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The Impact of Abortion Access on Violence Against Women

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Abstract

I document the effect of restrictions on abortion access on violence against women. Limiting access to abortion implies higher rates of unintended pregnancies and subsequent lower bargaining power for women. I start from the evidence of a sharp reduction in the abortion rate and an increase in fertility after the implementation of state laws regulating abortion in the U.S., to evaluate the impact of these restrictive policies on violence against women. I implement a generalized difference-in-difference model, finding that, depending on the initial distance, a one minute increase in time needed to reach the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality by 0-0.17 percent.

Keywords: *Abortion; Gender Violence; Intimate Partner Violence; Trap Law*
JEL Classification: I11; J12; J13; J16; J18; K23

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1 Introduction

The right to abortion gives women the entitlement to bodily self-determination and the possibility to decide whether and when to have kids. I claim that the lack of self-determination, caused by the loss of this fundamental right, decreases women's bargaining power in the private and public sphere and particularly among poor groups. This study tries to answer to the question of whether one of the aftermaths of lower access to the abortion service, with consequent decrease in bargaining power, is an increase in the likelihood of women to be victims of violence. The arrival of a child decreases women's socio-economic status, making them more vulnerable and hence raising their probability of suffering abuse. 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 capability of a woman to leave a relationship (Roberts et al., 2014). The relevance of my study lies in showing empirically the centrality of reproductive rights in women's empowerment process.

Some studies have tried to measure the impact of the impossibility to terminate a pregnancy on domestic violence through survey analysis. Several authors reported a higher prevalence of domestic violence among women seeking abortion services, finding that women who have abortions experience domestic violence and sexual assault at up to three times the rate of those who continue with their pregnancies (Aston and Bewley, 2009, Evins and Chescheir, 1996, Hall et al., 2014, Organization et al., 2013, Pinton et al., 2017, Taft and Watson, 2007). Besides, domestic violence tends to increase during pregnancy (Ellsberg et al., 2008). Roberts et al. (2014) use information from the Turnaway Study, a cohort study of women seeking abortions at 30 facilities across the U.S., and find that among women seeking abortion, having an abortion was associated with a reduction over time in physical violence from the man involved in the pregnancy, while carrying the pregnancy to term was not. They conclude that having a baby with an abusive man, compared to terminating the unwanted pregnancy, makes it harder to leave the abusive relationship.

Besides the relevance of these findings, there isn't, to the extent of my knowledge, any study that tries to quantify the causal impact of abortion access on IPV or that enlarges the definition of the dependent variable to the inclusion of types of violence other than IPV. The present analysis aims to fill these gaps.

I use a generalized difference-in-differences design with two-way fixed effects, exploiting Texas as a natural experiment. I start from the 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 Restrictions on Abortion Providers (TRAP) (Fischer et al., 2018, Lindo et al., 2020, Venator and Fletcher, 2020). I evaluate the effect of these restrictive policies on violence against women of reproductive age, which I call for simplicity *gender violence*. To the extent of my knowledge, this is the first study that finds a causal relationship between abortion access and violence against women, shedding light on a dramatic implication of anti-abortion policies.

Texas is a particular case since it experienced a dramatic cut in abortion facilities as a consequence of the policies. In July 2013 Texas House Bill 2 (HB2) took effect, whose clinics' requirements caused the closure of nearly half of Texas abortion clinics within the subsequent year. The change in clinics' accessibility started between the first and the second semester of 2013, when the first major requirement[1] went into effect (Figure 1). The assumption of the model 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 met the standards imposed by HB2, while other did not and had to shut down.

I find that, depending on the initial distance, a one minute increase in time needed to reach the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality by 0-0.16 percent. The effects seem to be largely driven by the impact of distance on intimate partner violence (IPV). 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

[1]The first provision require physicians at abortion clinics to have admitting privileges at a hospital within 30 miles of the facility. The other three requirements are described in section 2.

relatively close to a clinic before the closure.

The paper is organized as follows. Section 2 describes the juridical and economic background and shows the details of HB2. Section 3 explains the mechanism through which abortion access affects violence against women. Section 4 describes the data. Section 5 contains the details of my empirical strategy. Section 6 reports the main results. The last part of the paper is dedicated to some additional analysis and to robustness checks.

2 Background

Even if abortion in the U.S. has been legal since the Roe v. Wade (1973) decision of the U.S. Supreme Court, it is not easily accessible to most women. 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.[2] The lack of insurance coverage for abortion for low-income individuals is worsened by the fact that poor people have lower access to contraception. This, in turn, implies a higher likelihood of experiencing unwanted pregnancies. According to the Guttmacher Institute, 75% of abortion patients in 2014 were poor or low-income.[3] 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 directed primarily toward patients (demand-side policies) and include, for example, parental involvement requirements

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

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

for a minor's decision to terminate a pregnancy, and mandating 24-hour waiting periods between receiving information on abortion risks and the abortion procedure.

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

On July 18, 2013 Texas House Bill 2 (HB2) 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 the 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 admission. The implementation of this provision caused nearly half of Texas abortion clinics to close (Figure 1).

The ambulatory surgical center requirement took effect on October 3, 2014 but its enforcement was blocked two weeks later by the U.S. Supreme Court. Converting a clinic to meet these standards is both financially and time costly, as 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 HB2, 8 of the 41 Texas abortion clinics closed or stopped providing abortion services. Eleven more facilities closed or stopped providing abortions when HB2 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% 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 HB2. The majority opinion was that these provisions imposed an undue burden on access to abortion, without showing 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 have reopened. 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 30 minutes travel time by car from each clinic; the blue ones reflect a distance of one hour.

Lindo et al. (2020) estimate that, on average, clinics' closure due to HB2 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%, 28%, 38% and 44% respectively. They also find that 59% of this effect is due to congestion, meaning the reduction of clinics per-capita, rather than an increase in distance. 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 et al. (2018) estimate that abortion to Texas residents fell 16.7% and births rose 1.3% 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 the Wisconsin 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. The difference between the decreases 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 U.S. context, given the prevalence of unintended pregnancies. 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). The Guttmacher Institute evaluates that in 2011, there were 45 unintended pregnancies for every 1,000 women aged 15-44 in the United States (nearly 5% of reproductive-age women have an unintended pregnancy each year) and 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.[4]

The burden of an unintended child is particularly heavy for poor and vulnerable women, who constitute the group that experiences the highest rate of unintended pregnancies. These women cannot afford to resort to abortion within hospitals or private physicians' offices (which is a very expensive procedure) or to travel far away from home to reach the nearest abortion clinic, losing days of work and money for travel and hotel. In addi-

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

tion, 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, by 2012, 53 clinics to close, whose vast majority only provided nonabortion 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.

Finally, socioeconomic conditions are reported among IPV risk factors (Aizer, 2010, Capaldi et al., 2012), thus these women have on average a worse starting situation.

The relation between abortion and IPV is exacerbated by the fact that unintended pregnancies are more likely to occur to 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 likely to be also sexually assaulted, and this prevents them from using barrier contraceptives (Hall et al., 2014). Besides, they may choose to terminate the pregnancy to protect their child from a violent environment and the risk of suffering abuses.

3 The mechanisms through which lower abortion access increases violence against women

Reproductive rights and gender violence are linked by the loss of agency and bargaining power experienced by women as a consequence of unintended children. The arrival of a child decreases women's economic independence, making them more vulnerable, both within and outside the family, and increasing their likelihood of suffering abuse (Bettio and Ticci, 2017, McDonald, 2012, Romito and Gerin, 2002).

Several studies have estimated the negative impact of abortion access on women's socio-economic conditions. Increased legal access to the abortion procedure is associated with an increase in high school completion, employment rates, and labor force participation rates (Angrist and Evans, 1996, Kalist, 2004); a decrease in the likelihood of needing public assistance, living under the federal poverty line and working full time one year later (Foster et al., 2018); a higher probability for women of moving between occupations and into higher-paying occupations (Bahn et al., 2020). Moreover, teenage pregnancies may prevent girls from finishing high school or going to college. Estimates show that women with medium or higher education face less exposure to sexual, physical, or psychological abuse from partners or non-partners compared to low educated women (Bettio and Ticci, 2017).

