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Is TRAP a Trap?

The Impact of Abortion Access on Violence Against Women

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Caterina Muratori†

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. 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 persists after one year. The impact decreases as the initial distance from a clinic rises. The effect of distance on violence is higher for Hispanic women and it more than doubles for Black women.

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

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1 Introduction

On June 24, 2022, the U.S. Supreme Court issued a decision in Dobbs v. Jackson Women's Health Organization case, overruling both Roe v. Wade (1973) and Planned Parenthood v. Casey (1992). The court decision takes away the constitutional right to abortion and gives individual states the full power to regulate abortion. At the same time, in many other regions of the world, the debate on abortion has reignited and restrictions on abortion access are now at the center of political agendas. Although there is an extensive piece of literature that investigates the impact of restrictions on abortion access on reproductive outcomes, many second-order effects have not been addressed yet. I start from studies that estimate a sharp reduction in the abortion rate and an increase in the fertility rate after the implementation of many state laws regulating abortion in the U.S. – the so-called Targeted Regulations of Abortion Providers, or TRAP laws[1] (Fischer, Royer and White, 2018; Lindo et al., 2020*a*; Venator and Fletcher, 2020). I focus on Texas because it is a particularly interesting case since it experienced a dramatic cut in abortion facilities as a consequence of the TRAP policies.

The right to abortion gives women the possibility to decide whether and when to have children. I claim that the lack of choice in this domain decreases women's bargaining power in the private and public spheres and particularly among low-income individuals. This study addresses the question of whether part of the aftermath of lower access to the abortion services, with consequent decrease in bargaining power, is an increase in the likelihood of women to be victims of violence.

The arrival of a child lowers women's socio-economic status, making them more vulnerable and hence raising their probability of suffering abuse.[2] An unintended pregnancy may especially increase women's likelihood to suffer from intimate partner violence (IPV), as it also has a direct effect on the ability of a woman to leave a relationship (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) find that eight percent of women

[1]During the last two decades, many U.S. states have imposed additional regulations for abortion providers, targeted specifically at abortion clinics with the primary purpose of limiting access to abortion.

[2]This mechanism is investigated more thoroughly in Section 3.

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.

I use a generalized difference-in-differences design with two-way fixed effects (TWFE), exploiting Texas as a natural experiment. 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[3] of the bill went into effect (Figure 1). I evaluate the effect of Texas House Bill 2 on violence against women of reproductive age, which I call for simplicity *gender violence*.[4] 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[5] met the standards imposed by H-B2, while others did not and had to shut down. Event-study analyses, using both 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 this context.

The present study contributes to two strands of the literature. First, it adds to the literature on abortion and IPV an empirical estimation of the causal impact of abortion access on IPV. To the extent of my knowledge, this is the first study that finds a causal relationship between abortion access and violence, shedding light on a dramatic implication of anti-abortion policies. Some studies have tried to measure the impact on domestic violence of the impossibility to terminate a pregnancy through survey analysis. Several authors reported a higher prevalence of domestic violence among women seeking abortion services, finding that women who seek for abortions experience domestic violence and sexual assault at up to three times the rate of those who want to continue with their pregnancies (Aston and Bewley, 2009;

[3]The first provision required physicians at abortion clinics to have admitting privileges at a hospital within 30 miles of the facility. This and the other three requirements are described in Section 2.

[4]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.

[5]The randomness of clinic closure is investigated in Section 6

Evins and Chescheir, 1996; García-Moreno et al., 2013; Hall et al., 2014; Pinton et al., 2017; Taft and Watson, 2007). In addition, domestic violence tends to increase during pregnancy (Ellsberg et al., 2008). Using information from the Turnaway Study, Roberts et al. (2014) 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 also enlarge the definition of the dependent variable to include types of violence other than IPV (*gender violence*). 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 here. This contribution lies in having added many empirical tests on the randomness of treatment, as well as, having accounted for repeatedly treated units, and staggered and heterogeneous treatment.

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 up to 1.9 percent. This impact persisted after one year. 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[6]. 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 up to 4.8 percent.

The paper is organized as follows: Section 2 describes the juridical and economic background and provides details of HB-2. Section 3 explains the mechanism through which abortion access affects violence against women. The data are presented in Section 4, and Section 5 describes the empirical strategy. Section 6 explores the identification of the model and the main results are reported in Section 7. Section 8 is dedicated to sensitivity analysis, while Section 9 shows results from a placebo test. The last section concludes.

[6]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

On June 24, 2022, *Roe v. Wade* (1973)[7] was overruled by a decision of the U.S. Supreme Court in Dobbs v. Jackson Women's Health Organization case. As a consequence the constitutional right to abortion was taken away and individual states have now the full power to regulate abortion.[8] Even before the overruling of Roe v. Wade (1973), people seeking abortions could encounter substantial financial barriers. The Hyde Amendment (1976) currently bans the use of federal dollars for abortion coverage for people enrolled in Medicaid, the nation's main public health insurance program for low-income individuals. Similar restrictions apply to other federal programs and operate to deny abortion care or coverage to people with disabilities, Native Americans, prison inmates, poor and low-income individuals in the District of Columbia, military personnel, and federal employees.[9] The lack of insurance coverage for abortion for low-income individuals is worsened by the fact that poor people have lower access to contraception (Kavanaugh, Jones and Finer, 2011). This, in turn, implies a higher likelihood of experiencing unwanted pregnancies. According to the Guttmacher Institute[10] 75 percent of abortion patients in 2014 were poor or low-income.[11] Thus, most abortions (95%) are performed in specialized abortion clinics, rather than private physicians' offices or hospitals (Jones and Jerman, 2014) where the procedure is expensive. These clinics have been the main target of recent regulations introduced to limit abortion availability.

Early strategies to restrict abortion access were primarily directed toward patients (demand-side policies) and include, for example, parental involvement requirements for a minor's de-

[7]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, strucking 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.

[8]As of December 2022, 13 U.S. states have banned abortion completely (`https://www.nytimes.com/interactive/2022/us/abortion-laws-roe-v-wade.html`).

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

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

[11]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`)

cision to terminate a pregnancy, and mandating 24-hour waiting periods between receiving information on abortion risks and having the abortion procedure.

Recently, abortion opponents have shifted their focus to providers (supply-side policies), finding this a more effective strategy for restricting abortion access by limiting the definition of qualifying pregnancies and reducing the number of available providers (Fischer, Royer and White, 2018; Grossman et al., 2014; Lindo et al., 2020*a*; Venator and Fletcher, 2020). TRAP policies are supply-side policies. Examples of these policies include ordering clinics to meet requirements of ambulatory surgical centers and requiring that only physicians perform medical abortions. Between 2011 and 2017, 400 state laws regulating abortion have been adopted (Nash et al., 2018) causing a sharp reduction in abortion supply in many U.S. states.

On July 18, 2013 Texas House Bill 2 (HB-2) was signed into law. The bill imposed expensive and difficult-to-implement requirements on abortion facilities. It 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 in order 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 (i.e., 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 the end of 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, on average, clinics' closure due to HB-2 doubled the distance from a Texas resident to her nearest clinic. They 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 nation, 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.[12]

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).

