LUND UNIVERSITY

SCHOOL OF ECONOMICS AND MANAGEMENT

# Restrictions on Reproductive Health Care and Women's Labor Market Outcomes: Evidence from the United States

Linnea Falkeling and Karin Östling Svensson

MSc Economics

**Abstract**

This paper evaluates the impact of reproductive health care restrictions on women's labor market outcomes in the United States. We exploit the variation in restrictions on reproductive health care services across U.S. states and over time by using individual level data on fertility and labor market outcomes and state level data on reproductive health care restrictions. We apply a difference-in-differences framework using two-way fixed effects and expect that restrictions on reproductive services affect labor market outcomes through their effect on fertility. First stage estimations suggest that being exposed to more restrictive reproductive services increases the fertility rate with 9.1 percent in general, and an increase of 10.3 percent for black women when estimating sub-group effects. We find no evidence of a general effect on employment or hours worked, but some evidence that black women are employed to a lesser extent than other women when they are exposed to more restrictive reproductive health care services.

*Key words*: Reproductive health care, Abortion legislation, Fertility, Employment, Difference-in-Differences

Second Year Master's Thesis
June 2022
Supervisor: Therese Nilsson

# Contents



# List of Figures



# List of Tables

# 1 Introduction

The right to sexual and reproductive health is widely acknowledged in the developed world, but still many women face barriers to receive the reproductive health care services they require. Reproductive health care includes access to contraception, abortion services, counselling, prenatal care, and family planning (Schurr and Militz, 2020), and limiting access to these services can take different forms. World Health Organization (2020) report that many women lack access to contraceptives due to for example limited reproductive health care services and cultural or religious resistance. These services are often less accessible to the most vulnerable and poor women, therefore they stand to benefit the most by gaining greater access (Bahn et al., 2020). Moreover, facing barriers to safe, timely, geographically reachable, affordable, and non-discriminatory abortion can not only cause distress but also have financial and social implications and negatively impact women's possibilities of achieving an education as well as fully participating in society (World Health Organization, 2022). In this paper we examine the impact of restrictions on reproductive health care services and its effect on women's labor market outcomes in the U.S..

Abortion was legalized nationwide in the U.S. following the 1973 Roe v. Wade case (Kalist, 2004). Since then U.S. states have taken different directions in regards to women's access to reproductive health care. Gold and Nash (2012) report a trend of a "shrinking middle ground", where states either become more opposed to reproductive health care issues or moved to become more supportive of them. They estimate that the share of U.S. women living in states with limited abortion rights increased from 31 percent in 2000 to 55 percent in 2011. This share of women is at risk of increasing drastically as a recent document from the Supreme Court suggests that Roe v. Wade could be overturned in the summer of 2022, which could lead to a total abortion ban in almost half of U.S. states (Gerstein and Ward, 2022).

Since 2011, nearly 500 laws restricting abortion access have been passed by U.S. states (Center for Reproductive Rights, 2021). For example, in 2021 Texas imposed a law banning abortion after any fetal cardiac activity can be detected, which in practice means after around six weeks of pregnancy (Astor, 2021). Guttmacher Institute (2022e) reports that in 2022, 23 U.S. states apply so-called Targeted Restrictions on Abortion Providers (TRAP) laws, where states restrict access to abortion by for example imposing strict location and building requirements for abortion clinics without any evident benefit to the patient's health (Guttmacher Institute, 2022a). Moreover, there are many measures taken in order to restrict the individual from accessing abortion care. Twenty-five states require

a waiting period of typically 24 hours after receiving counselling before going through with an abortion (Guttmacher Institute, 2022d). Medoff (2009) reports that this type of counselling often consist of false or biased information regarding side-effects of having an abortion. Several states also impose restrictions on insurance coverage to not include abortion care (Guttmacher Institute 2022d; 2022c). While most restrictions concern abortion, some states impose restrictions on other reproductive health care services as well. For example, in six states a pharmacist has the right to refuse to distribute contraceptives (Guttmacher Institute, 2022b) and in 2016, 16 percent of women were unable to or experienced delays when attempting to access contraceptives (Johnston et al., 2017).

Many researchers have examined the link between access to reproductive health care services and women's labor market outcomes. Most of these studies have examined abortion legislation, finding a positive association between increased access to abortion and labor market outcomes, where this mechanism goes through a decrease in fertility (see eg. Kalist (2004); Bloom et al. (2009); Angrist and Evans (1999)). Kalist (2004) explains this mechanism, reasoning that improved access to abortion and contraceptives reduces unwanted pregnancies, and hence fertility rates, which in turn increases the labor force participation among women. Theoretically, Kalist (2004) further expects that women's human capital investments should increase when they gain access to abortion since the probability of a woman exiting the labor force due to an unexpected pregnancy is reduced. Thus, the rate of return to human capital will rise and labor market participation will increase (Kalist, 2004).

Empirically, the link between reproductive health care access and labor force participation has mainly been studied in the light of the 1973 Roe v. Wade case, and thus focused on the late 20th century. Less research has been made on access to abortion and other forms of reproductive health care in the U.S in the 21st century.

The aim of this paper is to examine whether restrictions on reproductive health care services have an impact on women's labor market outcomes at the extensive and the intensive margin. By exploiting variation in restrictions on reproductive services across U.S. states over time, and applying a difference-in-differences (DiD) framework using two-way fixed effects (TWFE), we examine the impact of changes in these restrictions on women's employment and hours worked between 2010 and 2021. Our hypothesis is that as states increase restrictions on reproductive services, there will be a decrease in women's employment and in the number of hours worked. We expect that this effect goes through fertility, as more restrictive reproductive services should increase the number of births and thus incentivize women to work less or not at all, to be able to stay at home

with their child. Testing the effect of more restrictive reproductive services on fertility, we find an increase of 9.1 percent in the number of births for women aged 15-29, with larger effects of 10.3 percent for young black women, but find no such effects for women aged 30-44. Examining the effect on women's employment and number of hours worked, we find no effect on employment, nor any impact on the number of hours worked for any age group.

The paper is organized as follows. The next section presents previous empirical findings on the impact of access to reproductive services on fertility and labor market outcomes. Section 3 describes the data used in this study, followed by Section 4 where we lay out the empirical specification. Section 5 presents the results and the paper ends with a concluding discussion in Section 6.

# 2 Previous Research

A large body of research studies the effects of reproductive health care access, including effects on fertility (see eg. Levine et al. (1999); Levine and Staiger (2004); Klerman (1999)), crime (see eg. Donohue and Levitt (2001); Lott and Whitley (2007); François et al. (2014)), child outcomes (see eg. Gruber et al. (1999); Rau et al. (2021)), and labor market outcomes (see eg. Angrist and Evans (1999); Kalist (2004); Bloom et al. (2009)). Different identification strategies have been used to identify causal effects, but most research on reproductive health care access focuses on abortion legislation and exploits variation in abortion legislation across space and over time (see eg. Angrist and Evans (1999); François et al. (2014); Kalist (2004)).

Previous research on the effects of changes in access to reproductive services in developed countries show evidence of increased access leading to a decrease in fertility. Levine and Staiger (2004) examine the impact of abortion restrictions on fertility in Eastern Europe and identify countries with severe restrictions, moderate restrictions, and very few restrictions. Similar to our study, Levine and Staiger (2004) uses shifts from one level of restrictions to another to assess the impact of changes in restrictions on fertility. The authors find that countries that changed from very restrictive laws to more liberal laws experienced a large reduction in fertility, while countries that changed from having modest restrictions to having abortions available on request experienced no such change. They explain that this is possibly due to a simultaneous increase in the pregnancy rate and in the abortion rate. Furthermore, González et al. (2021) examine the legalisation of abortion in Spain and Mølland (2016) analyzes abortion availability in Norway, both finding that increased access to abortion resulted in reduced fertility. Their findings on

labor market outcomes are more ambiguous. González et al. (2021) find no evidence of abortion access having an impact on labor market outcomes, while Mølland (2016) finds positive effects on earnings and labor market attachment in the short run that decreases in the long run. Madestam and Simeonova (2012) examine the impact of subsidized oral contraceptives in Sweden, finding that those eligible for the subsidy experienced a reduction in births and 2 percent higher earnings. In a panel of 97 countries, Bloom et al. (2009) estimate the effect of fertility on female labor force participation by using abortion legislation as an instrument for fertility. They find that lifting restrictions on abortion reduces fertility and estimate that a woman's labor supply during her reproductive life is reduced by two years after having a child.

In a U.S. setting, a majority of studies focus on the years prior to or around the 1973 Roe v. Wade case which legalized abortion at the national level. Levine et al. (1999) examine the effect of abortion legalization on fertility, using a DiD framework and exploiting different timings in abortion legalization across states. They find that states that legalized abortion prior to Roe v. Wade experienced a 4 percent decline in fertility compared to states where abortion was still illegal, with sub-group effects for teens of 12 percent, women older than 34 years (8 percent) and women aged 20-34 (2 percent). They also find larger effect for non-white women and unmarried women. Angrist and Evans (1999) examine the effect of abortion legalization in fifteen U.S. states prior to Roe v. Wade and its effect on schooling and labor market outcomes for teen women using an IV approach. They use the variation in fertility associated with changing abortion legislation to identify the effects of teen and out-of-wedlock childbearing on mothers' schooling and labor market outcomes. They find a negative first-stage effect on fertility that is larger for black women, while the reduced form results suggests that black teen women experienced an increase in schooling and improved labor market outcomes when abortion was legalized.

Similar to Angrist and Evans (1999) and Levine et al. (1999), Kalist (2004) also identifies heterogeneous sub-group effects for black women. He finds that the legalization of abortion has a positive effect of 2 percent on female labour force participation and that these effects are larger for black women, potentially explained by abortion rates being higher among black women than white women.

There are also studies on reproductive health care access which focus on other aspects than abortion legislation. Bailey (2006) uses exogenous variation in state consent laws to investigate the causal impact of birth control pills on the timing of a woman's first birth and women's labor force participation. The results show that legal access to the pill before the age of 21 reduces the likelihood of a first birth before age 22, increases

the number of women in the paid labor force, and raises the number of annual hours worked by 68-107 hours. One of the few studies examining several components of access to reproductive health care and its effect on labor market outcomes is Bahn et al. (2020). Similar to our study, they examine more recent U.S. legislation, focusing on indirect measures to reduce reproductive health care services by studying TRAP laws, public funding for abortion, and insurance coverage of contraception. They find that TRAP laws reduce labor mobility with 5-7 percent and decrease the likelihood of women reaching higher earning positions. Moreover, they find that public funding for medically necessary abortions increases occupational mobility and that insurance coverage for contraceptives increases employment by 34 percent, with larger effects for black women.