A lower economic position has an impact on the bargaining power of women both in the private and public sphere (Agarwal, 1997). Concerning the marketplace, lower economic opportunities decrease women's capability to contrast violence in the workplace because of the lack of outside options in the case of job loss. According to the review by McDonald (2012), women with irregular, contingent, or precarious employment contracts are particularly vulnerable to sexual harassment. In addition, lower economic opportunities force women to accept more dangerous job positions that may expose them to a higher likelihood of suffering abuse. For example, occupations that involve night shifts may require women to go back home at night, exposing them to a higher probability of being victims of violence by strangers.

The decrease in women's economic status, derived from the arrival of a child, is worsened by the fact that mothers are also likely to suffer a higher penalty in the workplace with respect to male workers and women without children. Correll et al. (2007) suggest that women, but not men, face discrimination based on their parental status, using both laboratory and field experiments. This is in accordance with the idea of the existence of a child penalty for women and a marriage premium for men (Blau and Kahn, 2017, Budig and England, 2001, Correll et al., 2007, Kleven et al., 2019). Besides, 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), decreasing their working opportunities. Bertrand et al. (2015) estimate how, after controlling for outside work, the majority of caring responsibilities still belong to women. 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 (Costa Dias et al., 2018, Hersch and Stratton, 1994).

In the household, the decrease in women's bargaining power, with the consequent increase 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 capability of a woman to leave a relationship for economic and emotional reasons (Bettio and Ticci, 2017, Sanders, 2007). Studies on underreporting of IPV testify to this fact. Even if domestic violence and sexual assault are a major burden of disease in the global female population[5] (Ellsberg et al., 2008), a relevant issue to address when measuring 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 compared with prevalence data, so that they constitute the so-called "iceberg" of domestic violence.[6] Evidence shows that the rate of reporting of IPV is lower for women in the early postpartum period (Keeling and Mason, 2011, Rubertsson et al., 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 perceive that to get help from the police, they must be prepared to end the relationship. Also, they find that 10% of the interviewed women declared to not calling the police to "*protect partner and preserve relationship*" (Fugate et al., 2005). These reasonings also apply to the workplace setting, where the fear to lose their job may push women to underreport sexual harassment. The results on underreporting are relevant to my econometric 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

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

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

level of reporting (e.g., concerning IPV, one could assume that the arrival of a child could make women more likely to denounce violence to protect their children). This evidence also suggests that my coefficients might represent an underestimation of the phenomenon.

An unwanted child has also an indirect effect on women's bargaining power within a couple through decreasing economic status. Women's bargaining power within the household is strictly related to their economic independence, which is heavily damaged by the arrival of a child. 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 (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 would be her bargaining power within the family (Gelles, 1976, Montero et al., 2012). Moreover, in the marriage market, women with children are typically less "eligible" than men with these characteristics, and this further decreases their willingness to leave the couple (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 et al., 1998). Aizer (2010) estimates that decreases in the wage gap reduce violence against women within the family.

To conclude, as underlined by Agarwal (1997), economic factors, together with lower bargaining power within the household, impact the bargaining power of a woman within the community, through a lower capacity of emigrate and a higher need for social support. This may have an impact on women's likelihood to contrast violence perpetrated by community members.

4 Data

The variables used in the analysis are summarized in Table 1 for the pooled sample period and in Table 2 for the periods before and after HB-2.

To measure violence, I use information on reported cases of violence against women for 100 Texas cities, taken from the Uniform Crime Reporting (UCR) Program Data. Uniform Crime Reporting is a city, county, state, and federal law enforcement program. It provides a nationwide view of crime based on the submission of crime information by law enforcement agencies. Within this program, each city law enforcement agency reports offenses that occur within its city boundaries.

I include in the analysis all the cases where the victim is female in reproductive age (15-49), the offender is male and the types of offense considered include assault, homicide, human trafficking, kidnapping and sexual offenses (see Appendix A). I will refer for simplicity to these several forms of violence as gender violence. In the second part of the analysis, I will only consider reported cases of intimate partner violence, i.e., the offender is a male partner/ex-partner of the victim.

Data are highly overdispersed, but mainly among municipalities, reflecting mostly differences in cities' populations. Thus, the inclusion of municipality fixed effects should greatly reduce overdispersion.

Data on clinics' opening and closing in Texas and neighboring states (Colorado, Louisiana, New Mexico, and Oklahoma) are taken from Lindo et al. (2020). A clinic is considered open (closed) in a semester (i.e. 6 month period) if it has been opened (closed) for at least three months.

I geocoded each abortion clinic in every semester of every year for the period 2010-2016. Then, I used the Stata command *georoute* to calculate the driving time (minutes) between each municipality that reports crimes to the *Uniform Crime Reporting Program* and the nearest clinic. Municipalities' centroids coordinates are taken from the Texas open data portal.[7]

I use the distance to the nearest clinic both at the same time and one year before the

[7] `data.texas.gov`

case of violence happens, since the consequences of an unintended pregnancy are visible both during pregnancy (Ellsberg et al. (2008) report that intimate partner violence tends to increase during pregnancy), and after the baby is born. I choose a one-year lag, and not a six-month lag, to control for the fact that lots of women could have tried to end their pregnancy at the end of the semester and so they could be still pregnant after six months.

County-level demographic controls (sex and race composition) are taken from the National Institute of Health Surveillance, Epidemiology and End Results (SEER), while county-level per capita income estimates are from the U.S. Bureau of Economic Activity (BEA). The unemployment rate by county is taken from the U.S. Bureau of Labor Statistics (BLS). Municipality-level demographic controls for race and sex composition come from the American Community Survey and information on municipality population is from the U.S. Bureau of Labor statistics.

5 Empirical Strategy

The baseline model is a generalized difference-in-difference design, that exploits within-municipality variation over time in distance to a clinic, controlling for cross-municipality time-varying shocks (Fischer et al., 2018, Lindo et al., 2020, 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. That is, the trajectory of the level of reported cases over time for municipalities with zero/small changes in distance is the path that they would have taken in municipalities with large changes in the absence of clinics' closure.

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 et al., 2018, Lindo et al., 2020, Lu and Slusky, 2019, Venator and Fletcher, 2020), with the inclusion of municipality and year fixed effects. As note by Lindo et al. (2020), *"while the possibility of overdispersion is the main theoretical argument that might favor alternative models, overdispersion is corrected by calculating sandwiched standard errors (Cameron and Trivedi, 2005). Moreover, the conditional fixed effects negative binomial model has been demonstrated to not 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 et al. (2018), all regressions are run using a Pseudo Maximum Likelihood estimator, which is a method known to solve this problem. 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:

$$E[GV_{i,c,t,y}|dist_{i,c,t,y}, \alpha_i, \delta_y, X_{c,y}, \Gamma_{i,y}] = exp(\beta_1 dist_{i,t,y} + \alpha_i + \delta_y + X'_{c,y}\beta_2 + \Gamma'_{i,y}\beta_3) \quad (1)$$

$GV_{i,c,t,y}$ (gender violence) is the number of reported cases of gender violence for municipality i in county c, in semester t of year y. $dist_{i,t,y}$ is the driving time from each municipality i to the nearest abortion clinic in semester t of year y and $y-1$ (or $t-2$ as defined in the tables). α_i is municipality fixed effect and δ_y is year fixed effect. $X'_{c,y}$ is the vector of county controls and $\Gamma'_{i,y}$ is the vector of municipality controls. In all models, the logarithm of municipality population is included as the exposure variable to account for the fact that municipalities vary widely in size and therefore have a different potential for offenses.

In Appendix F is reported the first-order analysis of the impact of clinics' closure on births and abortions. The estimates confirm the results by Fischer et al. (2018): distance to the nearest clinic has a negative effect on abortions and a positive effect on births. This confirms the hypothesis that abortion clinics' closure leads to an increase in the number of unintended pregnancies.

6 Results

6.1 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 HB2, while others did not and had to shut down. Clinics' opening and closing create a variation in geographic accessibility to abortion facilities that is randomly distributed within Texas territory. Therefore, treatment (change in distance) is good as randomly assigned and the control group is made by those municipalities that experienced no variation or very small variation in the access to abortion clinics.