The impact of restrictions on abortion access is particularly heavy in the American context, given the prevalence of unintended pregnancies.[13] The Guttmacher Institute estimates that in 2011, there were 45 unintended pregnancies for every 1,000 women aged 15-44 in the United States (i.e., 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. The unintended pregnancy rate is significantly higher in the United States than in many other developed countries.[14]

For poor and vulnerable women, the burden of an unintended child is particularly heavy. First, these women constitute the group that experiences the highest rate of unintended preg-

[12]To confirm the hypothesis that abortion clinics' closure leads to an increase in the number of unintended pregnancies, I replicate the analysis of the impact of distance to the nearest abortion clinic on abortions and births. Results confirm those by Fischer, Royer and White (2018), Venator and Fletcher (2020) and Myers (2021): an increase in the distance to the nearest clinic has a negative effect on abortions and a positive effect on births. Results are available upon request.

[13]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).

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

nancies: they cannot afford to turn to hospitals or private physicians' offices for an abortion (which is a very expensive procedure) 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. 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. Second, lower socioeconomic conditions are reported among IPV risk factors (Aizer, 2010; Capaldi et al., 2012), thus on average starting these women at a disadvantage.

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 Theoretical Framework

In this section, I analyze the channels through which lower abortion access, with a consequent higher probability of unintended children, may impact violence against women. An unintended pregnancy worsens women's socio-economic conditions mainly because (1) the cost of raising a child is very high (the additional costs associated with raising a child typically exceed $9,000 in annual expenses (Lino et al., 2017)); (2) teenage pregnancy may prevent girls from finishing high school or going to college; (3) being a mother limits a woman's opportunities on the job market, especially in light of studies on penalties to mothers in the workplace; (4) an increase in childcare and housework responsibilities due to the arrival of a child weighs more on women, limiting their professional opportunities. More difficult socioeconomic conditions impact women's agency and bargaining power, hence lowering their capacity to avert violence both in the public and private sphere (Agarwal, 1997; Bettio and Ticci, 2017; McDonald, 2012; Romito and Gerin, 2002). Concerning the latter, a lower economic status combined with the emotional aspects involved makes it harder for women to leave an abusive partner after the birth of a child (Bettio and Ticci, 2017; Biggs, Gould and Foster, 2013; Chibber et al., 2014; Sanders, 2007).

Several studies have estimated the positive relationship between abortion access and women's socioeconomic conditions. Increased legal access to the abortion procedure is associated with an increase in high school completion, employment rates, earnings, and labor force participation rates (Abboud, 2019; Angrist and Evans, 1999; Jones et al., 2021; Kalist, 2004; Lindo et al., 2020*b*); a decreased likelihood of needing public assistance, living under the federal poverty line and working full time one year later (Foster et al., 2018; Jones et al., 2021); and a higher probability of women moving between occupations and into higher-paying occupations (Bahn et al., 2020). Miller, Wherry and Foster (2020) estimate that women who were denied an abortion experience a significant increase in financial distress during the year that they give birth, compared to women who received a wanted abortion. These effects were particularly strong among Black women (Jones et al., 2021; Kalist, 2004; Lindo et al., 2020*b*). Moreover, teenage pregnancies may prevent girls from finishing high school or going to college. Schulkind and Sandler (2019) find that mothers who gave birth during the school year are 5.4 percentage points less likely to complete their high school education, and Jones and Pineda-Torres (2021) estimate that Black women first exposed to TRAP laws before age 18 are 2.1 percent less likely to initiate college and 5.8 percent less likely to complete college. Lower educational attainments worsen women's job perspectives and socioeconomic conditions, and estimates show that women with medium or high levels of education face less exposure to sexual, physical, or psychological abuse from partners or non-partners compared to less educated women (Bettio and Ticci, 2017).

Concerning the job market, 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. According to a review by McDonald (2012), women with irregular, contingent, or precarious employment contracts are particularly vulnerable to sexual harassment. 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.[15] The decrease in women's economic status resulting from the arrival of a child is worsened by the fact that women, but not men, are likely to suffer a penalty in the workplace for parenthood (Blau and Kahn, 2017; Budig and England, 2001; Correll, Benard and Paik, 2007; Kleven, Landais and Søgaard, 2019). Additionally, given the unequal division of housework between partners, an increase in housework responsibility due to the arrival of a child will weigh more on the shoulders of women (for a review on housework see Coltrane, 2000), limiting their employment opportunities. Bertrand, Kamenica and Pan (2015) estimate how, after controlling for outside work, the majority of caring responsibilities still belong to women.[16]

In the household, women's decrease in bargaining power, with the consequent rise in their likelihood of suffering from intimate partner violence, has a double determinant (Roberts et al., 2014). First, an unwanted child has a direct effect on the ability of a woman to leave a relationship for economic and emotional reasons (Bettio and Ticci, 2017; Biggs, Gould and Foster, 2013; Chibber et al., 2014; Sanders, 2007). Studies on underreporting of IPV testify to this fact. Even if domestic violence and sexual assault are a major burden for the global female population[17] (Ellsberg et al., 2008), a relevant issue to address when studying IPV is still underreporting. The problem of underreporting with IPV is so serious that reported cases of domestic violence represent only a very small part of the problem when

[15] 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 for 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).

[16] A piece 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 (Andersen and Andersen, 2017; Costa Dias, Joyce and Parodi, 2020; Hersch and Stratton, 1994; Rege and Solli, 2013).

[17] Reports based on national surveys indicate that the rate of physical intimate partner violence toward a partner one year before the interview for American couples ranges from 17% to 39% (Capaldi et al., 2012).

compared with prevalence data, so that they constitute the so-called "iceberg" of domestic violence.[18] Evidence shows that the rate of reporting of IPV is lower for women in the early postpartum period (Keeling and Mason, 2011; Rubertsson, Hildingsson and Rådestad, 2010). This may be because with the arrival of a child a woman becomes less likely to leave a relationship and more likely to protect the partner. Fugate et al. (2005) analyzed data from the Chicago Women's Health Risk Study, in which 491 abused women were interviewed in public health centers and a hospital. They find that many 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 preserve [the] relationship" (Fugate et al., 2005). These reasonings also apply to the workplace setting, where the fear of losing their job may push women to underreport sexual harassment. These findings on underreporting are relevant to my empirical analysis since they exclude the possibility that an increase in the number of reported cases of violence may be due to a possible increase in the level of reporting (e.g., concerning IPV, one could assume that the arrival of a child makes women more likely to denounce violence to protect their children).

The second way an unwanted child may decrease women's bargaining power within the household is indirect and works through a decrease in their economic status. Women's bargaining power within the household is strictly related to their economic independence, which, as argued, is significantly reduced by the arrival of a child.[19] In the original bargaining models of marriage (e.g., Manser and Brown, 1980; McElroy and Horney, 1981) 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 women's ability to physically survive outside the family, the greater her bargaining power within the

[18] `https://jech.bmj.com/content/58/7/536`.

