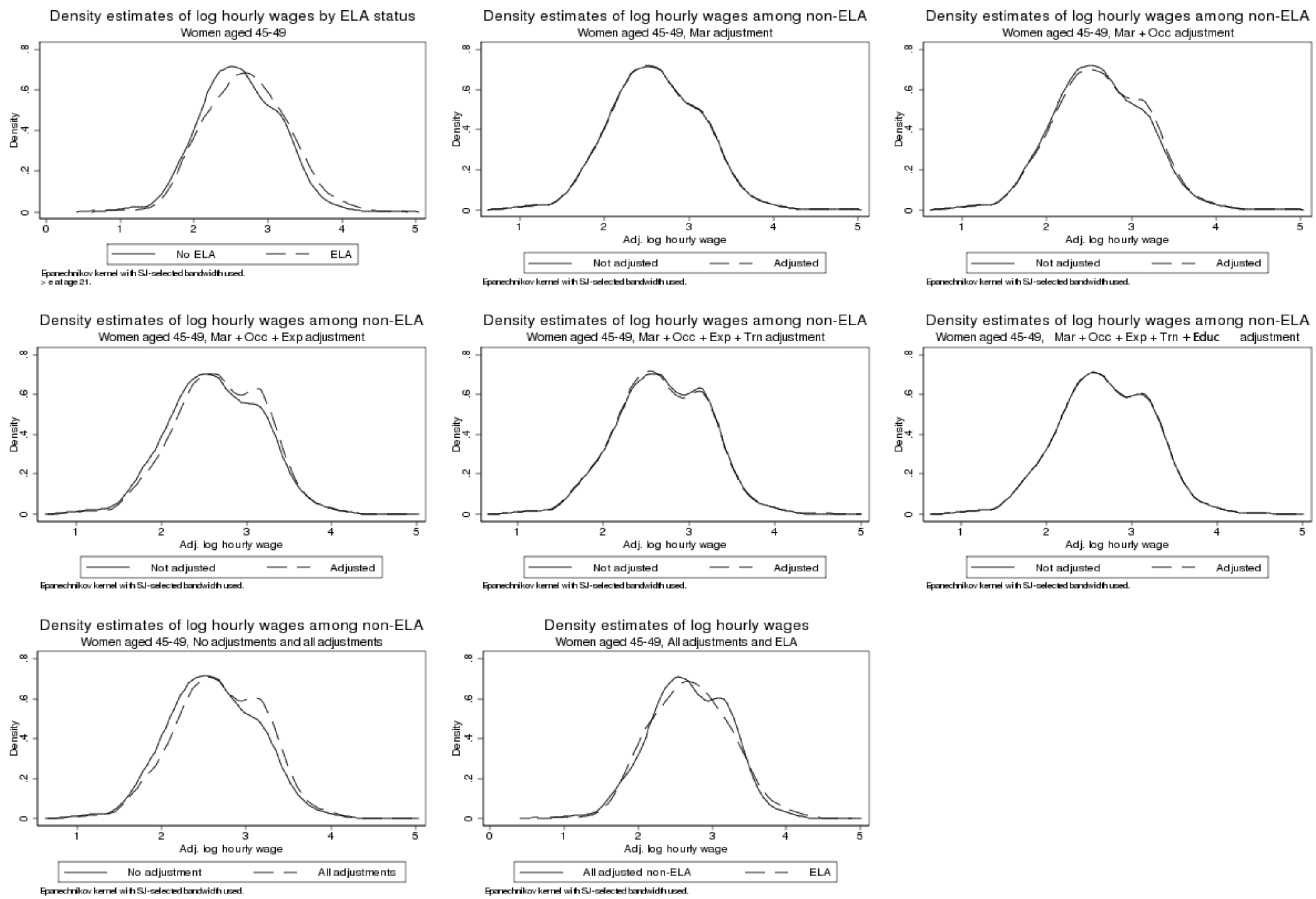
Sources: Panel A uses the 1964-2009 March *Current Population Survey*. Panel B uses the *National Longitudinal Survey of Young Women*.