Given the centrality of random assignment of treatment, this assumption needs a deeper discussion. Recall that provision (1) required all abortion providers to have admitting privileges at a hospital located within 30 miles of the abortion clinic. Since each clinic is located within a populated city, I can exclude the possibility of the existence of clinics that do not have a hospital within 30 miles. But, it could be the case that hospitals in more conservative areas are less likely to give admitting privileges. I can show that this is not the case by looking at the distribution of clinics' closure within Texas' boundaries, since there are no clusters of closings and they are spread on the whole territory. The geographic representation of clinics' closure shows my hypothesis. A superficial look at the post-policy distribution of clinics (Fig. 3) may suggest a cluster of closures in the western part of Texas. But the geographic distribution of closed clinics after HB2 shows that clinics have been shut down in the whole Texas territory and the western area remained unserved after 2013 only because it already had a very low number of clinics before the intervention.

Given the centrality of such assumption, I perform additional tests on its validity. I check whether some controls could have an impact on clinics' closures causing a failure of the randomness assumption. Results are reported in Appendix B. First, Poisson two-way fixed effect regression is used to estimate the impact of distance from each municipality to the nearest abortion clinic on the part of cases of gender violence predicted by the control variables. Firstly, the dependent variable is reported cases of gender violence and the independent variables are all controls. Then, the predicted cases are regressed on the regressor of interest (distance to the nearest clinic), including year 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 my measure of distance on all the control variables. For the OLS models, all the control variables are in logarithm, to avoid non-normal distributions. None of the estimated coefficient is statistically significant.

Provision (2) imposed all abortion facilities to meet the requirements of an ambulatory surgical center. Meeting 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 to. Anyway, this provision does not create a problem to the random assignment assumption since its enforcement was blocked two weeks after its implementation by the U.S. Supreme Court.

The identifying assumption underlying my generalized difference-in-differences strategy is that the only thing changing 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). To check the validity of the parallel trend assumption for average cases over time, I need to work on the subset of balanced observations. The trend in average cases over time changes completely if just one municipality is missing for one period, since municipalities vary greatly in size and population. To visually show the validity of the parallel trend assumption, I divide this subsample in two groups on the basis of the magnitude of the change in distance to the nearest clinic between the second half of 2013 and 2016. To keep more observations, I restrict the sample to the period 2011-2015 and to weight observations, I divide the number of cases by the population in each municipality and year. One group is made up by municipalities that have seen the travel time to their nearest clinic increase by more than 35 minutes after the first semester of 2013 and the other by municipalities whose distance changed less than 35 minutes. I arbitrarily choose this threshold, since I assume that changes in travel time lower than half an hour are likely to have no impact on the possibility to reach the nearest clinic. Despite that, these two groups are not the proper treatment and control group. Since I have continuous treatment, it is not possible to properly divide the treated units from the control ones.

Figures 4 and 5 confirm the validity of the parallel trend assumption for the trends in reported cases of gender violence and intimate partner violence. In both figures, the violence trends for municipalities whose distance change more that 35 minutes shows two increasing jumps: one after the policy implementation and the other one year after the implementation. This is consistent with the hypothesis that the effect of changes in access on gender violence and intimate partner violence appears both at the same time of clinics' closure and after a year. The smooth increase in the trend of the other group is due to the fact that this is not the real control group so it could include treated units.

To perform a formal test of this assumption, I estimate an event study, where I define the event in question as a closure that causes an increase in travel time to the nearest clinic higher than 10 minutes. I estimate equation 1 with the measure of travel time replaced by an indicator variable equal to one if the change in travel time since the last period exceeds 10 minutes. The regression includes leads and lags for the semesters surrounding the reference period, T. The indicator for period $T-1$ is omitted, meaning that the coefficients can be interpreted as the effect of a clinic closure that increases travel time by more than 10 minutes on gender violence cases relative to gender violence cases in the semester prior to the clinic closure. Using data for the three years prior to the closure and for the three years following the closure, 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 (Fig. 6 (a)). The only exception is a decrease in gender violence 3 years prior to clinics closure (first semester of the year), which however has small significance (at the 10 percent level). I also see a significant increase in violence in the semester of and the semester following the closure. In Figure 6 (b), I restrict the sample to a 2 years period on either side of the event. Again, I see no significant difference in pre-closure trends for municipalities that experience a closure relative to those that do not and a significant increase in gender violence in the semester of and the semester following the closure.

6.2 The effect of abortion access on gender violence

First, I estimate the impact of restricted access to abortion on gender violence. Table 3 reports the coefficients for the estimated effect of distance to the nearest abortion clinic on gender violence, while Table 4 reports marginal effects. Distance to the nearest clinic is measured in driving time (minutes). In each regression standard errors are clustered at the commuting zone level to account for both serial correlation in the outcome and overdispersion.

As shown by Table 3, column (1) a one minute increase in time needed to reach the nearest abortion clinic is associated with a 0.04 percent increase in the number of reported cases of gender violence per municipality in the same period.[8]

Following the literature (Fischer et al., 2018, Lindo et al., 2020, Venator and Fletcher, 2020), I assume that this relationship is non-linear, meaning that the effect is higher for municipalities relatively close to an abortion clinic before the implementation of the policy. Hence, I add a quadratic measure of distance. The quadratic version of distance shows the non-linear relationship: an additional minute increases the cost at a diminishing rate. As shown by columns (3), if the closest clinic is 0 minutes away, a one minute increase in time needed to reach the nearest abortion clinic is associated with a 0.17 percent increase in the number of reported cases of gender violence per municipality in the same period, with coefficients significant at the one percent level and robust to the inclusion of time-varying controls. The effect of a one minute increase reduces to 0.14 percent if the nearest clinic is 30 minutes away and to 0.11 percent

[8]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: $(e^{\beta} - 1) \cdot 100$

if the nearest clinic is 60 minutes away. The estimated marginal effects by starting distance from the nearest clinic are represented in Figure 7.

Table 5 and 6 show the impact of abortion access on gender violence one year after closure, confirming the existence of an additional effect with respect to the contemporaneous one. A one minute increase in the distance to the nearest clinic is associated with a 0.17 percent increase in the number of reported cases of gender violence per municipality the following year, if the closest clinic is within 0 minutes away. This is consistent with the fact that the economic vulnerability of a woman is likely to increase when the child is actually born, causing a further increase in the likelihood of suffering abuse. The effect of a one minute increase reduces to 0.12 percent if the nearest clinic is 30 minutes away and to 0.086 percent if the nearest clinic is 60 minutes away. The contemporaneous and lagged effects are equal for women living 0 minutes away from the closest clinic before clinics closure; for positive distances, the contemporaneous effect is increasingly higher than the lagged ones as the starting distances increase.

6.3 The effect of distance to the nearest clinic on intimate partner violence

In this section, 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. the victim is female in reproductive age and the offender is a male partner or spouse/ex-spouse of the victim.

Table 7 shows the estimated coefficients and Table 8 reports average marginal effects. If the closest clinic is 0 minutes away, a one minute increase in the distance to the nearest clinic is associated with a 0.16 percent increase in the number of reported cases of intimate partner violence per municipality at the same time and after a year. When the nearest clinic is 30 minutes away, a one minute increase in driving time is associated with an effect of 0.13 percent in the same period and 0.12 percent after a year. If the closet clinic is one hour away, the impact reduces to 0.10 percent in the same period and 0.076 percent after a year.

The results give evidence of the fact that a pregnancy traps women into violent relationships since the moment a woman realizes to be pregnant (Ellsberg et al., 2008), when looking at the contemporaneous coefficient. But they also are consistent with the evidence about intimate partner violence, that appears as a persistent and long-lasting phenomenon within a couple, as showed by the lagged effect.

Overall, looking at the size of the coefficient, it is very likely that a large part of the effect of restrictions on abortion access on gender violence is driven by the impact on IPV.

6.3.1 Seasonal trends in intimate partner violence

Several studies detect seasonal patterns in reported cases of IPV. Using 7 years of NIBRS data from one rural mountain state, Vazquez et al. (2005) find that there is a tendency for IPV incidents to occur late at night, on weekends, and on certain holidays. Similarly, Joshi and Sorenson (2010), using a dataset made of information from incident reports filed by police officers in response to service calls in a large U.S. city, find that a majority of IPV incidents occurred in between the evening and early morning hours, on weekends and on major American holidays.