[19] In contrast to the bargaining model, there are models of male backlash that predict that a wife's improved relative economic position increases violence, as it violates traditional gender norms and redefines the power relationship between the spouses, which could trigger a violent response from the husband (Macmillan and Gartner, 1999). According to this theory, the negative effect of increased female empowerment on IPV may be attenuated by a backlash effect. For updated empirical literature on the topic, see Cools and Kotsadam (2017), Bhalotra et al. (2018), Ericsson et al. (2019) and Guarnieri and Rainer (2021)

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). Hence, a woman’s outside options decrease as a child arrives, and this, in turn, lowers her bargaining power within the couple and increases the risk of IPV. 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.

4 Data

To investigate the relationship between abortion access and violence against women, I built a dataset where I merge a measure of distance to the nearest abortion clinic 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.

To measure violence, I use information on reported cases of violence against women for 63 Texas municipalities,[20] 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 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, relationships between victims and offenders, arrestees, and property involved in crimes. 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

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

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 municipality'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,[21] 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 sample. 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.

I include in the analysis all cases 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.[22] For simplicity, I will refer to these multiple forms of violence as gender violence. As shown in Table 1, the mean of the number of reported cases of gender violence increases after HB-2 implementation. In Section 7.3, the analysis is conducted on the subsample of reported cases of intimate partner violence, i.e., the offender is a male partner/ex-partner of the victim.

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 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

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

[22] See Appendix A for a description of the types of offenses considered.

Incident-Based Reporting System and the nearest clinic. Municipalities' centroid coordinates are taken from the Texas open data portal.[23] Table 1 shows how the average distance to the nearest clinic has almost doubled after the implementation of HB-2 within my sample of municipalities.

Distance from the nearest clinic has changed differently across counties after HB-2 implementation. Representing the sample's municipalities on a map that shows the magnitude of the variation in distance for each county, I checked whether my sample includes municipalities in every kind of county, including those where distance increased the most, i.e., more than 100 miles. Variations in distance in each county and municipalities in the sample are plotted in Figure E1 of Appendix E. In the main specification, I use the distance to the nearest clinic at the same time the case of violence occurs. I then check if the effect of clinics' closure persists after a year when the baby has actually been born. I choose a one-year lag, and not a shorter lag, to be sure to capture the effect of the actual birth of an unintended child, avoiding the possibility that some of the women in the sample could still be pregnant when evaluating the impact of variation in distance on violence; in fact, these women could have tried to end their pregnancy at the end of the six-month period and so they could be still pregnant after six months.

I add to the model some time-varying control variables at the county level, by averaging 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). The summary statistics of these variables are reported in Table 1.

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[24]. In Table E1 I will confirm the robustness of the results to the inclusion of such controls.

[23] `data.texas.gov`.

[24] Look at Figure A9 of Appendix A for a plot of these trends.

5 Empirical Strategy

I estimate the effect of access to abortion clinics 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 method relaxes the assumption on the correct specification of the density of the dependent variable, avoiding the risk of inconsistent estimates.

I estimate the following model:

$$E[GV_{i,c,t,y}|dist_{i,c,t,y}, X_{c,y}, \Gamma_{i,y}, \alpha_i, \delta_t] = exp(\beta_1 dist_{i,c,t,y} + X'_{c,y}\beta_2 + \alpha_i + \delta_t) \tag{1}$$

$GV_{i,c,t,y}$ (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 or $t-2$ of year y (a one-year lag is equivalent to a lag of two six-month periods). This set includes a linear measure of distance and a quadratic measure of distance, both measured in miles. α_i is the municipality fixed effect and δ_t is the six-month fixed effect[25]. 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.

6 Identification

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 creates 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).

Given the centrality of random assignment of treatment, this assumption needs a deeper discussion. Recall that provision (1) of HB-2 required all abortion providers to have admitting privileges at a hospital located within 30 miles of the abortion clinic. I verified that each clinic's municipality has a hospital inside these boundaries,[26] i.e., within 30 miles. However, it could be the case that hospitals in more conservative areas are less likely to grant admitting privileges. A simple look at the distribution of clinics' closure within Texas state borders shows that this is not the case, since there are no clusters of closures, which are instead spread across the entire state. A superficial look at the post-policy distribution of clinics (Figure 4) may suggest a cluster of closures in the western part of Texas. But the geographic

[25] 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.

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

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.

Given the centrality of such an assumption, additional tests are needed to confirm its validity. I check whether some controls could have an impact on clinics' closures, resulting in failure of the randomness assumption. Results are reported in Appendix B. In the first test, Poisson two-way fixed effect regression is used to 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). First, the dependent variable is reported cases of gender violence and the independent variables are all controls. Then the predicted cases are regressed on the variable of interest (distance to the nearest clinic), including six-month and municipality fixed effects. The coefficient is non-significant, confirming the hypothesis of random assignment of treatment.

To further investigate the issue, several OLS two-way fixed effect regressions are used to estimate the impact of distance on all the control variables (Table B2). For the OLS models, all the control variables are logarithmic, to avoid non-normal distributions. The estimation is made at the year level because all controls are collected on a yearly basis. None of the estimated coefficients is statistically significant, except for income that shows a negative effect only significant at the 90 percent level.

Finally, I check whether some controls may have an impact on the clinic's probability of being closed in each period (Table B3). All regressions include year and municipality 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. The ability to meet these standards may depend on a clinic's size, which, in turn, might be a consequence of the economic well-being of the municipality to which it belongs. In any case, this provision does not create a problem for the random assignment assumption since its enforcement was blocked two weeks after its implementation by the U.S. Supreme Court.

The identifying assumption underlying the generalized difference-in-differences strategy is that the only change at the exact time of the clinics' closures that impacted gender violence was the distance to the nearest abortion clinic, i.e., trends in gender violence would have been the same for treatment and control group in the absence of treatment (parallel trend assumption). I test this assumption by estimating an event study, where 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.[27] The regression includes leads and lags for the six-month periods surrounding the reference period, T. 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. Using data for the three years prior to the closure and for the three years following the closure (six six-month periods), I observe no significant difference in pre-closure reporting of cases of gender violence for municipalities that experience a closure relative to those that do not (Figure 5[28]). I also see a significant increase in violence in all periods following treatment.

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:

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

Table C2 of the Appendix 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.

[27]For the event study analysis, I use a balanced subsample of 420 observations.
[28]Regression coefficients can be found in Table C1 of Appendix C.

7 Results

7.1 The Effect of Abortion Access on Gender Violence

First, I estimate the impact of restricted access to abortion on gender violence. Table 2 reports the coefficients for the estimated effect of distance to the nearest abortion clinic on gender violence, with distance to the nearest clinic measured in miles. In each regression, standard errors are clustered at the municipal level to account for both serial correlation in the outcome and overdispersion.

As indicated by Table 2, column (1), when the closest clinic is 0 miles away, a 25-mile increase in distance to the nearest abortion clinic is associated with a 0.9 percent[29] 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 check the linearity of this relationship, by adding a quadratic measure of distance. The quadratic version of distance shows the non-linear relationship: an additional mile increases the cost at a diminishing rate, meaning that the effect is higher for municipalities relatively close to an abortion clinic before the implementation of the policy. Hence, women already far from the nearest clinic before HB-2 implementation suffered less from an increase in distance. Where the access to the closest abortion clinic was already difficult prior to 2013, meaning for example that women had to travel far away from home to seek an abortion, 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 Table 2, columns (3) – which includes time-varying controls and the

[29] 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 $(e^{\beta} - 1) \cdot 100$.

