THE EFFECTS OF LIMITING ACCESS TO ABORTION ON DOMESTIC VIOLENCE

by

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ABSTRACT

This paper estimates the effect of access to abortion on the number of reported domestic violence victims using abortion clinic closures in the state of Texas that resulted from a statewide policy change. Results suggest that a decrease in access to abortion services causes a decrease in the number of reported intimate partner victims, though the evidence is weak. When analyzing the effects by relationship type, the results appear to be driven by victims with no legal ties to their offender, such as dating partners. The results indicate that a law enforcement agency that no longer has an abortion clinic within 100 miles reports 13.8% fewer dating partner victims. Because domestic violence crimes are vastly underreported and the data I use are exposed to this issue, the results could be due to a change in the frequency of the crime or a change in the decision to report the crime. This is the first paper to estimate this causal relationship, contributing to the literature by analyzing a policy that could have unintended impacts on victims of domestic violence.

1. INTRODUCTION

In the United States, about one in four women will be a victim of physical violence, sexual violence, or stalking by an intimate partner in her lifetime (CDC, 2019). Domestic violence is a rampant issue and considered a public health problem because of its adverse short and long-term impacts on its victims, which far exceed the costs of injuries from physical abuse. For example, victims experience anxiety and post-traumatic stress disorder (PTSD) in the long-run, and roughly half of female homicide victims in the U.S. are killed by a current or former intimate partner (CDC, 2019). Women suffering this type of violence are also more likely to experience unintended pregnancies as compared to non-victims (Cripe et al., 2008). Because a child has the potential to act as a tether between a female victim and her abusive partner, she may seek an abortion in order to more easily leave her partner and prevent future violence towards her and her child (Roberts et al., 2014). Women seeking abortions experience high domestic violence rates, making it natural to wonder how the victims of this violence are affected by policies that affect access to abortion.

The goal of this paper is to explore how access to abortion affects domestic violence. This study aims to provide insight into the dynamics of abusive relationships and contribute to the evaluation of policies that change access to abortion services. Though there are several studies that examine how changes in abortion access or other family planning resources impact various women's health outcomes, this paper is the first to estimate the causal effect of access to abortion on domestic violence.

My empirical strategy exploits abortion clinic closures in Texas that resulted from Texas House Bill 2 (HB2), a statewide legislation passed in 2013 that imposed major restrictions on abortion clinics, causing roughly half of the state's clinics to close and the population's average distance to the nearest abortion clinic to nearly double. The isolated shocks to the supply of these clinics provide plausibly exogenous variation in access to abortion services across the state of Texas. Using Texas Department of Public Safety's family violence data at the law enforcement agency-level, I implement changes in distance from a law enforcement agency to the nearest operating abortion clinic to estimate the effect on the reported number of intimate partner victims, including spouses, ex-spouses, common-law spouses, and dating partners. The number of victims is a function of whether or not the crime is being reported to law enforcement.[1] Because of this, using these data will reflect changes in the reported domestic violence, which could be a result of a change in the reporting or a change in the frequency of the crime itself.

A priori, it is unclear what the effects of a decrease in access to abortion are on violence against intimate partners. There are several potential mechanisms that could lead to changes in domestic violence. The absence of abortion services may cause an increase in domestic violence because a woman cannot as easily escape the abusive relationship once she has a child with her abusive partner. On the contrary, it is possible that if a victim has a child because she no longer has abortion access, bringing a child into the relationship could encourage the abuser to decrease or stop their violent behavior. A decrease in access to abortion services may also affect domestic violence because of the

[1] Underreporting is a major issue for domestic violence, where only as much as 56% of these crimes are reported to law enforcement (Reaves, 2017).

information, advocacy, and outside resources the clinics provide regarding this violence. Most abortion clinics require screening of domestic violence for all patients and then teach victims what the signs of abuse are and how to safely leave an abusive partner (Planned Parenthood, 2014). In the absence of these resources and information, both the crime itself and the reporting of the crime could be impacted. For example, without the information provided by abortion clinics, it is possible that victims report the crime less often. This result could affect victims further by causing violence to increase because perpetrators are not held accountable as often. Again, because of the nature of the main outcome variable used in this paper, an effect detected in the analysis could be due to a change in the reporting of the crime or a change in the true rate of the crime.

The analysis also explores different types of intimate partner relationships because of the varying costs an unintended pregnancy might create for these different groups. For example, dating partners may have a greater marginal cost of carrying a pregnancy to term with an abusive partner compared to spouses because dating partners have no legal tie to their perpetrator before the child is born. Because a legal tie to a partner may be correlated with an increased reaction to a change in abortion access, it is important to analyze these groups individually.

I find weak evidence suggesting that a decrease in access to abortion causes a decrease in the reported number of intimate partner victims. These results appear to be largely driven by dating partners. More specifically, the estimates indicate that a law enforcement agency that no longer has a clinic within 100 miles reports 13.8% fewer dating partner victims. To explore whether this effect is a decrease in the reporting or a

decrease in the crime itself, I also use female homicides as an outcome variable because, unlike the majority of domestic violence crimes, homicides are nearly perfectly reported. However, the standard errors are sizable and the estimates are not statistically significant, leaving this mechanism unclear. I also separately analyze the effects on spouses, common-law spouses, and ex-spouses, and the estimates are too imprecise to make any conclusion about whether these groups are impacted.

The remainder of this paper is organized in the following manner: section 2 provides background, section 3 describes the data, section 4 explains the empirical model, section 6 and section 7 presents results and robustness checks, and section 8 concludes.

2. BACKGROUND

This section provides background on domestic violence, including who is most vulnerable and its adverse effects, and describes the current literature relevant to this paper. The section then provides a description of Texas House Bill 2 (HB2) and its use within the existing family planning and abortion access literature. The end of the section explains the probable correlation between abuse and abortions and the potential mechanisms for which access to abortion services may impact domestic violence.

Domestic Violence

Domestic violence refers to violence against current or former spouses and dating partners and does not require sexual intimacy.[2] The types of behavior considered domestic violence include physical violence, sexual violence, stalking, and emotional and psychological abuse. Although individuals of all ages, genders, races, ethnicities, and socio-economic statuses can be affected by intimate partner violence, some groups are more susceptible than others. Women between the ages of 18 and 24 are the most common victims of domestic violence, and because 75% of domestic violence is committed against women, this paper focuses on male-on-female[3] violence (Truman & Morgan, 2014). On average, abused women and abusive men are more likely to grow up in abusive households, have low education, and receive a low income (Aizer, 2010;

[2] Some states consider any violence that occurs within the home domestic violence, including violence against children, parents, or roommates. However, in this paper, domestic violence refers to violence against intimate partners. The paper also uses the term intimate partner violence interchangeably for domestic violence.

[3] Though same-sex relationships are also susceptible to domestic violence, this research concerns heterosexual relationships due to the paper's primary question regarding pregnancies and abortions.

Bowlus & Seitz, 2006). These three risk factors and their association with different races may help explain why non-white individuals are more often involved in abusive relationships.

The effects of domestic violence are detrimental to a victim's short-term and long-term health. Aside from short-term physical injury inflicted by abusive partners, victims of domestic violence experience depression, anxiety, and post-traumatic stress disorder, even long after the abuse is over (Office on Women's Health, 2017). Not only is a victim's health compromised because of this violence, but research also estimates that hospitalization for an assault while pregnant reduces the birth weight of the child by 163 grams (5.75 ounces), disproportionately affecting poor mothers (Aizer, 2011). In addition, the costs of domestic violence extend beyond health and the family. The National Coalition Against Domestic Violence (NCADV) reports that victims lose over 8 million days of paid work per year, inflicting both a productivity loss on employers and an income loss on victims. Carrell and Hoekstra (2012) expose another social cost of domestic violence, finding that children exposed to domestic violence also have adverse affects on their classmates' performances in school. However, as soon as the violence is reported to the court, the adverse effects disappear. Because these negative spillovers exist, assisting the families that suffer from domestic violence also benefits the public. Understanding the behavior surrounding domestic violence, who is most vulnerable, and its extensive adverse effects assists in guiding policy decisions.