Since IPV may occur more often during holidays, it could be particularly high in the second semester of each year with respect to the first one, since the former includes both summer vacations and Christmas holidays. To account for this fact, the impact of distance on IPV is estimated by including semester fixed effects, instead of year fixed effects. As shown by Table 16 of Appendix C, the coefficients of interest remain positive and significant, with magnitude similar to the main analysis.

7 Estimating the channels through which abortion access affects violence

My hypothesis is that on of the main channels through which abortion access impacts violence against women is by lowering their socio-economic conditions. The next two paragraphs give some empirical evidence on the validity of such assumption. First, the analysis focuses on the heterogeneous effect of distance to the nearest clinic on poorer women, since the economic burden that derives from an unintended pregnancy must have greater negative effects on economically disadvantaged women. Then, I will explore female high school dropout rate, to see whether an increase in distance to the closest abortion clinic may affect this measure of female educational outcomes.

7.1 The heterogenous effect of distance on gender violence

Beyond my assumption on the economic mechanism through which abortion access impacts violence, economically disadvantaged individuals might be more affected by the increase in distance to the nearest abortion clinic also because of their higher likelihood of experiencing unintended pregnancies. First, low-income women cannot turn towards 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..

Hence, I estimate the heterogenous effect of distance to the nearest clinic for municipalities in the top 25% of the distribution of the unemployment rate. I use unemployment rate, instead of per capita income, since the latter may hide large inequalities in income distribution.

I interact my continuous measure of distance with a dummy that takes value 1 if the municipality has an unemployment rate in the highest 25% of the distribution. Table 9 presents the estimated regression coefficients and indicate that there are heterogeneous effects across unemployment levels. The estimated effect of access is significantly larger for municipalities in the top 25% of the unemployment distribution and it is consistent to the inclusion of time varying socio-economic characteristics[9].

[9] Data on hispanic and black people per municipality have missing values for several municipalities in the top 25% of the unemployment distribution. Hence, I control for these two variables at the county level

7.2 Female dropouts

As mentioned above, evidence shows that increased legal access to the abortion procedure is associated with an increase in high school completion and women with medium or higher education face less exposure to sexual, physical, or psychological abuse compared to low educated women. Thus, I want to explore the link between distance to the nearest abortion clinic and female high school dropout rate.

Data on annual dropout rate for female students in grade 7-12 (from 12 to 17 years old) are from the Texas Education Agency. The annual dropout rate is calculated as the number of students who dropped out during the school year divided by the number of students enrolled during the school year, multiplied by 100.

I use two specifications, both including year and county fixed effects: a Generalized Linear Model with Gaussian family distribution and a Pseudo Maximum Likelihood Poisson model. Now the analysis is at the county-year level, since dropouts data are collected on a yearly basis. Looking at Table 10 when the closest clinic is 0 minutes away, the impact of a one minute increase in distance to the nearest clinic has an effect of 0.37-0.95 percent on the female dropout rate in high school within the same year, depending on the model used for the estimation. The effect reduces to 0.28-0.77 percent when the closest clinic is 30 minutes away and to 0.20-0.59 percent when the closest clinic is 60 minutes away[10].

8 Sub-sample analysis

8.1 Analysis on the balanced sub-sample

My sample is quiet unbalanced, thus I check the validity of my results on a balanced subsample of municipalities. I keep municipalities that have observations for the whole sample period. As shown by Table 11, when using the balanced subsample, the effects remain positive and strongly significant.

[10] I exclude from the list of control variables the *unemployment rate* since its inclusion could create an issue of reverse causality.

8.2 Geography based sub-sample

Since the western part of the country remains with zero clinics after HB2 implementation, it may be that cities in this area biases the results. Thus, I consider the sub-sample of municipalities that are located in the eastern part of Texas, as represented in Figure 8.

For this subsample the relation is linear and this is consistent with the fact that, given the high number of clinics prior to the bill implementation, all municipalities considered were relatively close to the nearest abortion clinic in the pre-policy period. As shown by Table 12, when I restrict the analysis to the subsample of eastern municipalities, results remain significant and coefficients increase. A one minute increase in time needed to reach the nearest abortion clinic is associate with a 0.3 percent increase in the number of reported cases of gender violence per municipality in the same period, when the nearest clinic is 0 minutes away (column (2)).

8.3 The substitution to self-induced abortion in Mexican bordering counties

As Chavkin et al. (2013) point out, where access to legal abortion services is restricted, women seek services under unsafe circumstances. The Guttmacher Institute reports that 56 million induced abortions took place each year during the period 2011-2014.[11] Of all abortions, an estimated 55% are safe (i.e., done using a recommended method and by an appropriately trained provider); 31% are less safe (meet either method or provider criterion); 14% are least safe (meet neither criterion).

According to a recent study, between 2011 and 2015 the number of Google searches using terms related to self-abortion increased from 119,000 to 700,000 and these searches were more common in states with the highest number of abortion restrictions (Stephens-Davidowitz, 2016). In Texas, another study estimated that at least 100,000 Texas residents had ever attempted to end a pregnancy on their own, though it is unknown what

[11] *Abortion Worldwide 2017: Uneven Progress and Unequal Access*, March 2018, www.guttmacher.org
Induced Abortion Worldwide: global incidence and trends, March 2018, www.guttmacher.org

methods they used (Grossman et al., 2015).

The drug most commonly used to self-induce an abortion is Cytotec, sold for the treatment of gastric ulcer, but containing Misoprostol, a drug that induces uterine contractions. Cytotec is only available by prescription in the United States, but can be obtained behind-the-counter at pharmacies in some countries, including Mexico. Jones (2011) estimates that, during the period 2008-2009, 1.2 percent of abortion clinics patients reported that they have self-induced abortion on their own using Misoprostol.

Texas is a particular case both because of the restrictions to abortion access and because it borders with Mexico. In 2012, 7 percent of Texas' abortion patients report having tried to end their pregnancy on their own (Grossman et al., 2014). Therefore, I test whether women living near the Mexican border are more likely to substitute abortion at clinics with self-induced abortion, experiencing a lower increase in unintended pregnancies and a subsequent lower increase in gender violence. The inclusion of such category of women should bias the results downward, so I look at the coefficient of the effect of travel time on violence when excluding counties close to Mexico (the list of excluded counties is reported in Appendix D).

Results are reported in Table 13. Coefficients are higher for both the contemporaneous and the lagged effect. This gives some evidence for the hypothesis of a substitution effect between abortion in clinics and self-induced abortion for women near the Mexican border. When access to abortion is restricted, women, especially the ones in areas near Mexico, can decide to self induce an abortion, avoiding the burden of unintended pregnancies and then decreasing the likelihood of suffering abuse.

9 Placebo test: the effect of distance on other crimes

To test the validity of my 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 involve, I consider only offenses where, if any, the victim is male. Since an unintended child may have a negative effect on the economic situation of a couple, I exclude from the placebo all property related crimes, but also all the violent crimes that could be related to the attempt of committing property crimes or to the jealousy towards another man (may be the case that some unintended pregnancies are the result of betrayal). The list of crimes considered is reported in Appendix E.1 and includes sex-related offenses, weapon law violation, bribery and purchasing prostitution.

I estimate my baseline model 1. As expected, coefficients are statistically insignificant for the linear specification when controlling for the socio-economic characteristics of the municipality. When using the quadratic specification, all coefficients are not significant. Results reported in Appendix E.2 give evidence on the validity of my results.

10 Conclusion

The results of my empirical analysis show that access to the abortion service has a sizable effect on the level of violence against women, in the private and public sphere. I find that, depending on the initial distance, a one minute increase in time needed to reach the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality by 0-0.17 percent in the same period and the following year. In accordance with the literature, that finds the effect of distance 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. When I restrict the analysis to intimate partner violence, I conclude that a large part of the effect of restrictions on abortion access on gender violence is driven by its impact on IPV.

Abortion access has been found to affect violence especially through a decrease in women's socio-economic conditions. Economically disadvantaged individuals appear to be more affected by the increase in distance to the nearest abortion clinic. This result is in accordance with poor individuals being more affected by a decrease in their economic opportunities as well as to experience a higher rate of unintended pregnancy as a consequence of lower abortion access. In addition, an increase in distance to the nearest clinic has been estimated to have a positive impact on female high school dropout rate.