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[30] 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.[31]

These results remain consistent in size and significance when (1) controls for Hispanic and Black female populations are added to the regression; (2) six-month fixed effects are replaced by year fixed effects in the main model, in light of the fact that the time-varying controls used are collected yearly; (3) re-estimating the main regression on a balanced subsample of municipalities[32], 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 percentile 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.

7.2 The Lagged Effect of Abortion Access on Gender Violence

I expect the previous effect to persist for some periods after clinics' closure because of the actual birth of the baby. To test this assumption, I include a lagged measure of distance

[30] 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.

[31] 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.

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

to the main regression (one year lag corresponding to two six-month periods lags). Table 3 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 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 but at a smaller rate than before, as shown by Figure 7.[33]

7.3 The Effect of Abortion Access on Intimate Partner Violence

Next, I disentangle the impact of abortion access on intimate partner violence, by including as dependent variable only reported cases of intimate partner violence, i.e., where the victim is a female of reproductive age and the offender is a male partner or spouse/ex-spouse of the victim. IPV offenses constitute 69.6 percent of the sample. Table 4 shows the estimated coefficients. 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 at the time and a 0.9 percent increase after a year. The effect of a 25-mile increase reduces as the initial distance increases.

Coefficients lose significance when including the quadratic of the distance in the lagged analysis. 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. For this reason, is not easy to capture this quadratic relationship. As shown here and in the next tables, when the sample varies, the relationship may appear as linear, plausibly because of the lack of western municipalities.

When looking at the contemporaneous coefficient, the results provide evidence of the fact that a pregnancy traps some women in violent relationships the moment they realize

[33] Figure D2 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.

they are pregnant[34] (Ellsberg et al., 2008). The results are also consistent with the evidence about intimate partner violence as a persistent phenomenon within a couple, as shown by the lagged effect.

Overall, looking at the size of the coefficients, it may seem that the effect of restrictions on abortion access on gender violence could be driven largely by the impact on IPV, especially when looking at the contemporaneous effect. To further investigate the role that IPV plays in explaining the effect of abortion access on gender violence, I look at the impact of distance on all forms of violence against women except for IPV, i.e., I exclude from the main analysis all the cases where the offender is a spouse, ex-spouse, or boyfriend of the victim. Looking at Table D1 of Appendix D, I can conclude that abortion access has a similar instantaneous impact on all forms of violence against women while, one year later, the effect appears larger for forms of violence other than IPV.

The decrease in the effect on IPV one year after clinics' closure may be driven by contrasting mechanisms. One possibility is that some women could have been able to leave the abusive partner after the birth of their baby, maybe motivated by the arrival of the child. On the opposite, as the rate of reporting of domestic violence decreases in the early postpartum period (Keeling and Mason, 2011; Rubertsson, Hildingsson and Rådestad, 2010), women may be less likely to report domestic violence after the birth of the baby.

7.4 Heterogeneity by Race

My hypothesis is that one of the main channels through which abortion access impacts violence against women is by lowering their 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

[34]Ellsberg et al. (2008) report that intimate partner violence tends to increase during pregnancy.

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 National Incident-Based Reporting System 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[35]. 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.[36]

The analysis on Hispanic women shows larger effects than the ones estimated on the entire population, both in the same period – up to 2.4 percent increase compared to the 1.9 percent increase found in the main analysis – and one year after the implementation of the policy – up to 1.9 percent compared to 1.2 percent increase in violence (Table 5). The analysis on Black women in Table 6 reveals much larger coefficients. When the nearest clinic is 0 miles away, a 25-mile increase in distance is associated with a contemporaneous 4.8 percent rise in gender violence cases against Black women. After one year the impact lowers to 3.5 percent.

With respect to the entire female population, the positive effect of an increase in the distance to the closest clinic offering abortion is larger for Hispanic women and has more than doubled for black and African American women. These results provide evidence of the validity of the mechanisms linking abortion access and gender violence identified at the beginning of the paper.

[35]U.S. Census Bureau

[36]U.S. Department of Commerce, Bureau of the Census, "Historical Income Tables: Households; 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.

8 Dynamic Treatment Effects

Although HB-2 was enforced on the exact same date for all clinics in Texas, not all distances to the nearest clinic changed in the same period, although most did. Three considerations should be noted: (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 only two weeks later by the Supreme Court, some clinics did temporarily shut down; (3) some time after the closures, certain other clinics managed to reopen because they were able to comply with the law. Figure E1 of Appendix E shows this situation. Each panel represents the yearly change in distance from every Texas county to the nearest abortion clinic, starting from 2013. Black dots represent the municipalities included in the sample. The treatment is dynamic and heterogeneous, 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. Again the event is the first period in which a municipality experience a positive change in distance since the previous period. The event study is plotted in Figure 8. The estimates show a clear and significant increase in cases of violence for several periods after the change in distance, confirming the validity of the two-way fixed effects design for the present analysis.

Some municipalities happened to be treated more than once. There is only one municipality in the sample treated three times, while the others are treated at most twice. Thus, I verify whether repeatedly treated observations could have created any sort of bias in 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 E5 of Appendix E. 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. Again, this new subsample is not able to capture the quadratic relationship, for the reason explained above.

9 Placebo Test: The Effect of Distance on Other Crimes

To investigate the validity of the results, 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, at least when conducting a contemporaneous analysis that does not consider long-term scenarios. The list of crimes considered is reported in Appendix F and includes sex-related offenses, weapon law violation, bribery, and purchasing prostitution. I estimate the baseline model 1, finding no robust coefficients. All coefficients are reported in Table F1 of the Appendix.

10 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 after one year. 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 IPV alone and when excluding IPV, I conclude 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 confirms the hypothesis on the key role of socio-economic conditions in explaining the mechanisms underlying the present paper. 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 main limitation of the study is related to the unbalancedness of the sample and the low number of observations. Unlike UCR, NIBRS covers only a limited set of localities as among participating states, not all police agencies are included. 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.

To the extent of my knowledge, this is the first study that finds a causal relationship between access to abortion and gender violence. 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.