Figure 5a. Decomposition of ELA Effect on Log Hourly Wage, ages 45-49



The figures show kernel density estimates of log hourly wages at ages 45-49 for women with and without *ELA*. Log hourly wages have been adjusted to control for cohort and state of residence fixed effects. The sequential graphs show how the distribution of log hourly wages of women without *ELA* changes when the wages are reweighted using the procedure described in the text and appendix to resemble the observable characteristics of women with *ELA*. The sequence of the graphs shows the marginal change of reweighting for each set of additional characteristics. The last graph compares the distributions of women with *ELA* and those without that have been reweighted for all five sets of characteristics.

Figure 5b. Decomposition of ELA Effect on Log Hourly Wage, ages 45-49: Reverse Order



The figures show kernel density estimates of log hourly wages at ages 45-49 for women with and without *ELA*. Log hourly wages have been adjusted to control for cohort and state of residence fixed effects. The sequential graphs show how the distribution of log hourly wages of women without *ELA* changes when the wages are reweighted using the procedure described in the text and appendix to resemble the observable characteristics of women with *ELA*. The sequence of the graphs shows the marginal change of reweighting for each set of additional characteristics. The last graph compares the distributions of women with *ELA* and those without that have been reweighted for all five sets of characteristics.

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

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

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

Table 2. The Impact of ELA on the Timing of First Marriage and Motherhood

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Marriage	Age at first marriage	1=Married before age 19	1=Married before age 20	1=Married before age 21	1=Married before age 22	1=Married before age 23	1=Marrie d before age 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
Panel B. Motherhood	Age at first birth	1=Birth before age 19	1=Birth before age 20	1=Birth before age 21	1=Birth before age 22	1=Birth before age 23	1=Birth before age 24
Mean of DV	*22.11*	*0.203*	*0.292*	*0.38*	*0.452*	*0.512*	*0.565*
ELA	0.236	-0.023	-0.034	-0.022	-0.009	-0.021	-0.011
	(0.285)	(0.020)	(0.028)	(0.029)	(0.028)	(0.029)	(0.030)
Observations	3384	3973	3988	3997	4001	4001	4001
(Pseudo) R-squared	0.05	0.04	0.04	0.03	0.03	0.03	0.03
Fixed effects	S, Y	S, Y	S, Y	S, Y	S, Y	S, Y	S,Y

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

Column (1) reports coefficients from an OLS regression of equation 2; columns (2) through (7) report mean marginal effects from probit specifications of equation 2. The sample in column1 is conditional on the outcome being observed, but the sample in columns 2 through 7 includes women who never get married (panel A) or give birth (panel B). 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-squareds. All regressions include vectors of state fixed effects (S) and cohort fixed effects (Y). Heteroskedasticity-robust standard errors are corrected for clustering at the state level and are presented in parentheses below each estimate.

Table 3. Wages Rates and Annual Income

	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 14-19	*5.61*	-0.135	-0.031	*2519.77*	-247.51	-0.248	*1581.0*	-450.06
		(0.308)	(0.029)		(612.01)	(0.069)***		(497.00)
ELA * Ages 20-24	*7.88*	-0.254	-0.033	*9943.43*	-954.01	-0.104	*7660.5*	-1299.56
		(0.283)	(0.023)		(637.53)	(0.048)**		(572.25)**
ELA * Ages 25-29	*9.60*	-0.242	-0.015	*15610.49*	167.80	0.078	*10911.4*	-151.20
		(0.319)	(0.026)		(723.88)	(0.047)*		(662.84)
ELA * Ages 30-34	*10.62*	0.408	0.030	*18116.04*	1004.18	0.117	*12452.0*	722.21
		(0.313)	(0.025)		(679.03)	(0.051)**		(639.17)
ELA * Ages 35-39	*11.74*	0.560	0.037	*21173.96*	1962.72	0.114	*15441.5*	1472.1
		(0.334)	(0.024)		(749.22)***	(0.046)**		(721.65)**
ELA * Ages 40-44	*12.84*	0.787	0.055	*24493.21*	2315.24	0.102	*19183.9*	2844.70
		(0.306)**	(0.022)**		(878.42)***	(0.045)**		(837.87)***
ELA * Ages 45-49	*14.29*	1.128	0.081	*28148.4*	2147.66	0.085	*25238.2*	3986.24
		(0.461)**	(0.031)**		(862.37)***	(0.048)*		(1000.45)***
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		0.21	0.26		0.013	0.102		0.012