While most previous studies have focused on single or few components of reproductive services such as abortion legalization or contraceptive access, the aim of this paper is to broaden the scope and investigate the full reproductive health care climate and its effect on women's labor market outcomes. To be able to capture even small nuances in restrictions on reproductive services across states and over years, we use an aggregate measure of the reproductive health care climate. Additionally, in contrast to many previous studies which investigate the effect of increased *access* to reproductive services, our study will examine the impact of implementing more *restrictions* on reproductive services. We thus contribute to the research field by providing evidence on the impact of many recent changes, even modest ones, in restrictions to reproductive services.

# 3 Data

We use data on labor market outcomes from IPUMS CPS, who provide data from the Annual Social and Economic Supplement of the Current Population Survey (ASEC) (Flood et al., 2021). The ASEC is collected in March every year, and asks individuals a variety of questions on social and economic conditions. Each individual participates in the survey at most two times, resulting in a repeated cross-sectional sample ranging from 2010 to 2021 (Flood and Pacas, 2017). Apart from economic and social conditions, the IPUMS CPS dataset provides demographic information on the individual's age, state of residence, race, as well as information about their children's age and their number of children. Following Kalist (2004) and Levine et al. (1999), we restrict the data to only include women aged 15-44 to capture women in fertile ages.

As a measure of the intensity of restrictions on reproductive services in each state we use information from NARAL Pro-Choice America (NARAL) (2019). They provide information on a variety of reproductive health care issues, including contraceptives, abortion,

and insurance coverage for reproductive services. While NARAL is an organization advocating reproductive freedom, we expect their data to be reliable since it is based on laws, regulations, and statutes and the information has been used in previous studies. For example, Jacobs and Stanfors (2015) use information from NARAL to examine the effect of abortion restrictions in U.S. states on women's contraceptive use.

Each year, NARAL release a report with information on the status of women's reproductive rights in U.S. states. In the annual report, each state is assigned a grade based on a number of factors either facilitating or obstructing access to reproductive health care (NARAL, 2019). Points are added for measures that facilitate access to reproductive health care, as for example if the state guarantees insurance coverage for abortion and contraceptives, if it has codified the protections of Roe v. Wade in the law, and if it has measures that prohibits violence and interference when entering or exiting an abortion clinic. Similarly, points are subtracted if a state for example has mandatory delays or biased counseling for abortion, if it prohibits insurance coverage for abortion, and if and to what extent TRAP laws are implemented (NARAL, 2019).[1] Based on these points, NARAL assigns each state a grade (A, B, C, D, F) in their yearly reports from 2009 to 2016 (NARAL, 2016). From 2017 to 2019 they instead assign each state a category (total access, strongly protected access, protected access, some access, restricted access and severely restricted access) based on the points. No state is assigned to the total access category (NARAL, 2019). This leaves us with states being assigned to five categories. The categories and the grades are based on the same criteria and to combine the categories and the grades, we map each grade to each category, where severely restricted access is mapped to grade F, and strongly protected access is mapped to grade A.

For the purpose of identification, we re-code the grades and assign each state a value between 1 and 5 based on the categorizations. 1 indicates low intensity of restrictions on reproductive services (corresponding to grade A) and 5 indicates high intensity (corresponding to grade F). States that have many restrictions are thus assigned the value 5, while states with few or no restrictions are assigned the value 1. With reports published from 2009-2019, we have 10 years of annual assessments of the restrictions on reproductive health care services in each state.[2] [3]

After restricting our sample to women aged 15-44 we have a sample of 501,852 observations.

[1] For information on all determinants in NARAL's grading system, see Table A.1 in the Appendix.

[2] District of Columbia is only assigned a grade until 2011, and we thus exclude it from our sample.

[3] Table A.3 in the Appendix provides an overview of the values assigned to each state spanning from 2009 to 2019.

### 3.1 Descriptive Statistics

Table 1 presents descriptive statistics by each value of restriction intensity, where Column 1 includes states with low restriction intensity and Column 5 includes states with high restriction intensity.[4] Note that over time states move across intensity categories, and residents in these states are consequently included in different columns at different times. Examining the age distribution, we see that no age group is overrepresented in any category nor across categories. Moreover, we note that women in states with more restrictive reproductive services to a larger extent had a child in the last year, which gives suggestive evidence that restrictions could be linked to higher fertility. The individual labor market variables vary somewhat across columns, and there is no apparent trend regarding women in states with lower or higher intensity of restrictions. Notably we see larger shares of black women in states with more restrictive reproductive services, while the share of married women is stable across columns. The state unemployment rate differs across columns, but similar to the individual labor market outcomes, the state unemployment rate follow no apparent trend across columns. Lastly, we note that the number of observations is evidently larger in the categories with the lowest and highest intensity. This is not surprising, but only in line with the findings of Gold and Nash (2012) on a shrinking middle ground with regards to reproductive health care access.

## 4 Empirical Specification

### 4.1 The Difference-in-Differences Setup

When estimating the effect of more restrictive reproductive services on women's labor market outcomes, one must be cautious with the problems of endogeneity. Simply estimating the effect through an ordinary least squares (OLS) regression with appropriate control variables can only attempt at estimating an association, due to the possibility of endogeneity in the model. In our case, it is possible that some unobserved variable could affect both restrictions to reproductive services and labor market outcomes. This would result in biased and inconsistent estimates (Angrist and Pischke, 2008). To avoid this problem, we make use of a quasi-experimental design and exploit the exogenous variation in restrictions on reproductive services over time and across U.S. states and apply a DiD framework with a TWFE approach. All states are treated to some extent, since every state has some reproductive health care legislation and regulations in place, but states have different treatment intensities, as indicated by the restriction intensities explained

[4]Supplementary descriptive statistics by age group can be found in Table A.2 in the Appendix.

Table 1: Descriptive Statistics by Restriction Intensity

| *Intensity of restrictions (1=low intensity, 5=high intensity)* | **(Full sample)** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
|---|---|---|---|---|---|---|
| *Population by age groups* | | | | | | |
| Percent of women age 15-19 | 17.95 | 17.96 | 18.01 | 18.20 | 18.39 | 18.10 |
| Percent of women age 20-24 | 14.00 | 14.74 | 13.93 | 13.54 | 13.57 | 14.04 |
| Percent of women age 25-29 | 15.43 | 15.35 | 15.20 | 15.18 | 15.25 | 15.70 |
| Percent of women age 30-34 | 17.16 | 16.68 | 17.17 | 17.61 | 16.71 | 17.17 |
| Percent of women age 35-39 | 17.72 | 17.20 | 17.80 | 17.45 | 17.86 | 17.54 |
| Percent of women age 40-44 | 17.73 | 18.07 | 17.89 | 18.02 | 18.21 | 17.45 |
| | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** |
| *Individual characteristics* | | | | | | |
| Had a child in the last year | 0.051 | 0.049 | 0.047 | 0.055 | 0.056 | 0.054 |
| | (0.219) | (0.216) | (0.211) | (0.228) | (0.230) | (0.227) |
| Worked last year | 0.051 | 0.644 | 0.660 | 0.728 | 0.675 | 0.660 |
| | (0.471) | (0.480) | (0.474) | (0.445) | (0.468) | (0.474) |
| Hours worked per week last year | 35.201 | 34.758 | 35.213 | 34.706 | 34.816 | 35.326 |
| | (11.740) | (11.957) | (11.947) | (12.128) | (11.924) | (11.641) |
| Share of black women | 0.121 | 0.080 | 0.121 | 0.085 | 0.131 | 0.151 |
| | (0.326) | (0.272) | (0.327) | (0.279) | (0.337) | (0.358) |
| Married | 0.421 | 0.410 | 0.406 | 0.437 | 0.440 | 0.430 |
| | (0.494) | (0.492) | (0.491) | (0.496) | (0.496) | (0.495) |
| *State level characteristics* | | | | | | |
| Unemployment rate | 6.427 | 7.736 | 6.238 | 5.620 | 8.477 | 6.014 |
| | (2.419) | (2.516) | (1.967) | (1.892) | (2.281) | (2.137) |
| Observations | 501,852 | 112,297 | 42,955 | 34,080 | 29,449 | 185,133 |

in Section 3.

The DiD framework allows us to estimate the effect of a state moving from a lower restriction intensity to a higher intensity, compared to states who maintain the same restriction intensity. The estimated effect is an intention-to-treat effect, meaning that the change in restrictions on reproductive services does not affect every woman in that state, but increases the probability of a woman being affected by this change.

## 4.2 The Parallel Trends Assumption

The underlying assumption for estimating causal effects within the DiD framework is the parallel trends assumption, stating that in absence of treatment the states should follow similar trends. In our TWFE model with a discrete treatment variable and treatment occurring at different times for different states, a classic visual pre-trends cannot be analyzed. We instead test this assumption by performing an event study, specified in Equation 1.