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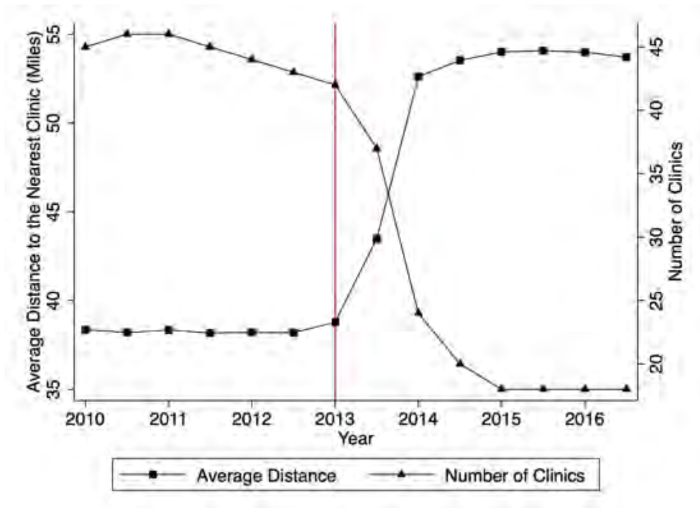
Figures

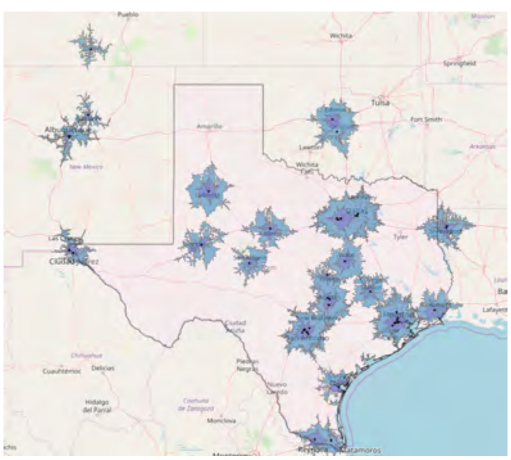
Figure 1: Number of abortion clinics and average distance from municipalities to the nearest abortion clinic in Texas



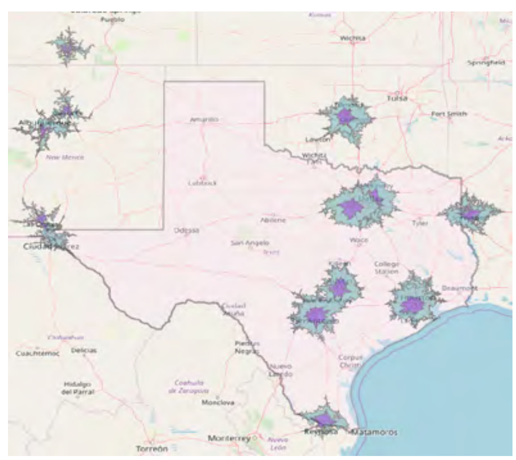
Note: Closure of abortion clinics after Texas HB-2 and increase in average distance from each municipality to the nearest clinic. The red vertical line represents the implementation of HB-2.
Source: Abortion clinic names and opening and closing dates are taken from Lindo et al. (2020*a*). The average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

Figure 2: Accessibility of abortion clinics in Texas and neighboring states, 2009 and 2016

(a) Abortion clinics in 2009

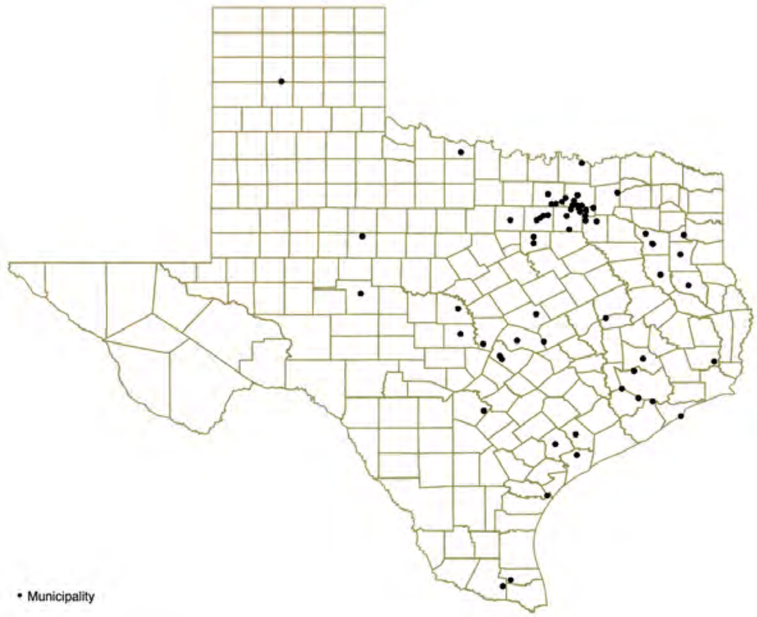


(b) Abortion clinics in 2016



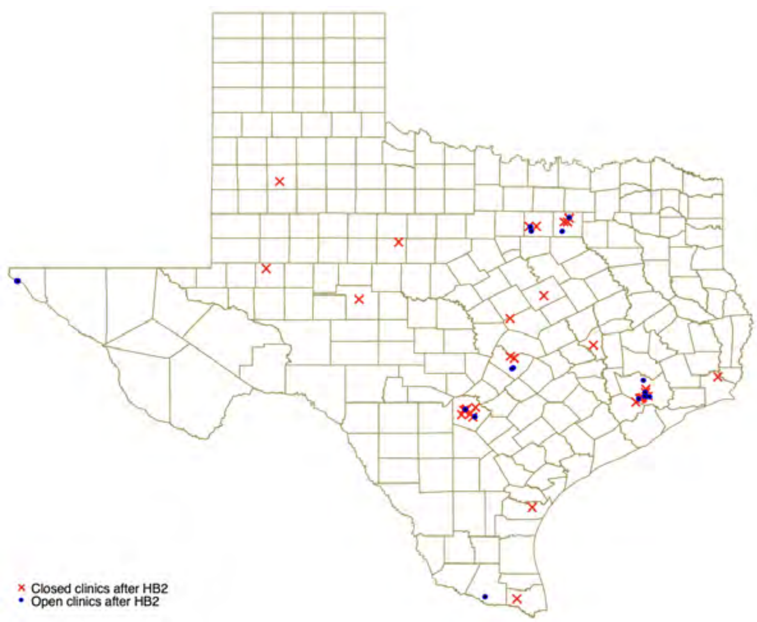
Note: Abortion clinics in Texas and neighboring states in 2009 and 2016. Around each point I drew 30-minute and one-hour isochrones to show geographic accessibility.

Figure 3: Municipalities in the sample



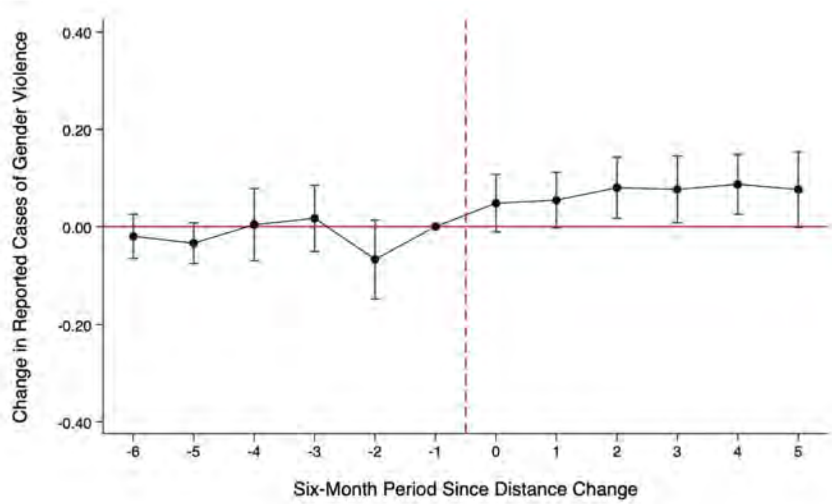
Note: Black points plot the municipalities included in the sample.

