# Is TRAP a Trap? The Impact of Abortion Access on Violence Against Women*

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**Abstract**

I evaluate the impact of abortion clinic closures on violence against women of reproductive age exploiting variation induced by a law that caused the closure of nearly half of Texas' clinics. I find sizable and nonlinear effects of travel distance on violence against women: a 25-mile increase in distance to reach the nearest clinic is estimated to increase the number of violent offenses by up to 1.9 percent and the effect decreases as the initial distance from a clinic rises. The largest effect is detected for Hispanic and Black women.

**JEL Classification:** I11; J12; J13; J16; J18; K23

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## 1. Introduction

Between the enactment of Roe v. Wade in 1973[1] and its overruling in June 2022[2], a total of 1,381 abortion restrictions in 12 US states have been enacted and 46% of those restrictions were enacted in the last decade alone (Nash and Ephross, 2022). This latest slate of restrictions is marked by decreasing emphasis on demand-side regulations such as mandatory waiting periods and parental involvement requirements for minors and increasing emphasis on supply-side regulations targeting providers such as admitting privileges requirements, hospital transfer policies, facility requirements, and outright bans (Fischer, Royer and White, 2018; Grossman et al., 2014; Lindo et al., 2020a; Venator and Fletcher, 2020). These supply-side restrictions have been called Targeted Regulations of Abortion Providers, or TRAP laws.

The prevalence of abortion suggests the effects of abortion restrictions are of fundamental interest to social scientists. Roughly 1 out of 5 pregnancies results in an abortion and current abortion rates indicate that 1 in 4 U.S. women will have an abortion in their reproductive lifetimes (Jones and Jerman, 2019). In line with one of the primary goals of abortion restriction policies, earlier research confirms the effectiveness of TRAP laws in decreasing the abortion rate and increasing the fertility rate (Brown et al., 2020; Fischer, Royer and White, 2018; Lindo et al.,

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[1] Roe v. Wade, 410 U.S. 113 (1973), was a landmark decision of the U.S. Supreme Court in which the Court ruled that the Constitution of the United States conferred the right to have an abortion, striking down many federal and state abortion laws. With this decision, the U.S. Supreme Court held that state governments could not regulate abortions performed in the first trimester of pregnancy and could regulate but not prohibit abortions in the second trimester. With a subsequent decision – Webster v. Reproductive Health Services (1989) – the Supreme Court reversed its previous trend and upheld several state abortion restrictions.

[2] As a consequence, within a year, 14 states have banned abortion completely and 6 states have reduced the gestational limit to limit its access (https://www.nytimes.com/interactive/2022/us/ abortion-laws-roe-v-wade.html)

2020a; Myers, 2021; Venator and Fletcher, 2020).

By increasing the likelihood of unintended pregnancies, abortion restrictions may have disruptive consequences on women's agency and bargaining power through decreasing economic conditions and lower capacity to leave abusive households. Lower economic conditions and bargaining power have been associated with increasing probability of suffering abuse (Agarwal, 1997; Bettio and Ticci, 2017; McDonald, 2012). The aim of the present study is to explore the impact of restrictive abortion policies on the prevalence of violence against women.

The additional costs associated with raising a child typically exceed $9,000 in annual expenses (Lino et al., 2017). Within the household, the increase in childcare and housework responsibilities weigh more on the shoulders of women as housework is estimated to be not equally divided between partners (Coltrane, 2000)[3]; contrarily to fathers, mothers are also likely to experience a child penalty on the workplace following childbirth (Blau and Kahn, 2017; Kleven, Landais and Søgaard, 2019). These child-related costs may decrease women's economic status (Foster et al., 2018; Jones et al., 2021; Miller, Wherry and Foster, 2020) and limit their educational and professional opportunities (Abboud, 2019; Angrist and Evans, 1999; Bahn et al., 2020; Jones and Pineda- Torres, 2021; Jones et al., 2021; Kalist, 2004; Lindo et al., 2020*b*; Schulkind and Sandler, 2019). Lower socioeconomic conditions have been found to be associated with women's violence victimization. Estimates show that women with medium or high levels of education face

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[3] Bertrand, Kamenica and Pan (2015) estimate how, after controlling for outside work, the majority of caring responsibilities still belong to women. A share of the significant part of the gender wage gap that cannot be explained by the usual explanatory factors is likely to be caused by women taking career breaks following childbirth (Costa Dias, Joyce and Parodi, 2020; Hersch and Stratton, 1994; Rege and Solli, 2013; Andersen 2017)

less exposure to sexual, physical, or psychological abuse from partners or non-partners compared to less educated women (Bettio and Ticci, 2017). According to a review by McDonald (2012), women with irregular, contingent, or precarious employment contracts are particularly vulnerable to sexual harassment.[4]

Within the household, an unintended pregnancy may increase women's likelihood of suffering from intimate partner violence (IPV), through its effect on the woman's ability to leave a relationship (Bettio and Ticci, 2017; Chibber et al., 2014; Roberts et al., 2014). Analyzing data from the Turnaway Study, a cohort study of women seeking abortions at 30 facilities across the U.S., Chibber et al. (2014) found that eight percent of women who mentioned partners as a reason for abortion identified having abusive partners as the main reason. Some of them explained that having a baby would be a deterrent to ending the abusive relationship. Evidence shows that the rate of reporting IPV is lower for women in the early postpartum period (Keeling and Mason, 2011; Rubertsson, Hildingsson and Rådestad, 2010). Fugate et al. (2005) analyzed data from the Chicago Women's Health Risk Study, finding that many abused women believe that to get help from the police, they must be prepared to end the relationship. Furthermore, they find that 10% of the interviewed women stated they did not call the police in order to "protect [their] partner and

[4] Lower economic standing decreases women's capacity to avoid violence in the workplace because of the lack of outside options in the case of job loss. In addition, a lower economic status forces women to accept more dangerous job positions that may be associated with a higher likelihood of suffering abuse. A simple example is made by occupations that involve night shifts which may expose women to a higher probability of being victims of violence by strangers. One interesting case is the one of sex work. Selling sex may be a viable option for women who need money and flexible working hours to support their children. Several studies indicate that the majority of prostitutes report having been raped and physically assaulted during the course of their activities and they are also disproportionately represented among female murder victims (Church et al., 2001; Farley and Barkan, 1998; Lowman, 2016).

preserve [the] relationship" (Fugate et al., 2005). The impact of childbirth on mothers' economic and emotional status makes it harder for them to leave the abusive partner (Bettio and Ticci, 2017; Chibber et al., 2014). In the original bargaining models of marriage[5] the threat point and the reservation utilities coincide with each other and correspond to the utility of divorce. The threat of divorce (or break up) becomes far less credible when a child arrives, for economic and emotional reasons. The premise here is that the greater a woman's ability to physically survive outside the family, the greater her bargaining power within the family (Gelles, 1976; Montero et al., 2012). Moreover, in the marriage market, mothers are typically less "eligible" than fathers, and this further decreases their willingness to leave a relationship (Agarwal, 1997). Results from a Finnish survey show that women who were unemployed, self-employed, or on maternity leave reported experiencing IPV more often (Heiskanen, Piispa and Aromaa, 1998). Aizer (2010) estimates that decreases in the wage gap reduce violence against women within the family, and Anderberg et al. (2016) estimate a positive relationship between female unemployment and domestic abuse.

I start from studies that estimate a sharp reduction in the abortion rate and an increase in the fertility rate after the implementation of TRAP laws (Brown et al., 2020; Fischer, Royer and White, 2018; Lindo et al., 2020a; Myers, 2021; Venator and Fletcher, 2020). The analysis focuses on Texas as it experienced a dramatic cut in abortion facilities as a consequence of TRAP policies. In July 2013, Texas House Bill 2 (HB-2) took effect, which caused the closure of nearly half of the state's abortion clinics within the subsequent year. The change in clinics' accessibility started between the first and the second half of 2013, when the first major requirement[6] of the bill went

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[5] See for example, Manser and Brown, 1980; McElroy and Horney, 1981.

[6] The first provision required physicians at abortion clinics to have admitting privileges at a hospital within 30 miles

into effect (Figure 1). I evaluate the effect of Texas House Bill 2 on violence against women of reproductive age, which I call for the sake of simplicity *gender violence*.[7] I use a generalized difference-in-differences approach and data on violent offenses from the National Incident Based Reporting System (NIBRS). The assumption underlying the identification strategy is that variations in the distance from a municipality to its nearest abortion clinic are exogenous since they are a consequence of the fact that some clinics randomly[8] met the standards imposed by H-B2, while others did not and had to shut down. Event-study analyses, using both Two-Way Fixed Effects (TWFE) and Sun and Abraham (2021) estimates, provide evidence in support of the parallel trends assumption, as well as evidence of a significant increase in violence after clinics' closure, confirming the validity of the TWFE model in the presence of heterogeneous and dynamic treatment.

To the extent of my knowledge, this is the first study in economics to empirically evaluate the impact of restrictive abortion regulations on violence against women. The present analysis contributes to three strands of the literature. First, it contributes to the literature on abortion and domestic violence. Previous studies had mainly looked at the link between pregnancy, abortion, and domestic abuse relying on survey data. Results show a higher prevalence of domestic violence among women seeking abortion services, with women who seek abortions experiencing domestic

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of the facility. This and the other three requirements are described in Section 2.

[7] The Council of Europe defines gender-based violence against women as *violence that is directed against a woman because she is a woman or that affects women disproportionately* (Council of Europe, 2011). This definition applies to the present case as the paper investigates forms of violence against women arising from decreasing access to abortion. The connection between abortion and violence makes the latter specific to the female population.

[8] The randomness of clinic closure is investigated in Section 5.

violence and sexual assault at up to three times the rate of those who want to continue with their pregnancies (Evins and Chescheir, 1996; Garcıa- Moreno et al., 2013; Hall et al., 2014; Taft and Watson, 2007). Ellsberg et al. (2008) reports that domestic violence tends to increase during pregnancy and Roberts et al. (2014), using information from the Turnaway Study, find that having an abortion was associated with a reduction over time in physical violence from the man involved in the pregnancy, compared with carrying the pregnancy to term. They conclude that having a baby with an abusive man, compared to terminating the unwanted pregnancy, makes it harder to leave the abusive relationship. With respect to these studies, (i) I enlarge the definition of the dependent variable to include types of violence against women other than IPV; (ii) I exploit administrative data on crime that with respect to survey data also capture more extreme forms of violence like homicides or kidnappings; and (iii) I evaluate the impact of restrictive abortion policies on violence against women. Second, the analysis contributes to the literature on the impact of TRAP laws on abortions and births that exploits the same setting and identification strategy used in this paper. The contribution lies in the inclusion of many empirical tests on the randomness of treatment, as well as, on checks that account for repeatedly treated units, and staggered and heterogeneous treatment. Finally, the present study contributes to the growing literature investigating unintended consequences of TRAP policies. Jones and Pineda-Torres (2021) find that exposure to TRAP law before age 18 reduce the probability of entering college and the probability of completing it by 1 to 3 percentage points. Bahn et al. (2020) finds that TRAP laws increased "job lock", with women in states with TRAP laws being less likely to move between occupations and into higher-paying occupations.

I find that, depending on the initial distance, a 25-mile increase in the distance to the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality by up to 1.9 percent. This impact persisted throughout the entire post-policy period in the sample, up to 3 years after HB-2 introduction. The relationship is non-linear, in the sense that the effect of distance on violence is lower for municipalities already far from their nearest abortion clinic, while it is larger for women living relatively close to a clinic before the closure.[9] The effect persists across different forms of violence, including IPV, offenses other than IPV, physical violence and sexual violence. Finally, the impact of an increase in distance is larger among Hispanic and Black women, with the latter group experiencing an increase in violence against them by up to 4.8 percent.

The well-documented benefit of the introduction of TRAP laws in terms of fertility outcome - with TRAP regulations associated with decreased abortion rates and increased birth rates (Lindo et al.2020a; Fischer, Royer and White 2018; Venator and Fletcher 2020; Brown et al. 2020; Myers, 2021) – are counterbalanced by their unintended positive impact on violence against women. The social and economic costs associated with violence against women in terms of women's physical, mental, sexual, and reproductive health are enormous (World Health Organization 2021). In addition, children who grow up in violent households may be victim of violence themselves and be more likely to suffer a range of behavioral and emotional problems and to perpetrate or experience violence later in life (World Health Organization 2021). In section 5.5, I present preliminary evidence on the relationship between TRAP laws and violence against minors who are likely to live in the abusive household. Results show a sizable and significant increase in violence on minors following the implementation of these laws.

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[9] This result is consistent with findings from Fischer, Royer and White (2018); Lindo et al. (2020*a*); Myers (2021); Venator and Fletcher (2020) of a diminishing marginal effect of travel distance on abortions.

# 2. Background

### 2.1 Texas HB-2

On July 18, 2013 Texas House Bill 2 (HB-2) was signed into law. The bill contains the following provisions: (1) all abortion providers must have admitting privileges at a hospital located within 30 miles of the abortion clinic, (2) all abortion facilities must meet the requirements of an ambulatory surgical center, (3) abortions after 20 weeks gestation are prohibited and (4) in accordance with Food and Drug Administration regulation, women must visit a doctor for each of the two doses of the abortion pill and, after taking the pill, the patient must be seen in a follow-up appointment within 14 days.

Provisions (1), (3), and (4) went into effect on November 1, 2013, causing the first wave of abortion clinic closures. Obtaining admitting privileges can take time since hospitals have to review a doctor's education, licensure, training, board certification and history of malpractice, and many hospitals require admitting doctors to meet a quota of admissions. The implementation of this provision caused nearly half of Texas abortion clinics to close (Figure 1).