To the extent of my knowledge, this is the first study that finds a causal relationship between abortion access and gender violence. The aim of this research is to enlarge the boundaries of the debate on abortion's policies, by acknowledging that lower access to abortion implies lower autonomy and agency for women, and, in turn, higher level of violence against them.

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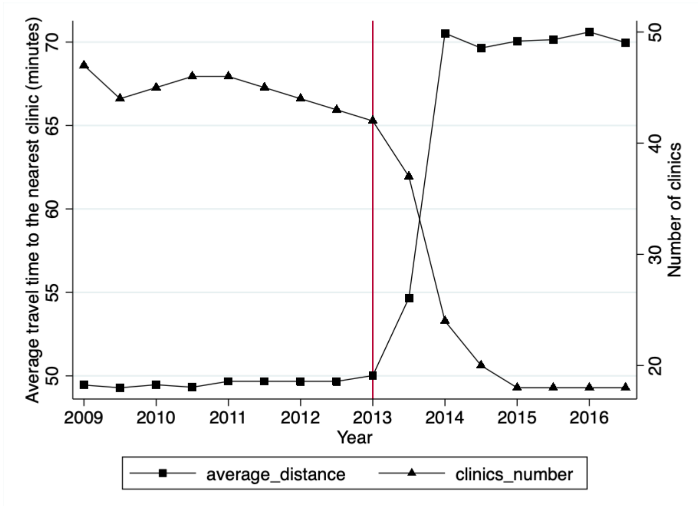
Figures

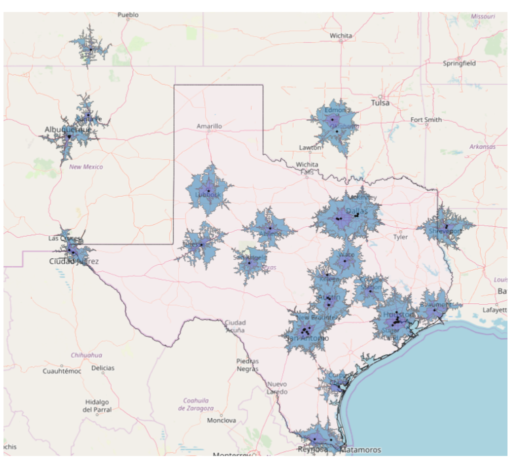
Figure 1: Abortion clinics closure and increase in average distance



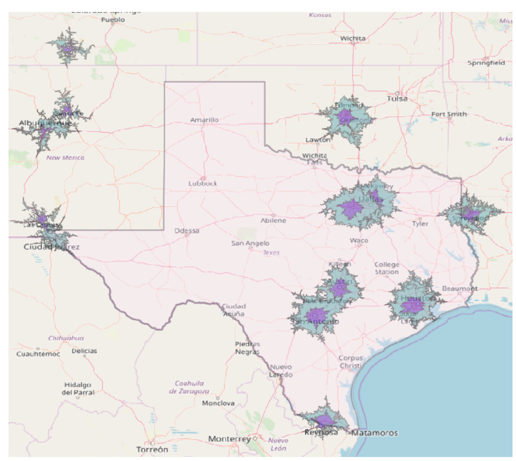
Note: Abortion clinics closure after Texas HB2 and increase in average distance from each municipality to the nearest clinic. The red horizontal line represents the implementation of the HB2 bill.
Source: Abortion clinics names and opening and closing dates are taken from Lindo et al. (2020). The average distance is calculated for all the municipalities of the sample for the period 2009-2016.

Figure 2: Abortion clinics accessibility

(a) Abortion clinics in 2009

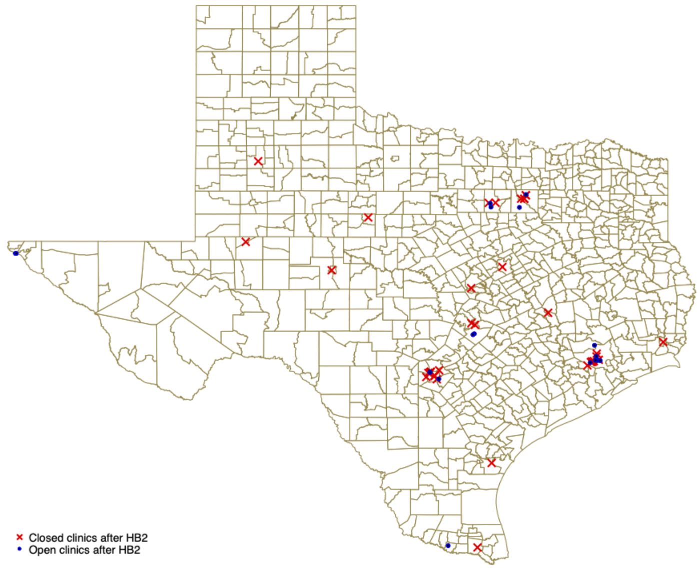


(b) Abortion clinics in 2016



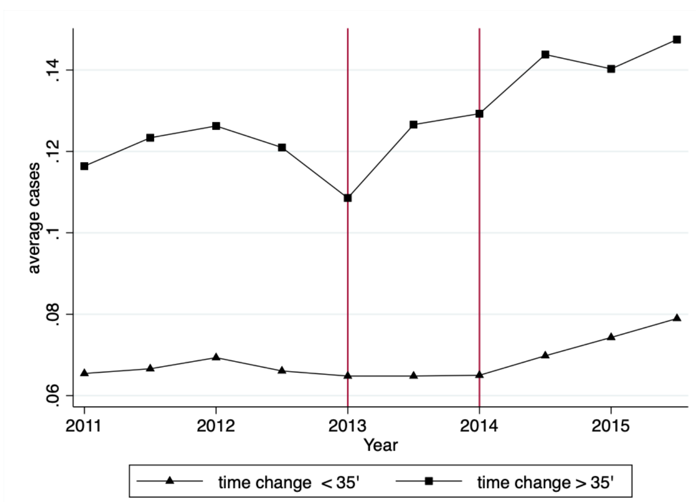
Note: Abortion clinics in Texas and neighboring states in 2009 and 2016. Around each point I drew 30 minutes and 1 hour isochrones to show geographic accessibility.

Figure 3: Randomness of abortion clinics' closure after HB2



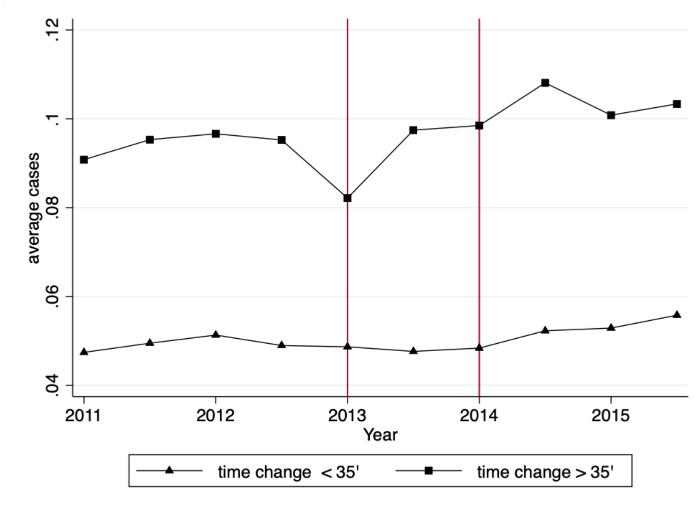
Note: Geographic distribution of abortion clinics after HB2. Crosses represent closed clinics, while points are the remained ones. The light brown lines mark the division of the state in counties.
Source: Abortion clinics' opening and closing dates are taken from Lindo et al. (2020).

Figure 4: Trends in reported cases of gender violence



Note: Average change in reported cases of gender violence weighted by municipality population. The two horizontal lines represent the period of HB2 implementation and one year after the implementation respectively.
Source: Data on reported cases of gender violence are from the Uniform Crime Reporting Program Data.