Figure 4: Open and closed abortion clinics in Texas after House Bill 2



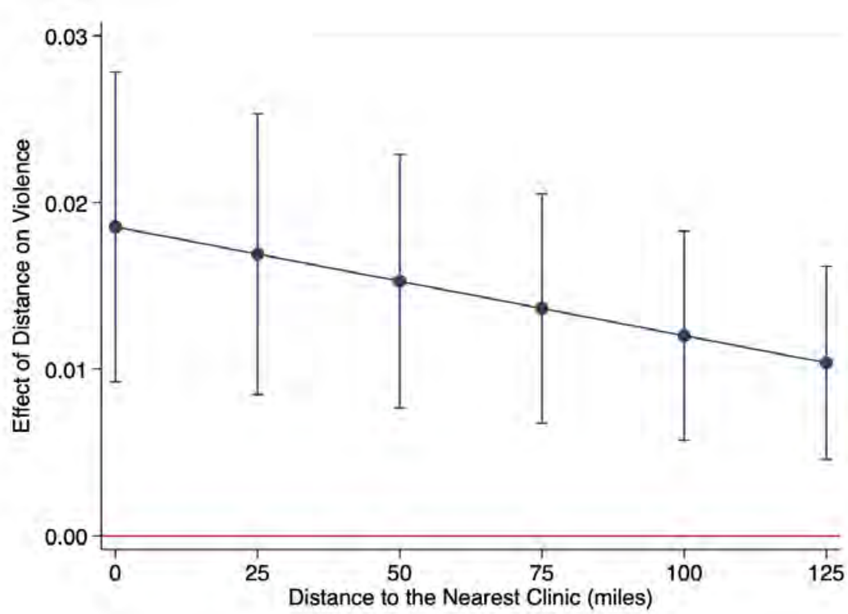
Note: Geographic distribution of abortion clinics after HB-2. Crosses represent closed clinics, while points are those that remain open. The light brown lines mark county borders.
Source: Abortion clinics' opening and closing dates are taken from Lindo et al. (2020*a*).

Figure 5: Event studies analysis of the impact of a positive variation in distance on gender violence, using TWFE estimates



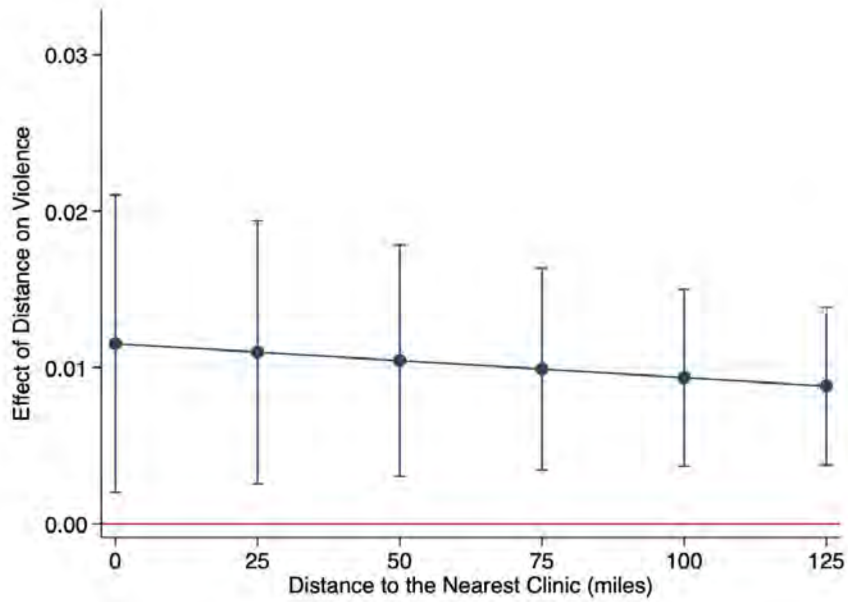
Note: The event studies are estimated through a two-way fixed effects Poisson model. This is equivalent to the model used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to six-month periods relative to the event. The event is defined as the first period in which distance increases. The first lag is omitted as it is the reference group. Horizontal lines represent 90% confidence intervals.

Figure 6: Effect of a 25-mile increase in distance on gender violence by starting level



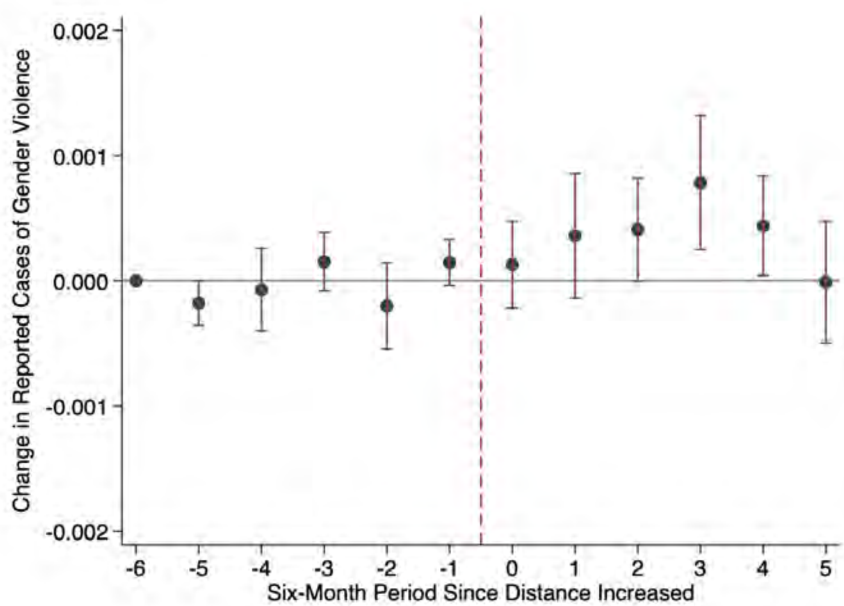
Note: Plot of estimated effects and 95% confidence intervals based on results in Column 3 of Table 2.

Figure 7: Lagged effect of a 25-mile increase in distance on gender violence by starting level



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

Figure 8: Event studies analysis of the impact of a positive variation in distance on gender violence, using Sun and Abraham estimates



Note: Sun and Abraham event study. The treatment is the first positive change in distance since the last period. The sample is restricted to 420 balanced observations. Horizontal lines represent 90% confidence intervals.