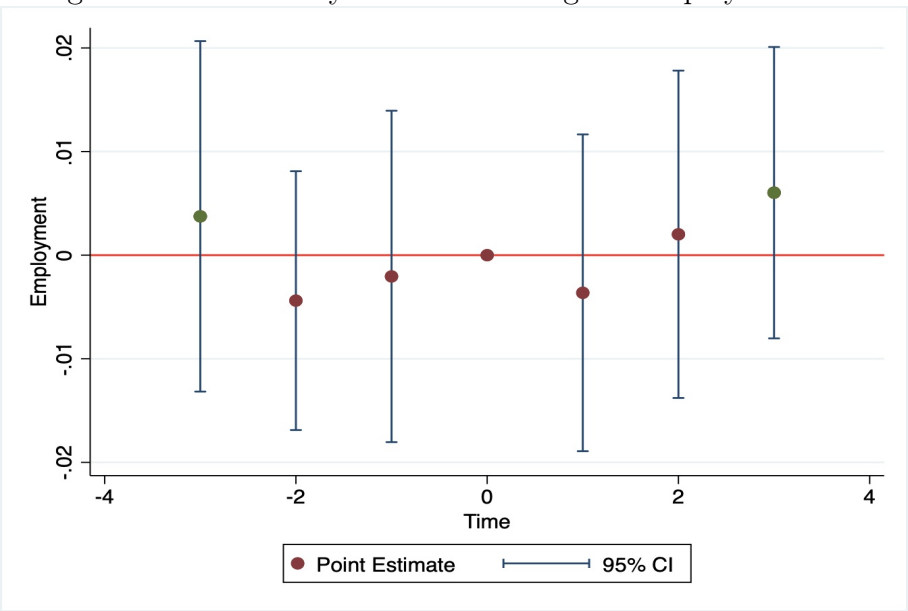
$$Outcome_{st} = \alpha_0 + \sum_{j=1}^{3} \beta_j (Lag\ j)_{st} + \sum_{k=0}^{3} \beta_k (Lead\ k)_{st} + \lambda X'_{st} + \delta_s + \gamma_t + \epsilon_{st} \qquad (1)$$

*Outcome* represents two labor market outcome variables, average hours worked and share of employed women, in state $s$ and time $t$. We include j=3 Lags and k=4 Leads, where k=0 represents the first year of the new treatment intensity and is set as reference period. $X'_{st}$ represents time-varying state controls: the share of married women, the unemployment rate, and the share of black women. $\delta_s$ represents state-fixed effects, $\gamma_t$ represents year fixed effects and $\epsilon_{st}$ is the error term.

Figures 1 and 2 present the event studies.[5] For the parallel trends assumption to be fulfilled, there should be no statistically significant effect on the dependent variable in the years prior to year zero. We can see that the parallel trends assumption is satisfied for both outcome variables, as both Figure 1 and Figure 2 show no effect significantly different from zero in the years prior to the reference year. However, we also note a lack of statistical significance in the years following year zero, which could be an indication that there might not be a statistically significant effect of more restrictive reproductive services on neither employment nor hours worked.

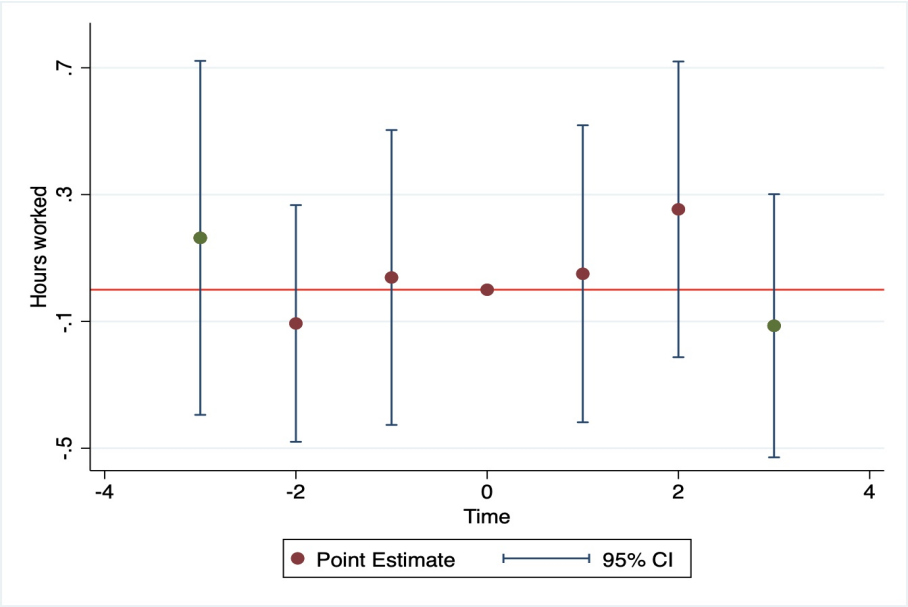
[5]We also perform event studies for younger and older women separately, following that the regressions are also performed for different age groups. These event studies show similar results and the graphs can be found in Figures A.1, A.2, A.3, and A.4 in the Appendix

Figure 1: Event Study: Extensive margin - Employment




Note: Time 0 represents the first year of a new restriction intensity. The event study is performed with probability weights.

Figure 2: Event Study: Intensive margin - Hours worked




Note: Time 0 represents the first year of a new restriction intensity. The event study is performed with probability weights.

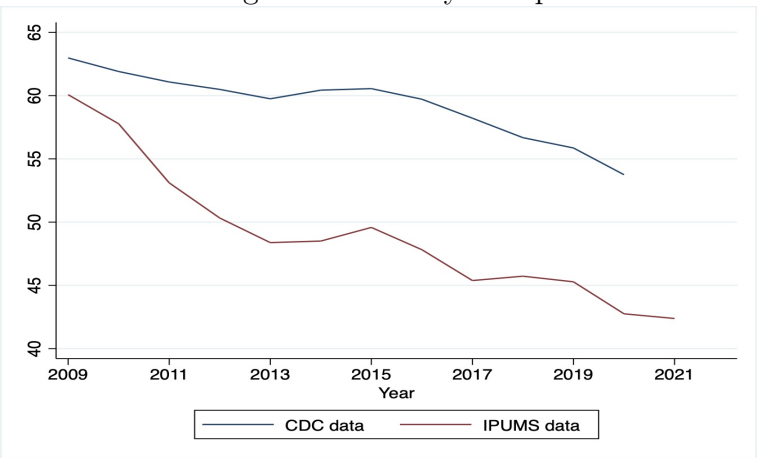
## 4.3 First Stage Specification

The mechanism through which restrictive reproductive services is expected to affect female labor market outcomes is through fertility, as more restrictive reproductive services should increase the number of births and thus incentivize women to work less or not at all, to be able to stay at home with their child (see Kalist, 2004). We therefore estimate a first stage effect of more restrictive reproductive services on fertility. First, we create a measure of the fertility rate using the IPUMS CPS data (Flood et al., 2021). The fertility rate is the number of births per 1000 women (Hamilton et al., 2021). Since the IPUMS CPS data does not provide information on when a child is born, we instead approximate the number of births each year by identifying women who have a child that is less than one year old at the time of the survey. We then calculate the approximate fertility rate by year using Equation 2:

$$Fertility\,rate = \frac{Number\ of\ children\ under\ 1\ year\ old}{Number\ of\ women} * 1000 \tag{2}$$

In order to validate our fertility measure we compare it to natality data from the CDC (2021), similar to what is done by Angrist and Evans (1999). The comparison is carried out on a national level and ideally we want to see that the trends in our sample follow the trends in the national fertility to be representative of the population. By visually examining Figure 3 we see that our fertility measure follows the CDC fertility measure over time although the magnitudes are different. Thus indicating that the fertility measure calculated from our sample is reasonably accurate.

Figure 3: Fertility Comparison




Source: CDC (2021), Flood et al. (2021)

We regress the restriction intensity measure on fertility. Ideally, we want to confirm the results from previous studies which have found higher levels of fertility when access to reproductive services is more restrictive (see eg. Angrist and Evans (1999); Levine and Staiger (2004); Klerman (1999)). We estimate the effects for both the full sample and for sub-samples of younger and older women separately to explore potential heterogeneous sub-group effects. The model is defined in Equation 3.

$$\begin{aligned} Fertility\,rate_{st} = \alpha_0 + \beta_1 Restriction\,Intensity_{st-2} + \beta_2 Black_{ist} \\ +\beta_3(Black_{ist} * Restriction\,Intensity_{st-2}) + \beta_4 Married_{ist} + \delta_s + \gamma_t + \epsilon_{ist} \end{aligned} \tag{3}$$

*Fertility rate* is defined by Equation 2, approximating the number of births per 1000 women in state $s$ and time $t$. $\beta_1$ captures the effect of a state moving from a lower restriction intensity to a higher restriction intensity. *Restriction Intensity* is the intensity measure in state $s$ and time *t-2*, meaning that it is lagged two years to allow the legislation to have an effect on fertility. One lag is made by default, since the data provided by NARAL is reported in January each year and thus represents the legislation climate of the previous year. The need for two lags comes from the fact that the data on births is reported annually in March, and thus represents the fertility rate between March in the previous year and March in the current year[6]. Additionally, the legislation on reproductive services must have been implemented before or in the beginning of a pregnancy for it to have an effect on fertility, and fertility cannot be measured until the pregnancy has passed. For these reasons we lag the *Restriction Intensity* variable two years to account for both the nature of the grading system and the fertility measure. Thereby allowing enough time to have passed from the moment of legislation to the moment of a birth.

Previous studies have found heterogeneous sub-group effects of access to reproductive health care on fertility. For example, Angrist and Evans (1999) have shown that the effect of access to reproductive services on fertility is larger for black women than for other women. We therefore include a dummy variable indicating if a woman is black. To further investigate the potential heterogeneous effect and capture the sub-group effect for black women we also include an interaction variable between the dummy and the restriction intensity measure. $\beta_3$ thus captures the additional effect of more restrictive reproductive services for black women. Since results from previous research have shown a larger effect for black women, we expect this coefficient to be positive. We perform all regressions for sub-samples of younger and older women separately to capture heterogeneous effects,

[6]For example, if some legislation occurred in 2015, we measure births between March 2016 and March 2017. This means that in the case of abortion, if a woman becomes pregnant in the end of 2015 she would either perform an abortion shortly thereafter or give birth in the autumn of 2016.

since many studies have found significant effects for young women (see eg. Levine et al. (1999)).

We also add a control for whether or not a woman is married, since being married could be an important determinant in the decision of having a child. There is a small chance of introducing endogeneity when including marital status as a control variable. This is a problem if the reproductive health care legislation and having children affect a woman's decision to get married, which might be expected to occur where traditions or norms regarding children born outside of marriage have a strong influence. Martinez et al. (2012) finds that only 5.2 percent of 15-44 year old U.S. pregnant women get married as a result of a pregnancy. We therefore expect this to be a relatively small problem and including marital status as a control is motivated by its probable effect on fertility. Nonetheless, we keep in mind this potential source of bias.

Lastly, factors such as the business cycle, political rule, and other factors that either change simultaneously in all states over time or that are constant in a particular state are captured by the state and time fixed effects, represented by $\delta_s$ and $\gamma_t$. $\epsilon_{ist}$ is the error term. All regressions are weighted with probability weights from IPUMS CPS, aiming to weight the data to be representative of the general population.