The ambulatory surgical center requirement took effect on October 3, 2014 but its enforcement was blocked two weeks later by the U.S. Supreme Court. Converting a clinic to meet these standards is costly both financially and in terms of time: there is a detailed licensing process, and clinics have to meet physical requirements such as certain room dimensions and corridor widths. This regulation affected the ability of several additional clinics to provide abortions, but only temporarily.

In April 2013, after the introduction of HB-2, eight of the 41 Texas abortion clinics closed or stopped providing abortion services. Eleven more facilities closed or stopped providing abortions when HB-2 was enforced, mainly because physicians experienced barriers to obtaining hospital admitting privileges. Although some clinics were able to reopen once physicians successfully obtained these privileges, others still closed, resulting in 19 licensed facilities providing abortions in Texas by July 2014, an overall 54 percent reduction in the number of facilities since April 2013 (Gerdts et al., 2016).

On June 27, 2016, with the *Whole Woman's Health v. Hellerstedt* decision, the United States Supreme Court struck down the admitting privileges provision and the ambulatory surgical center requirement of Texas HB-2. The majority opinion was that these provisions imposed an undue burden on access to abortion, without being seen to serve a legitimate interest in regulating women's health. But, one month after this decision, only three clinics that closed because of the bill reopened. In 2017, among the 27 abortion desert U.S. cities (that is, cities from which women have to travel more than 100 miles to reach the nearest abortion clinic), 10 were in Texas (Cartwright et al., 2018). Figure 2 represents the variation in the availability of abortion clinics in Texas and neighboring states from January 2009 to December 2016. The purple/blue isochrones give an idea of the geographic areas covered by each clinic: the purple ones represent an area of up to 30 minutes' travel time by car from each clinic; the blue ones reflect a distance of up to one hour.

Lindo et al. (2020*a*) estimate that, relative to having the nearest abortion provider within 50 miles, having the nearest abortion provider 50-100, 100- 150, 150-200 and more than 200 miles away reduces abortions by 16 percent, 28 percent, 38 percent, and 44 percent, respectively. These results are consistent with Grossman et al. (2017), who find that in Texas an increase in distance to the closest facility providing abortion services was associated with a decline in abortions between 2012 and 2014. Fischer, Royer and White (2018) estimate that abortion amongst Texas residents fell 16.7 percent and births rose 1.3 percent in counties that no longer had an abortion provider within 50 miles, after the implementation of policies restricting abortion access. Similarly, Venator and Fletcher (2020) analyze the effects of Wisconsin's restrictions on abortion access introduced between 2011 and 2013. They find that a 100-mile increase in distance to the nearest clinic is associated with 30.7 percent fewer abortions and 3.2 percent more births. Finally, two recent studies adopt a broader approach. Using data for 1,178 counties in 18 U.S. states, Brown et al. (2020) find that each additional mile to a provider was associated with a decrease of 0.011 in the abortion rate. Myers (2021) exploits a new dataset for the entire country, finding that an increase in travel distance from 0 to 100 miles is estimated to prevent 20.5 percent of women seeking an abortion from reaching a provider, and in turn to increase births by 2.4 percent. The difference between the decrease in the abortion rate and the increase in the fertility rate is consistent with women who could not terminate their pregnancy from a local provider, but who could decide to travel outside of Texas to have an abortion or to illegally self-induce an abortion (Grossman et al., 2010).

### 2.2 Unintended Pregnancies and Violence Against Women

The impact of abortion restrictions is likely to be larger in the US context than in many other western countries, given the prevalence of unintended pregnancies.[10] The unintended pregnancy rate is significantly higher in the United States than in many other developed countries.[11] The Guttmacher Institute estimates that in 2011, there were 45 unintended pregnancies for every

[10] The Guttmacher Institute defines an unintended pregnancy as a pregnancy that occurred when a woman wanted to become pregnant in the future but not at the time she became pregnant (unplanned) or a pregnancy that occurred when she did not want to become pregnant then or at any time in the future (unwanted).

[11] https://www.guttmacher.org/fact-sheet/unintended-pregnancy-united-states.

1,000 women aged 15-44 in the United States (that is, nearly 5 percent of reproductive-age women have an unintended pregnancy each year) and that nearly half (45%) of the 6.1 million pregnancies in the United States were unintended.

Poor and vulnerable women constitute the group that experiences the highest rate of unintended pregnancies.[12] Economic constraints reduce their ability to turn to hospitals or private physicians' offices for an abortion or to travel far away from home to reach the nearest abortion clinic, losing days of work and spending money on travel and hotels; in addition, they represent the group with the least access to contraception (Kavanaugh, Jones and Finer, 2011). According to the Guttmacher Institute[13] 75 percent of abortion patients in 2014 were poor or low-income.[14] This is especially true for Texas, wherein in 2011 a huge cut to public funds to family clinics, which provide free contraceptives to poor women and young girls, was implemented. Lu and Slusky (2019) estimate the effects of this budget cut, that caused 53 clinics to close by 2012, the vast majority of which only provided non-abortion family planning services. They estimate that an increase of 100 miles to the nearest clinic results in a 2.4 percent increase in the fertility rate for unmarried women. Packham (2017) finds that reducing funding for family planning services in Texas increased teen birth rates by approximately 3.4 percent over four years. In addition, as lower socioeconomic conditions are reported among IPV risk factors (Aizer, 2010; Capaldi et al., 2012), poor women

[12] https://www.guttmacher.org/fact-sheet/induced-abortion-united-states

[13] https://www.guttmacher.org/fact-sheet/induced-abortion-united-states

[14] Individuals are defined poor when they have an income below the federal poverty level of $15,730 for a family of two in 2014. Individuals are defined as low-income if they have an income of 100-199% of the federal poverty level (https://www.guttmacher.org/fact-sheet/induced-abortion-united-states).

are also  more likely to be involved into abusive relationships.

The relationship between abortion and IPV is exacerbated by the fact that unintended pregnancies are more likely to occur for women already involved in violent relationships (Aston and Bewley, 2009; Hall et al., 2014; Taft and Watson, 2007), since women who are physically assaulted by their partner are also more likely to be also sexually assaulted, and this prevents them from using barrier contraceptives (Hall et al., 2014). In addition, they may choose to terminate the pregnancy to protect a potential child from a violent environment and the risk of suffering abuse.

# 3. Data

To investigate the relationship between abortion access and violence against women, I built a dataset where I merge distance to the nearest abortion clinic measured in miles with the number of cases of gender violence for each municipality in the sample in any given period, for the years 2010 to 2016. The variables used in the analysis are summarized in Table 1 for the periods before and after HB-2.[15]

To measure violence, I use information on reported cases of violence against women for 63 Texas municipalities,[16] taken from the Uniform Crime Reporting Program Data: National Incident-Based Reporting System (NIBRS). NIBRS series is a component part of the Uniform Crime Reporting Program (UCR), a nationwide view of crime administered by the Federal Bureau of Investigation (FBI), based on the submission of crime information by participating law enforcement agencies. Unlike data reported through the UCR Program’s traditional Summary

[15] Summary statistics for the entire sample period (2010-2016) are reported in Table A1 of Appendix A.

[16] The list of the municipalities used for the analysis can be found in Appendix A.

Reporting System (SRS), NIBRS goes much deeper because of its ability to provide details on each single crime incident including information on victims, known offenders, and relationships between victims and offenders. Within this program, each city law enforcement agency reports offenses that occur within its municipal boundaries. Since the data collection is based on the voluntary submission of crime information by law enforcement agencies, data are completely missing or strongly imbalanced during my sample period for many municipalities, hence the dataset includes the subsample of Texas municipalities plotted in Figure 3.

The NIBRS reports offenses at the agency level and documents the municipality in which each agency is located. As a first check, I controlled that each municipality reported in the sample is covered only by a single agency and then I geolocated each agency using the mu- nicipality's coordinates to calculate changes in distance to the nearest clinic. As exposure variable, each regression includes the logarithm of the population covered by each agency,[17] and controls are built as averages across counties covered by each agency. For consistency purposes, agencies referred to counties instead of municipalities are dropped from the sam- ple. In 2016, lots of new agencies started reporting data to the NIBRS, but since they have data for only two periods of the entire sample period, they are dropped as well. Table A2 of Appendix A describes the sample selection. Since every agency referred to a geolocated municipality, the level of analysis considered is the municipal one.

UN Women includes as forms of violence against women intimate partner violence, sexual violence, femicide, human trafficking, female genital mutilation, child, early and forced marriage,

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[17] Agencies without such information are dropped from the sample.

online or technology-facilitated violence.[18] Following this broad definition of gender violence, I include in the analysis all offenses where the victim is a female of reproductive age (15- 49) and the offender is male, and the types of offense considered include assault, homicide, human trafficking, kidnapping, and sexual offenses.[19] [20] For simplicity, I will refer to these multiple forms of violence as *gender violence*. When decomposing the incidence of different types of violence in the sample, I find that: intimate partner violence constitutes the 69.6% of the offense, physical violence constitutes the 92.6% of the offenses, sexual violence constitutes the 7.37% of the offenses and prostitutions and human trafficking constitute the 0.03% of the offenses.

Looking at Table 1, I can detect some preliminary evidence on the relationship under study as the weighted mean of the number of reported cases of gender violence increase sizably after HB-2 implementation. Figure 4 plots the average time trends in gender violence offenses for municipalities that experience a positive change in distance compared to the ones whose distance did not change.[21] The figure shows a modest increase in violence for treated municipalities following the implementation of the law in 2013, while the trend for untreated cities remains stable for the entire sample period.

Data on clinics' opening and closing dates in Texas and neighboring states (Colorado, Louisiana, New Mexico, and Oklahoma) are taken from Lindo et al. (2020*a*). The inclusion of

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[18] https://www.unwomen.org/en/what-we-do/ending-violence-against-women/faqs/types-of-violence.

[19] See Appendix Table A3 for the list of the types of offenses considered.

[20] Similar to the case of physical IPV, studies suggest low socioeconomic status to be a risk factor for sexual violence victimization (Byrne et al. 1999, Breiding et al. 2014, Breiding et al. 2017)

[21] Average time trends in gender violence cases are calculated on the balanced subsample of observations.

clinics in Colorado, Louisiana, New Mexico, and Oklahoma needs to account for potential travel to clinics in neighboring states. A clinic is considered open (or closed) in a six-month period if it has been opened (or closed) for at least three months.

I geocoded each abortion clinic in every six-month period of every year for the period 2010-2016. Then, I used the Stata command *georoute* to calculate the travel distance (miles) between each municipality's geographic centroid that reports crimes to the National Incident-Based Reporting System and the nearest clinic. Table 1 shows how the average distance to the nearest clinic has almost doubled after the implementation of HB-2 within the sample of municipalities used for the analysis.

Distance from the nearest clinic has changed differently across counties after HB-2 implementation. Figure F1 of Appendix F plots variations in distance from each county to the nearest clinics and the municipalities included the sample. Although the subsample of municipalities used is restricted, it is able to capture changes in distance that go from 0 to more than 100 miles.

The model includes a number of time-varying control variables at the county level built as average values across counties covered by each agency. The main model includes the estimated income per capita taken from the U.S. Bureau of Economic Activity (BEA), the unemployment rate obtained from the U.S. Bureau of Labor Statistics, and the share of women of reproductive age calculated from the data by the National Institute of Health Surveillance, Epidemiology and End Results (SEER).[22] The summary statistics of these variables are reported in Table 1.

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[22] Including covariates for racial composition in each county may result in a problem of perfect collinearity with the municipal fixed effects, as the trends in the shares of White, Black, and Hispanics females are flat in the considered time period. A similar multicollinearity issue may arise using their absolute number due to the common trends in all these variables. Figure A3 of Appendix A plot the trends in the controls and in Table E1 I confirm the robustness of the

# 4. Empirical Strategy

I estimate the effect of abortion access on gender violence using a generalized difference-in-differences design that exploits within-municipality variation over time in distance to a clinic, controlling for cross-municipality time-varying shocks (Fischer, Royer and White, 2018; Lindo et al., 2020*a*; Venator and Fletcher, 2020). The causal interpretation is identified by the existence of a good counterfactual for the variation in cases that would have been observed for municipalities with larger changes in access if their access had changed very little. This counterfactual is constituted by the variation in the number of reported cases of gender violence for municipalities with small changes in access (Callaway, Goodman-Bacon and Sant'Anna, 2021).

Since the dependent variable is a discrete non-negative integer, taking the value 0 for several observations, I operationalize this strategy with a Poisson model specification (following Fischer, Royer and White, 2018; Lindo et al., 2020*a*; Lu and Slusky, 2019; Venator and Fletcher, 2020), with the inclusion of municipality and six-month fixed effects. Overdispersion, the main theoretical argument against this model, is corrected by calculating sandwiched standard errors (Cameron and Trivedi, 2005). In addition, the conditional fixed effects negative binomial model has been proven not to be a true fixed effects model (Allison and Waterman, 2002). Fixed effects Poisson Maximum Likelihood models may suffer from incidental parameter problem (Cameron and Trivedi, 2013). Thus, following Fischer, Royer and White (2018), all regressions are run using a Pseudo Maximum Likelihood estimator, a method known to solve this issue. In addition, this

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results to the inclusion of such controls.

method relaxes the assumption on the cor- rect specification of the density of the dependent variable, avoiding the risk of inconsistent estimates.