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

Wages are adjusted to 2000 dollars using the PCE deflator (BEA 2009). All regressions include vectors of state fixed effects (S); cohort fixed effects (Y); and age group fixed effects (A). Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate.

Table 4a. Human Capital Accumulation and Occupational Upgrading

		(1)		(2)		(3)
	Proportion enrolled in college	1= Enrolled in college	Mean grade completed	Highest grade completed	Proportion reporting training	1=Occupational training since last interview
ELA * Ages 14-19	0.492	0.007	10.175	-0.407	0.219	-0.031
		(0.038)		(0.117)***		(0.022)
ELA * Ages 20-24	0.241	0.047	12.094	0.087	0.203	-0.005
		(0.021)**		(0.133)		(0.012)
ELA * Ages 25-29	0.077	0.006	12.521	0.314	0.188	0.031
		(0.008)		(0.129)**		(0.011)***
ELA * Ages 30-34	0.072	0.003	12.851	0.265	0.245	0.027
		(0.012)		(0.130)**		(0.016)*
ELA * Ages 35-39	0.065	0.002	12.994	0.289	0.285	0.009
		(0.010)		(0.128)**		(0.016)
ELA * Ages 40-44	0.049	-0.009	13.133	0.281	0.310	0.020
		(0.009)		(0.133)**		(0.020)
ELA * Ages 45-49	0.029	ND	13.284	0.232	0.324	-0.020
				(0.143)		(0.019)
Fixed effects		Y, S, A		Y, S, A		Y, S, A
Observations		57373		78809		63013
Unique women		3702		4354		4323
(Pseudo) R-squared		0.14		0.14		0.03

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

Columns (1) and (3) report mean marginal effects from probit regressions; column (2) reports coefficients from an OLS regression. All regressions include vectors of state fixed effects (S); cohort fixed effects (Y); and age group fixed effects (A). Proportion enrolled in college is conditional on completing high school. Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate. "ND" indicates that disclosure requirements were not met for this estimate.

Table 4b. Human Capital Accumulation and Occupational Upgrading

		(1)		(2)		(3)		(4)
	Proportion in Professional Job	1= in Professional Job	Proportion in Non-traditional Job	1= in Non-traditional Job	Proportion in Clerical or Sales Job	1= in Clerical or Sales Job	Proportion in Other Job	1= in Other Job
ELA * Ages 14-19	0.009	0.002	0.007	ND	0.160	-0.052	0.192	0.007
		(0.003)				(0.019)***		(0.023)
ELA * Ages 20-24	0.087	0.008	0.044	0.007	0.304	-0.028	0.197	0.010
		(0.014)		(0.007)		(0.023)		(0.016)
ELA * Ages 25-29	0.163	0.047	0.080	0.020	0.248	0.002	0.193	0.020
		(0.020)**		(0.011)*		(0.023)		(0.017)
ELA * Ages 30-34	0.199	0.060	0.138	0.063	0.241	-0.016	0.206	0.018
		(0.022)***		(0.016)***		(0.020)		(0.017)
ELA * Ages 35-39	0.242	0.042	0.202	0.044	0.262	-0.031	0.204	0.015
		(0.025)*		(0.020)**		(0.020)		(0.021)
ELA * Ages 40-44	0.250	0.035	0.225	ND	0.272	-0.049	0.195	0.009
		(0.029)				(0.022)**		(0.024)
ELA * Ages 45-49	0.242	0.000	0.218	-0.010	0.223	-0.080	0.163	-0.003
		(0.023)		(0.017)		(0.019)***		(0.016)
Fixed effects		Y, S, A		Y, S, A		Y, S, A		Y, S, A
Observations		73737		73737		73737		73737
Unique women		4354		4354		4354		4354
(Pseudo) R-squared		0.07		0.09		0.02		0.02