## 4.4 Main Specification

Our main analysis concerns the effect of restrictive reproductive services on women's labor market outcomes. The outcome variable measuring the extensive margin is the percentage of women between the ages 20-44 in each state who reported that they "worked at a job or a business last year". The outcome variable capturing the intensive margin is the average hours worked per week last year reported by women aged 20-44 who participated in the labor force. Note here that, in line with Bloom et al. (2009), the sample is restricted to women aged 20-44, since those younger than 20 are likely too young to have been affected by their state's reproductive health care legislation. They are also likely to still be in school and not in the labor force and we exclude them for these reasons to avoid bias in our estimates. The two estimations are specified in Equation 4 and 5.

$$Employment_{st-1} = \alpha_0 + \beta_1 Restriction\ Intensity_{st-3} + \beta_2 Black_{ist} + \beta_3(Black_{ist} * Restriction\ Intensity_{st-3}) + \beta_4 Married_{ist} + State\ unemployment_{st} + \delta_s + \gamma_t + \epsilon_{ist} \quad (4)$$

$$Hours\ worked_{st-1} = \alpha_0 + \beta_1 Restriction\ Intensity_{st-3} + \beta_2 Black_{ist} + \beta_3(Black_{ist} * Restriction\ Intensity_{st-3}) + \beta_4 Married_{ist} + State\ unemployment_{st} + \delta_s + \gamma_t + \epsilon_{ist} \quad (5)$$

The outcome variables in both equations 4 and 5 are lagged one year due to the nature of the data, and represents labor market outcomes in state $s$ and year *t-1*. $\beta_1$ measures the effect of the restrictions on reproductive services in state $s$ and year *t-3*, defined by *Restriction Intensity*. *Restriction Intensity* is lagged three years and the reasoning behind this is that restrictions in year $t$ should have an effect on fertility in year *t+1* and any effect on labor market outcomes should be seen in year *t+2*. Since *Employment* and *Hours worked* are lagged one year by default, *Restriction Intensity* is lagged three years.

Similar to Equation 3, a dummy for whether a woman is black and an interaction term is included to capture sub-group effects for black women (as found by Angrist and Evans (1999) and Kalist (2004)). The interaction coefficient should be negative, since we expect that more restrictive reproductive services have a negative effect on labor market outcomes. In line with Bahn et al. (2020) and Agüero and Marks (2008) we control for marital status in both equations, since marital status can be an important determinant for labor market decisions, keeping in mind the possibility of introducing endogeneity (discussed in Section 4.3). Additionally, we control for the unemployment rate in state $s$ at time $t$, since unemployment can differ between states and over time, and would not be captured by the state and year fixed effects ($\delta_s$ and $\gamma_t$). Lastly, $\epsilon_{ist}$ is the error term and all regressions are weighted with probability weights.

There are possible threats to the empirical specification. First, there are potential spillover effects, mainly when it comes to abortion. Smith et al. (2022) estimates that in 2017, an average of 8 percent of women seeking an abortion traveled out of state to receive abortion care. This share is higher in states considered more opposed to abortion, where 13 percent are estimated to get out-of-state abortions. Spillover effects could create a downward bias in our estimations and it would be desirable to control for these. However, data on abortions reported by some states are either incomplete or not reported at all (Dreweke, 2015) and specific data on the number of women from a particular state traveling for out-of-state abortions does, to our knowledge, not exist. We perform our analysis while keeping in mind that our estimations are likely to be downward biased. Second, considering that we have repeated cross-sectional data, we cannot follow individ-

uals over time. This means that we cannot identify women who move across states that have different reproductive health care legislation, potentially generating a small bias in either direction as they might have been exposed to the legislation in their previous state of residence while being surveyed in their current state.

Recent developments in the DiD literature have discovered problems with the TWFE approach. Goodman-Bacon (2021) shows that in a DiD framework with different treatment timing, the TWFE estimator only equals the weighted average of all possible DiD estimations done with any two states over any two time periods in the sample. The problem comes from the fact that when units have various treatment timing, treated units can act as both control and treatment. Goodman-Bacon et al. (2019) have developed a module to decompose the TWFE estimation to account for this problem, but this module demands a binary treatment variable. We cannot decompose our TWFE estimations since creating a binary treatment variable with for example high and low treatment intensity would not only remove the variation in restriction intensity, but also provide very few states that make changes that are drastic enough to cross the threshold from low to high restriction intensity in such a binary setting. Instead we simply recognize the potential bias uncovered with this development in the DiD literature.[7]

# 5 Results

This section presents the regressions results. First, we examine fertility as the mechanism through which more restrictions on reproductive services are expected to affect women's labor market outcomes. Second, we present the main results starting with the estimations for the effect of increased restrictions on the extensive margin followed by the estimations of the effect on the intensive margin. Third, we present a robustness check where we perform placebo tests in order to validate our specification.

## 5.1 First Stage Results

Table 2 shows the first stage regressions and measures the effect of an increase in state level restriction intensity on fertility.[8] The results are to be interpreted as the unit increase in the number of births per 1000 women when a state moves from a lower restriction intensity to a higher intensity, for example changing from a value of 4 to a value of 5

[7]For further description and discussion on these recent developments, see Wooldridge (2021), de Chaisemartin and D'Haultfœuille (2022) and Callaway and Sant'Anna (2021)

[8]We have tested for non-linearity in the effect of increased restriction intensity on fertility but find no evidence of heterogeneous effects at different restriction intensities.

on the intensity scale. The aim of the first stage regressions is to examine whether or not the mechanism through which more restrictive reproductive services affects labor market outcomes is credible. This credibility can be validated through the F-statistic of a regression, where an F-statistic larger than 10 is considered appropriate (Staiger and Stock, 1997). We thus include the F-statistics of each regression in the table. The table also includes the mean fertility rate of each sub-sample.

Examining the full sample in Columns 1 and 2, we see no statistically significant effect of more restrictive reproductive services on fertility, but the signs are positive and the F-statistic is above 10. The positive coefficients are in line with previous research that found a negative association between access to reproductive services and fertility (see eg. Angrist and Evans (1999); Levine et al. (1999)). When including the interaction between being black and restriction intensity in Column 3, we see that the interaction coefficient is positive and statistically significant, suggesting that black women's fertility is more affected by changes in restrictions on reproductive services.

As discussed above, restrictions on reproductive health care services can show heterogeneous effects on fertility for different age groups, as found by for example Levine et al. (1999). We thus split our sample into younger and older women aged 15-29 and 30-44 respectively in Columns 4-9. For younger women (Columns 4 to 6) we find a statistically significant and positive effect of increased restriction intensity on fertility, and an F-statistic above ten for all specifications. As can be seen in Column 4, the estimates show an increase of 4.7 births per 1000 women when increasing restriction intensity, which translates to an increase in fertility of 9.1 percent for young women. In Column 5 we include the interaction variable between being black and the restriction intensity. The interaction coefficient is statistically insignificant, while the effect of more restrictions on reproductive services for non-black women show a statistically significant increase of 4.6 births, or 9.0 percent, per 1000 women. The effect for non-black women decreases to 4.3 births, or 8.5 percent, when including a dummy for being married in Column 6, and we find a statistically significant increase of 5.3 births, or 10.3 percent, for black women as indicated by the interaction term. However, this interaction term is only statistically significant when controlling for being married in Column 6, and the estimate might therefore not be stable to including covariates.

The sub-sample with women aged 30-44 (Columns 7, 8, and 9) show no statistically significant effect of increased restriction intensity on fertility for this age group, although the coefficients are positive. This result, along with the low F-statistic in Column 7 and 8, indicates that we might not find an effect of more restrictive reproductive services on

labor market outcomes for this age group in our main regressions, since we expect the effect to go through fertility.

In general, these first stage results confirm that there is a positive effect of more restrictive reproductive services on fertility for younger women, with larger effects for young black women. These results imply that an effect of increased restriction intensity on labor market outcomes should run through fertility, which further motivates our main analysis of investigating the effect of more restrictive reproductive services on female labor market outcomes, particularly for young women.

Table 2: First stage: Fertility

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Full sample | Full sample | Full sample | 15-29 | 15-29 | 15-29 | 30-44 | 30-44 | 30-44 |
| Restriction Int. | 2.552 | 2.500 | 2.494 | 4.685* | 4.627* | 4.359* | 0.262 | 0.248 | 0.223 |
| | (1.350) | (1.351) | (1.348) | (1.799) | (1.796) | (1.726) | (1.603) | (1.599) | (1.600) |
| Black | 0.408 | -1.028 | 0.392 | -0.167 | -1.792 | 0.292 | 0.588* | 0.217 | -0.949 |
| | (0.333) | (0.955) | (0.925) | (0.415) | (1.215) | (0.963) | (0.224) | (0.506) | (0.531) |
| Black*Restriction Int. | | 0.386 | 0.485* | | 0.435 | 0.927*** | | 0.100 | 0.0742 |
| | | (0.216) | (0.201) | | (0.270) | (0.238) | | (0.135) | (0.140) |
| Married | | | 7.908*** | | | 33.52*** | | | -3.909*** |
| | | | (1.005) | | | (1.711) | | | (0.231) |
| Constant | 51.09*** | 51.27*** | 47.80*** | 52.93*** | 53.13*** | 46.75*** | 49.45*** | 49.50*** | 52.20*** |
| | (4.485) | (4.476) | (4.462) | (6.181) | (6.166) | (6.010) | (5.235) | (5.219) | (5.235) |
| Observations | 426246 | 426246 | 426246 | 202348 | 202348 | 202348 | 223898 | 223898 | 223898 |
| $R^2$ | 0.042 | 0.042 | 0.052 | 0.146 | 0.146 | 0.242 | 0.042 | 0.042 | 0.044 |
| F | 15.23 | 19.40 | 25.97 | 34.47 | 33.20 | 84.46 | 4.519 | 4.299 | 43.30 |
| Sample mean | 49.65 | 49.65 | 49.65 | 51.22 | 51.22 | 51.22 | 48.24 | 48.24 | 48.24 |

Standard errors in parentheses.

Standard errors are clustered at the state level. Regressions are weighted using probability weights. The full sample includes women aged 15-44.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

## 5.2 Main Results

The estimates of the effect of increased restriction intensity on women's labor market outcomes are presented in Tables 3 and 4.[9] We begin by examining the outcome at the extensive margin in Table 3. The dependent variable is the percentage of women who worked last year by state and year. The interpretation of the main variable of interest, *Restriction intensity*, is that increasing the restriction intensity by one unit results in a percentage point change in the share of employed women.