I estimate the following model:

$$E\left[GV_{i,c,t,y} \middle| Dist_{i,c,t,y}, X_{c,y}, \ \Gamma_{i,y}, \alpha_i, \delta_t\right] = \exp\left(\beta_1 Dist_{i,c,t,y} + , X_{c,y}\beta_2 + \ \alpha_i + \ \delta_t\right) \qquad (1)$$

$GV_{i,c,t,y}$ (Gender Violence) is the number of reported cases of gender violence for municipality $i$ in counties $c$, in period (six-month) $t$ of year $y$. $Dist_{i,c,t,y}$ is a set of measures of access from each municipality $i$ to the nearest abortion clinic in the six-month period $t$. This set includes a linear measure of distance and a quadratic measure of distance, both measured in miles. $\alpha_i$ is the municipality fixed effect and $\delta_t$ is the six-month fixed effect.[23] The inclusion of municipality fixed effects should greatly reduce overdispersion, which is mainly due to differences in cities' characteristics. $X_{c,y}$ is the vector of county controls. In all models, the logarithm of the population covered by each agency is included as the exposure variable to account for the fact that agencies vary widely in size and therefore have a different potential for offenses. In each regression, standard errors are clustered at the municipal level to account for both serial correlation in the outcome and overdispersion.

There are a number of threats to identification, which I attempt to address in turn: (1) randomness of treatment, (2) non-parallel pre-treatment or post-treatment trends in the outcomes under study, and (3) the potential presence of heterogeneous and dynamic treatment effects by adoption timing.

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[23] In Appendix E, Table E2 I test the validity of the main results to the use of year fixed effects instead of six-month fixed effects.

Suggestive evidence on random assignment of treatment is presented in Figure 5. A superficial look at the post-policy distribution of clinics may suggest a cluster of closures in the western part of Texas. But the geographic distribution of clinics closed after HB-2 reveals that clinics have been shut down across the entire state and the western portion remained unserved after 2013 only because it already had a very low number of clinics before the intervention. Random assignment of treatment is empirically tested through a number of regression-based tests. Appendix B includes a discussion on randomness of treatment, the description of the tests implemented and the corresponding estimated results.

Then, to explore parallel pre-treatment trends and descriptively examine whether changes in the outcomes under study predate the enactment of HB-2 (i.e., policy endogeneity), I produce an event study that allows to decompose the estimated treatment effect over time. I define the event in question as a closure that causes a positive increase in the distance to the nearest clinic. I estimate Equation 1 with the measure of distance replaced by an indicator variable equal to 1 if distance has increased since the last period.[24] The regression includes leads and lags for the six-month periods surrounding the reference period, $T$ and endpoints are binned. The indicator for the first lead is omitted, meaning that the coefficients can be interpreted as the effect of a clinic closure that increases distance from the nearest clinic on gender violence cases relative to gender violence cases in the six-month period prior to the clinic closure. I further investigate the validity of the parallel trend assumption in Appendix C by estimating the impact of post-policy changes in distance on pre-policy trends in reported cases of gender violence (Table C2).

[24] For the event study analysis, I use a balanced subsample of 420 observations.

Finally, an important concern with the TWFE estimates (including those used to generate event study coefficients) is that, in the presence of heterogeneous and dynamic treatment effects, TWFE estimates may be biased (Goodman-Bacon, 2021; Sun & Abraham, 2021). To account for this possibility, I implement a Sun and Abraham event study known to account for heterogenous and dynamic treatment effects. Results are presented and discussed in section 5.2.

# 5. Results

### 5.1 The Effect of Abortion Access on Gender Violence

Table 2, Panel I present TWFE estimates of the effect of distance to the nearest abortion clinic on gender violence. Across specifications that include fixed effects (column 1) and add a wide set of time-varying controls (column 2), I find that a 25-mile increase in distance to the nearest abortion clinic is associated with a 0.9 percent[25] increase in the number of reported cases of gender violence per municipality in the same period, with coefficients significant at the one percent level. Following the literature (Fischer, Royer and White, 2018; Lindo et al., 2020*a*; Myers, 2021; Venator and Fletcher, 2020), I assume this relationship to be non-linear, meaning that the effect is higher for municipalities relatively close to an abortion clinic before the implementation of the policy. On the contrary, women already far from the nearest clinic before HB-2 implementation suffered less from an increase in distance. I estimate a model that includes distance and distance squared and estimated coefficients confirms that any additional mile increases the cost at a

[25] Since the model is a Poisson, the percentage effect of a one-unit change in the regressor on the dependent variable is computed using the transformation $(e^{\beta} - 1) \cdot 100$.

diminishing rate. Where the access to the closest abortion clinic was already difficult prior to 2013, meaning that women had already to travel far away from home to reach the closest abortion clinic, additional miles to the nearest clinic do not affect the pool of women who are able to take days off work or/and time away from family to have the procedure. On the contrary, women who used to have relatively easy access to abortion prior to HB-2 are the ones for whom an increase in distance determines a significant change of scenario, shifting from their being able to complete the procedure in few hours to the need for days off work and/or away from family to reach the nearest clinic.

As shown by Panel I of Table 2, columns (3) – which includes time-varying controls and the quadratic of the distance– if the closest clinic is 0 miles away, a 25-mile increase in distance to the nearest abortion clinic is associated with a 1.9 percent increase[26] in the number of reported cases of gender violence per municipality in the same period, with coefficients significant at the one percent level. The effect of a 25-mile increase reduces as the starting distance increases, according to the coefficient of the squared measure of distance. Figure 6 plots the estimated effects by starting distance from the nearest clinic.[27]

These results remain consistent in size and significance across a series of robustness checks: (1) control for Hispanic and Black female populations are added to the regression; (2) replace six-month fixed effects with year fixed effects, in light of the fact that the time-varying controls used are collected yearly; (3) re-estimate the regression on a balanced sub-sample of

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[26] I estimated the effect of a 25-mile variation to show more interpretable results. The effect of a one-mile increase is 0.08 percent.

[27] Figure D1 of Appendix D shows the effects of an increase in distance of 50, 100 and 150 miles for different level of pre-policy distance to the nearest clinic.

municipalities,[28] to reassure that the strong unbalancedness of the whole dataset doesn't bias results. All these estimates are shown in Appendix E, Tables E1 through E3.

Another concern is linked to the possibility that the effect might be driven by the agencies covering the largest municipalities or by municipalities whose distances change the most. Hence, I first drop all the observations whose reference population exceeds the 90th per- centile of the distribution. Next, all the municipalities for which the distance has increased more than 150 miles are excluded from the sample. For these last two subsamples, the relationship is linear given that they are located in the most populated part of Texas, so they are all relatively close to the nearest clinic before the implementation of the policy. Results are reported in Table E4. Coefficients remain consistent, but the effect appears slightly smaller when excluding the most affected cities.

The event study analysis plotted in Figure 7[29] confirms my main estimates and shows no significant difference in pre-closure number of gender violence offenses for municipalities that experience a closure relative to those that do not. The analysis also shows that the increase in violence following HB-2 is stable for the entire post-treatment periods included in the sample. To investigate the magnitude of the lagged effect, I include a lagged measure of distance into the main regression (one year lag corresponding to two six-month periods lags). Table 2, Panel II shows the impact of abortion access on gender violence one year after closure, confirming the existence of a lagged effect with respect to the contemporaneous one. This is consistent with the fact that the economic vulnerability of a woman is likely to increase when the child is actually born, causing an

[28] This subsample only includes municipalities that have observations for the entire sample period.

[29] Regression coefficients can be found in Table C1 of Appendix C.

increase in the likelihood of suffering abuse. A 25-mile increase in the distance to the nearest clinic is associated with a 1.2 percent increase in the number of reported cases of gender violence per municipality the following year, if the closest clinic is 0 miles away. The effect of a 25-mile increase reduces according to the initial distance as shown by Figure D2.[30]

If access to abortion clinics had remained at pre-HB2 levels, there would have been 13,351 gender violence offenses reported in my dataset rather than the 13,759 observed in 2014 and 15,516.5 offenses reported in 2015 rather than 15,743 reported in the dataset. This corresponds to 4.4% more offenses in 2014-2015 because of HB-2. The size of this effect is significantly smaller that the first stage effect of HB-2 on the abortion rate calculated by Lindo et al. (2020). They estimate that if access to abortion clinics had remained at pre-HB2 levels, Texas women would have had 122,315 legal abortions in 2014–2015 rather than the 107,830 observed in the abortion surveillance data, an estimated reduction of 14,485 abortions due to HB2. This corresponds to about 12 percent less abortions in 2014-2015 because of HB-2. The estimated effect of distance on violence is around one third of the first stage effect of distance on abortions.

### 5.2 Dynamic Treatment Effects

Although HB-2 was enforced on the exact same date for all clinics in Texas, not all clinics closed in the same period, although most did. This is due to the fact that (1) the first wave of closures happened in April 2013 after the introduction of HB-2, while the second wave occurred after the enforcement of the law in November 2013; (2) requirement two of HB-2 went into effect one year after the first requirement (on October 3, 2014), and even if its enforcement was blocked

[30] Figure D3 of Appendix D shows the effects of an increase in distance of 50, 100 and 150 miles for different levels of pre-policy distance to the nearest clinic.

only two weeks later by the Supreme Court, some clinics did temporarily shut down; (3) after some periods from the closures certain other clinics managed to reopen because they were able to comply with the law. Figure F2 of Appendix F plots the yearly change in distance from every Texas county to the nearest abortion clinic in every post-policy year. Black dots represent the municipalities included in the sample. As shown in the four panels of Figure F1, the treatment is dynamic and some observations are treated more than once.

I implement a Sun and Abraham event study, known to account for heterogenous and dynamic treatment effects. Since this estimator relies on an OLS model, I use as dependent variable the logarithm of the share of each municipality's number of cases over the population covered by the agency. To have a balanced sample and keep more observations as possible, I drop the first and last six-month periods from the sample and then I drop municipalities with missing values within this new sample period. The event is defined as the first period in which a municipality experience a positive change in distance. The event-time window ranges from 6 six-month periods (3 years) prior to HB-2 to 5 six-month periods (2.5 years) following adoption and endpoints are binned. The indicator for the first lead is omitted, meaning that the coefficients can be interpreted as the effect of a clinic closure that increases distance from the nearest clinic on gender violence cases relative to gender violence cases in the six-month period prior to the clinic closure. The event study is plotted in Figure 8. The estimates show a significant increase in gender violence offenses for several periods after the change in distance, confirming the validity of the TWFE design for the present analysis.

As shown in Appendix figure F1, some municipalities happened to be treated more than once.[31] I verify whether repeatedly treated observations bias the results. For all repeatedly treated municipalities, I include only the time period during which they are treated the first time. Results are shown in Table F1 of Appendix F. Coefficients decrease in size, as I restrict the sample of treated units and exclude some of the cities with the largest jumps in distance variations. The sign and significance of coefficients remain consistent, indicating that repeatedly treated observations do not create any bias in my results. For the same reason discussed in Section 5.1, this subsample of observations is not able to capture the quadratic relationship.

### 5.3 Heterogeneity by Type of Violence

The definition of gender violence used here includes all possible violent offenses in which the victim is a female of reproductive age and the offender is a male. In order to claim that the effect of abortion restrictions on gender violence is not driven by a particular type of offense – e.g., IPV – I disentangle the effect of distance on different forms of violence against women: IPV[32], offenses other than IPV[33], physical violence and sexual violence. The estimated coefficients are plotted in figure 9. The composition of the sample (92.6% physical violence, 7.37% sexual violence, 69.6% IPV) makes the confidence intervals around the estimated coefficients for offenses other than IPV and sexual violence offenses imprecisely estimated because of the very

[31] There is only one municipality in the sample treated three times, while the other municipalities are treated at most twice.

[32] IPV includes offenses where the victim is a female of reproductive age and the offender is a male partner or ex-partner of the victim.

[33] Offenses other than IPV includes all offenses except for cases where the offender is a partner/ex-partner of the victim.

high number of zeros. Nonetheless, the figure shows a consistent pattern of increase in each of these forms of violence caused by a 25-mile increase in distance to the nearest clinic.

Estimated coefficients are reported in Panel 1-4 of Table 3. If the closest clinic is 0 miles away, a 25-mile increase in the distance to the nearest clinic is associated with a 1.9 percent increase in the number of reported cases of intimate partner violence per municipality, a 1.1 percent increase in offenses other than IPV, a 2.4 percent increase in physical violence and a 4 percent increase in sexual violence. The effect of a 25-mile increase reduces as the initial distance increases.

For cases other than IPV and sexual violence - that constitute a small share of the sample - there is no evidence of a quadratic relationship between distance and violence. In the subsample of Texas municipalities that I am using the majority of cases are concentrated in the most populated part of the country – the East – where all municipalities were relatively close to the nearest clinic prior to HB-2. For this reason, it is not easy to capture this quadratic relationship, especially with such a large share of zeros as for sexual violence offenses and offenses other than IPV.

### 5.4 Heterogeneity by Race

My hypothesis is that one of the main channels through which abortion access affects violence against women is through its impact on women socio-economic conditions. In order to give some empirical evidence on the validity of such an assumption, I estimate the effect of distance to the nearest clinic on disadvantaged women, since the economic burden that derives from an unintended pregnancy must have greater negative effects on poorer women.

Beyond my assumption on the economic mechanism through which abortion access impacts violence, economically disadvantaged individuals might be more affected by the increase in distance to the nearest abortion clinic also because of their higher likelihood of experiencing unintended pregnancies. First, low-income women cannot turn to private physicians' offices and hospitals to obtain an abortion; second, they cannot afford to pay for travel and accommodation to reach a distant clinic; finally, they have lower access to contraceptives.

