Assisted Reproductive Technology and Women's Choice to Pursue Professional Careers *

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Abstract

We examine the impact of assisted reproductive technology on women's choice to pursue professional careers. We hypothesize that the availability of assisted reproductive technology increases the expected benefits of a professional degree by allowing women to delay childbearing in their 20s and 30s while establishing their careers, thereby reaping greater financial benefit from human capital investment. We exploit the state and time level variation in the enactment of insurance mandates to cover infertility treatments in employer sponsored health plans, as well as cohort variation in women's age at the time the laws are passed. These insurance mandates dramatically increase access to assisted reproductive technology. Using a triple-difference strategy, we find that a mandate to cover assisted reproductive technology does increase the probability that a woman chooses to invest in a professional degree and to work in a professional career.

Keywords: Occupational choice, insurance mandates, fertility, professional careers, professional degrees, assisted reproductive technology.
JEL codes: I13, I26, J13, J24

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1 Introduction

Advances in reproductive technology have had a significant impact on women's labor force choices. For example, as shown in Goldin and Katz (2002), the ability to delay childbearing through the use of oral contraceptives increased women's age at first marriage, allowing them more time to invest in professional degrees. However, the expected benefits of investing in a professional degree are likely to be mitigated if a woman postpones having children until merely the early years of her career. This is due to the well documented fact that taking breaks from work early in one's career can result in significant penalties to lifetime earnings (Miller, 2011). Moreover, these costs have climbed over the past decades as the fraction of women entering professional and more highly compensated occupations has increased.[1]

Another factor entering into a woman's career decision is a biological constraint. If women delay childbearing, they are faced with declining fecundity as they age. While contraceptives make it easier for women to postpone childbearing, there remains a biological limit to that delay. It can be argued that the costs of interruption to a professional career diminish the further into the future a woman can delay having children, since a longer time horizon at work allows her to better establish her career. For example, she may acquire more job-specific human capital or obtain a promotion before taking time off for childbearing. Once she has accumulated more experience, a woman is better able to retain her value to her employer, and thus mitigate potential losses to her lifetime earnings.

In this paper, we examine the impact of assisted reproductive technology (ART) on women's

[1] For example, women received only 5 percent of law degrees conferred in the United States in 1970, but 47 percent of new law degrees in 2010. (NCES 2011)

education and career choices. By potentially expanding the time horizon for childbearing to a later point in a woman's professional career, ART may increase the expected returns to investing in professional degrees with a resulting change in occupational choice. Although earlier research has shown that the increased efficacy and availability of assisted reproductive technology (ART) has had a significant impact on the use of infertility treatments (Bitler and Schmidt, 2012), marriage timing (Abramowitz, 2014), fertility among older women (Schmidt 2007, Buckles 2013) and allocation of labor supply over the life cycle (Buckles, 2007), the existing research does not speak directly to the impact of ART on human capital investment and career choice.

ART is distinct from other reproductive therapies in a crucial way. Not only is ART extremely expensive, but unlike other reproductive technologies such as contraceptives, it is generally not covered by insurance. Moreover, to the extent that health insurance coverage for ART is available, it is typically the result of state government mandates. Thus, in states where such mandates exist the costs of postponing childbearing in order to invest in human capital should be lower. In particular, this cost decrease will be the most significant to women in careers that carry high wage returns to advanced schooling and intensive on the job training. This characteristic of the ART mandates generates an interesting policy question: are women who reside in states where ART coverage is mandated more likely to invest in career oriented human capital?

Using a a difference in difference in differences (triple-difference) approach, we find that the presence of a mandate to cover ART increases the probability that college educated women are in professional occupations by 1.1 percentage points, and increases the probability of completing a professional degree by 2.1 percentage points. Our definition of treatment by the mandate allows us to interpret our results as the causal effect of access to ART on occupational choice. These results support the hypothesis that young women view the early years of a professional career as an investment period, in which declining fertility is a significant cost of investment.

The remainder of the paper is organized as follows: section 2 provides a brief background on infertility and infertility insurance mandates, section 3 presents the conceptual framework, section 4 describes the dataset, section 5 explains our empirical strategy, section 6 discusses the results, section 7 shows robustness checks, and section 8 concludes.

2 Background

According to the Centers for Disease Control and Prevention (CDC), fecundity begins to decrease rapidly after a woman turns 35, and roughly one third of couples that wait to conceive until after the woman is 35 will experience fertility problems. While maternal age is not the only cause of infertility, it is the most common factor seen when a couple has difficulty conceiving. There are many medical options for treating infertility, including drugs to encourage ovulation, intrauterine insemination, surgery, or a class of procedures known collectively as assisted reproductive technology (ART). ART includes all fertility treatments in which both ova and sperm are manipulated in the laboratory, and primarily refers to in-vitro fertilization (IVF) and extensions of this procedure.[2]

We use data from the 2002 National Survey of Family Growth to describe differences in the use of infertility treatments by age group. Figure 1 shows the fraction of women who received advice by age group; this rate is clearly increasing with age. Figure 2 shows the use of various infertility treatments broken down by type of treatment and age group. Ovulation drugs are the most common type of treatment, followed by artificial insemination, surgery and in-vitro fertilization (IVF). Less invasive and less expensive treatments, such as medications that induce ovulation,[3] are the most common infertility treatment chosen for younger women (women aged 25 to 34). Infertile women older than 40 are more likely to be treated with artificial insemination and IVF. The use of surgery is higher among women who are older than 30.

Infertility treatments are extremely expensive and generally not covered by health insurance. In 2006, Resolve.org quoted the American Society of Reproductive medicine in reporting the average cost of one IVF cycle to be $12,400 (2006 dollars).[4] Online cost calculators available from https://www.univfy.com/ and http://ivfcostcalculator.com/ estimate that the average cost for medication, pre-cycle procedures, and one cycle of IVF for a 35 year old woman is approximately $19,000 to $20,000 (2014 dollars). Moreover, it often takes multiple cycles to achieve a pregnancy: the CDC reports that 22% of all IVF cycles performed in 2009

[2] Other types of medical treatment included in ART are gamete intrafallopian transfer (GIFT), zygote intrafallopian transfer (ZIFT), embryo cryopreservation, egg or embryo donation, and gestational carriers (http://www.resolve.org/family-building-options/ivf-art/).

[3] For example: Clomid.

[4] http://www.resolve.org/family-building-options/insurance_coverage/the-costs-of-infertility-treatment.html. This figure does not appear to include the costs of ovulation medication and pre-cycle procedures.

resulted in a live birth, but this success rate varies with maternal age and health. As a result, in the absence of insurance coverage it is realistic to expect out of pocket expenses close to $100,000 to achieve a healthy birth through IVF. The high cost of ART is the primary obstacle that keeps women with infertility from receiving this form of treatment. The total cost of infertility treatments also includes lost wages, travel and accommodation costs, medical testing costs, and cryogenic storage of embryos.[5]

Currently only 25% of health care plans contain coverage for any form of fertility treatments and the extent of this coverage varies by state (William M. Mercer Company 1997; Bitler and Schmidt 2012). Virtually all coverage of ART procedures only exists due to state level legislation. Although most health insurance companies do not cover ART as a matter of course, there are now fifteen states which mandate that group health plans (generally employer-sponsored health plans) must include coverage for certain infertility treatments.

Hamilton and McManus (2012) and Bitler and Schmidt (2012) both demonstrate that IVF usage increased substantially as a result of the mandates to cover ART. This suggests that insurance companies are unlikely to cover ART treatments in the absence of a such a mandate. These policy changes provide a unique source of exogenous variation in access to ART, because they vary at the state and year level independently of individual women's career choices. To date only nine states (Arkansas, Connecticut, Hawaii, Illinois, Maryland, Massachusetts, Montana, New Jersey and Rhode Island) have passed laws explicitly mandating coverage of IVF in employer health care plans. New York, Ohio and West Virginia have passed laws that

[5] http://www.sart.org

mandate coverage of some fertility treatments but exclude IVF. Texas, California and Louisiana have passed laws that only require insurers to offer such plans to employers; the employers still have the option to choose plans that do not include ART coverage. Table 1 summarizes the laws passed from the different states and their timing. Our coding is based on information collected on the RESOLVE website and on the coding previously done by Bitler and Schmidt (2012).[6] We refer to the group of nine states with a mandate to cover ART as the "mandate states". The three states requiring coverage of some fertility procedures but not IVF, and the three states mandating only that insurance companies offer such coverage are referred to as "weak mandate" states.

In our analysis, we focus on the impact of the mandates to cover infertility treatments that include IVF: that is, our treatment group is made up of the mandate states. Our control group comprises states that never passed any type of fertility coverage mandate. As in Buckles (2007, 2013), we completely exclude the weak mandate states from our main analysis of the mandates. Including these states as non-treatment states might compromise the control group and decrease the precision of the estimated treatment effect.[7]

Previous studies show that the insurance mandates affect ART utilization and fertility outcomes among older women. In particular, Schmidt (2007) finds that the mandates increase

[6]Our coding of the mandate states is identical to the coding of Bitler and Schmidt (2012) for all states but Connecticut and Ohio. Connecticut passed a law on 2005, which replaces the pre-existing law from 1989. Our coding reflects this change. According to Bitler and Schmidt, Ohio had a mandate to cover between 1990 and 1997; after 1997, the mandate became a mandate to offer. We were unable to find supporting documents for this statement; here we consider Ohio as having a mandate to offer IVF coverage since 1990.