Examining the coefficients for the variable on restriction intensity in Table 3, we find that all coefficients are statistically insignificant and very small. Looking at the full sample in

[9]We have tested for non-linear effects, but find no evidence of increased restriction intensity having different effects on employment at different levels of restriction intensities.

Columns 1 through 3, we see no effect of increased restriction intensity on employment. However, in Columns 2 and 3 the interaction term between being black and the restriction intensity is negative and statistically significant. This implies that while we find no effect for non-black women, there seems to be a statistically significant effect of black women being employed to a lesser extent than non-black women when exposed to increased restriction intensity. We can, however, not draw any conclusion of the magnitude of the effect since the coefficient for *Restriction Intensity* is insignificant.

Turning to the sub-samples in Columns 4 to 9, we find no significant effect of more restrictive reproductive services in either regression. However, we note that the coefficients are positive in the sub-sample with younger women and negative in the sub-sample with older women. This is surprising, as the first stage results show a positive effect of increased restriction intensity on fertility for younger women but not for older women. If anything, we would thus have expected the coefficients to be negative for the younger age groups, which would indicate that young women work less when exposed to more restrictive reproductive services. Similar to the coefficients for the interaction term in the full sample, we note that the coefficients for this term are statistically significant for older women in Columns 8 and 9, suggesting that it is the older age group who drive this effect in the full sample. The coefficients are stable in regards to including covariates, suggesting that black older women are employed to a statistically significant lesser extent than non-black older women when exposed to more restrictive reproductive services.

Table 4 shows the regression estimates for the intensive margin, where we examine the effect of more restrictive reproductive services on average hours worked per week. The restriction intensity coefficients are interpreted as the unit increase in average hours worked per week when increasing the restriction intensity by one unit. The coefficients of the restriction intensity variable are negative in all columns, which is in line with our expectations that more restrictive reproductive services should reduce the number of hours worked. However, similar to the results in Table 3, we find no statistically significant effect of increased restriction intensity on the average number of hours worked. No statistically significant effect is found on the interaction terms either, in contrast to the estimates at the extensive margin. Therefore, we cannot draw any further conclusions from the regression estimates in Table 4.

The results from our main analysis show that there seems to be no general effect of more restrictive reproductive services on either labor market outcome. However, there are some indications that black women aged 30-44 are employed to a lesser extent than other women when exposed to increased restriction intensity.

Table 3: Main Regression: Extensive Margin - Employment

| | (1) Full sample | (2) Full sample | (3) Full sample | (4) 20-29 | (5) 20-29 | (6) 20-29 | (7) 30-44 | (8) 30-44 | (9) 30-44 |
|---|---|---|---|---|---|---|---|---|---|
| Restriction Int. | 0.00389 | 0.00400 | 0.00395 | 0.0103 | 0.0104 | 0.0101 | -0.000306 | -0.000257 | -0.000298 |
| | (0.00445) | (0.00444) | (0.00454) | (0.00615) | (0.00611) | (0.00612) | (0.00404) | (0.00404) | (0.00414) |
| Black | -0.000854 | 0.00183* | 0.00474*** | -0.00101 | 0.000844 | 0.00244 | -0.000108 | 0.00121 | 0.00114 |
| | (0.000465) | (0.000714) | (0.000888) | (0.000797) | (0.00260) | (0.00264) | (0.000289) | (0.000668) | (0.000718) |
| Black*Restriction Int. | | -0.000722** | -0.000517* | | -0.000497 | -0.0000506 | | -0.000355* | -0.000367* |
| | | (0.000226) | (0.000209) | | (0.000609) | (0.000653) | | (0.000165) | (0.000168) |
| Married | | | 0.0138*** | | | 0.0196*** | | | -0.000342 |
| | | | (0.00227) | | | (0.00245) | | | (0.000320) |
| State unemployment | | | -0.00109 | | | -0.000417 | | | -0.00150 |
| | | | (0.00172) | | | (0.00294) | | | (0.00157) |
| Constant | 0.714*** | 0.713*** | 0.716*** | 0.671*** | 0.670*** | 0.669*** | 0.742*** | 0.742*** | 0.756*** |
| | (0.0149) | (0.0149) | (0.0226) | (0.0203) | (0.0201) | (0.0361) | (0.0136) | (0.0135) | (0.0166) |
| Observations | 340464 | 340464 | 340464 | 122109 | 122109 | 122109 | 218355 | 218355 | 218355 |
| $R^2$ | 0.414 | 0.414 | 0.425 | 0.449 | 0.449 | 0.461 | 0.506 | 0.506 | 0.507 |
| Sample mean | 0.746 | 0.746 | 0.746 | 0.726 | 0.726 | 0.726 | 0.756 | 0.756 | 0.756 |

Standard errors in parentheses.

Standard errors are clustered at the state level. Regressions are weighted using probability weights. The full sample includes women aged 20-44.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Main Regression: Intensive Margin - Hours Worked

| | (1) Full sample | (2) Full sample | (3) Full sample | (4) 20-29 | (5) 20-29 | (6) 20-29 | (7) 30-44 | (8) 30-44 | (9) 30-44 |
|---|---|---|---|---|---|---|---|---|---|
| Restriction Int. | -0.0575 | -0.0541 | -0.0582 | -0.0614 | -0.0611 | -0.0907 | -0.0209 | -0.0207 | -0.0210 |
| | (0.134) | (0.133) | (0.127) | (0.218) | (0.218) | (0.206) | (0.0991) | (0.0993) | (0.0987) |
| Black | -0.106** | -0.0142 | 0.343*** | -0.0699 | -0.0618 | 0.0979 | -0.00661 | -0.000492 | -0.00297 |
| | (0.0366) | (0.0569) | (0.0473) | (0.0469) | (0.140) | (0.137) | (0.00824) | (0.0176) | (0.0183) |
| Black*Restriction Int. | | -0.0247 | 0.00143 | | -0.00215 | 0.0408 | | -0.00165 | -0.00179 |
| | | (0.0188) | (0.0160) | | (0.0331) | (0.0329) | | (0.00404) | (0.00412) |
| Married | | | 1.714*** | | | 1.923*** | | | -0.00933 |
| | | | (0.0649) | | | (0.0822) | | | (0.00929) |
| State unemployment | | | -0.0505 | | | -0.101 | | | -0.0130 |
| | | | (0.0533) | | | (0.0725) | | | (0.0561) |
| Constant | 36.30*** | 36.29*** | 35.87*** | 34.46*** | 34.46*** | 34.89*** | 37.45*** | 37.45*** | 37.57*** |
| | (0.423) | (0.420) | (0.597) | (0.704) | (0.702) | (0.847) | (0.324) | (0.325) | (0.624) |
| Observations | 340464 | 340464 | 340464 | 122109 | 122109 | 122109 | 218355 | 218355 | 218355 |
| $R^2$ | 0.071 | 0.071 | 0.161 | 0.069 | 0.069 | 0.131 | 0.318 | 0.318 | 0.318 |
| Sample mean | 36.442 | 36.442 | 36.442 | 34.602 | 34.602 | 34.602 | 37.432 | 37.432 | 37.432 |

Standard errors in parentheses.

Standard errors are clustered at the state level. Regressions are weighted using probability weights. The full sample includes women aged 20-44.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

## 5.3 Placebo Tests

As a robustness check we estimate the first stage regression and the regressions from the main analysis on labor market outcomes, but change the sample. Instead of restricting the samples to women aged 15-44 and women aged 20-44 respectively for the first stage regression and the main regressions, we restrict the samples to older women aged 45-65. Here we expect to see no effect as these women are typically not in fertile ages anymore and should not be affected by changes in reproductive services. The first stage results for women aged 45-65 are found in Table 5. As expected, the estimations show no statistically significant effect of increased restriction intensity on fertility, which is reassuring for the robustness of our specification. In Table 6 we turn to the effect of increased restriction intensity on employment and hours worked per week for women aged 45-65. We find no statistically significant effect of restrictive reproductive services on either labor market outcome, which further verifies the robustness of our specifications.

Table 5: Robustness check: First stage (age 45-65)

| | (1) | (2) | (3) |
|---|---|---|---|
| | 45-65 | 45-65 | 45-65 |
| Restriction Int. | -0.0413 | -0.0422 | -0.0421 |
| | (0.167) | (0.166) | (0.166) |
| Black | 0.0407 | 0.00764 | 0.0170 |
| | (0.0204) | (0.0818) | (0.0830) |
| Black*Restriction Int. | | 0.00892 | 0.00884 |
| | | (0.0194) | (0.0194) |
| Married | | | 0.0317* |
| | | | (0.0150) |
| Constant | 0.715 | 0.718 | 0.696 |
| | (0.538) | (0.533) | (0.531) |
| Observations | 276765 | 276765 | 276765 |
| $R^2$ | 0.034 | 0.034 | 0.034 |
| F | 3.922 | 4.171 | 4.164 |
| Sample mean | 0.676 | 0.676 | 0.676 |

Standard errors are in parentheses and are clustered at the state level. Regressions are weighted using probability weights. The full sample includes women aged 45-65.
$^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table 6: Robustness check: Labor market outcomes (age 45-65)

| | Employment | | | Hours Worked | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | 45-65 | 45-65 | 45-65 | 45-65 | 45-65 | 45-65 |
| Restriction Int. | -0.00199 | -0.00207 | -0.00205 | 0.0331 | 0.0328 | 0.0330 |
| | (0.00485) | (0.00485) | (0.00482) | (0.122) | (0.122) | (0.122) |
| Black | 0.00517*** | 0.00262 | 0.00429 | 0.0457*** | 0.0339 | 0.0436 |
| | (0.000854) | (0.00229) | (0.00223) | (0.0112) | (0.0228) | (0.0229) |
| Black*Restriction Int. | | 0.000690 | 0.000678 | | 0.00320 | 0.00318 |
| | | (0.000550) | (0.000552) | | (0.00680) | (0.00671) |
| Married | | | -0.00140*** | | | -0.00830*** |
| | | | (0.000171) | | | (0.00235) |
| State unemployment | | | 0.000445 | | | 0.00758 |
| | | | (0.00172) | | | (0.0296) |
| Constant | 0.683*** | 0.683*** | 0.682*** | 37.38*** | 37.38*** | 37.33*** |
| | (0.0156) | (0.0156) | (0.0226) | (0.406) | (0.406) | (0.489) |
| Observations | 271042 | 271042 | 271042 | 271042 | 271042 | 271042 |
| $R^2$ | 0.149 | 0.149 | 0.150 | 0.121 | 0.121 | 0.121 |