I exploit the fact that the NIBRS collects information on the race of the victim. First, I restrict the sample to all the offenses where the victim is of Hispanic origin since Hispanic individuals account for around 40 percent of the entire Texas population.[34] Then, I restrict the analysis to all the offenses where the victim is *Black or African American*, as the Black population constitutes one of the most economically and socially disadvantaged groups in the U.S. society – in 2016, the median household income of Hispanics was $49,887 and the one of Black Americans was $41,323, compared with $68,059 for non-Hispanic white Americans.[35]

The analysis on Hispanic women shows larger effects than the ones estimated on the entire population – up to 2.4 percent increase compared to the 1.9 percent increase found in the main analysis (Panel I, Table 4).

The analysis on Black women, in Panel II of Table 4, reveals much larger coefficients. When the nearest clinic is 0 miles away, a 25-mile increase in distance is associated with a 4.8 percent rise in gender violence cases against Black women.

With respect to the entire female population, the positive effect of an increase in the

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[34] U.S. Census Bureau

[35] U.S. Department of Commerce, Bureau of the Census, "Historical Income Tables: House- holds; Table H-5. Race and Hispanic Origin of Householder-Households by Median and Mean Income," 2017, https://www2.census.gov/programs-surveys/cps/tables/time-series/ historical-income-households/h05.xls.

distance to the closest clinic offering abortion is larger for Hispanic women and has more than doubled for black and African American women.

### 5.5 The Effect of Abortion Access on Violence Against Minors

An increase in intimate partner violence is likely to lead to an increase in violence within the household, that in turn can affect children living in the same home. I investigated the impact of distance to the closest clinic on violence against a minor (individuals aged 18 and younger) who is the child or stepchild of the victim and the offender is male. This specification is chosen to represent an increase in violence against minors from a man living in the household.

Estimates are presented in Table 5, for contemporaneous (Panel I) and lagged (Panel II) changes in distance. Coefficients show an increase in violence starting one year after clinic closure, with an estimated effect ranging from 3.6 to 8.4 percent. This result may reflect both children being trapped in abusive households or children born in abusive household resulting from unintended pregnancies. I speculate that coefficients for minors are higher than those of women as IPV has a particular low rate of reporting compared to other offenses.

### 5.6 Placebo Test: The Effect of Distance on Other Crimes

I perform a placebo test by estimating the effect of distance to the nearest abortion clinic on other crimes. To limit the analysis to crimes where the decrease in women's bargaining power is not involved, I consider only offenses where the victim, if any, is male. An unintended child may also have a negative effect on the economic situation of a couple, so lower access to abortion would generally increase the level of crime because of the consequently lower average socioeconomic conditions of the population. To account for this, I choose a list of crimes that are likely to be unrelated to a sudden decrease in socioeconomic status, especially in a short-run perspective. The list of crimes considered is reported in Appendix Table G1 and includes sex-related offenses, weapon law violation, bribery, and purchasing prostitution. I estimate the baseline model 1, finding non-significant coefficients of opposite sign with respect to the main estimates. All coefficients are reported in Table G2 of the Appendix.

# 6. Discussion and Conclusion

Results from the present analysis show that access to abortion services has a sizable effect on the incidence of violence against women of reproductive age, both in the private and public spheres. I find that, depending on the initial distance, a 25-mile increase in distance to the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality up to 1.9 percent, and the effect persists for the entire sample period, up to three years after clinics 'closure. In accordance with the literature that finds the effect of distance on abortions and births being a decreasing function of distance, the relationship of interest is non-linear, meaning that the effect is higher for municipalities relatively close to an abortion clinic before the implementation of the policy. Looking at the effect of distance on different forms of violence, evidence suggests that restrictions on abortion access have an impact on all forms of violence against women, not only IPV. In light of the evidence on the underreporting of violence, a phenomenon that tends to increase after the birth of a child, these results are likely to largely underestimate the effect of abortion access on violence.

Heterogeneous analyses by race of the victim provide evidence in support of the hypothesis on the key role of socio-economic conditions in explaining the mechanisms through which abortion restrictions impact gender violence. The effect is larger for the subsample of Hispanic women and more than doubles for Black women. Most disadvantaged women suffer the most from restrictions to abortion access, as they are more likely to experience unintended pregnancies in the first place, they have less means to obtain an abortion despite the limitations in access caused by clinics' closure, and they are more vulnerable to adverse socio-economic shocks.

The present analysis presents some limitations related to data constraints. Unlike UCR, NIBRS covers only a limited set of localities as among participating states not all police agencies are included. This results in an unbalanced panel that includes a subsample of Texas municipalities. Also, the paper external validity is partially limited by the characteristics of the municipalities included in the sample. Nearly 94% of municipalities in the dataset are located in urbanized areas implying that very few income-constrained rural areas are included in the sample. The effect of TRAP laws on more disadvantaged women is likely to be larger as shown by the evidence on heterogeneous effects by race of the victim presented in this paper. Thus the exclusion of rural municipalities may reduce the size of the estimated coefficients of the impact of abortion access on violence against women so that my results are an underestimation of the real effect. As its coverage grows, NIBRS will become a better source of information on violence against women allowing researchers to study the phenomenon on more representative samples.

The finding from this research broadens the boundaries of the debate on abortion policies that has reignited in recent years. Acknowledging that lower access to abortion implies lower autonomy and agency for women and, in turn, a higher risk of violence against them is concerning. This is especially true in light of the increasing number of state-based restrictions that limit women's access to abortion care in the U.S. as in many other regions of the world. Policies that restrict abortion provision may result in more women being unable to terminate unwanted pregnancies, potentially exposing them to higher risks of suffering abuse from partners and non-partners.

The discourse surrounding Targeted Regulation of Abortion Providers (TRAP) laws in the United States is deeply polarized. While proponents argue these laws are essential for maintaining high health and safety standards in abortion services, critics view them as strategic efforts to limit abortion access under the guise of safety. To the extent of my knowledge, no empirical study evaluates the effect of TRAP laws on women and children's health outcomes. One well-documented benefit of these laws is their significant influence on fertility outcomes, with TRAP regulations associated with decreased abortion rates and increased birth rates (Lindo et al.2020a; Fischer, Royer and White 2018; Venator and Fletcher 2020; Brown et al. 2020; Myers, 2021). This result aligns with the objective of many western countries of increasing their fertility rate to contrast the cost of population aging (Lee and Mason, 2011). On the opposite, a growing literature is investigating the costs of TRAP policies. The enforcement of TRAP laws has been linked to decreased educational opportunities for women, particularly teenagers (Jones and Pineda-Torres, 2021). This educational setback represents a significant societal cost, limiting young women's future employment prospects and earning potential. This effect is exacerbated by the disruptive impact of pregnancy on female employment opportunities (Bahn et al., 2020). Finally, this study sheds light on another cost associated with TRAP laws, which is the increase in the likelihood of women to be victim of violence. The social and economic costs associated with violence against women are enormous. Violence against women negatively impact women's physical, mental, sexual, and reproductive health. Specifically, violence against women (1) can lead to fatal outcomes like homicide or suicide, or to injuries; (2) increases the risk of unintended pregnancies, induced abortions, gynecological problems, and sexually transmitted infections, including HIV; (3) increases the risk of depression, post-traumatic stress and other anxiety disorders, sleep difficulties, eating disorders, and suicide attempts; (4) increase risk of acquiring sexually transmitted infections, including HIV; (5) increase likelihood of miscarriage, stillbirth, pre-term delivery and low birth weight babies; (6) finally, children who grow up in families where there is violence may suffer a range of behavioral and emotional problems. These can also be associated with perpetrating or experiencing violence later in life (World Health Organization 2021). The reduction in abortion rates and possible improvements in health and safety standards that have not been scientifically evaluated yet are overshadowed by the substantial costs incurred through detrimental effects on women's education, employment opportunities, and physical and mental health. The socio-economic and health-related costs of TRAP laws seem to outweigh their purported benefits, suggesting that these laws may be more harmful to society than beneficial.

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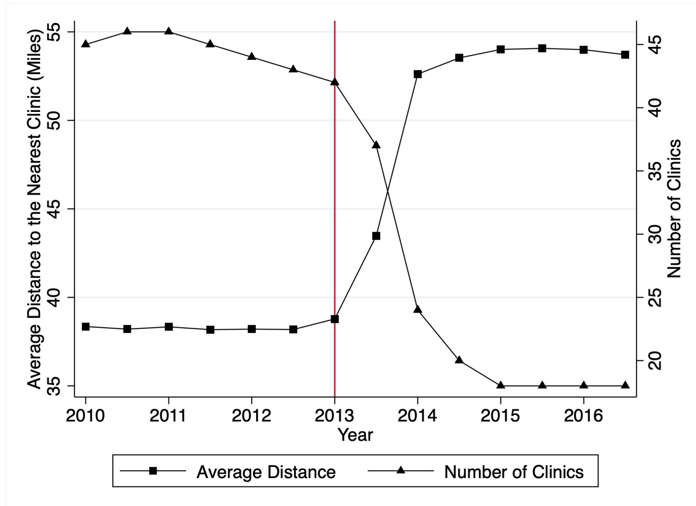
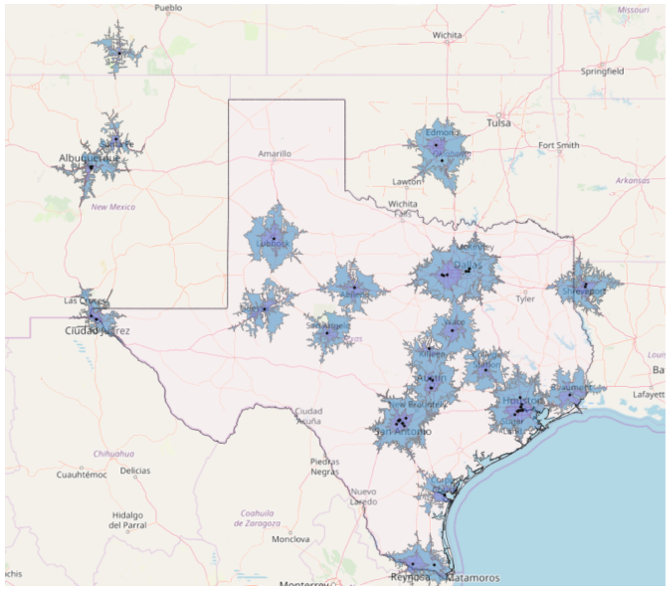
**Figure 1: Number of Abortion Clinics and Average Distance from Texas Municipalities to the Nearest Abortion Clinic, 2010-2016**



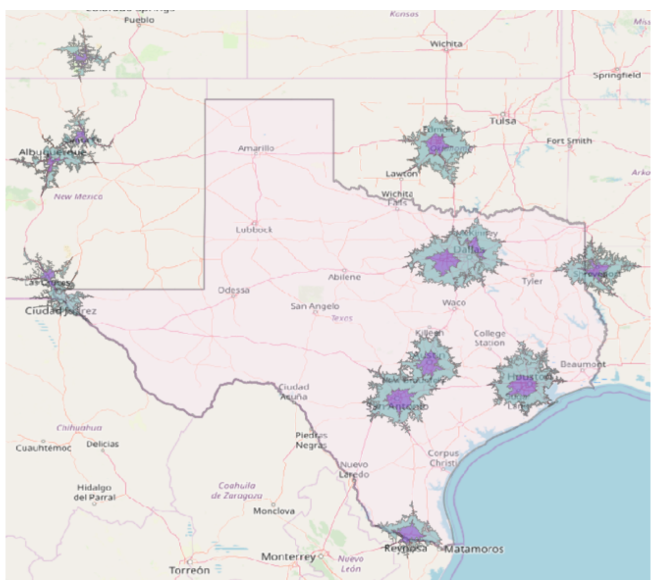

Note: Closure of abortion clinics after Texas HB-2 and increase in average distance from each municipality in the sample to the nearest abortion clinic. The red vertical line represents the implementation of HB-2. Source: Abortion clinic names and opening and closing dates are taken from Lindo et al. (2020*a*). The average distance is calculated for all the municipalities of the sample for the period 2010 to 2016.