[7]As a robustness check, we also check the results when all "weak mandate" states are included in the treatment group; these results are discussed in Section 4.

first birth rates for older women. Machado and Sanz-de-Galdeano (2011) show that the infertility insurance mandates increase age at first birth without changing total completed fertility. Furthermore, these first order results are concentrated among older women belonging to a relatively high socioeconomic status (Bitler and Schmidt 2012, Buckles 2013).[8] Ohinata (2011) and Abramowitz (2012) find evidence that mandates delay the timing of the first child among white, highly educated women. The heterogeneous first order effects of these mandates suggest that any impact on higher education should also be limited to a particular type of woman. In the context of this study, we expect the mandates to be relevant for the career decisions of college educated women who were relatively young when a mandate was introduced. Importantly, Buckles (2007) finds that mandates increased participation in the labor force for women younger than 35 and decreased participation for those women older than 35. This suggests a reallocation of labor supply away from later in the life cycle to earlier in the life cycle resulting from delayed fertility breaks.

3 Conceptual Framework

In this section, we use a simple conceptual framework to explain how infertility insurance mandates may affect a woman's decision to invest in a professional degree. This framework is inspired by a model of human capital formation (Becker, 1975) combined with a model of the

[8]Women with a higher socioeconomic status and more education are more likely to hold employer-based health insurance (Bitler and Schmidt 2012, p. 125) and to afford the high out-of-pocket expenditure associated with the use of infertility treatments even with insurance coverage.

effect of motherhood timing on wages (Miller, 2011).

Individuals invest in graduate education if the expected benefits of the education exceed the expected costs. Expected costs include the direct costs of a graduate degree–such as tuition and books–as well as the opportunity cost that is due to foregone earnings. Expected benefits are measured as the increase in lifetime earnings that result from obtaining the degree. Suppose that a woman can start working immediately after graduating from college (at age 22) or she can invest in a professional degree. Figure 3 depicts the shift in the age-wage profile that occurs with a graduate degree. The blue line represents the age-wage profile of a person who enters the labor force directly after college and continues working for the duration of her career life. The red line represents the profile of someone who invests in further schooling after college. We assume that wages increase with experience; this is reflected in the positive slope of both age-wage profiles. It is standard to also assume that the age-wage profile is concave: wages grow faster in the early phases of the career due to investment in training and positive but decreasing returns to experience. If a woman invests in a professional degree, her earnings are negative while she is in school. After graduation, her wages are higher than the wages of bachelor graduates of the same age and increase at a faster rate, reflecting intensive training and higher returns to experience.

Using data from the NLSY79,[9] Miller (2011) shows that women experience a reduction in their wages after motherhood, which is consistent with a fixed cost of motherhood and a downward shift in the wage profile. She also finds that motherhood causes women's returns to expe-

[9] National Longitudinal Survey of Youth 1979

rience to decrease after motherhood, resulting in a flatter wage profile and diminished average earnings for the duration of the life cycle. Miller notes that the decline in returns to experience may be especially strong for women working in professional occupations, because these occupations exhibit particularly steep earnings profiles early in the career (p. 1077). Accordingly, in this framework we focus on the reduction in earnings caused by a decline in returns to experience.

Figure 4 depicts the conceptual earnings penalty of fertility for professional women. A professional woman who never has a child and works continuously will remain on the solid red path, and one who interrupts her career for a period of time will move onto a dotted red path. The two dotted red profiles illustrate the impact of an interruption early in the career (when the profile slope is very steep) versus an interruption later in the career (after the profile has flattened). Following Miller (2011), we assume that women may have up to one child and motherhood causes a career interruption that lasts for a fixed amount of time. During a career interruption, women earn no wages. Upon returning to work after a maternity interruption, women's wages are lower than their pre-interruption wages: Miller refers to this drop as a "motherhood penalty." Moreover, after returning to work their wages grow at a slower rate than before, due to relatively lower returns to experience for mothers than for non-mothers.

As shown in Figure 4, motherhood reduces the expected benefits of a professional degree through four channels: foregone wages during career interruption, reduced wages when returning to work, reduced experience and lower returns to experience. Since foregone wages increase with women's age at motherhood, this channel predicts a greater cost associated with interruptions later in the career. Reduced wages when returning to work–possibly due to human capital depreciation–make early motherhood more costly in terms of lifetime earnings. Wage penalties resulting from career interruptions and lower returns to experience are decreasing in age at motherhood. As a result, these two channels are linked to a greater cost for interruptions that occur early in the career. Miller provides empirical evidence that the magnitude of these last three channels dominates the foregone wages effect. Delaying motherhood results in higher earnings over the course of the life cycle, as a result of both increased wages and greater hours worked (Miller, 2011). These results are supported by Buckles' (2007) findings that ART mandates increase wages for older women, suggesting that the ability to delay can meaningfully decrease the motherhood wage penalty.

Given the empirical evidence that a delayed interruption is significantly less costly to professional women than is an early interruption, we assume that women would tend to delay fertility if it were not for the biological constraints.[10] By allowing women to delay the birth of their first child, infertility insurance mandates raise the expected benefit of a graduate degree.

4 Data

In this section, we describe the dataset on women's careers and present summary statistics for the main sample used in the empirical analysis. The primary data set comes from the nation-

[10]This is a strongly simplified model of reality in which we ignore all other costs of delayed fertility other than the cost of lost earnings. These ignored costs certainly include personal preferences for fertility timing.

ally representative March Supplement of the 1977-2012 Current Population Survey (CPS).[11] The CPS contains information on women's education and occupation as well as their demographic characteristics. Throughout the analysis we restrict the sample to civilian women aged 35 to 64 with at least a college degree,[12] since only women with a college degree can apply for a professional degree program.[13] The age restrictions are intended to capture working age women who are beyond college and who are likely to have already completed their professional degree had they decided to invest in one.[14] In fact, the majority of women who invest in professional degrees complete their degree by age 35 (Figure 5).

Using the Integrated Public Use Microdata Series (IPUMS) classification for occupations, we define a woman to be in a professional occupation if she is currently working or if she has worked in one of the following occupations in the past 5 years: attorney, judge, physician, dentist or veterinarian. Accordingly, we identify professional degrees as JDs, LLS degrees, MDs, and DDS/DMDs.[15] From 1992 onward, the CPS data allow us to observe if a woman has completed a professional degree, although the particular field of degree is not explicitly stated. Prior to 1992 only the total years of post-secondary schooling is observable, but not the type of degree

[11] We exclude survey years before 1977 because prior to 1977 not all the states are identified individually.

[12] Prior to 1992, post-high school degrees cannot be differentiated in the CPS. For survey years 1977-1991,we define an individual as having a college degree if she has completed four years of college. From 1992 onwards, a respondent is said to have a college degree only if she has a Bachelor's Degree or higher.

[13] A degree in veterinary sciences is an exception. However, most of those who apply for veterinary school have a bachelor degree.

[14] Most traditional students will have completed college by age 25. See Goldin, Katz, Kuziemko (2006).

[15] These occupations and degrees correspond directly to the observations for which the variable *EDUC* is equal to 124 in the March CPS data.

completed. Hence we use only the CPS 1992-2012 for the analysis of completed education.

Panel A of Table 2 shows the descriptive statistics for the 1977-2012 sample from the March CPS, constructed with use of sampling weights.[16] The main sample used in the analysis includes mandate states and states that did not pass any mandates. We refer to this sample as the "mandate sample": Columns 1 to 3 show the statistics for the mandate sample. For robustness, we also use a "weak mandate sample" which includes both the mandate states and the weak mandate states in the treatment group: Columns 4 to 6 describe the weak mandate sample.[17] The mandate and weak mandate samples exhibit similar key characteristics.

Panel B of Table 2 represents only observations from the years 1992-2012, which is the dataset used for the analysis of completed degrees. Again, the first three columns are for the mandate sample, and the next three are for the weak mandate sample. As in Panel A, descriptive statistics for the mandate sample and weak mandate sample are comparable.

Conditional upon college completion, approximately 3.9% of American women have a professional degree, and 3.3% have a doctoral degree. 2.2% of women report working in a professional occupation. Given the small fraction of women who complete a professional degree or work in a professional occupation, even a modest effect of a mandate to cover IVF would be meaningful. The sample size for the occupational choice analysis is 196,489 and includes survey data from 1977 to 2012. The sample for analysis of educational choice contains 128,044

[16] Following the IPUMS documentation, we weight each observation by the variable *wtsupp*.
[17] This less restrictive treatment group is made up of both states that enacted legislation to mandate IVF coverage and also those passing mandates to offer ART coverage, or mandates to cover infertility treatments but explicitly excluding IVF related procedures.

women, and spans 1992 to 2012.[18] We also control for other demographic characteristics (marital status, race, age): although these characteristics are unlikely to be correlated with the presence of a mandate,[19] they are certainly correlated with the outcomes of interest and including them improves the efficiency of the estimation. Roughly 84% of our sample is white, 89% have been married at least once, and the mean age at the time of survey is 46.8 years.

There is a difference in the rate of treatment for the occupation sample (Panel A) and the degree sample (Panel B). 9.6% of the occupation sample were treated with a mandate to cover before age 35. Due to the data restrictions mentioned above, only 1.2% of the degree sample were treated by age 35.[20]

5 Empirical Strategy

5.1 Endogeneity of mandate timing

There may be a concern that states' decisions to pass a mandate are a function of state-level economic and cultural characteristics that are correlated with women's career decisions. For example, a state that has a high proportion of women working as attorneys and physicians might as a result have a greater propensity to pass an ART coverage mandate. Buckles (2007) argues that the mandates were not the result of lobbying efforts by professional women who

[18]This sample excludes all states that passed a mandate before 1992.

[19]It should be noted that Abramowitz (2014) finds a relationship of the mandates on marriage timing. However, controlling for marital status does not change our results in the triple difference specification, as discussed in section 5.

[20]Between 1992 and 2012, only Connecticut and New Jersey passed an insurance mandate to cover infertility treatments.

wanted to delay fertility. To explore this issue further, we use data from the 1980 Census (since West Virginia is excluded, 1980 is prior to the passage of any mandates).