Standard errors are in parentheses and are clustered at the state level. Regressions are weighted using probability weights. The full sample inludes women aged 45-65.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

# 6 Concluding Discussion

In this study we examine whether restrictions on reproductive health care services have an impact on women's labor market outcomes at the extensive and intensive margin. We do this by exploiting the variation in restrictions on reproductive services across states and over time in the U.S. through a difference-in-differences framework. First, we confirm that there is a positive effect of more restrictive reproductive services on fertility for young women (age 15-29), in line with the evidence found by Levine et al. (1999), Bailey (2006), Angrist and Evans (1999), and Klerman (1999). This serves as a mechanism through which reproductive services can affect labor market outcomes. We find a general increase in fertility of 9.1 percent for young women when exposed to more restrictive reproductive services. When looking at specific subgroups, we find that the increase for young non-black women is between 8.5 to 9.0 percent, while the effect for young black women is 10.3 percent. As noted in Section 5.1, this estimate is only statistically significant when including a control for marital status. Since marital status might introduce some endogeneity into the model, the preciseness of this estimate should be interpreted with caution. Furthermore, the magnitude of the estimates is large in comparison to the findings of previous studies. Levine et al. (1999) find an average decrease in fertility of 4 percent as a result of abortion legalisation, where the decrease is estimated to 12 percent

for teen women but only 2 percent for women aged 20-34. Since (Levine et al., 1999) examines more drastic changes in legislation than we do, our findings are unreasonably high in comparison. This could indicate that there is some unobserved factor that vary differently over time in different states, which would not be captured by the state-fixed effects in our DiD framework. Still, our results imply that restrictions on reproductive services have a negative impact on fertility and that black women are more affected than other women, possibly driven by abortion rates being higher for black women, as discussed by Kalist (2004).

Our main analysis investigates whether there is an effect of more restrictive reproductive services on labor market outcomes. The results suggest that there is no general effect for the full sample of women aged 20-44 on employment or hours worked, in contrast to previous research (see eg. Angrist and Evans (1999); Kalist (2004); Bahn et al. (2020)). But looking at heterogeneous sub-group effects, we do find some evidence on the extensive margin suggesting that black women are employed to a lesser extent than other women when exposed to more restrictive reproductive services. This is in line with previous evidence that effects are larger for black women (Angrist and Evans (1999); Kalist (2004)). We expected to find effects for younger women, however in contrast to previous research the effect that we find seems to be driven by older black women (aged 30-44). This is surprising as we find no statistically significant effect of increased restriction intensity on fertility for this group. The tendency of older black women being employed to a lesser extent than other women when exposed to more restrictive reproductive services does therefore not appear to be the result of an increase in fertility. Instead, there might be some other factor affected by increased restriction intensity that could affect employment negatively for older black women.

In general, the main results give little support to the hypothesis that more restrictive reproductive services affect labor market outcomes. If anything, we would have expected to find stronger estimates on the intensive margin, since it is likely that the decision to work or not comes at a greater threshold than the decision on how many hours to work. But, the estimates are not precisely estimated for neither intensive nor extensive margins.

There are several possible explanations to why our results show small and mostly insignificant effects, in contrast to previous research. First, in contrast to other studies, we estimate the effect of large and small changes in the legislation on reproductive health care services. While most other studies examine drastic changes in abortion legislation, where it for example changes from being prohibited to being legal, we estimate effects of both small and large changes in restrictions on reproductive health care services. Our re-

sults suggests that a small change in restriction on reproductive services might not hinder women from utilizing reproductive health care services. For example, if a woman does not wish to have a child, she would likely go to great lengths to receive an abortion even though state legislation has made abortion less accessible. Such an effort could be out-of-state abortions, a factor contributing to spillover effects. Moreover, these spillover effects can provide a downward bias and thus potentially explain the low impact of increased restriction intensity on labor market outcomes. Second, it is likely that other studies, such as Angrist and Evans (1999), find larger effects on women's labor market outcomes because they focus on the years around Roe v. Wade (i.e the 1970s and the 1980s). It is likely that women's labor market choices are different today than at that time. In the 1970s and 1980s it might have been more common for women to reduce their working hours or quit their jobs in order to take care of their child. Nowadays, partners may share the responsibility of having a child and use child care services to a larger extent than before. Therefore, having a child would not necessarily affect women's labor market choices as much today as during the time around Roe v. Wade. Third, there is a possibility that, contrary to evidence from previous research, some women enter the labor force or increase their hours worked as a result of having children to be able to provide for them. This would result in a smaller net effect.

In conclusion, we show that the effect of more restrictive reproductive services varies by race and age. An increase in restriction intensity has a positive effect on fertility for young women, and is larger for black young women. However, this does not translate into an effect on labor market outcomes on either the intensive or the extensive margin. While our estimates suggests that restricting reproductive services does not impact labor market outcomes, women's access to reproductive health care holds an intrinsic value that entitles women to their reproductive autonomy. Importantly, our results should not be interpreted as indicating that reproductive health care services can be restricted without affecting women's lives, only that modest changes might not affect women's labor market outcomes.

In the changing U.S. landscape with a shrinking middle ground and a discussion of overturning Roe v. Wade in the Supreme Court, it is all the more relevant to continue research on the implications of more states moving in the direction of restricting women's access to reproductive health care. While most previous research has focused on drastic changes, future research should further investigate the implications of introducing modest restrictions in reproductive health care as this seems to be a growing trend in U.S. states.

# References


J. Agüero and M. Marks. Motherhood and Female Labor Force Participation: Evidence from Infertility Shocks. *American Economic Review*, 98(2):500–504, 2008. doi: 10.1257/aer.98.2.500. Accessed 2022-04-13.

J. D. Angrist and W. N. Evans. Schooling and Labor Market Consequences of the 1970 State Abortion Reforms. *Research in Labor Economics*, 18:75–113, 1999. URL `https://doi.org/10.1016/S0147-9121(99)18020-8`. Accessed 2022-03-25.

J. D. Angrist and J.-S. Pischke. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, Princeton, 2008.

M. Astor. Here's What the Texas Abortion Law Says, 2021. URL `https://www.nytimes.com/article/abortion-law-texas.html?` Published 2021-09-09, Accessed 2022-04-13.

K. Bahn, A. Kugler, M. Mahoney, and A. McGrew. Do US TRAP Laws Trap Women Into Bad Jobs? *Feminist Economics*, 26(1):44–97, 2020. URL `https://doi.org/10.1080/13545701.2019.1622029`. Accessed 2022-04-26.

M. J. Bailey. More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply. *The Quarterly Journal of Economics*, 121(1):289–320, 2006. URL `https://doi.org/10.1093/qje/121.1.289`. Accessed 2022-04-13.

D. E. Bloom, D. Canning, G. Fink, and J. E. Finlay. Fertility, Female Labor Force Participation, and the Demographic Dividend. *Journal of Economic Growth*, 14(2):79–101, 2009. URL `https://www.jstor.org/stable/27750779`. Accessed 2022-05-10.

B. Callaway and P. H. Sant'Anna. Difference-in-Differences With Multiple Time Periods. *Journal of Econometrics*, 225:200–230, 2021. URL `https://doi.org/10.1016/j.jeconom.2020.12.001`. Accessed 2022-05-19.

CDC. About Natality, 2007-2020, 2021. URL `http://wonder.cdc.gov/natality-current.html`. Accessed 2022-04-04.

Center for Reproductive Rights. United States, 2021. URL `https://reproductiverights.org/our-regions/united-states/`. Accessed 2022-03-30.

C. de Chaisemartin and X. D'Haultfœuille. Two-Way Fixed Effects and Differences-in-Differences With Heterogeneous Treatment Effects: A Survey. *SSRN*, 2022. URL `https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3980758`. Accessed 2022-05-19.

J. Donohue and S. Levitt. The Impact of Legalized Abortion on Crime. *Quarterly Journal of Economics*, 116:379–420, 2001. URL https://doi.org/10.1162/00335530151144050. Accessed 2022-05-09.

J. Dreweke. Abortion Reporting: Promoting Public Health, Not Politics. *Guttmacher policy Review*, 18(2), 2015. URL https://www.guttmacher.org/gpr/2015/06/abortion-reporting-promoting-public-health-not-politics. Accessed 2022-04-20.

S. Flood and J. Pacas. Using the Annual Social and Economic Supplement with Current Population Survey Panels. *Journal of Economic and Social Measurement*, 42(3-4):225–248, 2017. doi: 10.3233/JEM-180447. Accessed 2022-04-13.

S. Flood, M. King, R. Rodgers, S. Ruggles, J. R. Warren, and M. Westberry. Integrated Public Use Microdata Series, Current Population Survey: Version 9.0 [dataset], 2021. URL https://doi.org/10.18128/D030.V9.0. Accessed 2022-03-28.

A. François, R. Magni-Berton, and L. Weill. Abortion and Crime: Cross-Country Evidence from Europe. *International Review of Law and Economics*, 40:24–35, 2014. URL http://dx.doi.org/10.1016/j.irle.2014.08.001. Accessed 2022-04-28.

J. Gerstein and A. Ward. Supreme Court Has Voted to Overturn Abortion Rights, Draft Opinion Shows. *Politico*, 2022. URL https://www.politico.com/news/2022/05/02/supreme-court-abortion-draft-opinion-00029473. Published 2022-05-02, Accessed 2022-05-13.

R. B. Gold and E. Nash. Troubling Trend: More States Hostile to Abortion Rights as Middle Ground Shrinks. *Guttmacher Policy Review*, 15(1), 2012. URL https://www.guttmacher.org/gpr/2012/03/troubling-trend-more-states-hostile-abortion-rights-middle-ground-shrinks. Accessed 2022-05-02.

L. González, S. Jiménez-Martín, N. Nollenberger, and J. V. Castello. The Effect of Access to Legal Abortion on Fertility, Marriage, and Long-term Outcomes for Women. Working paper, Barcelona School of Economics, 2021. URL https://bse.eu/research/working-papers/effect-abortion-legalization-fertility-marriage-and-long-term-outcomes-women. Accessed 2022-05-13.