**Figure 2: Accessibility of Abortion Clinics in Texas and Neighboring States, 2009 and 2016**

(a) Abortion clinics in 2009

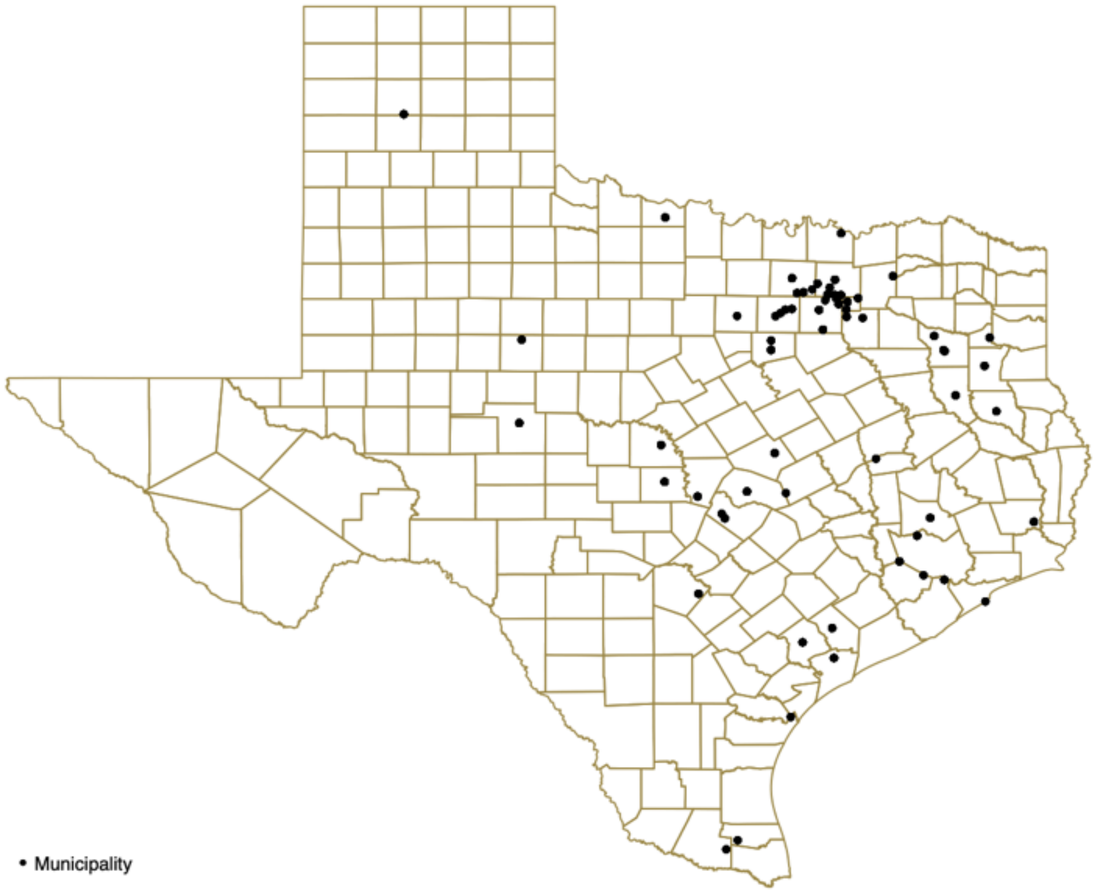



(b) Abortion clinics in 2016



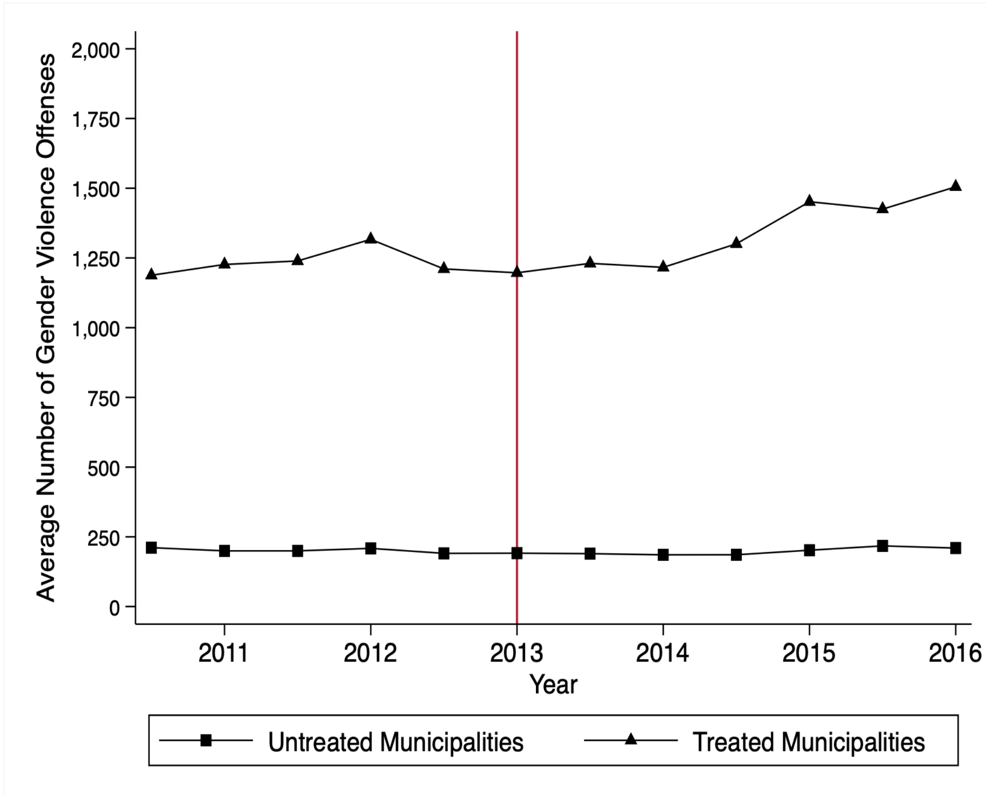

Note: Abortion clinics in Texas and neighboring states in 2009 and 2016. The purple and blue isochrones around each black point draw respectively an area of 30-minute and one-hour distance from the point.

**Figure 3: Municipalities Included in the Sample**



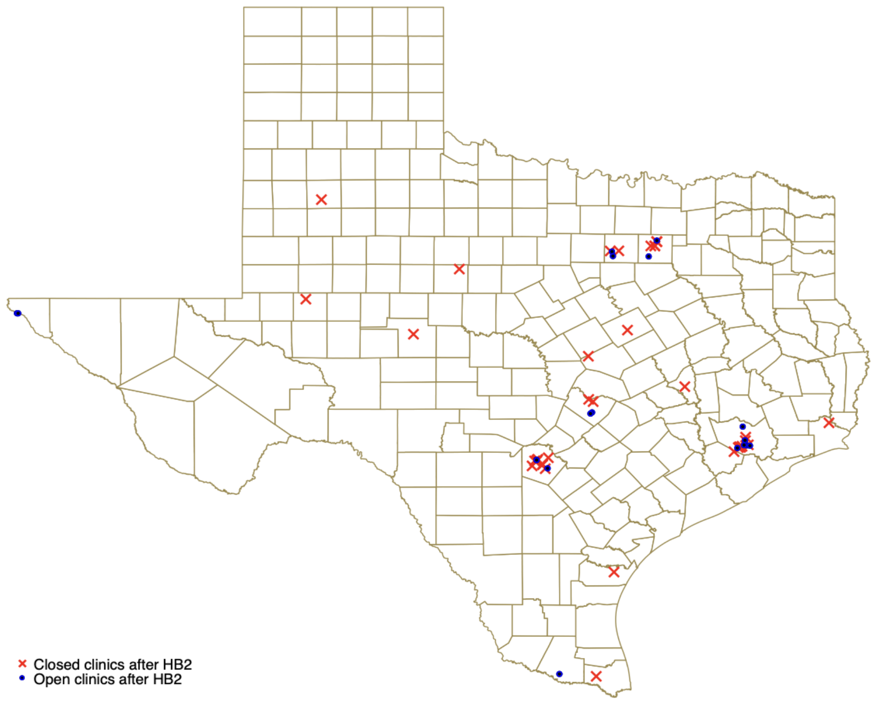

Note: Black points plot the municipalities included in the sample.

**Figure 4: Time Trends in Gender Violence, 2010-2016**



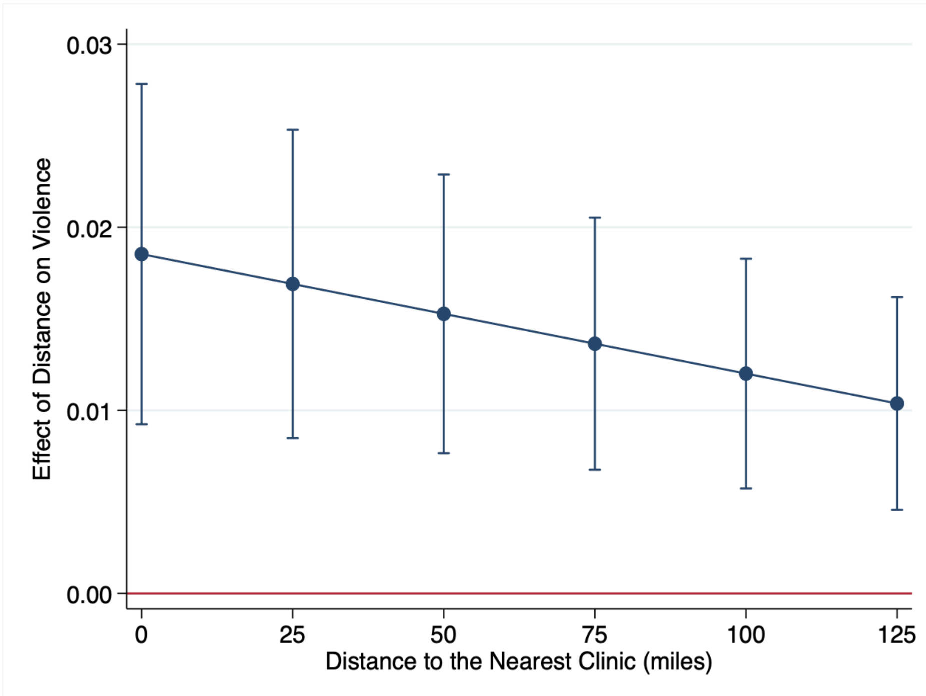

Note: Average time trends in gender violence for the subsample of treated and untreated municipalities. Treated municipalities are the ones that experienced a positive change in distance to the nearest abortion clinic during the sample period. Trends are calculated on the balanced subsample of observations.

**Figure 5: Open and Closed Abortion Clinics in Texas After House Bill 2**



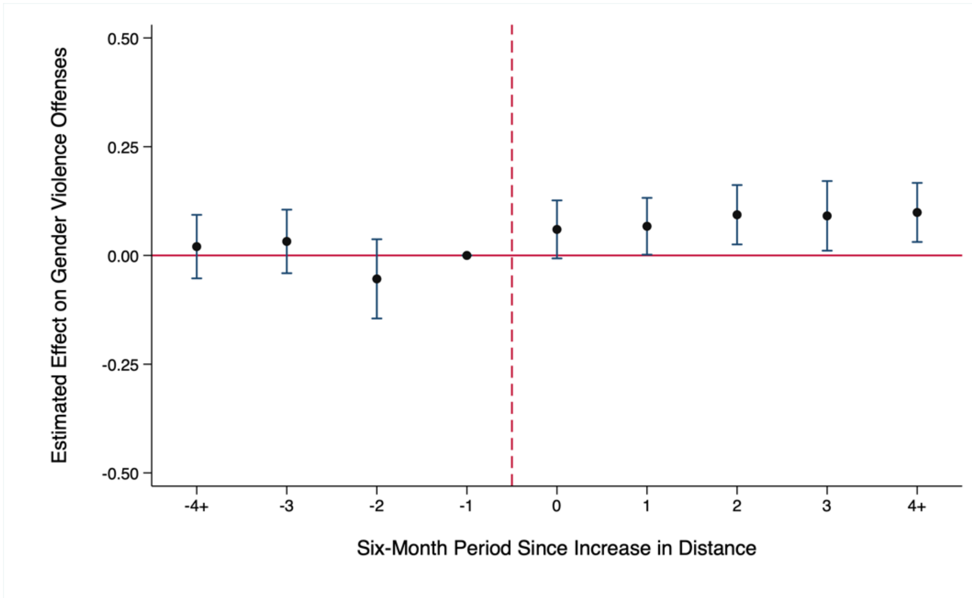

Note: Geographic distribution of abortion clinics after HB-2. Crosses represent closed clinics, while points are those that remain open. The light brown lines mark county borders.
Source: Abortion clinics' opening and closing dates are taken from Lindo et al. (2020a).

**Figure 6: Effect of 25-mile Increase in Distance to the Nearest Abortion Clinic on Gender Violence by Starting Level, 2010-2016**



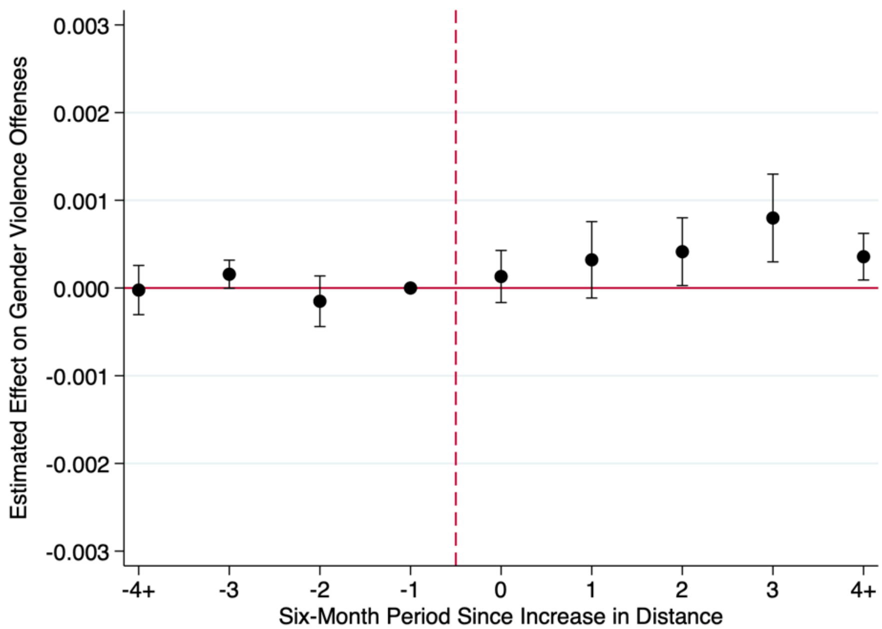

Note: Plot of estimated effects and 95% confidence intervals based on results in Column 3 of Table 2.