Table 3 shows the difference in means for a set of state-level characteristics between the treatment group and the control group in 1980.[21] The treatment and control groups display statistically different means for completed fertility and percentage working in agriculture: women in the treatment states have 0.2 fewer children than women in the control states, and a lower fraction of the population is employed in agriculture in treatment states compared to control states. However, the majority of measurable state level characteristics do not vary significantly between mandate and non-mandate states. In general these two groups are not obviously differentiated.

Additionally, we test whether state level characteristics observed before the mandates can predict the policy changes using multivariate regression analysis. We regress the probability that a state passed a mandate to cover on various state level indicators obtained from the 1980 Census. We find that lower completed fertility and older age at first marriage of women aged 35 to 44 are correlated with passing a mandate to cover. The results are reported in the Appendix (Table A1).

In light of these two factors, as well as potential unobservable factors, we control for state level characteristics in the following way. In the empirical strategy illustrated below, we always include state fixed effects to control for unobservable, time invariant state-level factors that

[21] When choosing which controls to add, we followed previous research on the effect of the availability of the pill on women's career. We include most of the controls in Table 2 of Bailey (2006).

might affect completed fertility as well as women's choice to invest in professional degrees. We also include a set of survey year dummies interacted with a dummy for living in an ever-treated state to account for any time-specific shocks that affected only mandate states.

5.2 Differences-in-differences-in-differences

One option for estimating the treatment effect is a difference-in-differences strategy that compares outcomes of women across states and years. For this estimation strategy to be valid, mandate and non-mandate states should exhibit parallel trends prior to the enactment of the mandates. Figure 6 displays the weighted average of the fraction of women in professional occupations in states with a mandate to cover ART: years are normalized such that year zero reflects the period in which a mandate was passed. The graph shows an increase in the rate of women in professional occupations that begins several years after the mandate was passed (t=4). The rate keeps increasing in the subsequent years. This is consistent with an effect of the mandate as it takes at least four years for women to apply to graduate school, enroll and graduate with a professional degree. However, there is also evidence of a smaller increase in women working in professional careers at the time of the mandate (year zero). This increase cannot be due to the laws as it takes some time for changes in women's investment behavior to be reflected by changes in degrees. The concern is that states that passed an ART coverage mandate and states that did not pass such a law were following different trends before the legislation. If this was the case, when using a difference-in-differences analysis one could wrongly interpret differences in pre-existing trends as a causal effect of the law.

To overcome this concern, we estimate the effect of mandates to cover infertility treatments using a triple difference specification (DDD). Since the mandates should only affect career choices of women who are relatively young when the laws are enacted, we can exploit the inter-cohort variation to identify the causal effect of the mandates. We can estimate the impact of insurance mandates by comparing the outcomes of women across states, calendar years and birth cohorts.[22] Here, women who were older than 35 when the mandate was passed are used as a within-state control group.[23] We assume that most women over the age of 35 will not be facing the decision of whether or not to invest in a professional degree or career (recall Figure 5). Importantly, we observe the occupations and education of women in the CPS even if they are not currently in the labor force.[24]

We consider an individual woman to be treated if her state of residence enacts legislation mandating ART coverage by the time she turns 35. Although age 35 also coincides with the age at which fertility starts to decline dramatically, our focus is on the timing of the investment decision rather than the timing of fertility.

[22] Gruber (1994) first used a DDD strategy to study the impact of insurance mandates to cover maternity benefits on women's wages and employment. In the context of infertility insurance mandates, Schmidt (2007) first used a DDD strategy to study the effect of the mandates on fertility.

[23] Our identification strategy is close to Schmidt (2007), with two main differences. First, she uses older women as treatment group and younger women as control group. Second, Schmidt (2007) exploits variation across women's age in the calendar year. Instead, here we exploit variation across women's age at the time of the mandates.

[24] This is conditional on having worked in the past 5 years. This is a necessary condition in order for us to use older women as a control group. If we only observed the data for working women, older women would form a poor control group given that the ART mandates have a negative effect on their labor supply, as shown by Buckles (2007).

We estimate the following regression using a linear probability model:

$$\begin{aligned}Pr(Y_{iskt}=1) = \alpha + &\beta_1 \mathit{EverMandate}_s \times \mathit{PostMandate}_{st} \times \mathit{35orYoungerMandate}_{sk} \\ &+ \beta_2 \mathit{EverMandate}_s \times \mathit{PostMandate}_{st} + \beta_3 \mathit{EverMandate}_s \times \gamma_k \\ &+ \beta_4' X_{iskt} + \sum_{kt} \mu_{kt} \sum_s \delta_s \times t + \delta_s + \gamma_k + \tau_t + \epsilon \end{aligned} \tag{1}$$

EverMandate$_s$ is equal to 1 if state s enacted a law requiring ART at any time during the span of our dataset. *PostMandate*$_{st}$ takes value 1 if the survey year is at least four years after the mandate year in a mandate state, and defined as 0 for all years for all control states. *35orYoungerMandate*$_{sk}$ is equal to 1 if cohort k in state s experienced a mandate by the time they turned 35; it is defined as 0 for all cohorts older than 35 at the time of a mandate and for all cohorts living in a control state. δ_s is a vector of state fixed effects that account for time-invariant unobservable factors at the state level. τ_t denotes year fixed effects that control for macroeconomic conditions or changes in federal law that are common to all states. γ_k represents cohort fixed effects that account for policy changes that affect all women born in the same year. *EverMandate*$_s \times \gamma_k$ is an interaction of *EverMandate*$_s$ and cohort fixed effects. This interaction term controls for cohort-specific changes within states that passed a mandate to cover infertility treatments. $\sum_{kt} \mu_{kt}$ are cohort-year fixed effects that control for any time-varying policy or shocks that affect all women born in the same year.[25]

[25] Our specification could be also written as:

$$\begin{aligned}Pr(Y_{iskt}=1) = \alpha + &\beta_1 \mathit{Mandate}_{t-4} \times \mathit{35orYoungerMandate}_{sk} + \beta_2 \mathit{Mandate}_{t-4} + \\ &\beta_3 \mathit{EverMandate}_s \times \gamma_k + \beta_4' X_{iskt} + \sum_{kt} \mu_{kt} \sum_s \delta_s \times t + \delta_s + \gamma_k + \tau_t + \epsilon\end{aligned}$$

Additionally, we control for individual race, age (at survey year) and marital status characteristics. Finally, to address the concern that the mandates may be endogenous, we include state-specific linear and quadratic time trends: $(\sum_s \delta_s \times t)$ and $(\sum_s \delta_s \times t^2)$. Throughout our analysis, we cluster the standard errors at the state level to account for within group serial correlation in the error terms (Bertrand et al., 2004).

β_4 measures the DDD effect of the mandates on relative outcomes for women 35 or younger at the time of the mandate, before and after the mandates were enacted, in states that passed the mandates compared to control states. The identifying assumption requires that, in the absence of the mandate, differences in outcomes between older women and younger women would have evolved similarly in states that passed a mandate and states that did not pass a mandate (Gruber 1994).

6 Results

Table 4 displays the results of the analysis pertaining to the mandate to cover treatment effect on the probability that a woman works in a professional occupation. In Column 1, we measure the raw difference in professional occupation rates between women in the treatment states and women in the mandate states, for the period 1977 - 2012. The coefficient on *EverMandate* indicates that college educated women in the treatment states are 0.6 percentage points more

where $Mandate_{t-4}$ is an indicator that takes value one after the mandate was passed in a mandate state. This notation is similar to the one used by Schmidt (2007).

likely to work in a professional occupation than college educated women in the control states. The regression specification used in Column 1 does not contain any additional controls, so this coefficient measures the average difference between the two groups across time and cohorts. In Column 2, the variable *(EverMandate)X(PostMandate)* is the interaction of the treatment group indicator (*EverMandate*) and a binary variable (*PostMandate*) that equals one when the woman is observed after the legislation has been enacted in her state. This specification is the difference in differences result across states and time. The mandates increased professional occupation rates among women in treatment states by 0.6 percentage points more than professional rates increased within control states during the sample period. Inclusion of state fixed effects (Column 3) in place of the *EverMandate* indicator does not alter the magnitude of this estimate greatly, but the estimated coefficient decreases slightly to 0.005 and becomes marginally insignificant.

In Columns 4 through 7, we examine the triple difference effect in order to account for the variation across state, time, and birth cohorts simultaneously. The treatment should only affect women who are relatively young at the time of the mandate. Hence we look at variation between birth cohorts who were younger than 36 years old and the time of the mandate and those birth cohorts who were already at least 36 years old. The triple difference effect is the coefficient on the variable *(EverMandate)X(PostMandate)X(35orYounger)*. The estimated triple difference effect in Column 4 is 0.010 and significant at the 1% level. The effect is robust to the inclusion of a full set of fixed effects: state, survey year, birth cohort, and the interactions *(Cohort)X(Survey Year)* and *(Cohort)X(Ever Mandate)*.

In Column 5, we add demographic controls for race and marital status to the regression model used in Column 4. The inclusion of these additional factors does not change the coefficient of interest. This strengthens the result in Column 4, as it implies no correlation between treatment and individual level demographic characteristics beyond the state, year, and cohort level trends. The signs of the demographic controls are as expected. There is a significant and negative sign on the *Black* indicator and a positive sign for *Other Race*–primarily due to the greater tendency of Asian Americans to work in professional occupations. *Ever Married* is negative and significant, which is consistent with the fact that never married women are more attached to the labor force than married women, and tend to have greater levels of human capital. Additionally, we examine the impact of state specific time trends. Neither the inclusion of linear time trends (Column 6) nor quadratic time trends (Column 7) diminish the magnitude or significance of the triple difference treatment effect. The magnitude of the effect actually increases slightly when state specific trends are included. All together, these findings confirm our hypothesis that the mandates to cover infertility treatments appreciably increase the probability that women enter a professional occupation.