Though there is a long-standing focus in the economic literature on crime, only a narrow section of this literature concerns domestic violence despite it being a widespread

issue. The existing literature discusses the relationship between the labor market and violence against women, policies aiming to reduce domestic violence, and unintended impacts of policies with primary goals unrelated to domestic violence. The literature regarding labor market outcomes finds that a positive change in the labor market for women, such as an increase in women's wages and employment, decreases domestic violence committed against women (Aizer, 2010; Anderberg et al., 2016; Bowlus & Seitz, 2006). For example, Aizer (2010) uses exogenous changes in the demand for labor in female-dominated industries to investigate how changes in wages impact domestic abuse against women and finds that a decrease in the gender wage gap reduces intimate partner violence. This research indicates that women with monetary resources outside of their partner have less incentive to stay with their abuser because they do not have to rely as heavily on the security of their partner's income.

The literature also analyzes programs and policies that aim to combat domestic violence such as Lethality Assessment Programs (LAP),[4] mandatory arrest laws,[5] and no-drop policies.[6] However, the findings show varied effectiveness of these programs and policies. Research finds that LAP and no-drop policies are successful in deterring perpetrators and decreasing domestic violence (Aizer & Bó, 2009; Koppa, 2018), while mandatory arrest laws have been found to exacerbate the issue by increasing intimate

[4] Law enforcement agencies adopt Lethality Assessment Programs to directly assist victims of domestic violence. The programs teach law enforcement officers how to identify victims at risk of fatality by an intimate partner and provide them with awareness, resources, and safety plans.
[5] Mandatory arrest laws are state-wide policies that aim to deter perpetrators from committing assaults against intimate partners by requiring law enforcement officers to arrest the suspected offender when arriving at a scene that appears to be a domestic violence case. These laws are controversial due to the suspicion that they create more danger for victims because perpetrators may retaliate after being released from jail or victims may refrain from reporting the crime in the first place.
[6] No-drop policies prohibit the persecutor from dropping charges after a perpetrator has been charged with a domestic violence crime.

partner homicides (Iyengar, 2009). Because the behavior of victims and perpetrators can be unpredictable and unintuitive, conducting these analyses is necessary in order to ensure the policies produce their intended effect by helping victims instead of further harming them.

In addition to evaluating policies that are intentionally aimed at decreasing domestic violence, there also exists a subsection of the literature that analyzes policies affecting domestic abuse as a byproduct of the policies' main intent. For example, Bobonis et al. (2013), Hidbrobo and Fernald (2013), and Angelucci (2008) find that policies that increase cash transfers to women decrease domestic violence. Though the primary goal of policies that increase cash transfers to low-income women is not to explicitly assist victims of intimate partner violence, these policies do supply additional positive impacts. My paper contributes to the overall domestic violence literature and more specifically to this narrower subsection of studies by analyzing a policy that has the potential for unintended effects on domestic violence. This research will assist in understanding the complex behavior of victims and perpetrators involved in domestic violence, helping to create better informed policies that have the potential to impact domestic violence in positive or negative ways.

Abortion Access and Texas House Bill 2

Texas HB2 is known as a Targeted Restrictions on Abortion Providers (TRAP) law and was passed in July of 2013. The bill had four provisions: doctors providing abortions must have admitting privileges to a hospital within 30 miles of the abortion clinic, abortions are no longer legal 20 weeks after fertilization, women taking abortion-

inducing pills must visit the doctor for both doses as well as for a follow-up appointment within two weeks, and abortion clinics must abide by the same state requirements as ambulatory surgical centers that specify details such as hallway width and procedure room size (Texas Policy Evaluation Project, 2014). The first three requirements went into effect on November 1, 2013, and the last requirement went into effect in October of 2014 but was suspended two weeks later until the Supreme Court decided this portion of the bill was unconstitutional in 2016 (*Whole Woman's Health v. Hellerstedt*, 2016).[7] All of these provisions raised costs for clinics and made maintaining licensure a major challenge, and as a result, over half of the state's abortion clinics closed. After this sudden shock in the supply of abortion providers, the population's average distance to the nearest abortion clinic for an individual nearly doubled. Figure 1 uses the data from this paper's main analysis and shows this dramatic change in November 2013, where the average distance to the nearest operating abortion clinic increases from 21 miles to about 42 miles. Individuals in certain areas were impacted differently than others. Women in rural or smaller cities were impacted through an increase in distance to an abortion clinic. Women residing in larger cities, such as Austin, Houston, and Dallas, still had access to clinics but were affected by congestion in remaining clinics due to one or two clinics in the area closing down. The analysis in this paper focuses on the changes in distance rather than the changes in congestion.

In the current literature, economists exploit the abortion clinic closures resulting

[7] The case also decided that the provision of HB2 requiring admitting privileges to a hospital within 30 miles was unconstitutional. However, this portion of the law was enforced throughout the time period analyzed in this paper. Even after the Supreme Court ruled these two provisions unconstitutional, the majority of the abortion clinics that had closed as a result of this bill remained closed.

from HB2 to estimate how a change in access to abortion affects outcomes such as abortion rate, birth rate, and contraceptive use, and this paper expands on the outcomes relating to women's health using the same variation. The papers utilizing these closures use difference-in-differences approaches and conduct their analyses at the county-level using driving distance to nearest abortion clinic from a county's population centroid as their measure for access to abortion services. Fischer et al. (2018) find that counties in Texas that no longer had an abortion provider within 50 miles experienced a 16.7 percent decrease in the abortion rate after Texas HB2. Lindo et al. (2019) similarly find that increasing the travel distance to the nearest abortion clinic from 0-50 miles to 50-100 miles reduces the abortion rate by 16 percent. They also find that a decrease in access has less of an impact when the closest clinic is already more than 50 miles away.[8] The increase in distance to the nearest abortion clinic for the average resident of Texas serves as an extra cost to receiving an abortion. Because the closures provide useful quasi-experimental variation in abortion access, the papers mentioned here help motivate the model in my paper.[9]

Though it may be intuitive to predict that birth rates would noticeably increase after a large reduction in abortion rates, both Fischer et al. (2018) and Lindo et al. (2019) find that this is not necessarily the case. Fischer et al. (2018) finds a 1.3 percent increase in births for counties that no longer had abortion clinic within 50 miles and Lindo et al. (2019) find little evidence of an increase in births with a change in access to abortion.

[8] Lindo et al. (2019) also find that congestion at an abortion facility affects the abortion rate, reporting that a 100,000 women per clinic increase results in a seven percent decrease in abortions.
[9] Neither Fischer et al. (2018) nor Lindo et al. (2019) discuss the anticipation of the law or the closures. It is possible that individuals rushed to receive abortions before closures, which would cause estimates to be overstated.