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

Columns (1) through (4) report mean marginal effects from probit regressions. All regressions include vectors of state fixed effects (S); cohort fixed effects (Y); and age group fixed effects (A). Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate. "ND" indicates that disclosure requirements were not met for this estimate. By construction, the sum of the proportions in the four categories equals the employment rate. For more information on job categories, see the Data Appendix.

Table 5. Labor-Force Attachment

		(1)		(2)		(3)
	Proportion in the labor force	Labor force participation	Mean usual weekly hours (excl. zeros)	Usual weekly hours (excl. zeros)	Mean Cumulative Experience	Cumulative Experience in Hours
ELA * Ages 14-19	0.421	-0.046	24.7	-4.205	509	ND
		(0.024)**		(0.663)***		
ELA * Ages 20-24	0.624	-0.027	35.0	-0.320	2723	-774.4
		(0.019)		(0.518)		(358.8)**
ELA * Ages 25-29	0.637	0.038	36.3	0.842	5929	-1009.5
		(0.020)**		(0.386)**		(430.7)**
ELA * Ages 30-34	0.683	0.037	35.6	0.980	10758	293.3
		(0.021)*		(0.313)***		(407.8)
ELA * Ages 35-39	0.756	0.013	36.0	1.738	16097	901.9
		(0.021)		(0.405)***		(559.8)
ELA * Ages 40-44	0.798	-0.005	36.9	1.306	22608	2406.6
		(0.019)		(0.458)***		(767.2)***
ELA * Ages 45-49	0.798	-0.015	37.9	0.386	30009	1365.9
		(0.019)		(0.463)		(987.0)
Fixed effects		Y, S, A		Y, S, A		Y, S, A
Observations		74075		52250		61736
Unique women		4354		4257		4329
(Pseudo) R-squared		0.05		0.08		0.62

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

Column (1) reports mean marginal effects from a probit regression; columns (2) and (3) reports coefficients from OLS regressions. The R-squared for column (1) is a pseudo (McFadden's) R-squared. All regressions include vectors of state fixed effects (S); cohort fixed effects (Y); and age group fixed effects (A). Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate. "ND" indicates that disclosure requirements were not met for this estimate. For construction of cumulative experience, see the Data Appendix.

Table 6. Heterogeneity in the Growth of Real Hourly Wages

	(1)	(2)	(3)	(4)	(5)
Sample	Lower third of IQ distribution	Middle third of IQ distribution	Upper third of IQ distribution	No College	Some College
ELA * Age 14-19	-0.065	-0.115	-0.622	-0.869	-0.638
	(0.558)	(0.438)	(0.638)	(0.216)***	(0.527)
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	0.2	0.23	0.17	0.26
Mean of DV for 14-19	3.22	3.89	4.07	3.02	3.49
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 and runs it on separate groups. 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 regressions include vectors of state fixed effects, cohort fixed effects, and age group fixed effects. Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate.

Table 7. Heterogeneity in Highest Grade Completed

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Lower third of IQ distribution	Middle third of IQ distribution	Upper third of IQ distribution	Lower third SES distribution	Middle third SES distribution	Upper third SES distribution
ELA * Age 14-19	-0.606	-0.067	-0.111	-0.251	-0.720	-0.375
	(0.247)**	(0.212)	(0.170)	(0.134)*	(0.187)***	(0.247)
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 14-19	10.59	10.81	10.95	9.64	10.36	10.59
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 (2) of table 4 and runs it on separate groups indicated as indicated. 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. All regressions include vectors of state fixed effects, cohort fixed effects, and age group fixed effects. Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate.