The estimate for the coefficient on the triple difference treatment effect is 0.010 and statistically significant at the 1% level in Columns 4 and 5, implying that introduction of a mandate to cover ART increases the probability of being in a professional occupation by 1 percentage point.[26] Within the context of 2.1% of women in the sample working in a professional occupation for the whole sample, this is a large effect. Given that women's rate of professional

[26]This coefficient is represented by β_1 in Equation 1.

occupation increased by 1.9 percentage points between 1985 and 2012, and that the share of women exposed to a mandate increased by 0.169, a coefficient of 0.010 suggests that the ART mandates explain roughly 10% of the increase in professional occupations among women nationally ($0.169 \times 0.010 = 0.17$ percentage points).

We follow a similar procedure to examine the effect of the mandates to cover ART on the probability that a woman completes a professional degree. The results are displayed in Table 5: Column 1 shows the raw group difference in professional degrees, Columns 2 and 3 present the difference in differences across states and time, and Columns 4-7 show the triple difference effect, which is our preferred measure of the treatment effect.

As with the occupation analysis, we see that the most meaningful variation in the outcome is due to variation across three dimensions rather than simply across states and time. This underlines the critical difference in the treatment of women who still face an human capital investment decision and those who are past the investment period of their lives. The raw difference in treatment group and control group shown in Column 1 is positive in the direction of the treatment group, but with a t-statistic of only 1.6. The sign of the difference indicates a greater propensity for investment among treatment state residents, as we anticipate. The state-time variation is positive, but insignificant as well. However, we do observe a positive and significant triple difference effect across treatment groups, survey years, and age cohorts. Various forms of the triple difference specification are presented in Columns 4-7, and all of these specifications include the pairwise interactions of *(CohortXYear)* and *(CohortXEverMandate)*, as well as state, survey year, and birth cohort fixed effects. The triple difference treatment effect is robust to the inclusion of demographic controls (Column 5) and state level linear and quadratic time trends (Columns 6 and 7, respectively). In Column 4, the effect of the mandate is 0.021 and significant at the 5% level. This indicates that the difference between relatively young cohorts and relatively older cohorts is 2.1 percentage points wider in the treated states than in the control states.

The magnitude of this effect indicates that approximately 25% of the increase in professional degrees among women can be attributed to the ART mandates. Between 1992 and 2012 the fraction of women with a professional degree increased from 3.3% to 4.4% and the treatment rate in our sample expanded from 3.9% to 16.9% (a change of 13 percentage points). The predicted change between 1992 to 2012 is $0.13 \times 0.021 = 0.27$ percentage points, which is about one fourth of the observed 1.1 percentage point change.

Taken as a whole, these results do provide evidence that legislation which mandates that group health insurance plans cover ART (specifically, IVF procedures) have a significant causal impact on women's occupational choice, and increases the likelihood that women enter professional careers. These findings are consistent with a conceptual framework that predicts that the ability to reallocate fertility from early in the life cycle to later in the life cycle increases the benefits to women of pursuing a professional degree.

In Table 6, we consider the difference in the effect of a mandate to cover ART and simply a mandate to offer plans that include ART coverage. We expand the set of states in the treatment group to include states that passed a mandate to offer insurance for infertility treatments. The effect of a mandate to cover is estimated as the triple difference effect *(EverMan-*

date)X(PostMandate)X(35orYoungerMandate), while the the effect of the weaker laws are measured as the triple difference effect *(EverWeakMandate)X(PostWeakMandate)X(35orYounger-WeakMandate)*. (Recall that states that passed a mandate to cover or a mandate to offer are referred to collectively as the weak mandate treatment group.) Pooled regression of professional occupation on both types of treatment yields results that show the strong mandate effect is significantly larger than that of a weak mandate (testing for equality between the two coefficients produces a p-value less than 0.01). The same result holds for the effect of the mandates on professional degree–in fact the weak mandate has a negative effect on investment in professional education in Columns 5 and 6.

6.1 Replication within US Census

As a supplement to the CPS dataset, we also use data from the decennial US Census (5% public use samples from 1980, 1990, and 2000, downloaded from IPUMS). For comparability with the CPS results, the Census sample is also made up of all college-educated women aged 35 to 64 at the time of the survey, for whom we can observe a three-digit occupation code (this is conditional on having worked in the past 5 years). The Census data adds value to the analysis because of the increased sample size and the ability to measure education data starting in 1980. However, the education data for 1980 is not completely comparable with the more recent years. We can identify professional degrees and doctoral degrees in the 1990 and 2000 Census samples, but the education information for the 1980 sample is less detailed. In 1980, post-secondary education is specified only as the number of years of college completed by the individual. Hence, within the 1980 Census sample we use an outcome that combines professional degrees and doctoral degrees, defined as having completed at least seven years of college. While this is not ideal, the alternative is to forgo the 1980 sample from the analysis. Given that six states enacted a mandate to cover IVF between 1980 and 1990, the 1980 sample adds considerable value.[27]

The impact of a mandate to cover within the Census dataset broadly corroborates the findings from Table 4. The Census results are shown in Table A2. There is a positive and significant impact of the mandates on occupational choice, although magnitude is noticeably smaller in the Census dataset (the estimated coefficient on the triple interaction term is 0.006). The effect is still sizeable: if a mandate to cover increases the probability of professional occupation by 0.6 percentage points, this would still be a 28% increase in the rate of professional occupations for the given sample. The inclusion of state specific time trends reduces the coefficient to 0.004, and the effect is not statistically significant at conventional terms. A mandate has a positive and significant effect on the probability of obtaining a professional degree in the Census sample, although the magnitude of the effect (1.2 percentage points) is smaller than the analogous result in the CPS sample (2.1 percentage points). However, the effect is still quite large: if a mandate to cover raises the probability of completing a professional degree by 1.2 percentage points, this is an 18% increase for the given sample. This result is robust to the inclusion of state specific time trends (linear and quadratic).

[27]We consider the results from the Census data as a complementary robustness check to the CPS analysis, but given the limitations of the education data in the 1980 Census as well as the lack of annual data, we prefer the CPS as the primary dataset for our analysis and inference.

Overall, the analysis of Census data confirms that a mandate to cover infertility treatments increased the probability that women enter professional careers. These results are especially important for the analysis of educational outcomes. Because data on professional degrees are not available in the CPS before 1992, the identification of the treatment effect on professional degrees using CPS data relies only on two states that passed a mandate to cover after 1992 (New Jersey and Connecticut). When we use Census data, the treatment group includes six states that passed a mandate to cover between 1980 and 2000 (Arkansas, Hawaii, Illinois, Maryland, Massachusetts and Rhode Island).

As an additional robustness check, we also use the unweighted CPS dataset. The unweighted regression results are qualitatively very similar to the main results in Tables 4 and 5 although the magnitude of the mandate coefficients is a bit smaller.[28]

7 Robustness Checks

We conduct several further robustness checks to corroborate and strengthen our findings. We consider the impact of endogenous migration and potential omission of family background characteristics, and we also refine the control group by dropping states which are very dissimilar from our treatment states.

[28] These results are available upon request.

7.1 Endogenous migration

There could be a concern that professional women are more likely to immigrate into an ART mandate state than are non-professionals, since the benefits of such migration are possibly larger for professional women. If this form of endogenous migration is present within the cohorts who were young relative to the mandate, our estimated treatment effect will be biased upwards.

Columns 1 and 2 of Table A3 report estimates for the determinants of being a "new migrant." New migrants are defined as women who were living outside the state one year before the survey.[29] We apply both the difference-in-differences and triple difference specifications, as in the earlier analyses.The mandates increased the rates of new migrant in the population, and the effect is largest for women who were 35 or younger at the time of the mandates.

However, dropping new migrants from the sample does not affect our main results. For comparison, Columns 3 and 5 report the main result (DDD) for occupational and educational outcomes using the survey years in which the migration variable is available. The estimates are very similar to those in Tables 4 and 5. In Columns 4 and 6 we drop new migrants. If anything, the point estimate increases when migrants are excluded, suggesting that women who move in mandate states are less likely to be professionals than women who were in ART mandate states already. Therefore, although there is some evidence that ART mandates attracted younger women from other states, it does not seem the case that they attracted professional women. This result alleviates the concern that endogenous migration biases the results up-

[29] This variable is available for a subset of years.

ward.

7.2 Family background and wealth

We also consider the importance of family background in determining both education and occupation outcomes. Ideally, we would control for parental wealth, however this information is not observable in the CPS. For a subset of our sample, we can observe various forms of non-wage income. In particular, we consider the importance of interest, dividend, and rental income. The sum of these variables serves as a reasonably good proxy for wealth, and intergenerational asset wealth tends to be highly correlated. Again, we find that including a control for non-wage income does not meaningfully change the mandate effect for either occupation or degree: results are shown in the Appendix (Table A4).

7.3 Refined control group

Finally, in order to improve the comparability of the treatment group's outcomes with the outcomes of the control group, we refine the control group for each policy change by utilizing the *synth* programming package developed by Abadie et al. (2010). The *synth* program generates a synthetic control group that is a weighted average of all non-treatment states during the treatment time period. The weights are chosen to match observable characteristics of the treatment group during the relevant period. We use the synthetic control group vector of weights to determine which states are the least similar to each of our treatment states–these states are dropped from the control group.[30] The remaining control group states and the refined control group regression results are shown in the Appendix. Refining the control group increases the magnitude and precision of the mandate effect: these results also indicate that the mandate to cover ART will increase the probability that relatively young women will invest in a professional career.