Because the decrease in abortion rate is sizeable and significant in both papers, it is not clear whether the associated relatively small change in birth rate is due to an increase in the use of contraceptives, an increase in self-administered abortions,[10] or some other behavioral change. With economically significant estimates concerning fertility outcomes, these results prompt additional questions regarding how Texas HB2 and similar policies affect other aspects of women's health.[11] Policy makers claim that laws such as Texas HB2 aim to protect women's health (Eckholm, 2013), making it vital to analyze all aspects of women's health in order to properly evaluate the costs and benefits of laws that limit access to abortion. Domestic violence is regarded as one of the main health issues that women face, and therefore, this paper contributes by causally estimating this important relationship.

The paper that is most similar to my study estimates the effect of being denied an abortion on subsequent domestic violence by using the gestation cutoff as a random assignment to be approved or denied an abortion (Roberts et al., 2014). The study finds that women who are just beyond the gestation cutoff and carry the pregnancy to term (the treatment group) experience higher rates of domestic abuse after a year compared to those who are just before the gestation cutoff and receive an abortion (the control group). However, the sorting between the treatment and control groups is likely not random. Women experiencing abuse prior to the pregnancy could be more likely to have a later

[10] Women can self-induce abortions by obtaining an over-the-counter drug sold in Mexico called misoprostol (Eckholm, 2013).

[11] Other studies have found that Texas's funding cuts to family planning clinics (which, in Texas, are legally different from abortion clinics and cannot provide abortions) and the subsequent decrease in access to these clinics do not decrease abortions as these policies are intended (Fischer et al., 2018; Packham, 2017). These policies do compromise other aspects of women's health, however, with research showing a decrease in access to family planning clinics results in a decrease in annual clinical breast exams, mammograms, and Pap tests (Lu & Slusky, 2016).

term abortion because of the controlling behavior of their abusers, causing an increase in the probability of victims being beyond the gestation cutoff and being denied abortions. Because of this, the estimates are potentially biased away from zero.

The existing literature regarding both domestic violence and access to abortion are important in guiding my research. The domestic violence literature assists in explaining the complex dynamics of abusive relationships and its consequences as well as analyzing significant policies that relate to this issue. The literature concerning access to abortion services provides substantial evidence that limiting abortion access results in a decrease in the abortion rate, suggesting that these policies could affect individuals' lives in other ways. This paper contributes to all of these aspects of the literature in that it attempts to further explain the behavior of domestic violence victims and perpetrators and examine a potential unintended impact of a policy that changes access to abortion. Additionally, this will be the first paper to estimate a causal relationship between access to abortion and domestic violence.

The Link between Abuse and Abortions

Though the causal estimate of the effect of access to abortions on domestic violence has not yet been studied before this paper, the prevalence of abuse among pregnant women has, providing intuition for the relationship studied in this paper. For example, women with unintended pregnancies report significantly higher rates of abuse than those with intended pregnancies (Goodwin et al., 2000). The reason for this could be that victims of domestic violence are vulnerable to sexual and reproductive coercion, which involves a perpetrator aiming to control their partner's reproductive health by

attempting to impregnate a partner without her consent, manipulate or interfere with birth control methods, coerce a partner into having unprotected sex, or control the outcomes of a pregnancy. Those seeking abortions are primarily women with unintended pregnancies, and this suggests a possible link between abuse and abortions. To further emphasize the potential correlation between abortions and abuse, Wu et al. (2005) find that repeat abortions are more common among victims of abuse compared to non-victims. In addition, women seeking abortions share similar characteristics to those experiencing abuse, namely low income individuals and females between the ages of 20 and 29. Perhaps most importantly to this paper, most domestic violence occurs between dating partners, which is also the most common group seeking abortions (Jerman et al., 2016; Sorenson & Spear, 2018). The type of relationship between the victim and the perpetrator is central to this paper's analysis because of this correlation. The connections between unintended pregnancies and abuse generate the important question that this paper attempts to answer: how are abused women impacted with no access to abortion services and what are the reasons behind these potential effects?

The effects of a decrease in abortion access on domestic violence are ambiguous a priori. To hypothesize how victims are affected, it is useful to consider the costs and benefits of leaving an abusive relationship. The benefits of leaving an abusive relationship may be more obvious than the costs: leaving an abuser could provide a victim with better long-term health (both mentally and physically) and prevent a child from being exposed to abuse. The victim's main costs of leaving are losing the financial stability that a partner provides, losing a home to live in, and risking retaliation from a

partner. With this, it is easier to understand the costs and benefits of carrying a pregnancy to term and receiving an abortion. One cost of having a child with an abuser is that there would exist a permanent tie between a woman and her abusive partner, creating an additional barrier to leaving the abusive relationship. It could also expose the child to abuse. Women who were reporting abuse in the Turnaway Study, a study that involved interviewing women who all sought abortions but did not all receive them, gave these two costs as primary reasons for wanting to end a pregnancy (Chibber et al., 2014). On the contrary, a cost of having an abortion is the risk of retaliation from the abusive partner. The perceived benefits of carrying the pregnancy to term is that being pregnant could provide safety from physical abuse if the perpetrator is unwilling to risk hurting the baby.[12] A victim may also have the hope that bringing a child into the relationship will encourage the abuser to end their violent behavior.

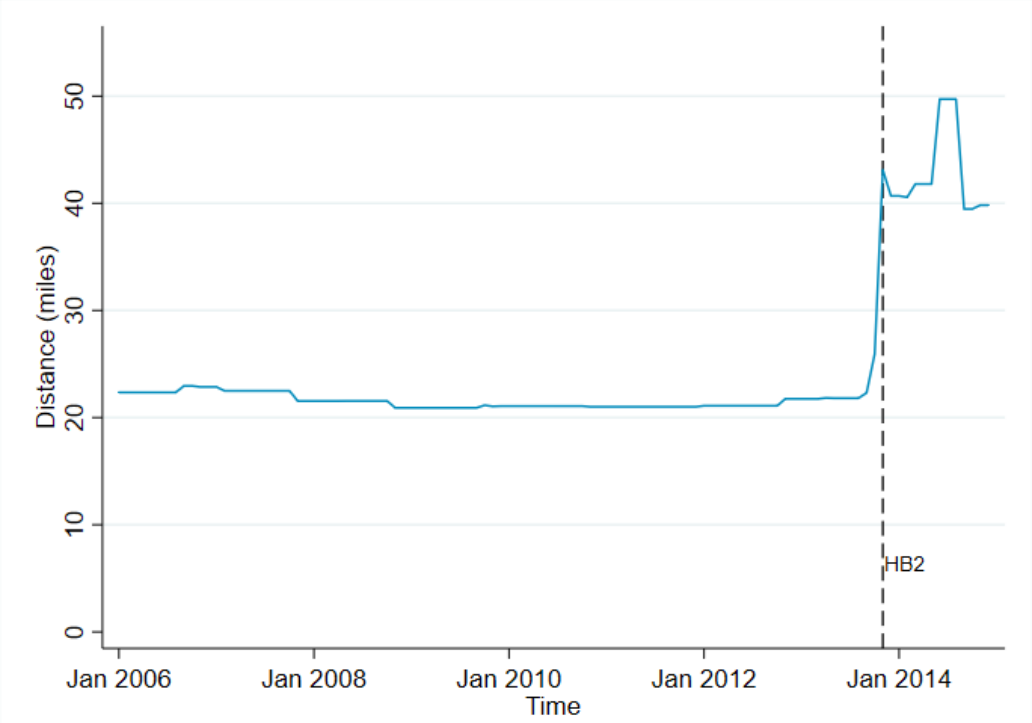
Considering these costs and benefits, there are several potential mechanisms that could lead to an effect on domestic violence. The absence of abortion services may cause violence to increase because the additional barrier to leaving the relationship causes a woman to stay with her abusive partner. On the contrary, it is possible that when a victim has a child that they otherwise would have aborted with access to abortion services, their partner decreases or stops their abusive behavior once a child is brought into the relationship. Though a victim may believe that a pregnancy could encourage her abuser to change, there is little evidence showing that pregnancy or a child provides protection

[12] During the time period considered in this paper, assault against a pregnant woman had no additional penalty compared to any other assault in the state of Texas (Odessa American, 2019). However, Texas does have a fetal homicide law, which allows the murder of an unborn child (excluding abortions) to be prosecuted as any other murder regardless of the stage of gestation (National Conference of State Legislatures, 2018).

from abuse (Bailey, 2010). Additionally, if an abortion brings conflict into a relationship when the decision is made unilaterally, a decrease in access may cause a decrease in violence.