Table 8. Heterogeneity in Labor-Force Participation

	(1)	(2)	(3)	(4)	(5)
Sample	Lower third of IQ distribution	Middle third of IQ distribution	Upper third of IQ distribution	No College	Some College
ELA * Age 14-19	-0.005	0.033	0.025	-0.075	-0.012
	(0.046)	(0.040)	(0.059)	(0.029)***	(0.038)
ELA * Age 20-24	-0.053	0.024	-0.031	-0.012	-0.065
	(0.055)	(0.045)	(0.035)	(0.025)	(0.034)**
ELA * Age 25-29	0.034	0.090	0.043	0.011	0.055
	(0.044)	(0.054)*	(0.038)	(0.023)	(0.026)**
ELA * Age 30-34	-0.007	0.103	0.032	0.007	0.075
	(0.041)	(0.047)**	(0.041)	(0.026)	(0.034)**
ELA * Age 35-39	0.007	0.061	0.018	-0.001	0.023
	(0.033)	(0.041)	(0.028)	(0.031)	(0.028)
ELA * Age 40-44	-0.013	0.018	-0.008	-0.009	-0.009
	(0.053)	(0.036)	(0.028)	(0.030)	(0.022)
ELA * Age 45-49	-0.021	0.016	-0.013	-0.020	-0.017
	(0.048)	(0.031)	(0.030)	(0.027)	(0.037)
Observations	12469	16531	20181	47925	26150
Unique women	790	975	1112	2898	1456
R-squared	0.052	0.060	0.063	0.048	0.077
Mean of DV for 14-19	0.435	0.503	0.462	0.431	0.405
Mean of DV for 20-24	0.616	0.664	0.658	0.606	0.657
Mean of DV for 25-29	0.606	0.619	0.697	0.571	0.771
Mean of DV for 30-34	0.687	0.666	0.705	0.651	0.774
Mean of DV for 35-39	0.731	0.755	0.802	0.729	0.805
Mean of DV for 40-44	0.773	0.809	0.853	0.772	0.845
Mean of DV for 45-49	0.740	0.842	0.863	0.765	0.851

This table uses the specification in column (5) of table 5 and runs it on separate groups indicated as indicated. 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. Estimates are mean marginal effects from a probit regression, and the R-squared statistics are pseudo (McFadden's) R-squared. All regressions include vectors of state fixed effects, cohort fixed effects, and age group fixed effects. Heteroskedasticity-robust standard errors are clustered at the state level and presented in parentheses below each estimate.