7.4 Placebo tests

We also conduct various placebo tests on our results, both with respect to the outcome that we defined and the timing of the mandates. In our primary analysis, we focus on a very narrow group of occupations which are known for intense investment during a worker's early career. If the effect of the mandates on career choice is not due to the impact on the investment decision, testing the result of the mandates on groups of occupation that are less investment intense early in the life cycle should reveal little or no influence of the policy changes. We test a broad range of occupation categories that require at least a college degree, within the same CPS sample as our original results.

Table A6 reports DDD coefficients from equation 1 on various alternative outcomes. The table lists the 13 categories that we consider as placebo outcomes, as well as our outcome of interests for comparison. The only categories that experience a positive and significant effect apart from our outcome of interest are the two sub categories that make up our category "Professional Occupations." The effect of the mandate on physicians is positive and signifi-

[30] We drop states that are assigned a weight equal to zero.

cant (0.004*), as is the effect for lawyers and judges (0.008***). It is notable that the impact for "other health professionals," a group that includes (RNs, Pharmacists, Nutritionists, Therapists, Physician Assistants) is significantly negative, and quite large in magnitude (-0.016**). Although we do not have sufficient data to conclude anything definitive, these results are consistent with a mechanism that changes the outcome for marginal women away from a career in the "other health professionals" category (traditionally dominated by pink collar occupations that require relatively highly skilled workers) and into a professional career in law or medicine.

We also control for the five year period prior to treatment to test for endogeneity, and do not find significant evidence of endogeneity. In addition, we conduct a placebo test in which we assign false mandate events among the control states and use the true mandate states as the control group (ignoring the actual mandates). In this placebo test, there was virtually no effect of the false mandates. These results are available upon request.

8 Conclusion

In this paper, we measure the impact on women's occupational choice of access to assisted reproductive technology, measured as an exogenous cost reduction due to state level mandates for health insurance plans. The empirical findings show evidence that mandates to cover IVF and other ART treatments do increase the probability that women choose to invest in professional degrees and work in professional occupations. In contrast, the mandates that only require insurance companies to offer ART coverage plans, but lack universal coverage among group sponsored plans, do not have a significant effect on women's choice to enter a professional career. This is consistent with our expectation that lack of universal coverage for high cost procedures does not allow for sufficient risk sharing to reduce costs for individuals. Our results are robust to the inclusion of state-specific time trends, key robustness checks, and are replicable to a reasonable degree using data from the US Census.

The results presented in this paper suggest that policies that help women to push back motherhood may help increase women's participation in professional occupations. Recently, a number of high-tech companies started offering fertility preservation to their employees (Miller 2014).[31] Our study suggests that these type of benefits could increase women's investment in highly skilled professional occupations. In particular, it is possible that the ability to delay fertility can encourage highly capable women to substitute away from traditionally pink collar careers in nursing, professional therapist jobs, and physician assistant jobs, and into the male dominated occupations in law and medicine.

[31]This practice is likely to grow more common with widely publicized announcements of such employee benefits by Facebook and Apple. http://www.nbcnews.com/news/us-news/perk-facebook-apple-now-pay-women-freeze-eggs-n225011

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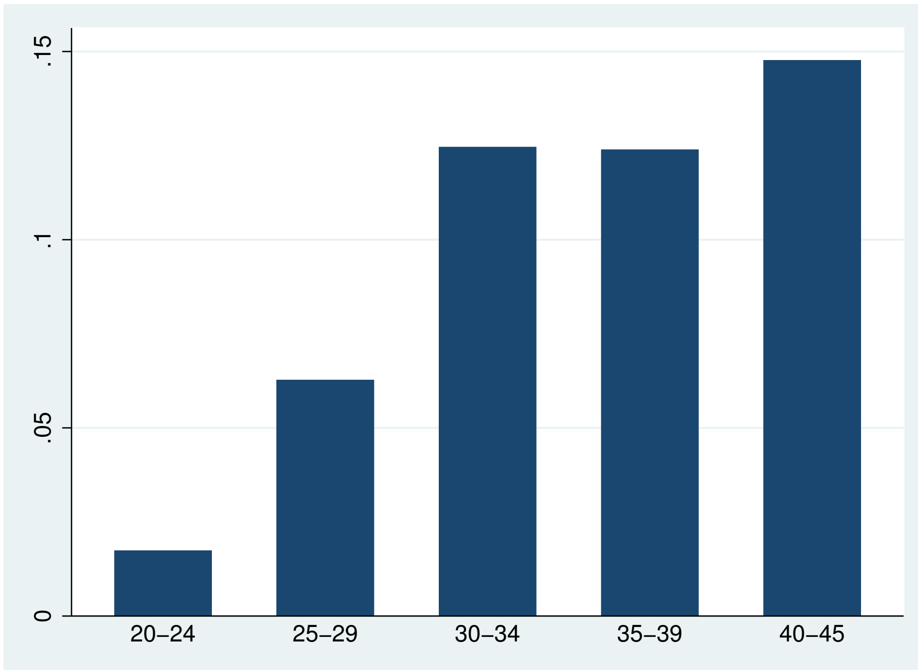
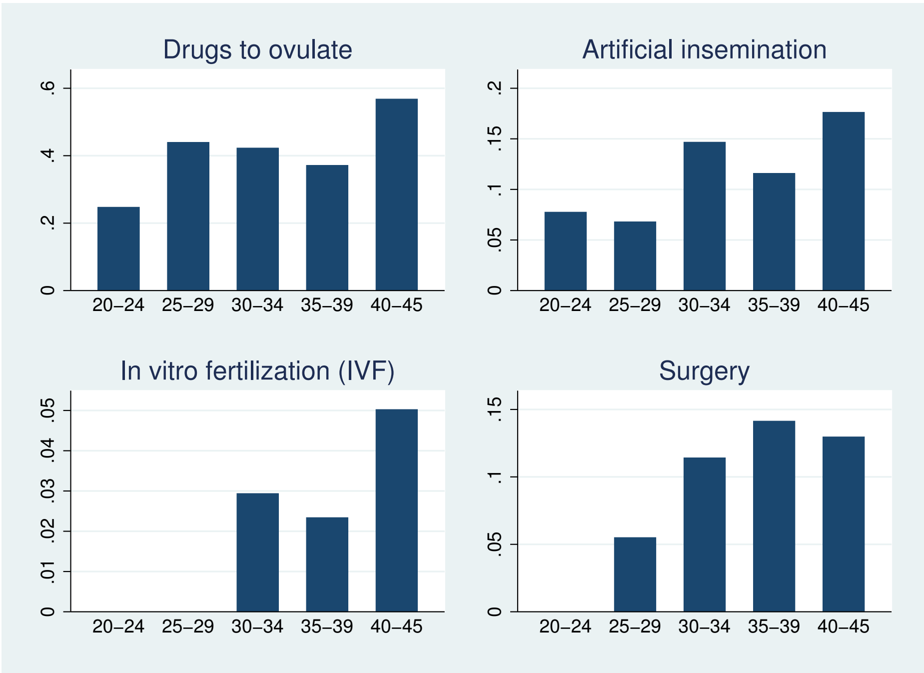
Figure 1: The Number of Women Receiving Advice for Infertility Treatment Increases with Age



Notes: Data from 2002 National Survey of Family Growth, weighted to represent U.S. aggregate population

Figure 2: The Number of Women Using Infertility Treatments Increases with Age



Notes: Data from 2002 National Survey of Family Growth, weighted to represent U.S. aggregate population