The reduction in access to abortion services also translates to a reduction in the important information and resources that the clinics provide regarding domestic violence. Abortion clinics screen patients for domestic violence and then provide education and external resources to victims, such as shelters, counseling, and hotlines (Planned Parenthood, 2014). The lack of these resources could result in a decrease in the reporting of the crime because victims are unaware of their situation and do not have an advocate encouraging them to report the crime. It could also result in an increase in the violence because victims are no longer aware of the resources that will help them leave and, if there is a decrease in reporting, perpetrators are not held accountable for their crimes. Although the effects of access to abortion on violence against intimate partners are unclear, this research aims to provide insight into the behavior of individuals involved in these violent relationships.

Figure 1: Average distance to nearest abortion clinic for a Texas resident over time



Notes: The graph uses the law enforcement agency-by-month data from this study. The vertical line represents the implementation of Texas House Bill 2 on November 1, 2013, which caused abortion clinic closures across the state. The distance to the nearest clinic is the straight-line distance from a law enforcement agency to it's nearest operating abortion clinic, and that distance is averaged over law enforcement agencies in each month, weighted by the law enforcement agency's population covered.

3. DATA

I use distance to nearest abortion clinic as a measure of access to abortion services to estimate the effects on the number of domestic violence victims reported by Texas law enforcement agencies. My data are at the law enforcement agency-by-month level from 2006 to 2014 using 895 agencies. These panel data are unbalanced due to some agencies having no violence to report in various months and some agencies failing to report, which I explain and address in this section.

Crime Data

I use the number of domestic violence victims as my outcome variable from Texas Department of Public Safety's Family Violence Crime Reports (2015). These monthly family violence data are at the law enforcement agency level and include the total number of family violence victims and the number of victims by relationship to the offender, where there are 17 relationship categories.[13] The main outcome variable is the sum of all victims that are considered intimate partners. There are four categories that include intimate partners: spouse, ex-spouse, common-law spouse, and other. In Texas, dating violence is included in family violence, but there is no explicit category for dating partners in the data, so these victims fall under the category called *other*.[14] It is reasonable to assume that the majority of victims in this category are dating partners because, as mentioned in the previous section, most domestic violence occurs between

[13] Relationship categories in the family violence data include spouse, common-law spouse, ex-spouse, parent, child, sibling, grandparent, grandchild, in-law, stepparent, stepchild, stepsibling, foster parent, foster child, roommate, other, and unknown. Table A1 shows the percentage of family violence by relationship type.

[14] In 2001, Texas Senate Bill 68 of the 77th Legislature changed family violence to include dating violence.

dating partners. In addition, there are few other familial relationships that can fall into this category that are not already accounted for by the other 16 categories: aunt, uncle, niece, nephew, and cousin. It is possible that the inclusion of these miscellaneous victim-offender relationships presents measurement error in the dependent variable and may cause the standard errors to be overstated due to additional noise. However, the victims in this category that are not dating partners are not likely to be affected by changes in access to abortion, so the inclusion of these miscellaneous victims are unlikely to introduce bias to the estimates.

The family violence data do not specify the gender of victims, but because over 75% of victims of domestic violence are female (Truman & Morgan, 2014), I assume that this same proportion is represented in the data. It is important to note that domestic violence data from law enforcement agencies underestimate the number of victims because only about half of domestic violence is reported to the police (Reaves, 2017), and the data I use are not likely to be immune to this issue. The number of victims in the data is dependent on the crime being reported to law enforcement, and because the violence is consistently underreported, it will not be clear whether an effect detected using these data is due to changes in the decision to report the crime or the true frequency of it.

The law enforcement agencies appearing in the family violence data are matched with the U.S. Department of Justice's Law Enforcement Agency Identifiers Crosswalk data in order to have more detailed information about each agency. The Crosswalk data include agency type (such as local police department and county sheriff's office), whether the agency has reported at least one crime since 1985 to the Uniform Crime

Reporting (UCR) program, the county in which the agency is located, whether the agency is in a census place, and the agency's address. With this information, I exclude miscellaneous agencies such as university and airport police and use only data from county sheriff's departments and local police departments. I exclude the law enforcement agencies that have reported zero offenses of all crimes or have not reported since 1985. I also use the law enforcement agency's population covered from the Federal Bureau of Investigation's UCR Offenses Known and Clearances by Arrest, which varies by year and represents the number of people for which a particular law enforcement agency is responsible within their jurisdiction. I exclude any agency that does not have the population covered variable. After narrowing down the law enforcement agencies in this way, there are 895 agencies left in the sample. Of these agencies, 641 are local police departments and 254 are county sheriff's departments. 387 agencies are classified as rural areas because they are non-census places, and 508 are classified as urban areas.

The biggest weakness to these data is that law enforcement agencies only appear in the family violence data if they have at least one victim of family violence in a given month. Because of this, it is difficult to deduce from these data whether or not an agency is missing in a particular month because it did not report or because it had zero victims of family violence. Nearly 29% of agencies are missing in more than half of the months throughout the time period, and the mean number of family violence victims for these agencies is 1.5 victims in the observed months. For this reason, I assume that many of these agencies had no victims of family violence and impute zeros for these agencies based on the agency's mean number of family violence victims. For example, if a law

enforcement agency's mean number of family violence victims per month is less than four, I impute zeros for that agency in all missing months. Table 1 shows the mean and standard deviation of all family violence victims and intimate partner victims associated with no imputed zeros and different rules for imputing zeros ranging from two to ten. The mean and standard deviation change very minimally as I increase the threshold for imputing zeros. This is reassuring, as it communicates that imputing zeros does not change the data in a problematic way and my results should not be sensitive to these changes. I use six as the rule for my main results and introduce different rules as a robustness check.

Because the main outcome variable is a function of reporting and an effect detected could be a result in a change in reporting, I also implement homicides as an additional dependent variable. Using UCR's Supplementary Homicide Reports, I use female homicides, male-on-female homicides, and female spousal homicides at the law enforcement agency-by-month level. Homicides are nearly perfectly reported, so a change in this violence may provide a better indication about the effect on domestic violence rather than the decision to report the crime.

Access to Abortion

I use distance from a law enforcement agency to its nearest operating abortion clinic as a proxy for access to abortion. The assumption is that this distance is a good proxy for access to abortion for the population that the police agency serves. I use the same set of abortion clinics and their operation dates implemented in Fischer et al. (2018). The abortion clinics include all Texas clinics offering abortions at any time

between 2006 and 2014 as well as clinics in near-by states (Colorado, New Mexico, Louisiana, Oklahoma, Kansas, and Arkansas) that may serve populations near the Texas border. Using the addresses of all abortion clinics and law enforcement agencies, I acquire all of these institutions' geographical coordinates using a geocoding service created by Texas A&M University and calculate the straight-line distance from the geographical coordinates of the law enforcement agencies to those of the abortion clinics. Because Texas lacks major geological features that would cause driving distance to vary greatly from a straight-line distance, I implement the straight-line distance. Fischer et al. (2018) confirms that the straight-line distance serves as a good proxy for driving distance because their results are not sensitive to the use of either of these metrics. With the constructed distances, I create binary measures of access to abortions: no clinic within 25 miles, no clinic within 50 miles, and no clinic within 100 miles.