A. Goodman-Bacon. Difference-in-Differences With Variation in Treatment Timing. *Jour-*

*nal of Econometrics*, 225:254–277, 2021. URL https://doi.org/10.1016/j.jeconom.2021.03.014. Accessed 2022-05-19.

A. Goodman-Bacon, T. Goldring, and A. Nichols. BACONDECOMP: Stata Module to perform a Bacon decomposition of difference-in-differences. 2019. URL https://ideas.repec.org/c/boc/bocode/s458676.html. Accessed 2022-05-19.

J. Gruber, P. Levine, and D. Staiger. Abortion Legalization and Child Living Circumstances: Who is the "Marginal Child"? *Quarterly Journal of Economics*, 114(1): 263–291, 1999. URL https://www.jstor.org/stable/2586953. Accessed 2022-04-02.

Guttmacher Institute. Counseling and Waiting Periods for Abortion, 2022a. URL https://www.guttmacher.org/state-policy/explore/counseling-and-waiting-periods-abortion. Accessed 2022-04-28.

Guttmacher Institute. Emergency Contraception, 2022b. URL https://www.guttmacher.org/state-policy/explore/emergency-contraception. Accessed 2022-04-28.

Guttmacher Institute. Regulating Insurance Coverage of Abortion, 2022c. URL https://www.guttmacher.org/state-policy/explore/regulating-insurance-coverage-abortion. Accessed 2022-05-02.

Guttmacher Institute. An Overview of Abortion Laws, 2022d. URL https://www.guttmacher.org/state-policy/explore/overview-abortion-laws. Accessed 2022-05-02.

Guttmacher Institute. Targeted Regulation of Abortion Providers, 2022e. URL https://www.guttmacher.org/state-policy/explore/targeted-regulation-abortion-providers. Accessed 2022-04-28.

B. E. Hamilton, J. A. Martin, and M. J. Osterman. Births: Provisional Data for 2020, 2021. URL http://www.prochoiceamerica.org/wp-content/uploads/2019/02/WhoDecides2019-DigitalEdition.pdf. Accessed 2022-05-18.

J. Jacobs and M. Stanfors. State Abortion Context and US Women's Contraceptive Choices, 1999-2010. *Perspectives on Sexual and Reproductive Health*, 47(2):71–82, 2015. URL doi:10.1363/47e3015. Accessed 2022-05-09.

E. M. Johnston, B. Courtot, and G. M. Kenney. Access to Contraception in 2016 and What it Means to Women. *Urban Institute*, 2017. URL https://www.urban.org/sites/default/files/publication/87691/

2001113-access-to-contraception-in-2016-and-what-it-means-to-women.pdf. Accessed 2022-04-28.

D. E. Kalist. Abortion and Female Labor Force Participation: Evidence Prior to Roe v. Wade. *Journal of Labor Research*, 25:503–514, 2004. URL https://doi.org/10.1007/s12122-004-1028-3. Accessed 2022-04-20.

J. A. Klerman. U.S. Abortion Policy and Fertility. *AEA Papers and Proceedings*, 89(2): 261–264, 1999. doi: 10.1257/aer.89.2.261. Accessed 2022-04-25.

P. Levine, D. Staiger, T. Kane, and D. Zimmerman. Roe v Wade and American Fertility. *American Journal of Public Health*, 89(2):199–203, 1999. URL https://doi.org/10.2105/AJPH.89.2.199. Accessed 2022-03-25.

P. B. Levine and D. Staiger. Abortion Policy and Fertility Outcomes: The Eastern European Experience. *Journal of Law and Economics*, 47(1):223–243, 2004. URL https://doi.org/10.1086/380475. Accessed 2022-04-25.

J. R. Lott and J. Whitley. Abortion and Crime: Unwanted Children and Out-of-wedlock Births. *Economic Inquiry*, 45:304–324, 2007. URL https://doi.org/10.1111/j.1465-7295.2006.00040.x. Accessed 2022-04-28.

A. Madestam and E. Simeonova. Children of the Pill: The Effect of Subsidizing Oral Contraceptives on Children's Health and Wellbeing. Working paper, 2012. URL http://www.ne.su.se/polopoly_fs/1.152096.1384252247%21/menu/standard/file/Children%20of%20the%20Pill.pdf. Accessed 2022-05-13.

G. Martinez, K. Daniels, and A. Chandra. Fertility of Men and Women Aged 15–44 Years in the United States: National Survey of Family Growth, 2006–2010. *National Health Statistics Reports*, (51), 2012. URL https://www.cdc.gov/nchs/data/nhsr/nhsr051.pdf. Accessed 2022-04-22.

M. Medoff. Biased abortion counseling laws and abortion demand. *The Social Science Journal*, 46:632–643, 2009. doi: 10.1016/j.soscij.2009.05.001. Accessed 2022-05-13.

E. Mølland. Benefits from delay? The effect of abortion availability on young women and their children. *Labour Economics*, 43:6–28, 2016. URL https://doi.org/10.1016/j.labeco.2016.06.011. Accessed 2022-05-13.

NARAL. Who Decides?, 2009. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/04/2009-Who-Decides.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2010. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/04/2010-Who-Decides.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2011. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/04/2011-Who-Decides.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2012. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/04/2012-Who-Decides.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2013. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/04/2013-Who-Decides.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2014. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/04/2014-Who-Decides.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2015. URL http://www.prochoiceamerica.org/wp-content/uploads/2019/02/WhoDecides2019-DigitalEdition.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2016. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/01/WHODecides-2016-Final.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2017. URL http://www.prochoiceamerica.org/wp-content/uploads/2017/01/WhoDecides2017-DigitalEdition3.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2018. URL http://www.prochoiceamerica.org/wp-content/uploads/2018/02/WhoDecides2018-withCover-2.pdf. Accessed 2022-03-25.

NARAL. Who Decides?, 2019. URL http://www.prochoiceamerica.org/wp-content/uploads/2019/02/WhoDecides2019-DigitalEdition.pdf. Accessed 2022-03-25.

T. Rau, M. Sarzosa, and S. Urzúa. The Children of the Missed Pill. *Journal of Health Economics*, 79, 2021. URL https://doi.org/10.1016/j.jhealeco.2021.102496. Accessed 2022-05-02.

C. Schurr and E. Militz. Reproductive Rights. In *International Encyklopedia of Human Geography*, pages 435–442, 2020. URL https://doi.org/10.1016/B978-0-08-102295-5.10234-3. Accessed 2022-04-13.

M. H. Smith, Z. Muzyczka, P. Chakraborty, E. Johns-Wolfe, J. Higgins, D. Bessett, and A. H. Norris. Abortion Travel Within the United States: An Observational Study of Cross-State Movement to Obtain Abortion Care in 2017. *The Lancet Regional Health - America*, 10, 2022. URL https://doi.org/10.1016/j.lana.2022.100214. Accessed 2022-05-06.

D. Staiger and J. H. Stock. Instrumental Variables Regression with Weak Instruments. *Econometrica*, 65(3):557–586, 1997. URL https://doi.org/10.2307/2171753. Accessed 2022-05-05.

J. M. Wooldridge. Two-Way Fixed Effects, The Two-Way Mundlak Regression, and Difference-in-Difference Estimators. *SSRN*, 2021. URL https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3906345. Accessed 2022-05-19.

World Health Organization. Family planning/contraception methods, 2020. URL https://www.who.int/news-room/fact-sheets/detail/family-planning-contraception. Accessed 2022-05-13.

World Health Organization. Abortion, 2022. URL https://www.who.int/health-topics/abortion#tab=tab_2. Accessed 2022-05-13.

# A Appendix

Table A.1: Determinants for NARAL state grading

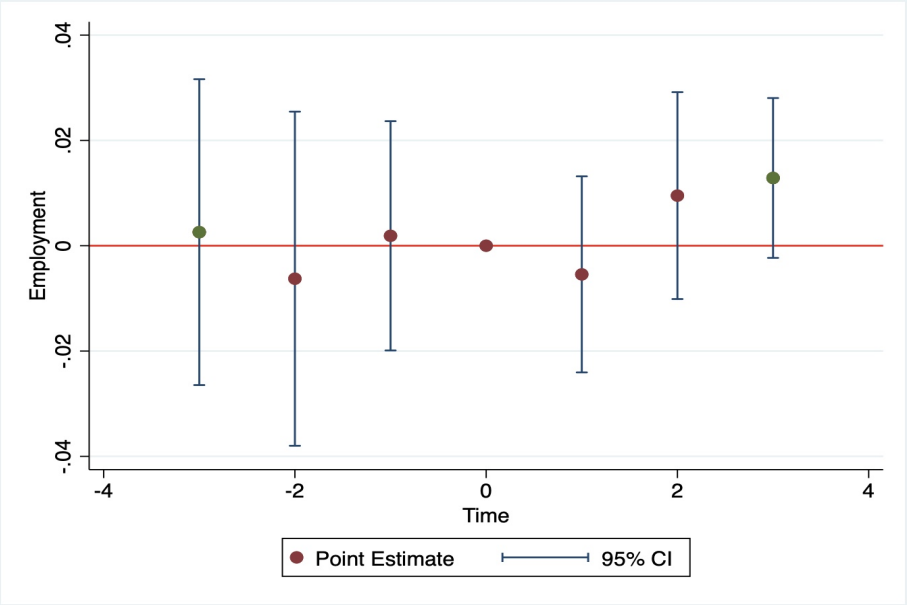
| ***Points are added if a state ...*** | ***Points are subtracted if a state ...*** |
|---|---|
| Allows certain qualified health-care professionals to provide abortion care | Impose TRAP laws. The severity and extent of the TRAP laws determines how many points to subtract. |
| Has codified the protections of Roe v. Wade, and has a state constitution beyond the U.S. constitution protecting the right to choose. | Has bans on abortions after certain point in pregnancy or bans on certain abortion procedures. |
| Has legislated abortion clinic protection, as for example if the state penalize offenders who interfer with entry or exit to a clinic. The number of points are based on the strength of protection. | Has inadequate or lack of health expectations in terms of the fetus when imposing post-viability abortion bans, or if the state have determined a specific time in pregnancy of viability. |
| Has limited funding and referrals to crisis pregnancy centers (centers misleading women about their reproductive health-care options). | Requires the woman to take part of biased counseling before having an abortion. |
| Has ensured that survivors of sexual assault are provided with counseling about and access to emergency contraceptives. | Requires delays and/or multiple visits to the abortion clinic when wanting to have an abortion. |
| Has guaranteed insurance coverage for abortion care. | Has bans on counseling or referrals to abortion care. |
| Arrange increased family-planning services through Medicaid or a state amendment. | Funds crisis pregnancy centers and requires/refers women to these centers. |
| Has a medical assistance program providing abortion coverage without restrictions. | Has measures that prohibits insurance coverage for abortion care. |
| Ensures that insurance coverage includes access to contraceptives to the same extent as other prescription medication. | To a large extent allows individuals and organizations to refuse providing or counseling women to reproductive health services. |
| Has a Medicaid program that covers emergency contraception without prescription. | Has regulations regarding who has to consent or be notified when a minor seeks an abortion. |
| Guarantees women birth control prescriptions. | |