Tables

Table 1: Population-weighted summary statistics, before and after House Bill 2

	Before HB-2		After HB-2	
	Mean	Standard dev.	Mean	Standard dev.
Cases of Gender Violence	714.657	879.416	853.556	1022.568
Distance to the Nearest Clinic (Miles)	28.265	34.553	46.655	66.757
Agency Population	264,140.9	286,973.3	30,4247.3	318,881.6
County Population	1,475,198	685,030	1,499,208	744,626
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	0.012	0.141
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 a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_t$ (25 *miles*)	0.009	0.009	0.019
	(0.003)	(0.003)	(0.005)
$Distance_t^2$ (25 *miles*)			-0.0008
			(0.0003)
Municipality and Six-Month FE	Yes	Yes	Yes
Time-Varying Controls	No	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 3: Estimated lagged effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_{t-2}$ (25 *miles*)	0.008	0.008	0.012
	(0.003)	(0.003)	(0.005)
$Distance^2_{t-2}$ (25 *miles*)			−0.0003
			(0.0003)
Municipality and Six-Month FE	Yes	Yes	Yes
Time-Varying Controls	No	Yes	Yes
Number of Observations	673	673	673

Note: Estimated lagged 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 a 25-mile increase in distance on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)	(5) (IPV)	(6) (IPV)
$Distance_t$ (25 *miles*)	0.010	0.009	0.019			
	(0.003)	(0.003)	(0.004)			
$Distance^2_t$ (25 *miles*)			−0.0008			
			(0.0002)			
$Distance_{(t-2)}$ (25 *miles*)				0.009	0.007	0.007
				(0.003)	(0.003)	(0.005)
$Distance^2_{(t-2)}$ (25 *miles*)						0.0002
						(0.0002)
Municipality and Six-Month FE	Yes	Yes	Yes	Yes	Yes	
Time-Varying Controls	No	Yes	Yes	No	Yes	Yes
Number of Observations	636	636	636	636	636	636

Note: Estimated effect of distance to the nearest abortion clinic on intimate partner violence (IPV) 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 on violence against Hispanic women.

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 *miles*)	−0.002	−0.009	0.024			
	(0.006)	(0.006)	(0.004)			
$Distance_t^2$ (25 *miles*)			−0.0022			
			(0.0003)			
$Distance_{(t-2)}$ (25 *miles*)				−0.003	−0.002	0.019
				(0.006)	(0.006)	(0.005)
$Distance_{(t-2)}^2$ (25 *miles*)						−0.002
						(0.0003)
Municipality and Six-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	Yes	No	Yes	Yes
Number of Observations	654	654	654	654	654	654

Note: Estimated effect of distance to the nearest abortion clinic on violence against Hispanic 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 6: Estimated effect of a 25-mile increase in distance on violence against Black women.

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 *miles*)	0.018	0.021	0.048			
	(0.006)	(0.006)	(0.017)			
$Distance_t^2$ (25 *miles*)			−0.0020			
			(0.001)			
$Distance_{(t-2)}$ (25 *miles*)				0.018	0.020	0.035
				(0.008)	(0.007)	(0.018)
$Distance_{(t-2)}^2$ (25 *miles*)						−0.0012
						(0.001)
Municipality and Six-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	Yes	No	Yes	Yes
Number of Observations	614	614	614	614	614	614

Note: Estimated effect of distance to the nearest abortion clinic on violence against Black 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.

Appendix A Data Description

Table A1: Population-weighted summary statistics, 2010-2016

	2010-2016				
	Mean	Standard deviation	Minimum	Maximum	N
Cases of Gender Violence	783.3459	954.721	0	2,836	673
Distance to the Nearest Clinic (Miles)	37.359	53.738	1.77	276.65	673
Agency Population	283,974.6	303,610.5	686	851,849	673
County population	1,486,992	714,494.2	3,258	2,587,462	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

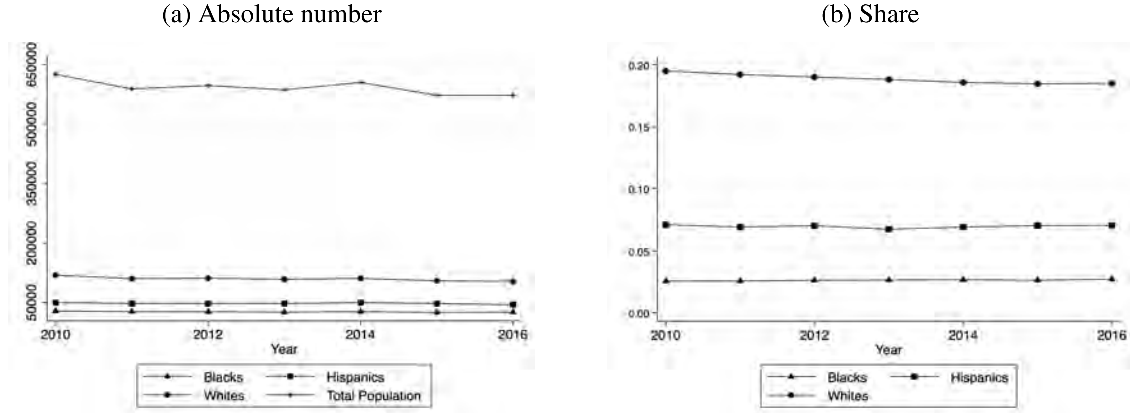
Type of Offense

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

Trends in Racial Composition

Figure A9: Trends in racial composition

(a) Absolute number (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

Table B1: The effect of distance on the predicted level of gender violence

	(1) (GV)	(2) (Predicted GV)
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 effect of distance on covariates

	(1) (Pop.)	(2) (Income)	(3) (Unemp. rate)	(4) (Fem. 15-49)
Distance (miles)	−0.0002			
	(0.0001)			
Distance (miles)		−0.0002		
		(0.0001)		
Distance (miles)			0.0002	
			(0.0003)	
Distance (miles)				-0.00002
				(0.00003)
Municipality FE and Year FE	Yes	Yes	Yes	Yes
Number of Observations	347	347	347	347

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

Table B3: The effect of covariates on the clinics' probability of closure

	(1) (Probability of closure)
County Population (log)	−0.625
	(0.640)
Income per Capita (log)	0.614
	(0.926)
Unemployment Rate (log)	0.577
	(0.927)
Share of Females aged 15-49 (log)	0.430
	(3.06)
Municipality and Six-Month FE	Yes
Number of Observations	812

Note: Estimated effect of 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 municipality period level. All explanatory variables are logarithms. Robust standard errors are reported in parentheses and are clustered at the municipal level.

Appendix C Parallel Trend

Table C1: Event study: Effect of an increase in distance on gender violence

	(1) (Gender Violence)
T=-6	-0.02004
	(0.02758)
T=-5	-0.03410
	(0.02547)
T=-4	0.00397
	(0.04516)
T=-3	0.01706
	(0.04134)
T=-2	-0.06718
	(0.04885)
Event (T=0)	0.04815
	(0.03600)
T= 1	0.05444
	(0.03480)
T= 2	0.08007
	(0.03813)
T= 3	0.07656
	(0.04174)
T= 4	0.08705
	(0.03757)
T= 5	0.07634
	(0.04732)
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.