Figure 3: Conceptual Framework: Costs and Benefits of a Professional Degree

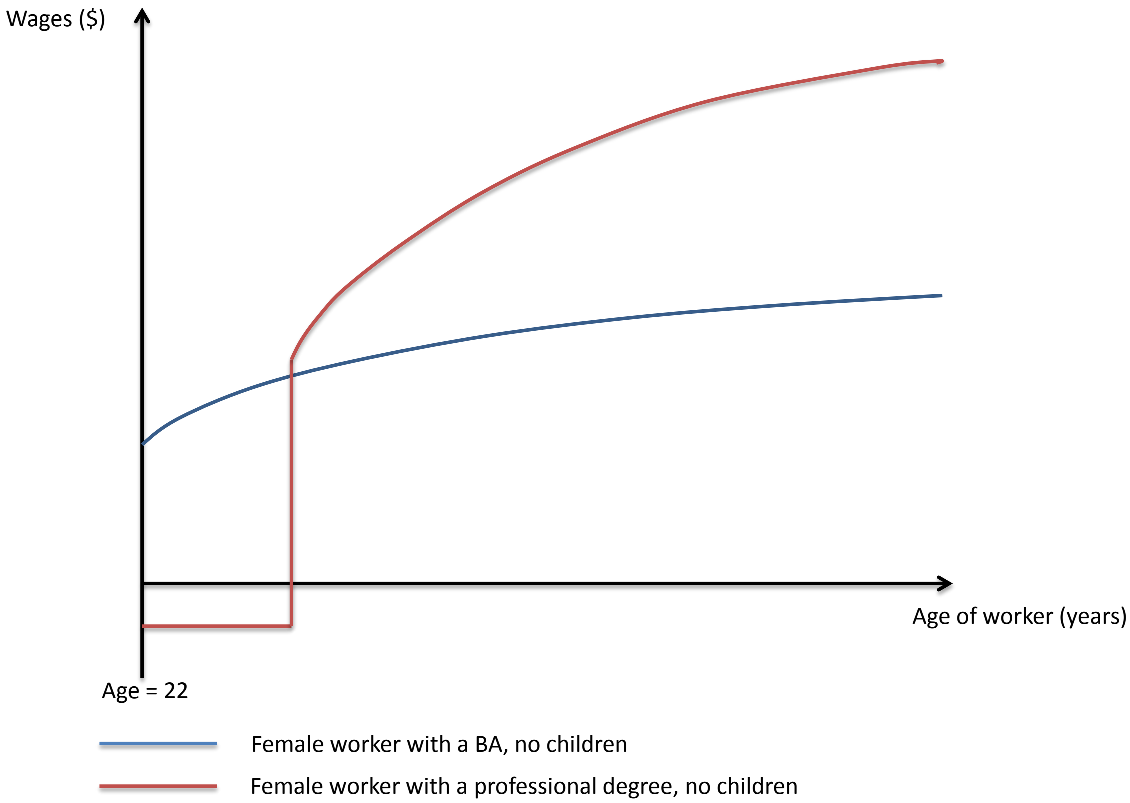


Figure 4: Conceptual Framework: Returns to Motherhood Delay

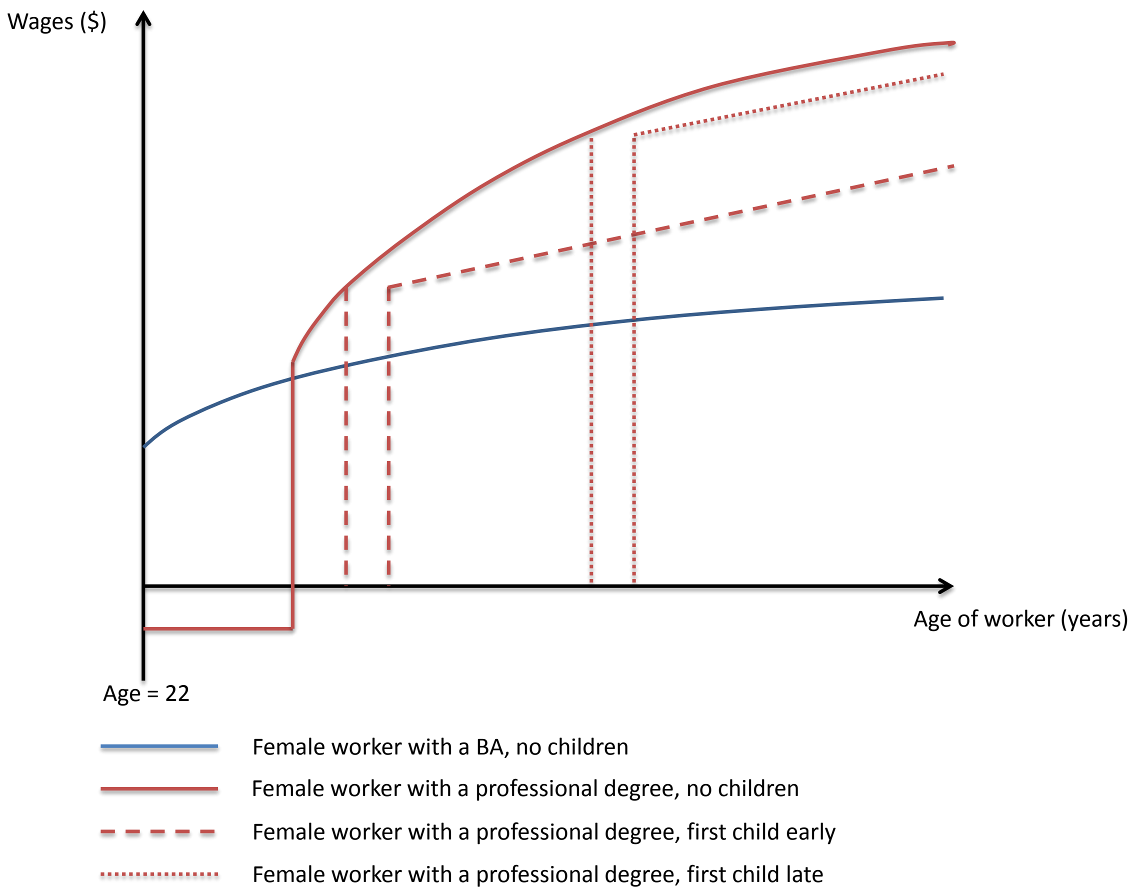
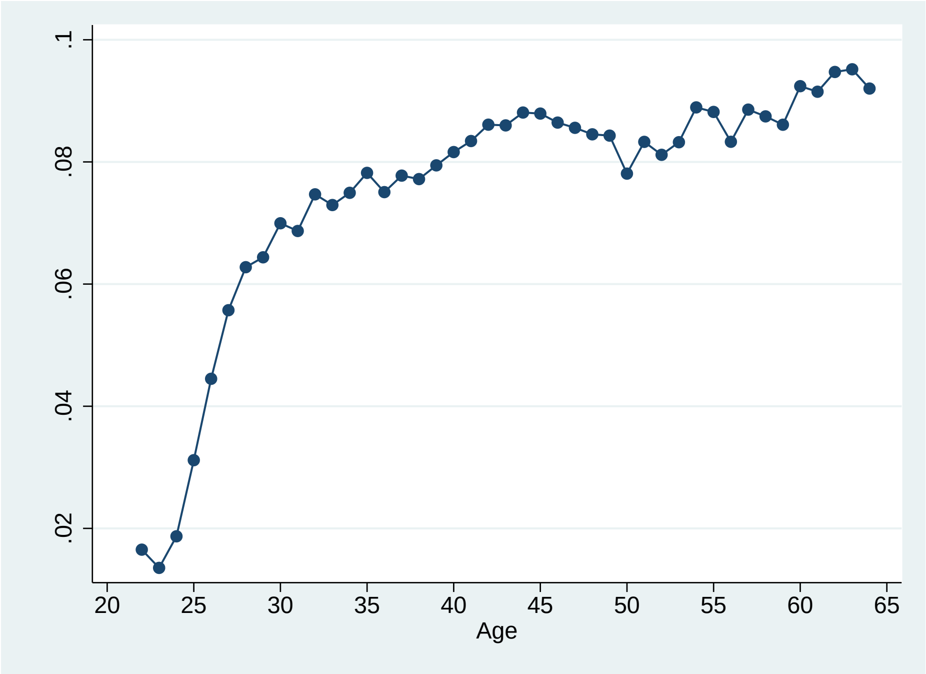
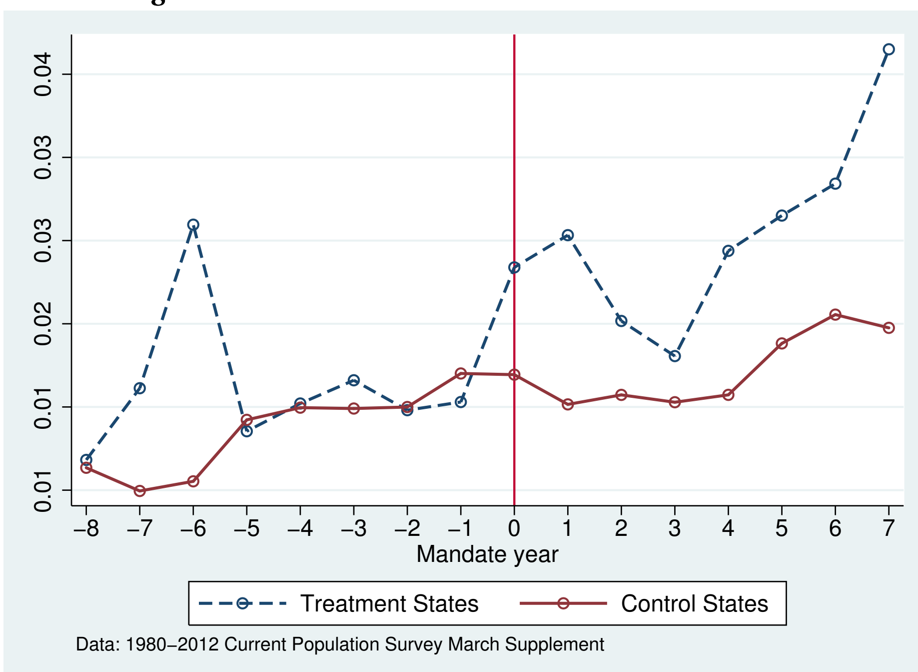


Figure 5: Professional Degrees by Age



Notes: Data from 2000 Census. Sample: college educated women aged 22-64. Data are weighted to represent U.S. aggregate population.

Figure 6: Increase in the Rate of Professional Careers



Notes: Data from the March Supplement of the Current Population Survey. Share of college educated women aged 35-64 working in a professional occupation.

Table 1: Infertility Insurance Mandates

ART Coverage Legislation by State				
State	Year	Type of coverage	Mandate	Weak mandate
Arkansas	1987	Mandate to cover IVF	x	x
California	1989	Mandate to offer. NO IVF		x
Connecticut	2005	Mandate to cover IVF	x	x
Hawaii	1987	Mandate to cover IVF	x	x
Illinois	1991	Mandate to cover IVF	x	x
Louisiana	2001	Mandate to offer		x
Maryland	1985	Mandate to cover IVF	x	x
Massachusetts	1987	Mandate to cover IVF	x	x
Montana	1987	Mandate to cover IVF	x	x
New Jersey	2001	Mandate to cover IVF	x	x
New York	1990	Mandate to cover.NO IVF		x
Ohio	1991	Mandate to cover.NO IVF		x
Rhode Island	1989	Mandate to cover IVF	x	x
Texas	1987	Mandate to offer		x
West Virginia	1977	Mandate to cover.NO IVF		x

Notes: Data from www.resolve.org and Bitler and Schmidt (2012)