The closures resulting from Texas HB2 provide within law enforcement agency variation in abortion access overtime. The policy produces plausibly exogenous variation in access to abortions because individual clinics were unable to anticipate and adjust to the harsh restrictions of this statewide policy. Figure 2 shows how many law enforcement agencies in the sample do not have an abortion clinic within 25, 50, and 100 miles over the sample period. The graph shows a dramatic increase in the number of law enforcement agencies that do not have a clinic within these distances when the enforcement of HB2 began on November 1, 2013.

Supporting Data

I control for other county-by-year characteristics that change over time including

per capita income (Bureau of Economic Analysis, 2019), unemployment rate (Bureau of Labor Statistics, 2015), and population characteristics (SEER, 2016). Population characteristics include age, race, and gender composition of a county's population. More specifically, I control for the fractions of each county's population that are non-Hispanic white, non-Hispanic black, Hispanic, and other. I also include the fraction of females ages 15 to 44 in five-year groupings due to this being the demographic that is seeking abortions and are the most likely victims of domestic violence. I use a law enforcement agency's population covered (the number of people within a particular law enforcement agency's jurisdiction) as the exposure variable in Poisson regressions.

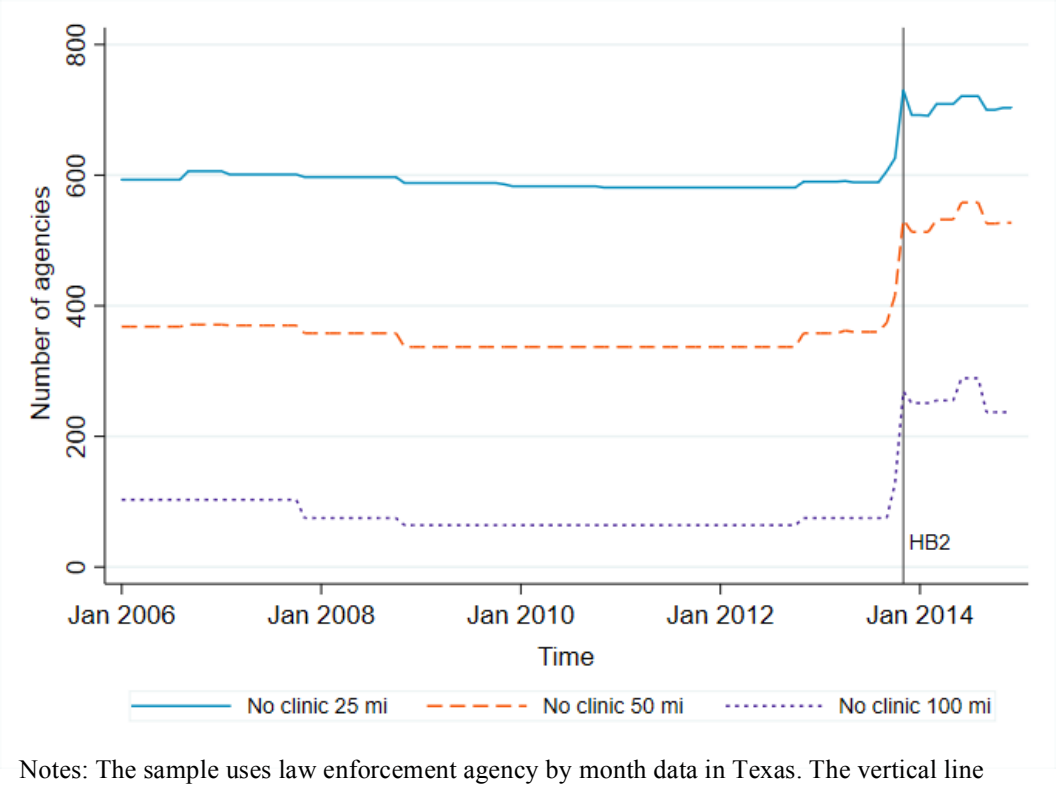
Summary statistics are reported in Table 2. The mean number of intimate partner victims reported by a law enforcement agency in a given month is 4.55 per 10,000 people while the mean number of all victims of family violence is 6.83 per 10,000 people. The average distance to the nearest abortion clinic nearly doubles after Texas enacts HB2, with the average distance to the nearest clinic being 21 miles in 2012 and increasing to nearly 42 miles in 2014. The population with no abortion clinic within 25 miles increases from 25% in 2012 to 37% in 2014, and 13% of the population has no clinic within 100 miles in 2014.

Table 1: Summary statistics of number of victims with different imputing rules

	Rule	Mean	Std. Dev.	Obs.
Family violence victims per 10,000 people	None	7.055	3.750	67,367
	2	6.924	3.836	89,658
	4	6.843	3.885	95,715
	6	6.828	3.894	96,506
	8	6.825	3.895	96,591
	10	6.822	3.897	96,639
Intimate partner victims per 10,000 people	None	4.702	2.801	67,367
	2	4.614	2.847	89,658
	4	4.561	2.873	95,715
	6	4.551	2.878	96,506
	8	4.549	2.879	96,591
	10	4.547	2.879	96,639

Notes: Zeros are imputed for an agency's missing months based on whether that agency's mean number of family violence victims is below a given rule. For example, if the mean number of family violence victims for an agency is less than 4 for each month between 2006 and 2014, that agency will have zeros for all missing months. None represents the sample that has no imputed zeros. Domestic violence victims includes victims classified under spouse, common-law spouse, and ex-spouse in Texas DPS Family Violence data. The summary statistics are weighted by each law enforcement agency's population covered.

Figure 2: The number of agencies without access to abortion clinics overtime



Notes: The sample uses law enforcement agency by month data in Texas. The vertical line represents the beginning of the enforcement of Texas House Bill 2, which was November 1, 2013.

Table 2: Summary statistics

Variable	Mean	Std. Dev.
Victims per 10,000 people		
All family violence	6.83	3.89
All intimate partners	4.55	2.88
Spouse	1.48	1.04
Ex-spouse	0.31	0.37
Common-law spouse	1.15	1.20
Other	1.61	1.65
Distance measures		
2012 distance to nearest clinic	21.17	28.94
2012 no clinic within 25 miles	0.25	0.43
2012 no clinic within 50 miles	0.13	0.34
2012 no clinic within 100 miles	0.04	0.19
2014 distance to nearest clinic	41.97	61.97
2014 no clinic within 25 miles	0.37	0.48
2014 no clinic within 50 miles	0.26	0.44
2014 no clinic within 100 miles	0.13	0.34
Economic controls		
Per capita income	40271.76	9981.07
Unemployment rate	6.27	1.85
Population characteristics		
Population covered	544738.60	702813.80
Proportion white non-Hispanic	0.46	0.20
Proportion black non-Hispanic	0.12	0.08
Proportion Hispanic	0.37	0.21
Proportion of other races/ethn.	0.05	0.03
Proportion of females 15 to 19	0.04	0.00
Proportion of females 20 to 24	0.04	0.01
Proportion of females 25 to 29	0.04	0.01
Proportion of females 30 to 34	0.04	0.01
Proportion of females 35 to 39	0.03	0.00
Proportion of females 40 to 44	0.03	0.00

Notes: There are 96,398 observations for the victim data from 895 law enforcement agencies. The summary statistics for victims and distance measures are weighted by each law enforcement agency's population covered. The distance measures are the straight-line distance from a law enforcement agency to the nearest abortion clinic and are summarized in the year before HB2 (2012) and the year after HB2 (2014). All economic controls and population characteristics except for population covered are averaged over county-years and are weighted by county population. Population covered is averaged over law enforcement agency-years and weighted by the population covered.