Table C2: The effect of distance changes after clinics' closure on trends in gender violence prior to closure

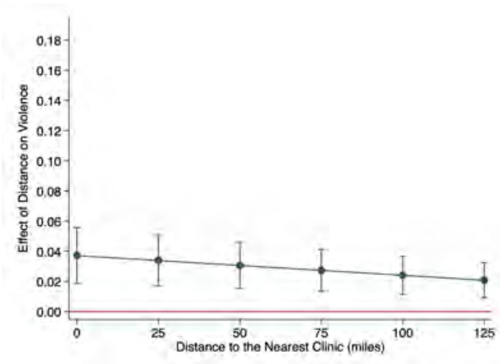
	(Δ *Distance*, 2013-2016)
Δ *GV*, 2010-2013	-0.486
	(0.609)
Number of Observations	34

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.

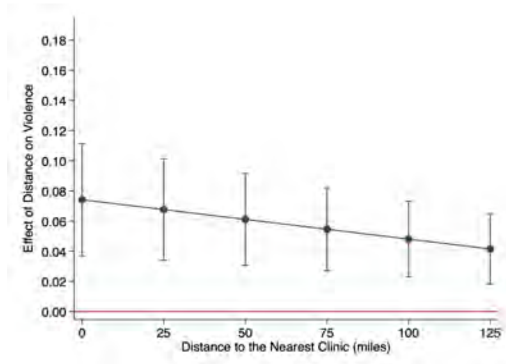
Appendix D Additional Results

Figure D1: Effect of an increase in distance on gender violence by starting level

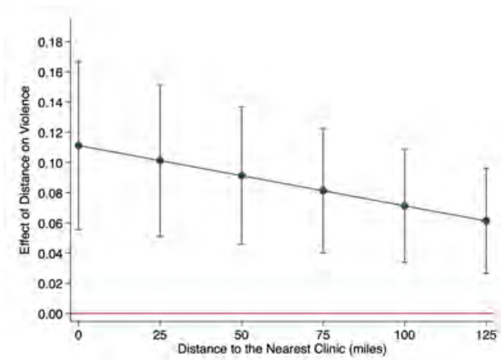
(a) Increase in Distance of 50 miles



(b) Increase in Distance of 100 miles



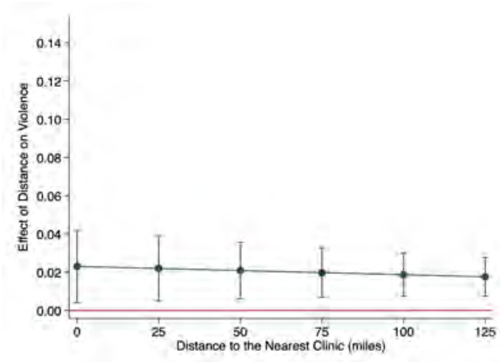
(c) Increase in Distance of 150 miles



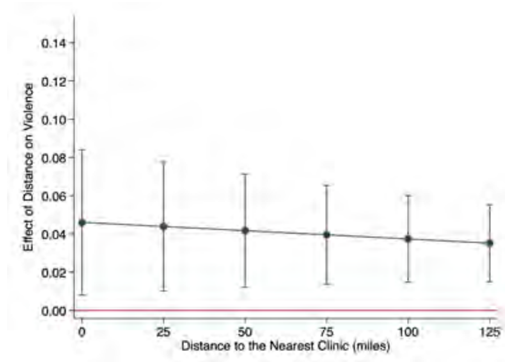
Note: Plot of estimated coefficients of the effect of distance on gender violence and 95% confidence intervals.

Figure D2: Lagged effect of an increase in distance on gender violence by starting level

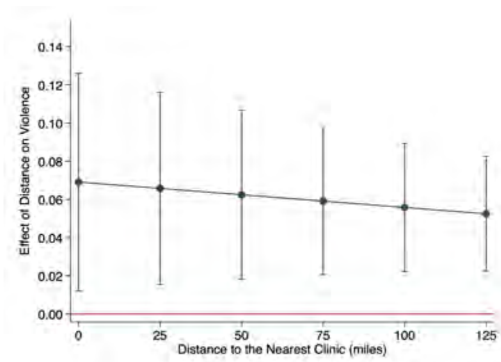
(a) Increase in Distance of 50 miles



(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.

Table D1: Estimated effect of a 25-mile increase in distance on all forms of gender violence except for IPV

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 *miles*)	0.010	0.011	0.011			
	(0.003)	(0.004)	(0.007)			
$Distance_t^2$ (25 *miles*)			0.00003			
			(0.0004)			
$Distance_{(t-2)}$ (25 *miles*)				0.011	0.013	0.014
				(0.005)	(0.004)	(0.008)
$Distance_{(t-2)}^2$ (25 *miles*)						−0.0001
						(0.0007)
Municipality and six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	Yes	No	Yes	Yes
Number of Observations	631	631	631	631	631	631

Note: Estimated effect of distance to the nearest abortion clinic on forms of gender violence other that intimate partner violence (GV) 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.

Appendix E Robustness Checks and Sensitivity Analysis

Table E1: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence, accounting for county racial composition

	(1) (Gender violence)	(2) (Gender violence)
$Distance_t$ (25 *miles*)	0.009	0.017
	(0.006)	(0.006)
$Distance_t^2$ (25 *miles*)		$-.0008$
		(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 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, 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 number of cases of gender violence, using year fixed effects

	(1) (GV)	(2) (GV)	(3) (GV)
$Distance_t$ (25 miles)	0.009	0.009	0.021
	(0.003)	(0.003)	(0.005)
$Distance_t^2$ (25 miles)			-0.001
			(0.0003)
Municipality FE and Year FE	Yes	Yes	Yes
Time-Varying Controls	No	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 on gender violence, using the balanced subsamples.

	(1) (GV)	(2) (GV)	(3) (GV)
$Distance_t$ $(25\ miles)$	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 FE	Yes	Yes	Yes
Time-Varying Controls	No	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 reported in parentheses and are clustered at the municipal level.

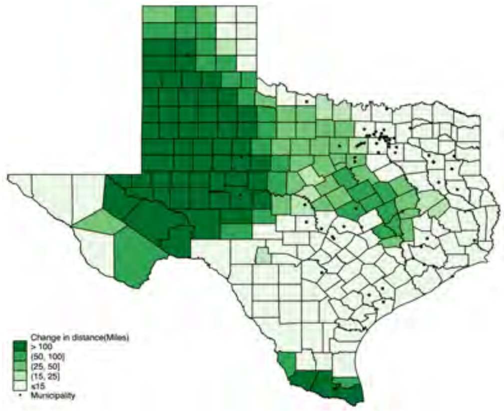
Table E4: Estimated effect of a 25-mile increase in distance on gender violence, using different samples.

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
	Pop. $<$ 90 percentile		Change $\leq$ 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 FE	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	No	Yes
Number of Observations	605	605	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.

Figure E1: 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]

[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 E5: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence, accounting for repeatedly treated observations

	(1) (Gender Violence)	(2) (Gender Violence)
$Distance_t$ $(25\ miles)$	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 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.

Appendix F Placebo Test

Type of offense

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

Table F1: Estimated effect of distance on other crimes

	(1) (OC)	(2) (OC)	(3) (OC)
$Distance_t$ (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 FE	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 other crimes (OC) 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.