Table 2: Sample Descriptive Statistics

Panel A: Sample for regressions with occupation as outcome

	Mandate Sample			Weak Mandate Sample		
	N	Mean	Std Dev	N	Mean	Std Dev
Professional occupation	196489	0.021	0.144	257341	0.022	0.147
Professional degree	154369	0.039	0.193	199549	0.039	0.194
Ph.D.	154369	0.033	0.179	199549	0.033	0.179
Professional degree or Ph.D.	154369	0.072	0.259	199549	0.072	0.259
Age	196489	46.857	8.156	257341	46.854	8.160
White	196489	0.864	0.343	257341	0.844	0.363
Black	196489	0.083	0.276	257341	0.082	0.275
Ever married	196489	0.897	0.304	257341	0.891	0.311
% women living in a mandate state	196489	0.260	0.438	257341	0.497	0.500
(EverMandate)X(PostMandate) X(35orYounger)	196489	0.096	0.295	257341	0.207	0.405

Panel B: Sample for regressions with education as outcome

	Mandate Sample			Weak Mandate Sample		
	N	Mean	Std Dev	N	Mean	Std Dev
Professional occupation	128044	0.022	0.148	129762	0.022	0.147
Professional degree	128044	0.038	0.191	129762	0.038	0.191
Ph.D.	128044	0.032	0.176	129762	0.032	0.176
Professional degree or Ph.D.	128044	0.070	0.255	129762	0.070	0.255
Age	128044	47.210	8.080	129762	47.205	8.080
White	128044	0.865	0.341	129762	0.864	0.343
Black	128044	0.083	0.277	129762	0.085	0.279
Ever married	128044	0.899	0.302	129762	0.899	0.301
% women living in a mandate state	128044	0.099	0.299	129762	0.118	0.323
(EverMandate)X(PostMandate) X(35orYounger)	128044	0.012	0.108	129762	0.015	0.121

Notes: Underlying data for Panel A are from the 1977-2012 March Supplement of the Current Population Survey. Data for Panel B are from the March CPS 1992-2012. Summary statistics computed using survey weights. Mandate is defined as "mandate to cover IVF". Weak Mandate is defined as "a mandate to offer IVF, a mandate to cover infertility treatments except IVF, or a mandate to cover IVF". State that passed a mandate to offer or a mandate to cover infertility treatments but not IVF are in the weak mandate sample but are excluded from the mandate sample.

Table 3: Comparison of Treatment and Control Groups

Variable	Mean in control states (no mandate)	Mean in treatment states (mandate to cover)	Difference	Student's T test	P-value
Labor Force Participation rate for women	0.51	0.52	-0.02	-1.09	0.30
Labor Force Participation rate for men	0.76	0.76	0.00	-0.28	0.79
Completed fertility of women aged 35 to 44	2.75	2.55	0.20	3.05	0.01
Unemployment rate for women	0.06	0.06	0.00	0.52	0.61
Unemployment rate for men	0.06	0.06	0.00	0.93	0.37
Age at first marriage of women aged 35 to 44	19.44	19.85	-0.41	-1.79	0.11
Percentage working in agriculture	0.03	0.01	0.01	3.03	0.01
Percentage Black	0.10	0.10	0.00	0.00	1.00
Average number of cars per household	1.48	1.53	-0.04	-1.15	0.27
Mean years of education for women	10.58	10.58	0.01	0.05	0.96
Mean wages among full-time employed women	10.35	10.31	0.04	0.60	0.56
Mean wages among full-time employed men	10.66	10.63	0.02	0.60	0.56
Percentage in poverty	0.15	0.13	0.02	1.37	0.20

Notes: Data from the 1980 Census. 44 observations.

Table 4: Effect of Mandate on Occupation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Professional occupation						
	D	DD	DD	DDD	DDD	DDD	DDD
EverMandate	0.006***	0.002					
	(0.002)	(0.001)					
(EverMandate)X(PostMandate)		0.006**	0.005	0.003	0.003	0.005	0.005
		(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
(EverMandate)X(PostMandate)X(35orYounger)				0.010***	0.010***	0.012***	0.012***
				(0.003)	(0.003)	(0.002)	(0.002)
Black					-0.007***	-0.007***	-0.007***
					(0.002)	(0.002)	(0.002)
Other race (non white, non Black)					0.014***	0.014***	0.014***
					(0.002)	(0.002)	(0.002)
EverMarried					-0.009***	-0.009***	-0.009***
					(0.002)	(0.002)	(0.002)
Constant	0.020***	0.005***	-0.005***	-0.010***	-0.001	0.132	0.065
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.089)	(0.045)
Survey year FE		X	X	X	X	X	X
State FE			X	X	X	X	X
Cohort FE				X	X	X	X
Cohort FE * Survey year FE				X	X	X	X
Cohort FE * (EverMandate)				X	X	X	X
State Specific Time Trends						Linear	Quadratic
Observations	196,489	196,489	196,489	196,489	196,489	196,489	196,489
R-squared	0.000	0.002	0.003	0.012	0.013	0.013	0.013

Notes: Robust standard errors, adjusted for clustering at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Underlying data from the 1977-2012 CPS
Professional occupations are defined as physicians, veterinarians, dentists, lawyers and judjes.
Mandate is legislation requiring employer sponsored health plans to cover ART.
Regressions are weighted using CPS survey weights. States that only passed a mandate to offer are excluded.
Column 1 shows the difference in average share of professionals between states that passed a mandate and states that did not pass a mandate (D).
Columns 2 and 3 (DD) show the difference-in-difference across states and survey years.
Columns 4 to 7 show the triple difference across (DDD) states, survey years and birth cohorts.

Table 5: Effect of Mandate on Degree

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Professional degree						
	D	DD	DD	DDD	DDD	DDD	DDD
EverMandate	0.004	0.003					
	(0.002)	(0.002)					
(EverMandate)X(PostMandate)		0.001	0.003	-0.000	-0.001	0.001	0.001
		(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
(EverMandate)X(PostMandate)X(35orYounger)				0.021**	0.021**	0.023**	0.023**
				(0.009)	(0.009)	(0.010)	(0.010)
Black					-0.009***	-0.009***	-0.009***
					(0.002)	(0.002)	(0.002)
Other race (non white, non Black)					0.011**	0.011**	0.011**
					(0.005)	(0.005)	(0.005)
EverMarried					-0.009**	-0.009**	-0.009**
					(0.003)	(0.003)	(0.003)
Constant	0.037***	0.031***	0.024***	0.011	0.021	-0.612***	-0.299***
	(0.002)	(0.005)	(0.004)	(0.021)	(0.023)	(0.153)	(0.073)
Survey year FE		X	X	X	X	X	X
State FE			X	X	X	X	X
Cohort FE				X	X	X	X
Cohort FE * Survey year FE				X	X	X	X
Cohort FE * (EverMandate)				X	X	X	X
State Specific Time Trends						Linear	Quadratic
Observations	128,044	128,044	128,044	128,044	128,044	128,044	128,044
R-squared	0.000	0.000	0.003	0.010	0.010	0.011	0.011

Notes: Robust standard errors, adjusted for clustering at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Underlying data from the 1992-2012 CPS
Professional occupations are defined as physicians, veterinarians, dentists, lawyers and judjes.
Mandate is legislation requiring employer sponsored health plans to cover ART.
Regressions are weighted using CPS survey weights. States that only passed a mandate to offer are excluded.
Column 1 shows the difference in average share of professionals between states that passed a mandate and states that did not pass a mandate (D).
Columns 2 and 3 (DD) show the difference-in-difference across states and survey years.
Columns 4 to 7 show the triple difference across (DDD) states, survey years and birth cohorts.

Table 6: Effect of Weak versus Strong Mandate

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Professional occupation			Professional degree		
(EverMandate)X(PostMandate)	0.005		0.006	0.000		0.001
	(0.004)		(0.004)	(0.003)		(0.003)
(EverMandate)X(PostMandate)X(35orYoungerMandate)	0.009***		0.010***	0.022***		0.020**
	(0.002)		(0.002)	(0.007)		(0.009)
(EverWeakMandate)X(PostWeakMandate)		0.006**	0.005*		0.039***	0.037***
		(0.003)	(0.003)		(0.003)	(0.003)
(EverWeakMandate)X(PostWeakMandate)X(35orYoungerWeakMandate)		-0.003	0.001		-0.022***	-0.007
		(0.002)	(0.002)		(0.002)	(0.004)
Black	-0.006***	-0.006***	-0.006***	-0.009***	-0.009***	-0.009***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Other race (non white, non Black)	0.009	0.008	0.009	0.011**	0.011**	0.011**
	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Ever Married	-0.009***	-0.009***	-0.009***	-0.009**	-0.009**	-0.009**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Constant	0.183*	0.192*	0.191*	-0.633***	-0.634***	-0.638***
	(0.095)	(0.098)	(0.097)	(0.150)	(0.150)	(0.151)
Observations	257,341	257,341	257,341	129,762	129,762	129,762
R-squared	0.010	0.010	0.010	0.011	0.011	0.011
P-value Test Weak Treatment = Strong Treatment			2.31e-05			2.51e-06

Notes: Robust standard errors, adjusted for clustering at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Underlying data in Columns 1-3 are from the 1977-2012 CPS. Underlying data in Columns 4-6 are from the 1992-2012 CPS
Professional occupations are defined as physicians, veterinarians, dentists, lawyers and judjes.
Mandate is legislation requiring employer sponsored health plans to cover ART.
Regressions are weighted using CPS survey weights.
Each column shows the triple difference across (DDD) states, survey years and birth cohorts.