4. EMPIRICAL FRAMEWORK

The abortion clinic closures resulting from HB2 provide quasi-experimental variation in access to abortion services across law enforcement agencies over time. I identify the effects on domestic violence by comparing the reporting of domestic violence in agencies that have small changes in access to a clinic to those that have large changes in access to a clinic, controlling for changes in aggregate reporting over time, time-invariant agency characteristics, and time-varying population characteristics of counties in which the agencies reside. I estimate the following Fixed Effects Poisson model:

$$E[V_{amy}|Access_{amy}, \boldsymbol{X}_{cy}, \theta_a, \delta_{my}] = \exp(\beta Access_{amy} + \gamma \boldsymbol{X}_{cy} + \theta_a + \delta_{my})$$

The outcome variable V_{amy} is the number of victims of domestic violence reported by law enforcement agency a in month m and in year y. I estimate this model using a Fixed Effects Poisson estimator because the outcome variable is a count variable. I also use this estimator because of the ability to include fixed effects without the model suffering from inconsistency caused by the incidental parameters problem, a problem that exists in other non-linear models (Cameron & Trivedi, 2013).

The variable of interest $Access_{amy}$ is a binary measure of abortion clinic access, where abortion access is defined as one of the following: no clinic within 25 miles, no clinic within 50 miles, and no clinic within 100 miles. I use this binary measure of access to abortion services because the effect of distance is not likely to be linear. There is only one indicator for access to abortion in each regression. β can be interpreted as the percent change in the number of domestic violence victims within a law enforcement agency's district due to a change in access to abortion. For example, the coefficient estimate for a

regression that uses the 25-mile measure indicates that a law enforcement agency that no longer has a clinic within 25 miles reports a β percent change in victims. In this framework, the three different binary measures in access to abortion services allow the exploration of different degrees of treatment intensity.

To control for other confounding factors, I include $\boldsymbol{X}_{cy}$, a vector of characteristics in county *c* and in year *y* including unemployment rate, natural log of income per capita, and population characteristics. There are multiple law enforcement agencies *a* in a given county *c*. The population characteristics include proportions of the population by race and ethnicity (white non-Hispanic, black non-Hispanic, and Hispanic where other is the excluded group) and female proportions of the population from 15 to 44 in five-year groupings. θ_a represents law enforcement agency fixed effects, which account for time-invariant agency observed and unobserved characteristics. δ_{my} represents year-by-month fixed effects, which control for time-varying factors that impact the number of domestic violence victims similarly across all law enforcement agencies, such as aggregate seasonality of crime. I use law enforcement agencies' yearly population covered as the exposure variable in the main specification. The number of victims in an area is dependent on the potential number of victims (i.e. the population), and the exposure variable accounts for this in a Poisson model by including the log of the exposure variable as a regressor and constraining its coefficient to one. This will account for the large differences in population – and therefore the varying potential for victims – across law enforcement agencies. Standard errors are calculated to correct for potential clustering at the law enforcement agency level, addressing the concern that observations

within a law enforcement agency may be correlated. Calculating the standard errors in this way also solves the problem of overdispersion that is often present in Poisson models.

The identifying assumption is that without changes in access to abortion, the law enforcement agencies that experience a large change in access to abortion clinics would have similar changes in the number of reported victims to that of agencies that have small changes in access to clinics. In other words, the variation in access to abortion is uncorrelated with time-varying unobserved characteristics affecting the number of victims.[15]

With the inability to observe whether the relationship between access to abortion and the number of domestic violence victims detects a change in reporting or the true frequency of the violence, I also explore homicides as a dependent variable, where the rest of the model remains unchanged. As mentioned in the previous section, homicides are far less likely to go unreported compared to all other domestic violence crimes, providing a potential method to explore actual changes in this violence.

[15] The control group contains the areas that have no or relatively small changes in distance to the nearest abortion clinic, which includes cities where at least one clinic remains open. Though the distance may not change much for these residents, there are clinics in these areas that shut down due to HB2 and cause an increase in the number of patients at remaining clinics (Lindo et al., 2019). If this congestion causes the control group to also experience a decrease in access to abortion and results in similar changes in domestic violence to the treatment group, failing to account for congestion in the model will likely overstate the estimates of the effect of an increase in distance to an abortion clinic on domestic violence.

5. RESULTS

The results suggest weak evidence that a decrease in access to abortion causes a decrease in the number of intimate partner victims reported by a law enforcement agency. When stratifying the results by the four types of intimate partner relationships, dating partners appear to drive the main results. The estimates for spouses, ex-spouses, and common law spouses are too noisy to determine whether or not these groups are affected.

Table 3 displays estimates of the effect of access to abortion services on the number of all intimate partner victims. Recall that this includes victims categorized as spouse, common-law spouse, ex-spouse, and other, where other includes dating partners. Each estimate is from a different regression. Moving down each column, the measures of access to abortion are no clinic within 25, 50, and 100 miles. All columns include law enforcement agency fixed effects and year-by-month fixed effects; column 1 includes no controls, column 2 introduces the exposure variable, and column 3 (the preferred specification) introduces county-by-year controls. The coefficient with the 25-mile measure in the third column is -0.055 and indicates that a law enforcement agency that no longer has a clinic within 25 miles experiences a 5.5% decrease in the reported number of domestic violence victims. However, the estimate is not significant. The 50-mile and 100-mile measures are significant at the 10% level, indicating a 6.6% decrease and an 8.7% decrease in reported victims, respectively.

The preferred specification includes the county-by-year controls because they account for time-varying county characteristics that may explain changes in reported domestic violence and may also be correlated with access to abortion clinics. Assuming

the negative sign on the coefficient of interest is true, excluding unemployment could bias estimates towards zero because unemployment is potentially positively correlated with distance to abortion clinics and domestic violence. Excluding per capita income would also bias estimates towards zero because income is likely negatively correlated with distance to clinics, as urban areas typically have higher income, and it is also negatively correlated with domestic violence. Controlling for the fraction of females ages 15 to 44 in five year groupings may be important because changes in these populations overtime help explain the changes in violence in an area (i.e. more potential victims in the most vulnerable groups could be associated with more violence) and these fractions may also be correlated with distance to abortion clinics if certain age groups of women are more likely to live in urban versus rural areas. This same idea can be applied to race and ethnicity compositions.

Because only 14% of abortion patients are married, there is likely varying demand for abortions between different types of victim-offender relationships, indicating that an unintended pregnancy may have different implications for different types of relationships and analyzing these relationships separately is important to this analysis (Jerman et al., 2016). For example, individuals who are married already have a legal tie to their partner without a child, whereas dating partners do not have these same ties. Because of this, the additional tie a child creates for a partnership is much larger for those who are dating compared to those who are married – a victim who is dating her abusive partner and is considering leaving him may have a greater marginal cost of carrying a pregnancy to term compared to victims who are already in a legally binding relationship. Thus, dating

partners may have a higher demand for abortions and may experience more obvious effects when access to an abortion clinic is removed.