Table 9. Heterogeneity in Marriage and Divorce Propensities

	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	A. Never Been Married					C. Ever Been Divorced				
	Lower third IQ dist.	Middle third IQ dist.	Upper third IQ dist.	No College	Some College	Lower third IQ dist.	Middle third IQ dist.	Upper third IQ dist.	No College	Some College
ELA * Age 14-19	-0.059	0.024	0.034	0.031	-0.011	ND	ND	ND	ND	ND
	(0.054)	(0.035)	(0.035)	(0.028)	(0.008)					
ELA * Age 20-24	-0.113	0.011	0.032	-0.022	-0.004	0.013	0.015	ND	0.004	-0.004
	(0.079)	(0.063)	(0.078)	(0.027)	(0.050)	(0.025)	(0.014)		(0.008)	(0.005)
ELA * Age 25-29	-0.030	0.021	0.017	-0.014	0.021	0.102	0.087	0.022	0.048	0.019
	(0.058)	(0.042)	(0.057)	(0.020)	(0.049)	(0.055)**	(0.035)**	(0.027)	(0.023)*	(0.017)
ELA * Age 30-34	-0.038	0.026	0.028	-0.011	0.026	0.070	0.075	0.030	0.019	0.021
	(0.049)	(0.032)	(0.044)	(0.019)	(0.035)	(0.067)	(0.046)	(0.033)	(0.028)	(0.025)
ELA * Age 35-39	-0.036	0.036	0.022	-0.022	0.018	0.044	0.069	0.033	0.003	0.012
	(0.041)	(0.030)	(0.041)	(0.016)	(0.030)	(0.077)	(0.048)	(0.038)	(0.028)	(0.032)
ELA * Age 40-44	-0.058	0.023	0.032	-0.024	0.007	0.020	0.023	0.032	-0.028	0.008
	(0.042)	(0.026)	(0.043)	(0.014)*	(0.028)	(0.080)	(0.052)	(0.042)	(0.029)	(0.036)
ELA * Age 45-49	-0.060	0.016	0.024	-0.026	0.013	0.022	0.033	0.015	-0.035	0.009
	(0.038)	(0.025)	(0.045)	(0.016)*	(0.030)	(0.082)	(0.052)	(0.043)	(0.031)	(0.039)
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	0.228	0.332	0.317	0.240	0.346	0.217	0.205	0.192	0.192	0.184
Mean of DV for 14-19	0.863	0.889	0.930	0.843	0.982	0.007	0.001	0.004	0.005	0.000
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 presents estimates from equation (1) from a probit. Estimates are from a separate regression on the indicated group. “ND” indicates that disclosure requirements were not met for this estimate. See table 8 notes.

Table 10. Summary of Log Hourly Wage Decompositions

Statistic	Total Difference	Effect of Education	Effect of Job Training	Effect of Experience	Effect of Occupation	Effect of Marriage	Unexplained Difference
Mean	0.091	0.029	-0.008	0.056	0.003	0.001	0.010
		(31.4)	(-8.6)	(61.8)	(2.9)	(1.2)	(11.4)
25th percentile	0.081	0.020	-0.009	0.067	0.000	0.002	0.002
		(25.1)	(-11.1)	(82.2)	(0.0)	(1.9)	(2.2)
50th percentile	0.102	0.045	-0.016	0.059	0.003	0.001	0.009
		(43.8)	(-15.4)	(57.6)	(3.4)	(1.2)	(9.0)
75th percentile	0.090	0.038	-0.009	0.051	0.009	0.005	-0.003
		(41.9)	(-10.0)	(56.1)	(9.7)	(6.0)	(-3.7)

Statistic	Total Difference	Effect of Marriage	Effect of Occupation	Effect of Experience	Effect of Job Training	Effect of Education	Unexplained Difference
Mean	0.091	0.004	0.026	0.054	-0.009	0.006	0.010
		(4.3)	(28.3)	(59.5)	(-9.7)	(6.3)	(11.4)
25th percentile	0.081	0.006	0.019	0.059	-0.008	0.003	0.002
		(7.0)	(23.5)	(73.0)	(-9.4)	(3.7)	(2.2)
50th percentile	0.102	0.001	0.038	0.058	-0.010	0.006	0.009
		(1.0)	(37.1)	(56.5)	(-9.6)	(6.0)	(9.0)
75th percentile	0.090	0.006	0.043	0.047	-0.015	0.012	-0.003
		(6.4)	(48.0)	(52.4)	(-16.1)	(12.9)	(-3.7)

The numbers represent the difference in log hourly wages at different points in the distribution between women with ELA and those without after reweighting for the specified factors. Percentage differences are in parentheses. The unexplained difference is the residual not accounted for by the five factors. The second panel reverses the order of the decompositions.

Data Appendix

This appendix summarizes the creation of the variables used in the analysis.. 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.

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 was used to check birth reports, but, in most cases, the modal reported year and month of birth was used.[30] 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.[31]

State of residence

The geocode 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 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.

ELA

By researching state laws, the authors compiled a listing of the years in which each state legally allowed unmarried women (of age 20) 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 ($r = 0.94$).