Appendix

Table A1: Endogeneity of Mandate Timing

Linear probability model	(1)
Dependent Variable	EverMandate
Percentage Black	0.745
	(1.306)
Mean years of education for women	-0.454
	(0.284)
Age at first marriage of women aged 35 to 44	0.480*
	(0.270)
Completed fertility of women aged 35 to 44	-0.919*
	(0.532)
Mean wages among full-time employed women	-1.500
	(1.266)
Mean wages among full-time employed men	3.004
	(2.081)
Unemployment rate for women	-9.051
	(13.127)
Unemployment rate for men	0.703
	(10.563)
Labor Force Participation rate for women	0.432
	(4.660)
Labor Force Participation rate for men	-3.357
	(5.411)
Percentage working in agriculture	-4.102
	(5.213)
Percentage in poverty	2.588
	(5.270)
Average number of cars per household	-0.184
	(0.780)
South	-0.283
	(0.246)
Constant	-15.531
	(12.779)
Observations	44
R^2	0.409

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Mandate is defined as "mandate to cover IVF".
Weak Mandate is defined as "a mandate to offer IVF, a mandate to cover infertility treatments except IVF, or a mandate to cover IVF".
Notes: Data from the 1980 Census. West Virginia is excluded.
States that only passed a mandate to offer are excluded.

Table A2: Triple Difference Effect of Mandate (Census data)

Linear Probability Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Professional occupation				Professional degree			
	Mean=.021 (SD=.144)				Mean=.0.068 (SD=.0.252)			
(EverMandate)X								
(PostMandate)X(35orYounger)	0.005*	0.006*	0.004	0.004	0.012**	0.013***	0.012**	0.012**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)	(0.006)
(EverMandate)X(PostMandate)	-0.001	-0.000	-0.000	-0.000	-0.015**	-0.015**	-0.005	-0.005
	(0.001)	(0.001)	(0.002)	(0.002)	(0.007)	(0.007)	(0.014)	(0.014)
Black		-0.006***	-0.006***	-0.006***		-0.007***	-0.007***	-0.007***
		(0.001)	(0.001)	(0.001)		(0.002)	(0.002)	(0.002)
Other race (non white,								
non black)		0.017***	0.017***	0.017***		0.034***	0.035***	0.035***
		(0.003)	(0.003)	(0.003)		(0.004)	(0.004)	(0.004)
EverMarried		-0.007***	-0.007***	-0.007***		-0.016***	-0.016***	-0.016***
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Constant	-0.000	0.007***	-0.104***	-0.047***	0.084***	0.101***	-0.848***	-0.374***
	(0.001)	(0.001)	(0.013)	(0.006)	(0.007)	(0.007)	(0.038)	(0.020)
Survey year fe	X	X	X	X	X	X	X	X
Cohort fe	X	X	X	X	X	X	X	X
State fe	X	X	X	X	X	X	X	X
Cohort fe * Survey year fe	X	X	X	X	X	X	X	X
Cohort fe *(EverMandate)	X	X	X	X	X	X	X	X
State Specific Time Trends			Linear	Quadratic			Linear	Quadratic
Observations	833,892	833,892	833,892	833,892	833,892	833,892	833,892	833,892
R-squared	0.005	0.006	0.006	0.006	0.008	0.009	0.010	0.010

Notes: Robust standard errors, adjusted for clustering at the state level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Underlying data from Census (5% samples) 1980-2000. States that passed a mandate to offer are excluded.
Regressions are weighted using IPUMS sampling weights.
Professional degrees include JDs, LLS degrees, MDs, and DDS/DMDs.
Professional occupations are defined as physicians, veterinarians, dentists, lawyers and judges.

Table A3: Robustness check: Migration

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Migrant		Professional occupation		Professional degree	
Sample	All		All	Non-migrants	All	Non-migrants
Specification	DD	DDD	DDD	DDD	DDD	DDD
(EverMandate)X(PostMandate)	0.003**	0.002	0.004	0.004	0.000	0.001
	(0.001)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
(EverMandate)X(PostMandate)X(35orYounger)		0.005***	0.012***	0.013***	0.021**	0.024***
		(0.001)	(0.003)	(0.003)	(0.010)	(0.008)
Black		0.000	-0.007***	-0.007***	-0.010***	-0.009***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Other race (non white, non Black)		0.023***	0.014***	0.014***	0.011**	0.011**
		(0.004)	(0.002)	(0.002)	(0.005)	(0.005)
EverMarried		-0.005***	-0.010***	-0.010***	-0.009**	-0.009**
		(0.001)	(0.002)	(0.002)	(0.003)	(0.004)
Constant	0.025***	0.001	-0.001	-0.001	0.022	0.023
	(0.004)	(0.003)	(0.003)	(0.003)	(0.023)	(0.023)
Survey year FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
Cohort FE		X	X	X	X	X
Cohort FE * Survey year FE		X	X	X	X	X
Cohort FE * (EverMandate)		X	X	X	X	X
Observations	181,158	181,158	181,158	176,626	124,513	121,585
R-squared	0.005	0.017	0.012	0.012	0.010	0.010

Notes: Robust standard errors, adjusted for clustering at the state level, are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1 Underlying data from the 1981-1984, 1986-1994, 1996-2012 CPS.
Mandate is legislation requiring employer sponsored health plans to cover ART.
A woman is a migrant if she moved between states or abroad in the past year.
Regressions are weighted using CPS survey weights. States that only passed a mandate to offer are excluded. Column 1 shows the difference-in-difference across states and survey years.
Columns 2 to 7 show the triple difference across (DDD) states, survey years and birth cohorts.
In Columns 5 and 7, migrants are excluded from the sample.

Table A4: Robustness check: Non-wage income

OLS Regression	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Professional Occupation			Professional Degree		
(EverMandate)X(PostMandate)X (35orYounger)	0.011***	0.011***	0.013***	0.020**	0.021**	0.023**
	(0.003)	(0.003)	(0.002)	(0.010)	(0.009)	(0.010)
(EverMandate)X(PostMandate)	0.004	0.004	0.006	-0.000	-0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)
Black		-0.005***	-0.005***		-0.009***	-0.009***
		(0.002)	(0.002)		(0.002)	(0.002)
Other race (non white, non black)		0.015***	0.015***		0.011**	0.011**
		(0.002)	(0.002)		(0.005)	(0.005)
EverMarried		-0.008***	-0.008***		-0.009**	-0.009**
		(0.002)	(0.002)		(0.003)	(0.003)
Dividends, Interests and Rents	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.011***	-0.003	0.149*	0.009	0.018	-0.607***
	(0.002)	(0.002)	(0.083)	(0.022)	(0.023)	(0.154)
Observations	196,489	196,489	196,489	128,044	128,044	128,044
R-squared	0.013	0.013	0.014	0.010	0.011	0.011

Notes: Standard errors in parentheses are robust to clustering at the state level *** p<0.01, ** p<0.05, * p<0.1
Underlying data from the 1977-2012 CPS (Columns 1 and 2); underlying data from the 1992-2012 CPS (3)-(6)).
Regressions are weighted using CPS survey weights. States that passed a mandate to offer are excluded.
In Columns 3 to 6, states that passed the law before 1992 are excluded.

Table A5: Selective Control Group

Triple difference regression with refined control group

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Professional Occupation			Professional Degree		
(EverMandate)X(PostMandate)X(35orYounger)	0.012***	0.012***	0.014***	0.016	0.016	0.020
	(0.003)	(0.003)	(0.002)	(0.013)	(0.013)	(0.014)
(EverMandate)X(PostMandate)		0.004	0.005	0.002	0.001	-0.001
		(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
Black		-0.006***	-0.006**		-0.009**	-0.009**
		(0.002)	(0.002)		(0.003)	(0.003)
Other race (non white, non black)		0.014***	0.014***		0.018*	0.018*
		(0.003)	(0.003)		(0.009)	(0.009)
EverMarried		-0.008***	-0.008***		-0.005	-0.005
		(0.002)	(0.002)		(0.005)	(0.005)
Constant	-0.012***	-0.003	-1.233***	0.041	0.047	-3.478***
	(0.002)	(0.003)	(0.099)	(0.051)	(0.054)	(0.226)
Observations	102,608	102,608	102,608	53,865	53,865	53,865
R-squared	0.018	0.018	0.019	0.019	0.020	0.020

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
The refined control group includes states that are assigned a positive weight using the synthetic control method.

Table A6: Alternative Occupation Outcomes

OCC1990 values	Outcome-Occupation Category	DDD Effect: estimated coefficient on (EverMandate)X(PostMandate)X(35orYounger)
84-88, 178,179	Professional (our definition)	0.012***
		(0.002)
3-200	Managerial and Professional Specialty*	-0.002
		(0.008)
43-200	Professional Specialty*	0.001
		(0.008)
2-153, 165-200, excluding 95	Non-traditional: Manag. & Professional Specialty* excluding nurses and K-12 teachers	0.010
		(0.014)
43-83	Engineers, Scientists, and Architects	-0.000
		(0.003)
43-89	Engineers, Scientists, Architects and Physicians	0.004
		(0.005)
44-83	STEM: Professional Specialty in Science, Mathematics, Computer Science, Engineering	0.000
		(0.003)
166-173	Social Scientists	-0.003*
		(0.002)
96	Pharmacists	0.000
		(0.001)
178,179	Lawyers and Judges	0.008***
		(0.003)
84-89	Physicians	0.004*
		(0.002)
95-106	Other Health Professionals	-0.016***
	(RNs, Pharmacists, Nutritionists, Therapists, Physician Assistants)	(0.005)
113-154	Post-secondary teachers	0.005
		(0.004)
155-163	K-12 Teachers	0.004
		(0.006)

Notes: Sample: CPS 1977-2012, College Educated Women Ages 35-65 (n=196,489)
*"Managerial" is defined by the CPS as OCC1990 equal to 3 - 37. "Professional Specialty" is defined as OCC1990 = 42 - 201.
Triple difference regression includes controls for demographic characteristics, state, year, cohort, cohortXyear, cohortXgroup, and state specific linear trends.
Coefficients calculated using sample weights. Robust standard errors (clustered at the state level) are in parentheses.
Significance of estimates denoted as follows: *** p<0.01, ** p<0.05, * p<0.1
Our definition of professional degree includes physicians, dentists, veterinarians, optometrists, podiatrists, other health and therapy, lawyers and judges.