Figure 5: Trends in reported cases of intimate partner violence

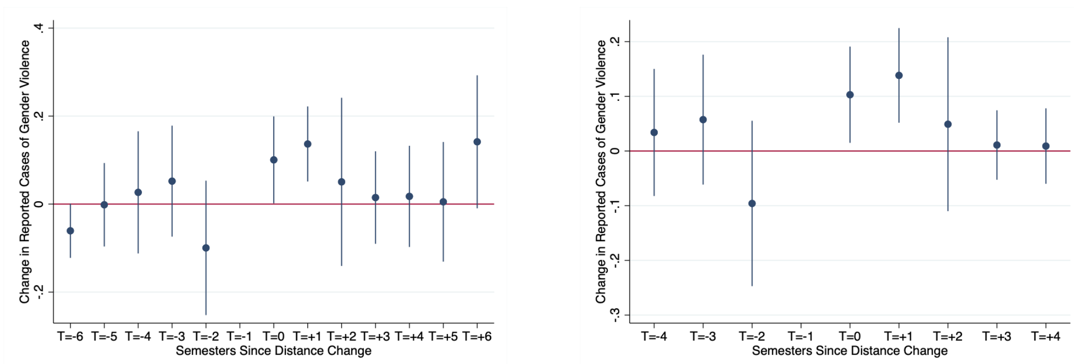


Note: Average change in reported cases of IPV weighted by municipality population. The two horizontal lines represent the period of HB2 implementation and one year after the implementation respectively.
Source: Data on reported cases of intimate partner violence are from the Uniform Crime Reporting Program Data.

Figure 6: Event Studies: Effect of Abortion Access on gender violence

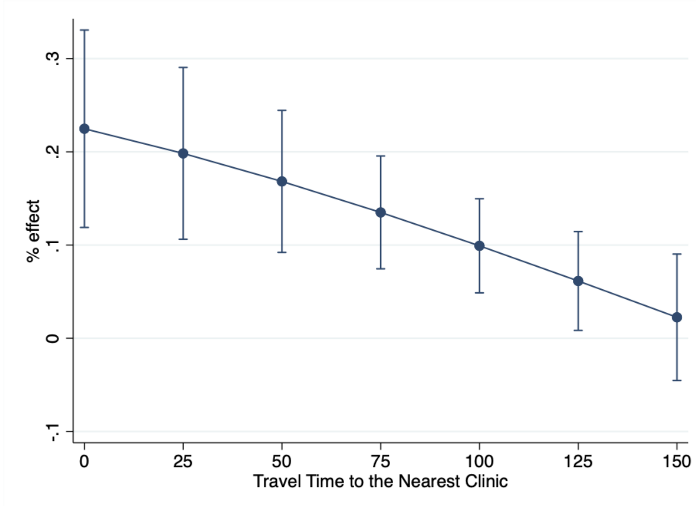
(a) 3 Years-10 minutes

(b) 2 Year - 10 minutes



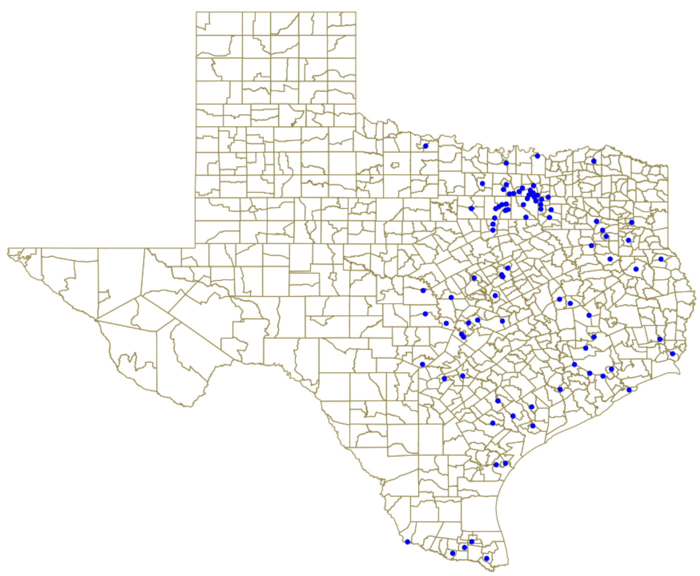
Note: The event studies are estimated through a Fixed Effects Poisson model which is equivalent to the model used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to semesters relative to the event. The event is defined as the first period in which a municipality switched from having a clinic to not having a clinic within the corresponding driving time. The semester prior the event is omitted as it is the reference group.

Figure 7: Average marginal effect of an increase in travel time by starting level



Note: Plot of estimated marginal effects and 95 percent confidence intervals based on results in Column 3 of Table 4.

Figure 8: Eastern municipalities



Note: Sample's municipalities located in the eastern part of Texas.

Tables

Table 1: Summary statistics

	2010-2016				
	Mean	Standard dev.	Min.	Max.	N
Cases of gender violence	153.4	370.27	0	2,844	884
Distance to the nearest clinic (minutes)	57.34	50.68	5.3	281.37	870
Distance squared	5,854.02	12,132.43	28.09	79,169.07	870
Municipality level controls					
(log) Population	10.28	1.51	6.27	14.19	884
Hispanic	17,939.44	40,363.61	18	278,125	830
Black	9,101.98	22,226.19	0	156,413	830
County level controls					
(log) Population	12.48	1.73	6.70	15.34	902
Hispanic share	0.28	0.18	0.04	0.96	902
Black share	0.11	0.07	0.003	0.35	902
Share of females (15-44)	0.23	0.03	0.14	0.27	902
(log) Per capita income ($)	10.51	0.23	9.84	11.50	902
Unemployment rate	5.9	1.78	2.77	13.65	902

Note: Population-weighted summary statistics calculated for 100 Texas cities for the period $2010-2016$.
Source: Abortion clinics opening and closing dates are taken from Lindo et al. (2020). The average distance is calculated by the author for all the municipalities in the sample. County-level demographic controls are taken from the National Institute of Health Surveillance, Epidemiology and End Results, while county-level per capita income 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. Municipality-level demographic controls come from the American Community Survey and information on municipality population is from the U.S. Bureau of Labor statistics.

Table 2: Summary statistics. Pre and post HB-2

	Pre HB2		Post HB2	
	Mean	Standard dev.	Mean	Standard dev.
Cases of gender violence	168.05	361.60	142.80	377.70
Distance to the nearest clinic (minutes)	41.75	29.97	69.62	59.93
Distance squared	2,638.88	3,642.24	8,431.52	15,545.98
Municipality level controls				
(log) Population	10.519	1.39	10.09	1.57
Hispanic	18,294.68	39,184.7	117,480.69	40,871.17
Black	9,331.12	21,541.62	8,854.30	22,570.57
County level controls				
(log) Population	12.63	1.65	12.36	1.78
Hispanic share	0.26	0.13	0.31	0.21
Black share	0.11	0.07	0.11	0.07
Share of females (15-44)	0.24	0.02	0.23	0.03
Log of per capita income	10.50	0.22	10.51	0.24
Unemployment rate	7.13	1.17	4.98	1.58

Note: Population-weighted summary statistics calculated for 100 Texas cities for the pre HB-2 period (2010-2012) and post HB-2 period (second semester of 2013 - 2016).
Source: Abortion clinics opening and closing dates are taken from Lindo et al. (2020). The average distance is calculated by the author for all the municipalities in the sample. County-level demographic controls are taken from the National Institute of Health Surveillance, Epidemiology and End Results, while county-level per capita income 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. Municipality-level demographic controls come from the American Community Survey and information on municipality population is from the U.S. Bureau of Labor statistics.