Table 4 reports estimates on the number of victims by the victim's specific relationship to offender, where columns 1 through 4 are spouse, common-law spouse, ex-spouse, and other.[16] For spouse, common-law spouse, and ex-spouse, the estimates have large standard errors and are not significant for any of the measures of access to abortion. However, the 100-mile estimate for victims classified as other is significant at the 10% level and indicates that when an agency moves from having at least one clinic within 100 miles to having no clinic within 100 miles, there is a 13.8% decrease in a law enforcement agency's reported number of victims in the other category. The coefficients for no clinic within 25 miles and no clinic within 50 miles in this column are not significant. The other category is likely comprised of mostly dating partners, so these results suggest that dating partners are affected by changes in access to abortion. Because most domestic violence occurs between dating partners and this group is also more likely to seek abortions than those in more committed relationships, it is reasonable to believe that this group would be more clearly affected by a change in access to abortion compared to the first three groups.[17] Figure 3 displays the 95% confidence intervals for all of the estimates in Table 4. The wide confidence intervals communicate how imprecise the estimates are. The results by relationship suggest that dating partners may

[16] The number of agencies and the total number of observations for these estimates differ across relationships and differ from those in Table 3 because the Poisson model drops any group that has all zero outcomes. Table A4 in Appendix A displays estimates using the exact same sample for each relationship, where I exclude any agency that has zero victims for spouse, ex-spouse, common-law spouse, or other in all months between 2006 and 2014. My estimates in Table 4 are not sensitive to these changes.

[17] In Appendix B, I explore the combination of common-law spouse victims and other victims because common-law spouses may have a similar legal tie to that of dating partners if couples who live together are accidentally categorized as common-law spouses.

be driving the main estimates, and the majority of the rest of this section focuses on dating partners by further analyzing the category of victims deemed other.

Due to the missing agencies in the original dataset being a primary flaw of the family violence data, I also conduct an analysis using only agencies that report in all months throughout the sample period in order to have a balanced panel. Table 5 shows the estimates for the effect of access to abortion services on the other category, where the first column uses the original sample that has imputed zeros, the second column uses only law enforcement agencies that report at least one victim of family violence in all months from 2006 to 2014, and the third column uses only law enforcement agencies that report at least one victim of family violence in all months from 2012 to 2014. The summary statistics of these samples are presented in Table A3 in Appendix A. Column three uses a narrowed sample period around the implementation of HB2. Because there were a few abortion clinic openings between 2006 and 2010 that cannot be considered exogenous, narrowing the sample period prevents the model from treating clinic openings in the same way as the closings. Comparing columns 2 and 3, narrowing the sample period increases the precision of the estimates despite decreasing the total number of observations in the sample. Using the balanced panels, the 25-mile and 50-mile estimates are negative and statistically significant in both columns 2 and 3. The first estimate in column 3 indicates that a law enforcement agency that no longer has a clinic within 25 miles reports 14.1% fewer family violence victims classified as other, and this coefficient is significant at the 5% level. In Appendix A, Table A5 presents estimates from a similar analysis where the outcome includes all intimate partner victims.

I also explore effects of access to abortion services by types of law enforcement agencies. Table 6 reports the effects of access to abortions on the number of victims considered other by three types of agencies. The first column uses local police departments, differing from the main sample by excluding county sheriffs' offices. The second and third column use rural and urban agencies, respectively. Unlike the previous analysis presented of the other category, all of the estimates presented here have large standard errors and are insignificant. In Appendix A, Table A6 replicates the estimates of Table 6 but uses all intimate partner victims.

It is ambiguous why the estimates are consistently negative because a decline in the number of reported victims could indicate either that the crime is occurring less often or that individuals are reporting the crime less often. It is a possibility that the violence decreases because without access to abortion, more women remain pregnant and cause perpetrators to refrain from physically assaulting their pregnant partners. In addition, bringing a child into the family may further encourage a perpetrator to stop their abusive behavior. However, this may not be likely because there is little evidence that perpetrators commit less abuse when their partner is pregnant (Bailey, 2010). Conversely, a decrease in reporting in the absence of a clinic could be a result of the absence of the information that abortion clinics provide. As mentioned previously, abortion clinics typically screen for domestic violence and then provide victims with resources if they want to seek help, such as counseling, shelter, or hotlines. It may be the case that the screening, resources, and advocacy that these clinics provide allow a victim to become aware of their situation, and therefore, they decide to report the crime; when these clinics

are taken away, reporting decreases and makes it appear as if the number of victims decreases.

To explore the mechanism for which the reported number of victims is decreasing with a decrease in access to abortion, I introduce homicides as an additional outcome variable. Ideally, homicides are more representative of any true changes in this crime because homicides are nearly perfectly reported. Table 7 shows the results of this analysis, where the first column uses all female homicides, the second column uses male-on-female homicides, and the third uses female spousal homicides. The estimates are also negative but are extremely imprecise; the upper-bound estimate for the 95% confidence interval on the 25-mile measure indicates a 23.3% increase in female homicides while the lower-bound indicates a 26.6% decrease. As the victim-offender relationship becomes more specific, the confidence interval grows wider. With insignificant estimates on all measures of access to abortion, these results cannot assist in determining the mechanism for which a decrease in access to abortion causes a decrease in a law enforcement agency's reported number of victims.

Overall, the results weakly suggest that a decrease in access to abortion results in a decrease in the reported number of dating partner victims. Though the estimates using the spousal relationships as outcome variables provide no insight into how abortion access affects domestic violence against these groups, the estimates using dating partners suggest that this group matters when analyzing policies that change access to abortion. More research is needed in order to determine why this group has fewer reported victims after a reduction in abortion access.

Table 3: Estimated effects of abortion access on number of domestic violence victims

	(1)	(2)	(3)
No clinic 25 mi	-0.038	-0.036	-0.055
	(0.033)	(0.035)	(0.035)
No clinic 50 mi	-0.039	-0.032	-0.066*
	(0.035)	(0.037)	(0.036)
No clinic 100 mi	-0.051	-0.053	-0.087*
	(0.041)	(0.043)	(0.047)
Agencies	895	895	895
N	96,398	96,398	96,398
Agency FE	X	X	X
Year-by-month FE	X	X	X
Exposure variable	-	X	X
County-by-year controls	-	-	X

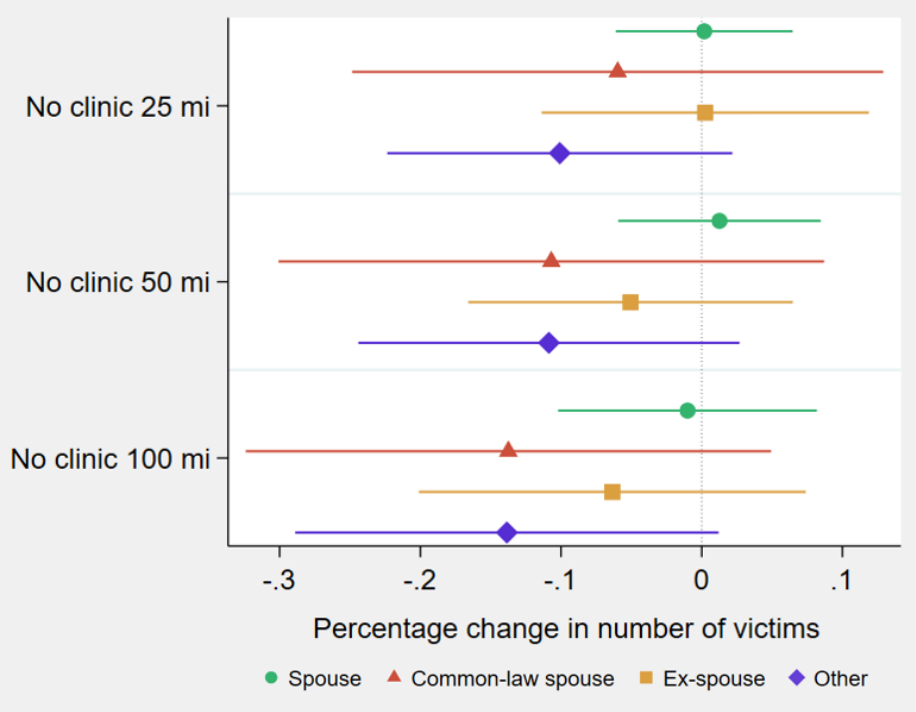
Notes: Each estimate is from a different regression based on a Fixed Effects Poisson model and measures the effect of access to abortion clinics on number of domestic violence victims, which includes the following relationship categories: spouse, common-law spouse, ex-spouse, and other, where other includes dating partners. The analysis uses Texas law enforcement agency-level data over all months between 2006 and 2014. All models have law enforcement agency fixed effects and year-by-month fixed effects. The exposure variable is population covered by a law enforcement agency. County-by-year level controls include: unemployment rate; log of per capita income; the faction of the population that is non-Hispanic white, non-Hispanic black, and Hispanic versus other race/ethnicity; and the fraction of the female population that is 15-44 in five year groupings. Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Estimated effects of number of victims by relationship type