Source: NARAL Pro-Choice America (2016)

Table A.2: Descriptive Statistics by Age Group

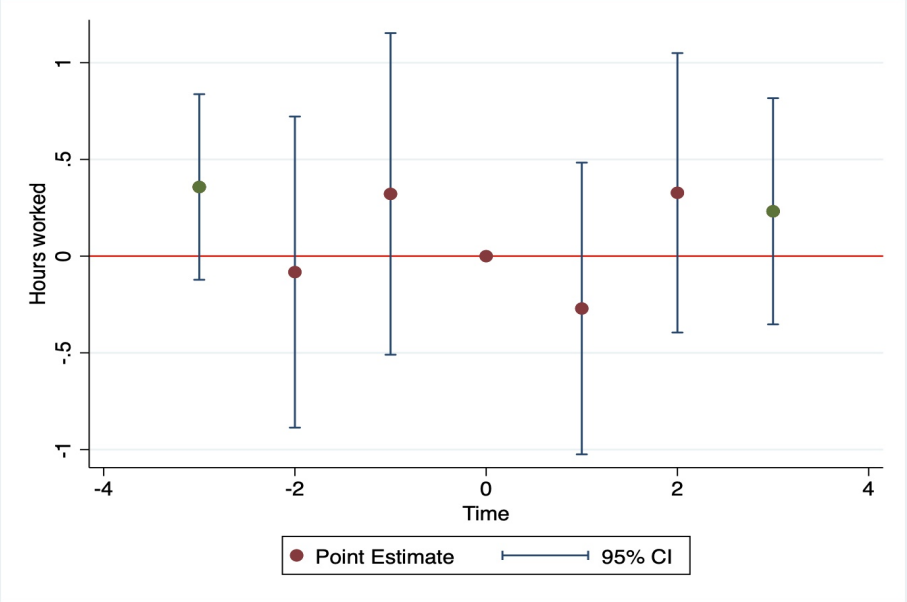
| *Age group* | **(15-44)** | **(20-44)** | **(15-29)** | **(20-29)** | **(30-44)** |
|---|---|---|---|---|---|
| | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** | **Mean (sd)** |
| *Individual characteristics* | | | | | |
| Had a child in the last year | 0.051 | 0.060 | 0.053 | 0.080 | 0.049 |
| | (0.2195767) | (0.237253) | (0.2240248) | (0.2707496) | (0.2154731) |
| Worked last year | 0.666 | 0.746 | 0.566 | 0.726 | 0.756 |
| | (0.471567) | (0.435551) | (0.495593) | (0.445784) | (0.42935) |
| Hours worked per week last year | 35.201 | 36.442 | 31.892 | 34.602 | 37.432 |
| | (11.7398) | (10.8833) | (12.487) | (11.086) | (10.6421) |
| Share of black women | 0.121 | 0.121 | 0.128 | 0.133 | 0.114 |
| | (0.3258122) | (0.3262328) | (0.3339497) | (0.339835) | (0.3181683) |
| Married | 0.421 | 0.512 | 0.179 | 0.283 | 0.640 |
| | (0.4937679) | (0.4998614) | (0.03832703) | (0.4506039) | (0.4801112) |
| *State level characteristics* | | | | | |
| Unemployment rate | 6.427 | 6.423 | 6.442 | 6.440 | 6.414 |
| | (2.4194887) | (2.4189032) | (2.42381) | (2.42486) | (2.4155218) |
| Observations | 501,852 | 411,750 | 237,813 | 147,711 | 264,039 |

Figure A.1: Event Study Extensive Margin (age 20-29)



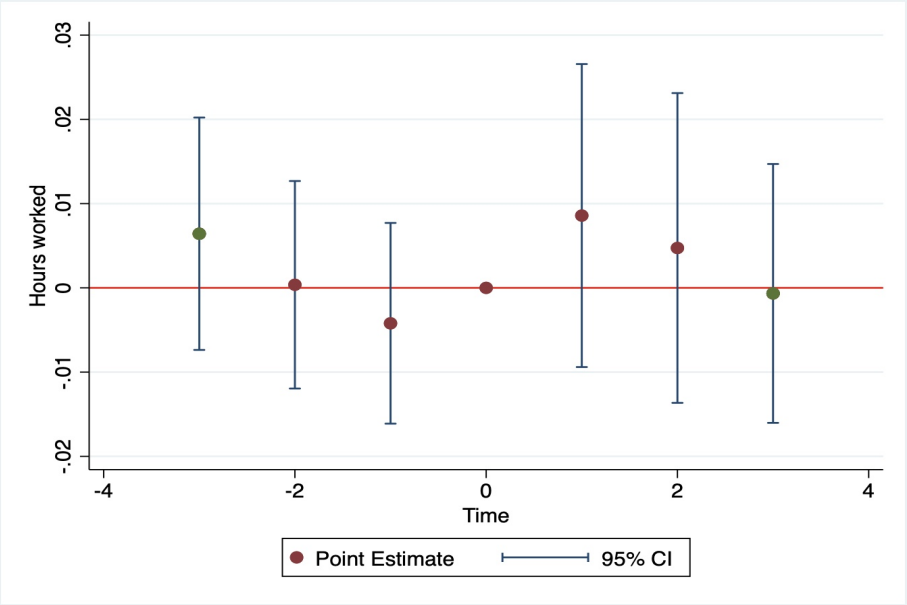

Note: Time 0 represents the first year of a new restriction intensity. The event study is performed with probability weights.

Figure A.2: Event Study Intensive Margin (age 20-29)



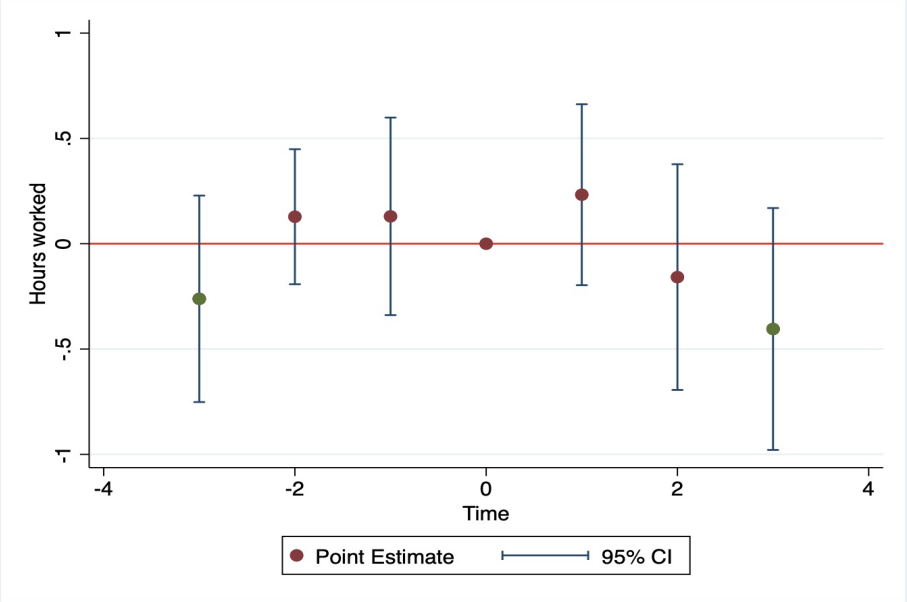

Note: Time 0 represents the first year of a new restriction intensity. The event study is performed with probability weights.

Figure A.3: Event Study Extensive Margin (age 30-44)




Note: Time 0 represents the first year of a new restriction intensity. The event study is performed with probability weights.

Figure A.4: Event Study Intensive Margin (age 30-44)




Note: Time 0 represents the first year of a new restriction intensity. The event study is performed with probability weights.

Table A.3: Reproductive Health Care Access Grade by State and Year

| State | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| AL | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| AK | 2 | 1 | 1 | 1 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| AZ | 2 | 4 | 4 | 4 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| AR | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| CA | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| CO | 4 | 4 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 |
| CT | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| DE | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 2 |
| FL | 4 | 4 | 4 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| GA | 4 | 4 | 4 | 4 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| HI | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| ID | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| IL | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| IN | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| IA | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 5 | 5 |
| KS | 4 | 4 | 4 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| KY | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| LA | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| ME | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| MD | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| MA | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 3 | 3 | 3 | 3 |
| MI | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| MN | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 |
| MS | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| MO | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| MT | 1 | 1 | 1 | 1 | 1 | 2 | 1 | 1 | 1 | 1 | 1 |
| NE | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| NV | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| NH | 1 | 1 | 1 | 2 | 3 | 3 | 3 | 3 | 3 | 3 | 3 |
| NJ | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| NM | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| NY | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| NC | 4 | 4 | 4 | 4 | 4 | 5 | 5 | 5 | 5 | 5 | 5 |
| ND | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| OH | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| OK | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| OR | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| PA | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| RI | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 5 | 4 | 5 | 4 |
| SC | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| SD | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| TN | 4 | 4 | 4 | 4 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| TX | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| UT | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| VT | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 2 | 2 | 2 |
| VA | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| WA | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| WV | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 4 | 4 | 5 | 5 |
| WI | 4 | 3 | 3 | 4 | 4 | 4 | 4 | 5 | 5 | 5 | 5 |
| WY | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 4 |

Source: NARAL 2009, 2010, 2011, 2012, 2013, 2014, 2015, 2016, 2017 2018, 2019