Table 3: Estimated effect of distance to the nearest abortion clinic on number of cases of gender violence

	(1)	(2)	(3)
	(Gender violence)	(Gender violence)	(Gender violence)
$Distance_t$ $(min.)$	.00040	.0014***	.0017***
	(.0002)	(.0003)	(.0004)
$Distance_t^2$ $(min.)$		$-.000004^{***}$	$-.000005^{**}$
		(.000001)	(.000002)
Municipality FE and year FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	869	869	828

Note: Estimated effect of travel time to the nearest abortion clinic on gender violence for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 4: Estimated average marginal effect of distance to the nearest abortion clinic on reported cases of gender violence

	(1) (Gender Violence)	(2) (Gender Violence)	(3) (Gender Violence)
$Distance_t$	0.05	0.18***	0.24***
	(.03)	(.04)	(.06)
$Distance_t^2$		−.0005***	−.0007***
		(.0002)	(.0002)
Municipality FE and Year FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	869	869	828

Note: Average marginal effect of one minute increase in travel time to the nearest abortion clinic on gender violence for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 5: Estimated lagged effect of distance to the nearest abortion clinic on gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_{t-2}$ *(min.)*	−.00005	.0014***	.0017***
	(.0003)	(.0004)	(.0004)
$Distance_{t-2}^2$ *(min.)*		−.000006***	−.000007***
		(.000002)	(.000002)
Municipality FE and year FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	855	855	830

Note: Estimated legged effect of travel time to the nearest abortion clinic on gender violence for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 6: Estimated average marginal effect of distance to the nearest abortion clinic on gender violence the following year

	(1) (Gender Violence)	(2) (Gender Violence)	(3) (Gender Violence)
$Distance_{t-2}$ (*min.*)	$-.006$	$.19^{***}$	$.24^{***}$
	(.05)	(.06)	(.06)
$Distance^2_{t-2}$ (*min.*)		$-.0007^{***}$	$-.001^{***}$
		(.0002)	(.0002)
Municipality FE and Year FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	855	855	830

Note: Average marginal effect of one minute increase in travel time to the nearest abortion clinic on gender violence the following year for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 7: Estimated effect of distance on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)	(5) (IPV)	(6) (IPV)
$Distance_t$ (*min.*)	.0003	$.0012^{***}$	$.0016^{***}$			
	(.0003)	(.0003)	(.0005)			
$Distance^2_t$ (*min.*)		$-.000003^{**}$	$-.000005^{**}$			
		(.000002)	(.000002)			
$Distance_{(t-2)}$ (*min.*)				$-.00012$	$.0010^{**}$	$.0016^{***}$
				(.0003)	(.0004)	(.0004)
$Distance^2_{(t-2)}$ (*min.*)					$-.000004^{***}$	$-.000007^{***}$
					(.000002)	(.000002)
Municipality FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	824	824	824	826	826	826

Note: Estimated effect of travel time to the nearest abortion clinic on intimate partner violence (IPV) for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 8: Estimated average marginal effect of distance to the nearest abortion clinic on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)	(5) (IPV)	(6) (IPV)
$Distance_t$	.028	.12***	.15***			
	(.02)	(.03)	(.05)			
$Distance^2_t$		−.0003**	−.0005**			
		(.0001)	(.0002)			
$Distance_{(t-2)}$				-.011	.09**	.15***
				(.03)	(.04)	(.04)
$Distance^2_{(t-2)}$					−.0004***	−.0007***
					(.0001)	(.0002)
Municipality FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	824	824	824	826	826	826

Note: Average marginal effect of one minute increase in travel time to the nearest abortion clinic on intimate partner violence (IPV) for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 9: Estimated effect of distance to the nearest abortion clinic on gender violence for municipalities in the top 25% of the unemployment rate distribution

	(1) (Gender violence)	(2) (Gender violence)
$Distance_t$ $(min.)$	$.0015^{***}$	$.0018^{***}$
	(.0003)	(.0004)
$Distance_t^2$ $(min.)$	$-.000004^{***}$	$-.000003^{***}$
	(.000001)	(.000001)
$Top\ 25\%\ unemployment$	$-.046$	$-.042$
	(.47)	(.04)
$Distance_t * top\ 25\%\ unemployment$	$.0074^{*}$	$.0082^{*}$
	(.004)	(.005)
$Distance_t^2 * top\ 25\%\ unemployment$	$-.00009^{*}$	$-.0001^{*}$
	(.00005)	(.00005)
Municipality FE and year FE	Yes	Yes
Time-varying controls	No	Yes
Number of observations	862	862

Note: Estimated effect of travel time to the nearest abortion clinic on gender violence for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per county, hispanic people per county, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 10: Estimated effect of distance to the nearest abortion clinic on female high school dropout rate

	GLM	GLM	Poisson	Poisson
	(1) (Dropout Rate)	(2) (Dropout Rate)	(3) (Dropout Rate)	(4) (Dropout Rate)
$Distance_t$	.099**	.095**	.004**	.0037*
	(.042)	(.046)	(.002)	(.002)
$Distance^2_t$	−.0003**	−.0003*	−.000015**	−.0000143**
	(.0001)	(.0001)	(.000006)	(.000006)
Municipality FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	1,764	1,677	1,652	1,595

Note: Estimated effect of one minute increase in travel time to the nearest abortion clinic on female high school dropout rate for all Texas' counties from 2010 to 2016. Estimates in columns (1) and (2) are based on a generalized linear with Gaussian family distribution and estimates in columns (3) and (4) are based on a two-way fixed effects Poisson model. The analysis is at the county-year level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is female population aged 15-19. Time-varying controls are black people, hispanic people and per capita income. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 11: Estimated effect of distance on gender violence, using the balanced subsample

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
$Distance_t$ $(min.)$	.0013***	.0012***		
	(.0003)	(.0003)		
$Distance^2_t$ $(min.)$	−.000003**	−.000002		
	(.000001)	(.000001)		
$Distance_{(t-2)}$ $(min.)$			.0016***	.0014***
			(.0004)	(.0004)
$Distance^2_{(t-2)}$ $(min.)$			−.000006***	−.000005**
			(.000002)	(.000002)
Municipality FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	532	518	532	518

Note: Estimated effect of travel time to the nearest abortion clinic on gender violence (GV), excluding municipalities with missing observations, from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 12: Estimated effect of distance to the nearest abortion clinic on gender violence for municipalities in the eastern part of Texas

	(1) (Gender violence)	(2) (Gender violence)
$Distance_t$ *(min.)*	.031*	.003*
	(.002)	(.001)
Municipality FE and year FE	Yes	Yes
Time-varying controls	No	No
Number of observations	755	755

Note: Estimated effect of travel time to the nearest abortion clinic on gender violence for municipalities in the eastern part of Texas, from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table 13: Estimated effect of distance on gender violence, excluding municipalities in counties on the Mexican border.

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
$Distance_t$ $(min.)$	.0015***	.0018***		
	(.0003)	(.0004)		
$Distance_t^2$ $(min.)$	−.000004***	−.000005***		
	(.000001)	(.000001)		
$Distance_{(t-2)}$ $(min.)$			.0015***	.0018***
			(.0004)	(.0005)
$Distance_{(t-2)}^2$ $(min.)$			−.000006***	−.000008***
			(.000001)	(.000002)
Municipality FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	821	794	821	796

Note: Estimated effect of distance to the nearest abortion clinic on gender violence (GV), excluding municipalities in counties near the Mexican border, from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are municipality population, black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Appendix A Variables description

A.1 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 Fonding
- Statutory Rape
- Human Trafficking - Involuntary Servitude
- Kidnaping/Abduction
- Pornography/Obscene Material
- Prostitution
- Assisting or Promoting Prostitution
- Purchasing Prostitution
- Forcible Rape
- Forcible Sodomy

Appendix B Tests for random assignment of treatment

Table 14: The effect of distance on the predicted level of gender violence

	(1) (GV)	(2) (Predicted GV)
Distance		-.00002
		(.00007)
Municipality population (log)	.39	
	(.24)	
Hispanic by municipality	.000002	
	(.000005)	
Black by municipality	-.0000007	
	(.000008)	
Unemployment rate by county	.04**	
	(.02)	
Per capita income by county (log)	-.004	
	(.04)	
Share of female aged 15-44 by county	1.15	
	(9.27)	
City FE and year FE	Yes	Yes
Number of observations	846	846

Note: Estimated effect of distance to the nearest abortion clinic on the part of gender violence predicted by controls (*predicted GV*). Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent level respectively.

Table 15: The effect of distance on covariates.

	(1) (Pop.)	(2) (Income)	(3) (Unemp. rate)	(4) (Hispanic)	(5) (Black)	(6) (Fem. 15-44)
Distance (minutes)	−.00018 (.0001)					
Distance (minutes)		-.0004 (.0006)				
Distance (minutes)			-.0001 (.0001)			
Distance (minutes)				.00002 (.0002)		
Distance (minutes)					.00002 (.0004)	
Distance (minutes)						.000002 (.00002)
City FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	960	960	960	846	808	850

Note: Estimated effect of distance to the nearest abortion clinic on controls. All explanatory variables are in logarithm. Estimates are based on a two-way fixed effects OLS model and the analysis is at the municipality-semester level. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent level respectively.