**Figure 7: Event Studies Analysis of the Impact of a Positive Variation in Distance on Gender Violence, Using TWFE Estimates, 2010-2016**



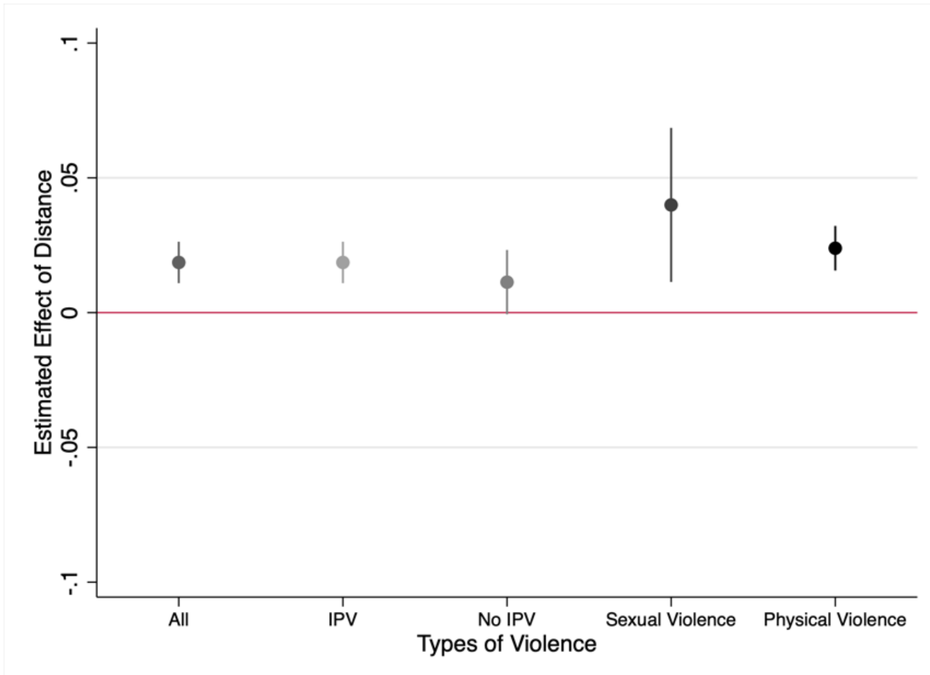

Note: The event study regression is estimated using data from the National Incident-based Reporting System from 2010 to 2016 and the balanced subsample of observations. The dependent variable is the count of gender violence offenses in each municipality. The event study regression is estimated via Poisson model and includes controls for municipality fixed effects, six-month fixed effects, and the following county-level controls: share of females of reproductive age (15-49), the logarithm of the county income per capita, and the unemployment rate. The exposure variable included in all regressions is the reference population of each reporting agency. The event-time window ranges from 4 six-month periods (2 years) prior to HB-2 to 4 six-month periods (2 years) following adoption. The reference period is six-month to HB-2. Circles indicate coefficient estimates. 95 percent confidence intervals that account for within municipality clustering are reported with vertical lines.

**Figure 8: Event Studies Analysis of the Impact of a Positive Variation in Distance on Gender Violence, Using Sun and Abraham Estimates, 2010-2016**




Note: The event study regression is estimated using data from the National Incident-based Reporting System from 2010 to 2016 and the balanced subsample of observations. The dependent variable is the count of gender violence offenses in each municipality. Estimates are generated with a Sun and Abraham event study and the regression controls for municipality fixed effects, six-month fixed effects, and the following county-level controls: share of females of reproductive age (15-49), the logarithm of the county income per capita, and the unemployment rate The exposure variable included in all regressions is the reference population of each reporting agency. The event-time window ranges from 4 six-month periods (2 years) prior to HB-2 to 4 six-month periods (2 years) following adoption. The reference period is 1 period to HB-2. Circles indicate coefficient estimates. 95 percent confidence intervals that account for within municipality clustering are reported with vertical lines.

**Figure 9. Heterogeneity in the Effect of Distance to the Nearest Abortion Clinic on Gender Violence by Type of Offense, 2010-2016**




Note: Estimates are generated via Poisson regressions using data drawn from the National Incident-Based Reporting System for 63 Texas municipalities from 2010 to 2016. The dependent variable is the count of gender violence offenses in each municipality. The analysis is at the six-month-municipality level and all regressions include municipality and six-month fixed effects, and the following county-level controls: share of females of reproductive age (15-49), the logarithm of the county income per capita, and the unemployment rate. The exposure variable included in all regressions is the reference population of each reporting agency. Circles indicate coefficient estimates. 95 percent confidence intervals that account for within municipality clustering are reported with vertical lines.

**Table 1: Population-Weighted Summary Statistics, Before and After Texas House Bill 2**

| | Before HB-2 | | After HB-2 | |
|---|---|---|---|---|
| | Mean | Standard Deviation | Mean | Standard Deviation |
| Cases of Gender Violence | 714.657 | 879.416 | 853.8088 | 1022.648 |
| Distance to the Nearest Clinic (Miles) | 28.265 | 34.553 | 46.658 | 66.768 |
| Agency Population | 264,140.9 | 286,973.3 | 30,4345.7 | 318,889.9 |
| County Population | 1,475,198 | 685,030 | 1,499,320 | 744,545.8 |
| Share of Hispanic Females (15-49) | 0.297 | 0.113 | 0.307 | 0.107 |
| Share of Black Females (15-49) | 0.159 | 0.058 | 0.160 | 0.060 |
| Share of Females (15-49) | 0.253 | 0.011 | 0.249 | 0.012 |
| Log (Income Per Capita $) | 10.701 | 0.113 | 10.831 | 0.107 |
| Unemployment Rate | 7.114 | 0.921 | 4.505 | 0.787 |
| Number of Observations | 343 | | 331 | |

Note: Population-weighted summary statistics calculated for 63 Texas municipalities for the pre-HB-2 period (2010 - first half of 2013) and post-HB-2 period (second half of 2013-2016).
Source: Abortion clinics opening and closing dates are taken from Lindo et al. (2020*a*). The average distance is calculated by the author for all the municipalities in the sample. Gender violence offenses and population covered by each agency are taken from the National Incident-Based Reporting System. County-level demographic controls are taken from the National Institute of Health Surveillance, Epidemiology and End Results, while county-level income per capita estimates are from the U.S. Bureau of Economic Activity. The unemployment rate by county is taken from the U.S. Bureau of Labor Statistics.

**Table 2. Estimated Effect of 25-Miles Increase in Distance to the Nearest Abortion Clinic on Gender Violence, 2010-2016**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel I: Contemporaneous Effect** | | |
| $Distance_t$ (25 Miles) | 0.009 | 0.009 | 0.019 |
| | (0.003) | (0.003) | (0.005) |
| $Distance_t^2$ (25 Miles) | | | -0.0008 |
| | | | (0.0003) |
| | **Panel II: Lagged Effect** | | |
| $Distance_{(t-2)}$ (25 Miles) | 0.008 | 0.008 | 0.012 |
| | (0.003) | (0.003) | (0.005) |
| $Distance^2_{(t-2)}$ (25 Miles) | | | -0.0003 |
| | | | (0.0003) |
| N | 673 | 673 | 673 |
| Municipality and Six-Month FE | Yes | Yes | Yes |
| Time-Varying Controls | No | Yes | Yes |

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

**Table 3. Estimated Effect of 25-Miles Increase in Distance to the Nearest Abortion Clinic on Gender Violence by Type of Offense, 2010-2016**

| | (1) | (2) | (3) |
|---|---|---|---|
| | **Panel I: Intimate Partner Violence** | | |
| $\text{Distance}_t$ (25 Miles) | 0.009 | 0.009 | 0.019 |
| | (0.003) | (0.003) | (0.005) |
| $\text{Distance}_t^2$ (25 Miles) | | | -0.0008 |
| | | | (0.0003) |
| | **Panel II: Non-IPV** | | |
| $\text{Distance}_t$ (25 Miles) | 0.010 | 0.012 | 0.011 |
| | (0.004) | (0.004) | (0.007) |
| $\text{Distance}_t^2$ (25 Miles) | | | 0.00004 |
| | | | (0.0004) |
| | **Panel III: Physical Violence** | | |
| $\text{Distance}_t$ (25 Miles) | 0.011 | 0.011 | 0.024 |
| | (0.004) | (0.004) | (0.005) |
| $\text{Distance}_t^2$ (25 Miles) | | | -0.0011 |
| | | | (0.0003) |
| | **Panel IV: Sexual Violence** | | |
| $\text{Distance}_t$ (25 Miles) | 0.058 | 0.045 | 0.039 |
| | (0.011) | (0.008) | (0.017) |
| $\text{Distance}_t^2$ (25 Miles) | | | 0.0005 |
| | | | 0.001 |
| N | 673 | 673 | 673 |
| Municipality and Six-Month FE | Yes | Yes | Yes |
| Time-Varying Controls | No | Yes | Yes |

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model and the analysis is at the six-month municipality level. All regressions include municipality and six- month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

**Table 4. Estimated Effect of 25-Miles Increase in Distance to the Nearest Abortion Clinic on Gender Violence by Race of the Victim, 2010-2016**

| | (1) | (2) | (3) |
|---|---|---|---|
| | | **Panel I: Hispanic Women** | |
| $Distance_t$ (25 Miles) | -0.002 | -0.001 | 0.024 |
| | (0.006) | (0.006) | (0.004) |
| $Distance_t^2$ (25 Miles) | | | -0.0023 |
| | | | (0.0004) |
| | | **Panel I: Black Women** | |
| $Distance_t$ (25 Miles) | 0.018 | 0.021 | 0.048 |
| | (0.006) | (0.006) | (0.017) |
| $Distance_t^2$ (25 Miles) | | | -0.002 |
| | | | (0.001) |
| N | 673 | 673 | 673 |
| Municipality and Six-Month FE | Yes | Yes | Yes |
| Time-Varying Controls | No | Yes | Yes |

Note: Estimated effect of distance to the nearest abortion clinic on violence against women for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

**Table 5. Estimated effect of a 25-mile Increase in Distance to the Nearest Abortion Clinic on Violence Against Minors, 2010-2016.**

| | (1) | (2) | (3) |
|---|---|---|---|
| Distance (25 miles) | 0.011 | 0.010 | 0.032 |
| | (0.013) | (0.013) | (0.023) |
| $Distance^2$ (25 miles) | | | -0.002 |
| | | | (0.002) |
| | | | |
| $Distance_{t-2}$ (25 miles) | 0.043 | 0.036 | 0.084 |
| | (0.008) | (0.009) | (0.034) |
| $Distance^2_{t-2}$ (25 miles) | | | -0.004 |
| | | | (0.003) |
| Number of Observations | 673 | 673 | 673 |
| Municipality and Six-Month FE | Yes | Yes | Yes |
| Time-Varying Controls | No | Yes | Yes |

Note: Estimated effect of distance to the nearest abortion clinic on violence on minors for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

# Appendix A Data Description

**Table A1. Population-weighted summary statistics, 2010-2016**

| | 2010-2016 | | | | |
|---|---|---|---|---|---|
| | **Mean** | **S.D.** | **Min.** | **Max.** | **N** |
| Cases of Gender Violence | 783.3459 | 954.721 | 0 | 2,836 | 673 |
| Distance to the Nearest Clinic | 37.359 | 53.738 | 1.77 | 276.65 | 673 |
| Agency Population | 283,974. | 303,610.5 | 686 | 851,849 | 673 |
| County population | 1,486,99 | 714,494.2 | 3,258 | 2,587,46 | 673 |
| Share of Hispanic Females (15-49) | 0.302 | 0.1099 | 0.048 | 0.924 | 673 |
| Share of Black Females (15-49) | 0.159 | 0.059 | 0.006 | 0.273 | 673 |
| Share of Females (15-49) | 0.252 | 0.0118 | 0.141 | 0.274 | 673 |
| Log (Income Per Capita $) | 10.765 | 0.128 | 9.891 | 11.055 | 673 |
| Unemployment Rate | 5.832 | 1.562 | 2.942 | 12.967 | 673 |

Note: Population-weighted summary statistics calculated for 63 Texas municipalities for the period 2010- 2016. Source: Abortion clinics' opening and closing dates are taken from Lindo et al. (2020*a*). The average distance is calculated by the author for all the municipalities in the sample. Gender violence offenses and population covered by each agency are taken from the National Incident-Based Reporting System. County- level demographic controls are taken from the National Institute of Health Surveillance, Epidemiology, and End Results, while county-level income per capita estimates are from the U.S. Bureau of Economic Activity. The unemployment rate by county is taken from the U.S. Bureau of Labor Statistics.

**Table A2. Sample selection**

| | |
|---|---|
| Initial Sample | 882 |
| Excluding Observation with at Most 2 Periods | 816 |
| Excluding County-level Observations | 687 |
| Excluding Observations Without Reference Population | 673 |
| Final Sample | 673 |

Note: Sample selection. Years 2010-2016. Source: National Incident-Based Reporting System.