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)

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[30] The exact code is available from the authors upon request.

[31] The early waves sampled respondents in the early months of the year but later waves sampled respondents in later months.

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.

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.[32]

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

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[32] Prior to 1995, the not-in-labor-force (NILF) categories that correspond to the main activity of the survey week included "going to school," "keeping house," "unable to work," and "other." From 1995 on, these categories include "retired," "disabled," and "other."

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.

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 (see above) and our best estimate for the number of weeks worked.

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.)

Nevertheless, 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 5 apply the first and second measures but exclude the third in the interest of maintaining a larger sample size.

Finally, for the descriptive figures, we employ a common baseline of age 24 by subtracting earlier experience between 1967 and that age. This addresses the fact that the *NLS-YW* lacks retrospective information on work hours prior to 1967.

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 scheme. "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 4b, 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.

Duncan index

The Duncan index is a socioeconomic measure of an occupation's prestige based on a weighted average of the typical education and income of men who held it around the middle of the last century (See Duncan, 1961). A variable for this index is available in the survey for respondents' current or most recent job for each survey wave through 1993.

Wages, earnings, and total family income

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. In all years but 1975, 1977, and 1980, there is also a variable for the total family income of the respondent if she is not a dependent living with her parents or guardians. Total family income is defined as the sum of individual income components for each adult family member and includes wage and salary income; business and farm income; interest, dividends, and rents; Social Security, pensions, disability, and worker's compensation; government assistance; and alimony, child support, and transfers from relatives. 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 and total family income are subject to censoring from above, with the top code varying across years. In the analysis, hourly wage outliers (less than 2 or more than 100 real dollars) are excluded, and the problem of top-coding in the other variables is addressed by using a censored normal regression.

IQ & 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). The agency that processed the *NLS-YW*, the Center for Human Resource Research (CHRR), converted these scores from various tests 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 http://jenni.uchicago.edu/evo-earn/IQ.pdf.) 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. Regressions, available upon request, show no correlation between each year's interview status and *ELA*.

Variables Used in Table 1 Balancing Tests

(1) **Father born in U.S.**: binary variable equal to one if a respondent's father was born in U.S./Canada . About 95% of sample had father born in U.S./Canada.

(2) **Mother born in U.S.**: binary variable equal to one if a respondent's mother was born in U.S./Canada. About 97% of sample had mother born in U.S./Canada. *This test did not meet disclosure restrictions and is not reported. However, there appeared to be no significant difference in the proportion of women with ELA with foreign born mothers.*

(3) **Rural residence:** binary variable equal to one if a respondent resided on a farm/ranch or in another rural area at age 14. About 25% of the sample lived in a rural area at age 14.

(4) **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% of the sample lived with two parents at age 14.

(5) **Father worked for pay:** binary variable equal to one if a respondent's father worked for pay when respondent was 14. About 93% of the sample had a father working for pay at age 14. (Note: This is *not* conditional on having a father in the HH).

(6) **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 62% 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).

(7) **Magazines in home:** binary variable equal to one if a respondent had magazines available at home when she was age 14. About 63% 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% 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% of the sample did.

(10) **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 17% of the sample had a father working in a professional job. (Note: This is conditional on having had a father working at age 14).

(11) **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 2% of the sample had a mother working in a professional job. (Note: This is conditional on having had a mother working at age 14).

(12) **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.

(13) **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.

(14) **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.

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

(16) **Duncan index of household head**: Duncan index socioeconomic job score of head of household when respondent was age 14; conditional on head (not necessarily father) working when respondent was 14. (The scale runs from 3 to 97).

(17) **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.

(18) **Respondent's IQ score**: continuous IQ score of respondent. Reference distribution is independent national norm, not empirical sample. Only 2/3 of entire sample had an IQ or achievement test administered; while these 2/3 were slightly above national norms, the presence of an IQ score is uncorrelated with *ELA.*

(19) **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).