Appendix C Seasonal trends in IPV

Table 16: Estimated effect of distance on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)
$Distance_t$ $(min.)$	$.0012^{***}$	$.0015^{**}$		
	(.0004)	(.0007)		
$Distance_t^2$ $(min.)$	$-.000003^{*}$	$-.000005^{*}$		
	(.000002)	(.000003)		
$Distance_{(t-2)}$ $(min.)$			$.0008^{*}$	$.0012^{**}$
			(.0005)	(.0005)
$Distance_{(t-2)}^2$ $(min.)$			$-.000003^{**}$	$-.000006^{**}$
			(.000002)	(.000002)
Municipality FE and semester FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	824	824	826	826

Note: Estimated effect of travel time to the nearest abortion clinic on intimate partner violence (IPV) for 100 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and semester fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, share of females in reproductive age (15-44) per county, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Appendix D Evidence on self-induced abortions

D.1 Counties near the Mexican border excluded from the analysis

- Brewster
- Brooks
- Cameron
- Culberson
- Dimmit
- El Paso
- Hidalgo
- Hudspeth
- Jeff Davis
- Jim Hogg
- Kinney
- Maverick
- Pecos
- Presidio
- Starr
- Terrel
- Val Verde
- Webb
- Willacy
- Zapata
- Zavala

Appendix E Placebo test

E.1 Type of offense

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

E.2 Estimated effect of distance to the nearest abortion clinic on other crimes

Table 17: Estimated effect of distance on other crimes

	(1) (OC)	(2) (OC)	(3) (OC)	(4) (OC)
$Distance_t$ (*min.*)	$-.0007^{**}$	-.0006	-.0008	-.0003
	(.0003)	(.0005)	(.001)	(.0008)
$Distance_t^2$ (*min.*)			.0000004	-.000001
			(.000003)	(.000004)
Municipality FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	828	718	828	718

Note: Estimated effect of travel time to the nearest abortion clinic on other crimes (OC) for 100 Texas cities from 2009 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the municipality-semester level. All regressions include municipality and year fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are black people per municipality, hispanic people per municipality, per capita income per county and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Appendix F The first order effect of clinics closure on abortions and births

To check the validity of the causality channel I pinned down, I replicate the estimation of the first order effect of distance on abortions and births, followed the analysis of Fisher et al (2018). Again I use a two-way Fixed Effect Poisson model, estimated by a pseudo maximum likelihood.
The baseline model is

$$E[Y_{c,t}|dist_{c,t}, \alpha_c, \delta_t, X_{c,t}] = exp(\beta_1 dist_{c,t} + \alpha_c + \delta_t + X'_{c,t}\beta_2) \tag{2}$$

$Y_{c,t}$ is either the number of abortion or the number of births by county c in year t.
$dist_{c,t}$ is a binary measure of access to abortion clinics for county c in year t.
α_c is county fixed effect and δ_t is year fixed effect. $X'_{c,t}$ is the vectors of time varying controls at the county level.
In all specifications the relevant population (women of childbearing age (15-44)) is included as the exposure variable to account for the fact that counties vary in size and hence have a different potential for births and abortions.
Data on abortions and births are from the Texas Department of State Health Services. Demographic controls are taken from the National Institute of Health Surveillance, Epidemiology and End Results (SEER), while per capita income estimates are from the U.S. Bureau of economic activity (BEA) and unemployment rate estimates from the U.S. Bureau of Labor Statistics (BLS). Time varying controls include unemployment rate, per capita income, race/ethnicity-specific populations and female in reproductive age. Summary statistics are reported in Table 18.
Following Fischer et al. (2018) I use a bin measure of access to abortion as explanatory variable:

- $\mathbb{1}(travel\ time > 25\ minutes)$
- $\mathbb{1}(travel\ distance > 60\ minutes)$

- $\mathbb{1}(travel\ time > 115\ minutes)$

In each regression, I include one single indicator for clinic access rather than the full set of indicators. As such, the coefficient on the 25-minutes dummy is the effect of passing from having an abortion clinic within a distance of 25 minutes by car to not having an abortion clinic within 25 minutes.

Results for the abortion analysis are reported in Table 19. Columns 1-6 show that the estimates are insensitive to the inclusion of time-varying controls. Using the full set of covariates, the 25-minutes, 60-minutes, and 115-minutes estimates indicate a 10%, 25%, and 36% decrease in abortion, respectively (column 2,4 and 6). Results for the birth analysis are reported in Table 20. The strongest results is for the 25-minutes estimate whose coefficient indicates a 11% increase in births (column 2). Results confirm my hypothesis of an increase in the number of births and a decrease in the number of abortions as a consequence of restrictions to abortion access.

Table 18: Summary statistics

	2009-2016				
	Mean	Standard dev.	Min	Max	N
Dependent variables					
Abortions	4751.64	5850.819	0	18914	2,032
Births	86.736	177.656	0	998	1,776
Explanatory variables					
Travel time to closest clinic (minutes)	41.362	49.747	6.23	303.05	2,032
Travel time squared	4,184.449	11,513.55	38.813	91,839.3	2,032
Time varying controls					
Females 15-44	315,954.2	330,873.2	9	1,007,817	2,032
Share of black women 15-44	.072	.076	0	.412	2,032
Share of hispanic women 15-44	.378	.235	.028	.971	2,032
Unemployment rate	6.317313	1.926	1.866	18.083	2,032
Per capita income	42,758.84	10444.1	18,792	162,378	2,032

Note: Population-weighted summary statistics calculated for the 254 Texas counties for the period $2009-2016$. The number of observations is 2032, that decreases to 1776 for births count since I didi not find information for 2009.
Source: data on abortions and births are from the Texas Department of State Health Services. Demographic controls are taken from the National Institute of Health Surveillance, Epidemiology and End Results, while per capita income estimates are from the U.S. Bureau of economic activity and unemployment rate estimates from the Bureau of Labor Statistics.

Table 19: Estimated effect of access to abortion on the number of abortions

	(1) (IVG)	(2) (IVG)	(3) (IVG)	(4) (IVG)	(5) (IVG)	(6) (IVG)
Clinics within 25 min.	$-.11$	$-.10$				
	(.09)	(.08)				
Clinics within 60 min.			$-.22^{***}$	$-.25^{***}$		
			(.05)	(.05)		
Clinics within 115 min.					$-.32^{***}$	$-.36^{***}$
					(.07)	(.07)
County FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes	No	Yes
Number of observations	2,032	2,032	2,032	2,032	2,032	2,032

Note: Estimated effect of distance to the nearest abortion clinic on abortions (IVG). Estimates are based on a two-way fixed effects Poisson model and the analysis is at the county-year level. All regressions include county and year fixed effects. The exposure variable included in all regressions is women in reproductive age (15-44 years old) per county. Time-varying controls are share of women in reproductive age who are black, share of women in reproductive age who are hispanic, unemployment rate and per capita income. Robust standard errors are reported in parentheses and are clustered at the county level. *, ** and *** indicate statistical significance at ten, five and one percent level respectively.

Table 20: Estimated effect of access to abortion on the number of births

	(1) (Births)	(2) (Births)	(3) (Births)	(4) (Births)	(5) (Births)	(6) (Births)
Clinics within 25 min.	.10**	.11**				
	(.049)	(.046)				
Clinics within 60 min.			-.07	-.07		
			(.07)	(.08)		
Clinics within 115 min.					.05	.05
					(.03)	(.03)
County FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes	No	Yes
County-specific trends	No	No	No	No	No	No
Number of observations	1,757	1,757	1,757	1,757	1,757	1,757

Note: Estimated effect of distance to the nearest abortion clinic on births. Estimates are based on a two way fixed effects Poisson model and the analysis is at the county-year level. All regressions include county and year fixed effects. The exposure variable included in all regressions is women in reproductive age (15-44 years old) per county. Time-varying controls are share of women in reproductive age who are black, share of women in reproductive age who are hispanic, unemployment rate and per capita income. Robust standard errors are reported in parentheses and are clustered at the county level. *, ** and *** indicate statistical significance at ten, five and one percent level respectively.