## Municipalities in the Sample

1. Allen
2. Amarillo
3. Aransas Pass
4. Bedford
5. Bee Cave
6. Cleburne
7. Conroe
8. Denton
9. Denton
10. Edna
11. Flower Mound
12. Forney
13. Fort Worth
14. Frisco
15. Galveston
16. Georgetown
17. Haltom City
18. Heath
19. Henderson
20. Highland Park
21. Iowa Park
22. Isd: East Central
23. Joshua
24. Katy
25. La Villa
26. Lakeway
27. Lancaster
28. Lewisville
29. Lindale
30. Llano
31. Longview
32. Lumberton
33. Lyford
34. Marble Falls
35. McKinney
36. Missouri City
37. Murphy

38. Nacogdoches
39. Normangee
40. North Richland Hills
41. Pearland
42. Plano
43. Port Lavaca
44. Richardson
45. Rockwall
46. Rowlett
47. Royse City
48. Rusk
49. Sachse
50. San Angelo
51. San Saba
52. Sweetwater
53. Temple
54. Terrell
55. Texas A&M Univ: Commerce
56. The Colony
57. Thorndale
58. Tomball
59. Tyler
60. Tyler Junior College
61. Victoria
62. Weatherford
63. Wylie

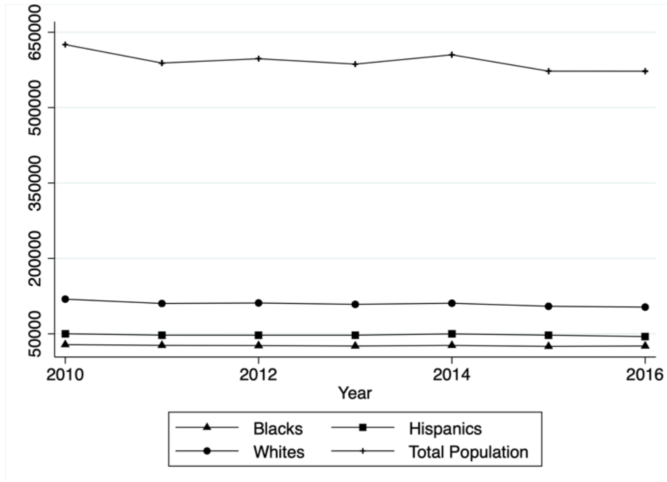
**Table A3. Type of Offenses**

| | | |
|---|---|---|
| Aggravated assault | Human trafficking – commercial sex acts | Pornography/obscene material |
| Simple assault | Sexual assault with an object | Prostitution |
| Intimidation | Forcible fondling | Assisting or promoting prostitution |
| Murder/nonnegligent manslaughter | Statutory rape | Purchasing prostitution |
| Negligent manslaughter | Human trafficking – involuntary servitude | Forcible rape |
| Justifiable homicide | Kidnaping/abduction | Forcible sodomy |

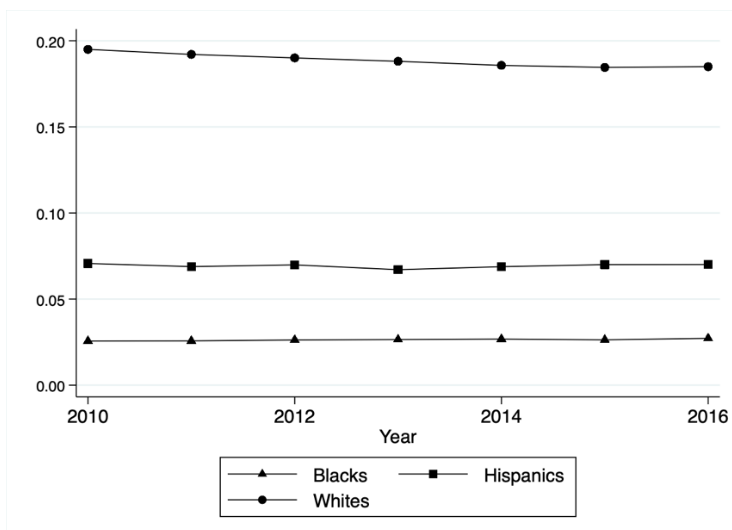
Source: NIBRS, 2010-2016

**Figure A3. Time Trends in Racial Composition, SEER 2010-2016**

Panel (a) Absolute number




Panel (b) Share




Note: Trends in the county absolute numbers and shares of females aged 15-49 of Hispanic, Black, and White ethnicity.

# Appendix B Random Assignment of Treatment

The basic assumption is that the variation in the distance from a municipality to its nearest abortion clinic is exogenous to the model, since it is a consequence of the fact that some clinics randomly met the standards imposed by HB-2 while others did not and had to shut down. The opening and closing of clinics create a variation in geographic accessibility to abortion facilities that is randomly distributed within the state of Texas. Therefore, treatment (change in distance) is good as randomly assigned and the control group is comprised of those municipalities that experienced no variation or very small variation in the access to abortion clinics (Callaway, Goodman-Bacon and Sant'Anna, 2021).

Provision (1) of HB-2 required all abortion providers to have admitting privileges at a hospital located within 30 miles of the abortion clinic. As first preliminary check on randomness of treatment, I verified that each clinic's municipality has a hospital inside these boundaries,[36] that is, within 30 miles. However, it could be the case that hospitals in more conservative areas are less likely to grant admitting privileges. The distribution of clinics' closure within Texas state borders shows a different pattern, since there are no clusters of closures, which are instead spread across the entire state (Figure 5).

Randomness of treatment is then tested using regression analysis. First, I check whether some controls could have an impact on clinics' closures, resulting in failure of the randomness assumption. In the first test, Poisson two-way fixed effect regression is used to

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[36] https://healthdata.dshs.texas.gov/dashboard/hospitals/texas-hospital-data

estimate the impact of distance from each municipality to the nearest abortion clinic on the portion of cases of gender violence predicted by the control variables (Table B1). In column (1), the dependent variable is reported cases of gender violence and the independent variables are all controls. As expected, macroeconomic conditions (*unemployment rate*) are risk factors for gender violence. For this reason, the main specification controls for both the unemployment rate and income per capita. In column (2), the predicted cases of violence are regressed on the variable of interest (distance to the nearest clinic). The coefficient is non-significant, providing evidence in support of randomness of treatment. To further investigate the issue, several OLS two-way fixed effect regressions are used to estimate a balance test of the impact of covariates on distance to the closest clinic (Table B2). For the OLS model, the measure of distance is logarithmic, to avoid non-normal distributions. None of the estimated coefficients is statistically significant, except for income per capita that shows a weak negative correlation with distance.

Finally, I check whether county characteristics have an impact on the clinic's probability of being closed in each period (Table B3). All regressions include six-month and county fixed effects. None of the coefficients is statistically significant. This gives credit to the assumption of randomness of the treatment and excludes the hypothesis of a reverse causality problem.

Provision (2) of HB-2 states that all abortion facilities must meet the requirements of an ambulatory surgical center. Even if clinics' characteristics may correlate with the clinic's location, the enforcement of this requirement was blocked two weeks after its implementation by the U.S. Supreme Court, only causing some short-term closures.

**Table B1: The Estimated Effect of Distance on the Predicted Gender Violence Offenses, 2010-2016**

| | (1) | (2) |
|---|---|---|
| | Gender violence | Predicted Gender Violence |
| Distance (miles) | | −0.0002 |
| | | (0.0001) |
| Unemployment Rate | 0.027 | |
| | (0.013) | |
| Income per Capita (log) | 0.380 | |
| | (0.497) | |
| Share of Females Aged 15-49 | −0.393 | |
| | (7.737) | |
| Municipality and Six-Month FE | Yes | Yes |
| Number of Observations | 673 | 673 |

Note: Estimated effect of distance to the nearest abortion clinic on the portion of gender violence predicted by controls (*Predicted GV*). Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. The reference population of each reporting agency is included in every regression as exposure variable. Robust standard errors are reported in parentheses and are clustered at the municipal level.

## Table B2: The Estimated Effect of Covariates on Distance, 2010-2016

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Log(Distance) | Log(Distance) | Log(Distance) | Log(Distance) |
| Log(Agency Pop.) | -0.355 | | | |
| | (0.370) | | | |
| Log(Income per Capita) | | -0.712 | | |
| | | (0.408) | | |
| Unemployment Rate | | | 0.058 | |
| | | | (0.086) | |
| Share of Females (15-49) | | | | -7.063 |
| | | | | (12.672) |
| Municipality FE and Six-Month FE | Yes | Yes | Yes | Yes |
| Number of Observations | 673 | 673 | 673 | 673 |

Note: Estimated effect of control variables on the distance to the nearest abortion clinic. Estimates are based on a OLS model, and the analysis is at the municipality-six-month level. Robust standard errors are reported in parentheses and are clustered at the municipal level.

**Table B3: Estimated Effect of Covariates on the Clinics' Probability of Closure, 2010-2016**

| | (1) |
|---|---|
| | Probability of Closure |
| County Population (log) | −0.731 |
| | (2.375) |
| Log(Income per Capita) | 0.127 |
| | (0.955) |
| Log(Unemployment Rate) | 0.060 |
| | (0.069) |
| Log(Share of Females aged 15-49) | 6.383 |
| | (6.427) |
| County and Six-Month FE | Yes |
| Number of Observations | 812 |

Note: Estimated effect of county covariates on the clinic's probability of closure in each period. Coefficients are estimated through a linear probability model, and the analysis is at the six-month-county level. All explanatory variables are logarithms. Robust standard errors are reported in parentheses and are clustered at the county level.

# Appendix C. Test for Parallel Trend Assumption

**Table C1: Event study: Effect of an increase in distance on gender violence, 2010-2016**

| | (1) |
|---|---|
| 4+ Periods Before | 0.0203 |
| | (0.0372) |
| 3 Periods Before | 0.0322 |
| | (0.0373) |
| 2 Periods Before | -0.0539 |
| | (0.0464) |
| 1 Period Before | - |
| | |
| Event | 0.0599 |
| | (0.0340) |
| 1 Period After | 0.0671 |
| | (0.0333) |
| 2 Periods After | 0.0936 |
| | (0.0348) |
| 3 Periods After | 0.0910 |
| | (0.0408) |
| 4+ Periods After | 0.0989 |
| | (0.0346) |
| Number of Observations | 420 |

Note: Estimated effect of an increase in distance on gender violence for 63 Texas cities from 2010 to 2016. The model is equivalent to the one used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to six-month periods relative to the event. The event is defined as the first period in which distance increases. The six-month period prior to the event is omitted as it is the reference group. Robust standard errors are reported in parentheses and are clustered at the municipal level.

To further investigate the parallel trend assumption, I test whether changes in distance faced by municipalities following the closures are predictive of pre-policy trends in reported cases of gender violence. I regress the change in cases between 2010 and 2013 on the change in distance between 2013 and 2016:

$$GV_{i,2013} - GV_{i,2010} = \beta_0 + \beta_1(dist_{i,2016} - dist_{i,2013}) + \varepsilon_i \qquad (2)$$

Table C2 below shows the results. There is no significant effect of distance changes in the post-policy period on trends in cases in the pre-policy period.

**Table C2: The effect of distance changes after clinics' closure on trends in gender violence prior to closure, 2010-2016**

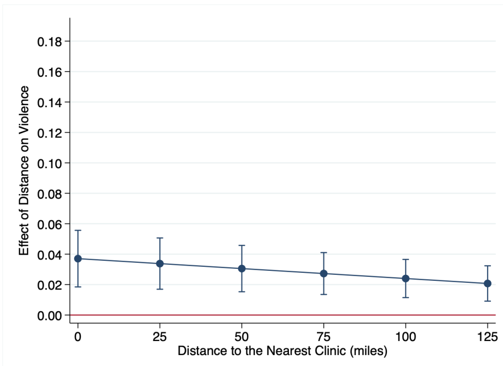
| | (1) |
|---|---|
| | $\Delta_{2013\text{-}2016}$ Distance |
| $\Delta_{2010\text{-}2013}$ Gender Violence | −0.486 |
| | (0.609) |
| Number of Observations | 34 |

Note: Estimated effect of changes in distance to the nearest abortion clinic between 2013 and 2016 on annual cases of gender violence between 2010 and 2013. Robust standard errors are reported in parentheses.

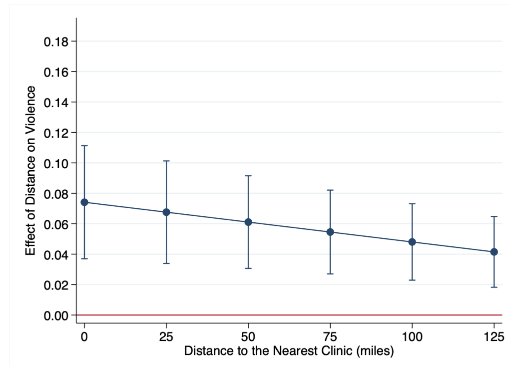
# Appendix D. Additional Results

**Figure D1: Effect of an Increase in Distance to the Nearest Abortion Clinic on Gender Violence by Starting Level, 2010-2016**

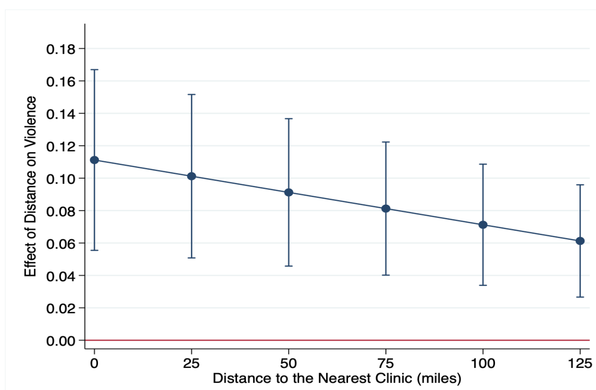
Panel (a) Increase in Distance of 50 miles




Panel (b) Increase in Distance of 100 miles

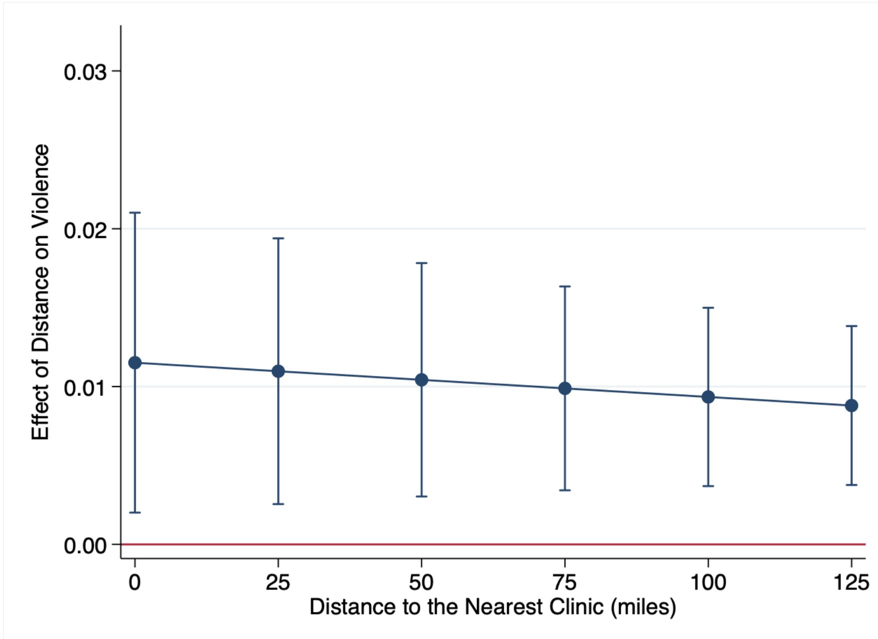



Panel (c) Increase in Distance of 150 miles




Note: Plot of estimated coefficients of the effect of distance on gender violence and 95% confidence intervals.

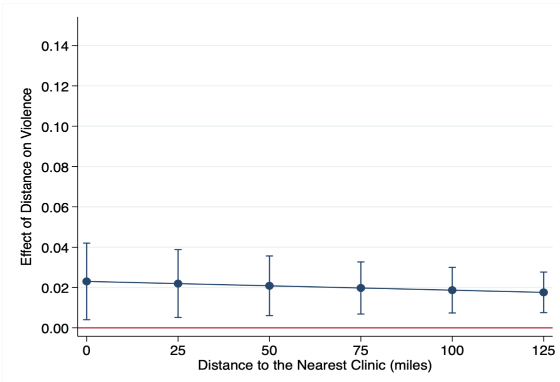
**Figure D2: Lagged effect of a 25-mile Increase in Distance to the Nearest Abortion Clinic on Gender Violence by Starting Level, 2010-2016**



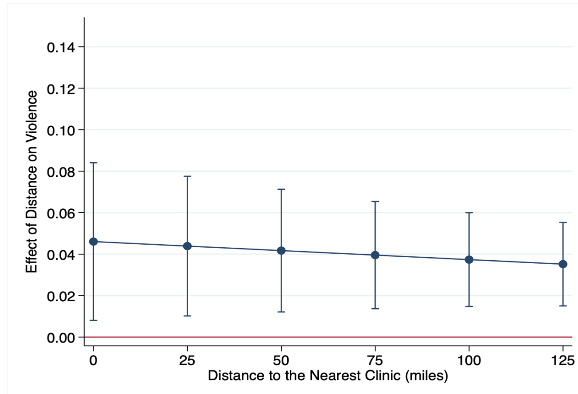

Note: Plot of estimated effects and 95% confidence intervals based on results in Column 3 of Table 3.

**Figure D3: Lagged Effect of an Increase in Distance to the Nearest Abortion Clinic on Gender Violence by Starting Level, 2010-2016**

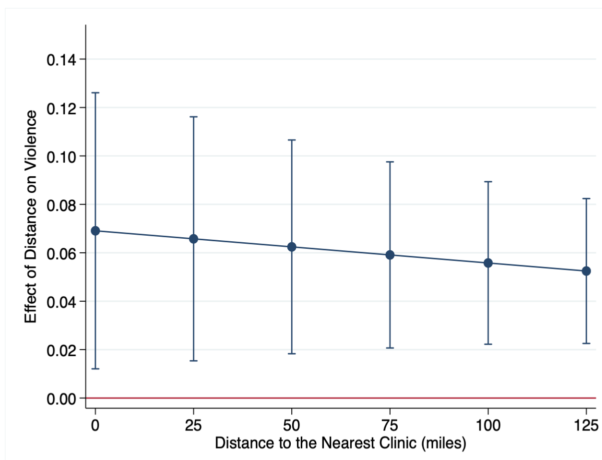
(a) Increase in Distance of 50 miles




(b) Increase in Distance of 100 miles




(c) Increase in Distance of 150 miles




Note: Plot of estimated coefficients of the effect of distance on gender violence and 95% confidence intervals.

# Appendix E. Robustness Checks and Sensitivity Analysis

**Table E1: Estimated Effect of a 25-mile Increase in Distance to the Nearest Abortion Clinic on Gender Violence, Accounting for County Racial Composition, 2010-2016**

| | (1) | (2) |
|---|---|---|
| *Distance$_t$* (25 *miles*) | 0.009 | 0.017 |
| | (0.006) | (0.006) |
| *Distance$_t^2$* (25 *miles*) | | -0.0008 |
| | | (0.0003) |
| Municipality and Six-Month FE | Yes | Yes |
| Time-Varying Controls | Yes | Yes |
| Number of Observations | 673 | 673 |

Note: Estimated effect of distance to the nearest abortion clinic on gen- der violence for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county in- come per capita, and unemployment rate per county, shares of Black and Hispanic females of reproductive age (15-49) per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

**Table E2: Estimated Effect of a 25-mile Increase in Distance to the Nearest Abortion Clinic on Gender Violence, Using Year Fixed Effects, 2010-2016**

| | (1) | (2) | (3) |
|---|---|---|---|
| *Distance$_t$* (25 *miles*) | 0.009 | 0.009 | 0.021 |
| | (0.003) | (0.003) | (0.005) |
| *Distance$_t^2$* (25 *miles*) | | | -0.001 |
| | | | (0.0003) |
| Municipality and Six-Month | Yes | Yes | Yes |
| Time-Varying Controls | Yes | Yes | Yes |
| Number of Observations | 673 | 673 | 673 |

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

**Table E3: Estimated Effect of a 25-mile Increase in Distance to the Nearest Abortion Clinic on Gender Violence, Using the Balanced Subsamples, 2010-2016**

| | (1) | (2) | (3) |
|---|---|---|---|
| $Distance_t$ (25 *miles*) | 0.010 | 0.010 | 0.019 |
| | (0.004) | (0.003) | (0.005) |
| $Distance_t^2$ (25 *miles*) | | | -0.0008 |
| | | | (0.0003) |
| Municipality and Six-Month | Yes | Yes | Yes |
| Time-Varying Controls | Yes | Yes | Yes |
| Number of Observations | 476 | 476 | 476 |

Note: Estimated effect of distance to the nearest abortion clinic on gender violence (GV), using different samples. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six- month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time- varying controls are share of females in reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are re- ported in parentheses and are clustered at the municipal level.

**Table E4: Estimated Effect of a 25-mile Increase in Distance on Gender Violence, Using Different Samples, 2010-2016**

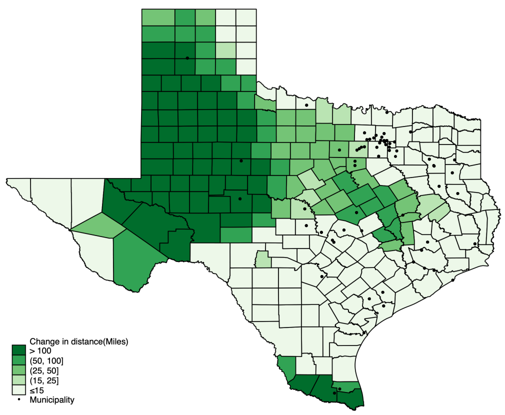
| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Pop. < 90 Percentile** | | **Change ≤ 150 Miles** | |
| $Distance_t$ (25 *miles*) | 0.013 | 0.013 | 0.0062 | 0.0074 |
| | (0.003) | (0.004) | (0.003) | (0.004) |
| Municipality and Six-Month | Yes | Yes | Yes | Yes |
| Time-Varying Controls | Yes | Yes | Yes | Yes |
| Number of Observations | 656 | 656 | 656 | 656 |

Note: Estimated effect of distance to the nearest abortion clinic on gender violence (GV), using different samples. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females in reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

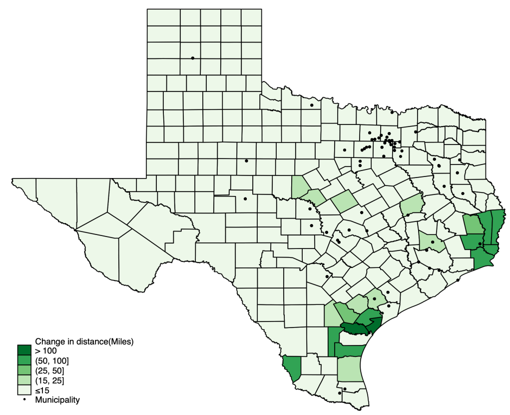
# Appendix F Dynamic Treatment Effects

**Figure F1: Yearly change in distance from each Texas county to the nearest abortion clinic and municipalities in the sample**

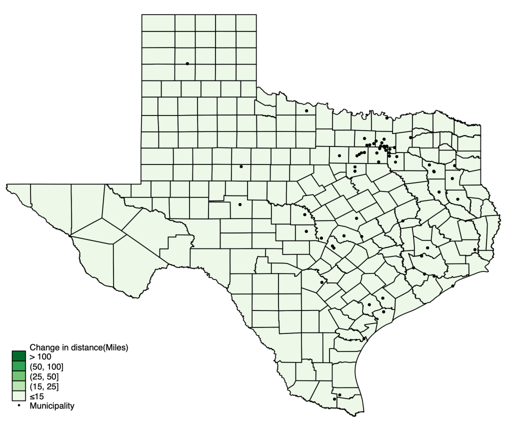
(a) Yearly county change in distance to the nearest abortion clinic from January 2013 to December 2013



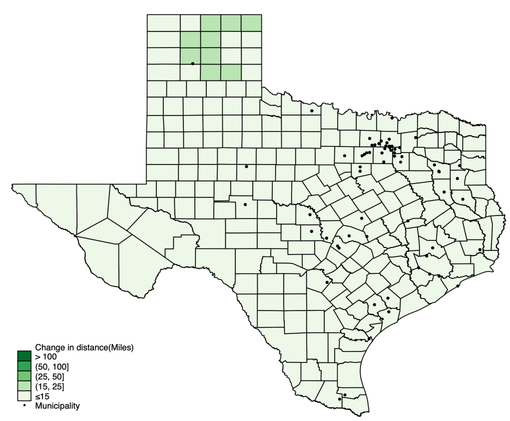

(b) Yearly county change in distance to the nearest abortion clinic from January 2014 to December 2014




(c) Yearly county change in distance to the nearest abortion clinic from January 2015 to December 2015




(d) Yearly county change in distance to the nearest abortion clinic from January 2016 to December 2016




Note: Yearly change in distance from each Texas county population centroid to the nearest abortion clinic. Black dots are municipalities in the sample.
Source: Travel distance from each county population-weighted centroid to the nearest abortion clinic is taken from the Myers Abortion Facility Database.[a]

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[a]Myers, C. (2021). County-by-month travel distances to nearest abortion provider, June 1, 2021. Retrieved from osf.io/pfxq3 DOI 10.17605/OSF.IO/8DG7R.

**Table F1: Estimated Effect of a 25-mile Increase in Distance to the Nearest Abortion Clinic on Gender Violence, Accounting for Repeatedly Treated Observations, 2010-2016**

| | (1) | (2) |
|---|---|---|
| $Distance_t$ (25 *miles*) | 0.007 | 0.007 |
| | (0.003) | (0.003) |
| Municipality and Six-Month FE | Yes | Yes |
| Time-Varying Controls | No | Yes |
| Number of Observations | 665 | 665 |

Note: Estimated effect of distance to the nearest abortion clinic on gen- der violence for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipal- ity level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of females of reproductive age (15-49) per county, the logarithm of the county income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.

# Appendix G. Placebo Test

**Table G2. Types of Offenses, Placebo Analysis**

| | |
|---|---|
| Forcible sex | Weapon law violation |
| Forcible sodomy | Bribery |
| Sexual assault | Obscene material/pornography |
| Forcible fondling | Purchasing prostitution |

Source: NIBRS, 2010-2016

**Table G2: Estimated Effect of 25-Miles Increase in Distance to the Nearest Abortion Clinics on Other Crimes, 2010-2016**

| | (1) | (2) | (3) |
|---|---|---|---|
| *Distance$_t$* (25 *miles*) | -0.018 | -0.005 | -0.026 |
| | (0.0078) | (0.009) | (0.016) |
| *Distance$_t^2$* (25 *miles*) | | | 0.0017 |
| | | | (0.0011) |
| Municipality and Six-Month | Yes | Yes | Yes |
| Time-Varying Controls | No | Yes | Yes |
| Number of Observations | 645 | 645 | 645 |

Note: Estimated effect of distance to the nearest abortion clinic on crimes other than *gender violence* for 63 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is the reference population of each reporting agency. Time-varying controls are share of Hispanics and Blacks, the logarithm of the income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the municipal level.