	Spouse	Common-law spouse	Ex-spouse	Other
No clinic 25 mi	0.002	-0.060	0.003	-0.101
	(0.032)	(0.096)	(0.059)	(0.063)
No clinic 50 mi	0.013	-0.107	-0.051	-0.109
	(0.037)	(0.099)	(0.059)	(0.069)
No clinic 100 mi	-0.010	-0.137	-0.064	-0.138*
	(0.047)	(0.095)	(0.070)	(0.077)
Agencies	882	885	785	848
N	94,994	95,318	84,518	91,322

Notes: This table displays estimates by the victim's relationship to the offender. Each estimate is from a different regression based on a Fixed Effects Poisson model, where the regressions include law enforcement agency fixed effects, year-by-month fixed effects, an exposure variable (law enforcement agency population covered), and county-by-year controls. Because Texas considers dating violence a part of family violence but the family violence data does not have an explicit category for dating partners, the other category includes dating partner victims. The number of agencies is different for each relationship type because the Poisson model drops any group with all zero outcomes. Table A4 in Appendix A solves this problem by using a consistent sample across all relationships. Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figure 3: 95% confidence intervals of estimates by relationship type



Notes: The graph shows the 95% confidence intervals of estimates from Table 4. Each confidence interval represents an estimate from a different regression.

Table 5: Estimated effects on number of dating partner victims using balanced panels

	Main results	All months 2006-2014	All months 2012-2014
No clinic 25 mi	-0.101	-0.141*	-0.154**
	(0.063)	(0.084)	(0.067)
No clinic 50 mi	-0.109	-0.163*	-0.129*
	(0.069)	(0.096)	(0.067)
No clinic 100 mi	-0.138*	-0.141	-0.089
	(0.077)	(0.100)	(0.082)
Agencies	848	203	203
N	91,322	21,924	7,308

Notes: This table uses agencies that provide a balanced panel (columns 2 and 3). The agency is included in the sample must report at least one victim of family violence in all months between 2006 and 2012 (the agency has no imputed zeros). The last column narrows around the implementation of HB2 and the majority of the closures. The outcome variable is the number of victims in the category called other, which includes dating partners. Each estimate is from a different regression based on a Fixed Effects Poisson model. Each regression includes law enforcement agency fixed effects, year-by-month fixed effects, an exposure variable (law enforcement agency population covered), and county-by-year controls. Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Effects on number of dating partner victims by law enforcement agency type

	Local Police Departments	Rural	Urban
No clinic 25 mi	-0.089	-0.079	-0.091
	(0.075)	(0.059)	(0.075)
No clinic 50 mi	-0.102	-0.098	-0.103
	(0.083)	(0.079)	(0.083)
No clinic 100 mi	-0.145	-0.082	-0.147
	(0.091)	(0.080)	(0.092)
Agencies	610	348	500
N	65,719	37,483	53,839

Notes: The analysis separates law enforcement agencies by type, where law enforcement agency data is from U.S. Department of Justice's Law Enforcement Agency Crosswalk data. Rural areas include law enforcement agencies that are not considered part of a census place and urban areas include law enforcement agencies that are considered part of a census place. The outcome variable is the number of victims in the category called other, which includes dating partners. Each estimate is from a different regression based on a Fixed Effects Poisson model and follows the controls of the main specification (Table 3 column 3). Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Estimated effects of access to abortion on female homicides

	Female homicides	Male-on-female homicides	Female spousal homicides
No clinic 25 mi	-0.016	-0.040	-0.106
	(0.127)	(0.151)	(0.244)
No clinic 50 mi	-0.113	-0.054	-0.095
	(0.144)	(0.169)	(0.297)
No clinic 100 mi	-0.054	0.077	0.043
	(0.177)	(0.182)	(0.393)
Agencies	404	369	266
N	43,632	39,852	28,728

Notes: Each estimate is from a different regression based on a Fixed Effects Poisson model and measures the effect of access to abortion clinics on number of homicides. Female spousal homicides includes the victim being a spouse, common-law spouse, and ex-spouse. The analysis uses Supplementary Homicide data at the Texas law enforcement agency-by-month level from 2006 to 2014. The exposure variable is population covered by a law enforcement agency. County-by-year level controls are included. Each column has a different number of agencies because the Poisson model drops any group that has all zero outcomes. Standard errors are in parentheses and allow errors to be correlated within law enforcement agency overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

6. ROBUSTNESS CHECKS

This section describes robustness checks and their results to test the sensitivity of the estimates to changes in the model, where the number of victims classified as other is the outcome variable. Tables in Appendix A display results of the same sensitivity checks conducted in this section but uses all intimate partners as the outcome variable.

First, I follow a similar approach used in Lindo et al. (2019) by using multiple measures of access to abortion in one regression where a law enforcement agency either has a clinic within 50 to 100 miles, 100 to 150 miles, or a clinic more than 150 miles away (where having a clinic within 50 miles is the omitted group). Table 8 shows the results from this analysis, where the outcome variable is the number of victims in the other category and each column represents a different regression. Each regression includes law enforcement agency fixed effects and year-by-month fixed effects. Moving across the table from left to right, each regression adds more controls and the third column is most similar to the preferred specification, aside from having multiple measures for access to abortion clinics. The results in column 3 indicate that relative to having an abortion clinic within 50 miles, having the nearest abortion clinic 50 to 100 miles away results in a 5.3% decrease in reported victims, having the nearest abortion clinic 100 to 150 miles away results in a 12.4% decrease in reported victims, and having the nearest abortion clinic more than 150 miles away results in a 17.1% decrease in reported victims. However, only the last two coefficients are significant at the 10% level. These results further suggest that dating partners are affected with a decrease in access to abortion and help support the sign of the estimates from the main analysis.

Because the main specification relies on imputing zeros for missing agencies in the family violence data, I also conduct an analysis using different imputing rules to further explore whether the imputing is concerning. Table 9 displays estimates on the number of victims using different rules for imputing zeros, where columns 1 through 5 use two, four, six, eight, and ten as the different rules, respectively. Recall that the rule used for the main analysis is six. The estimates and their standard errors do not change as the rule for imputing zeros becomes less strict (moving further to the right on the table), so the results in my main specification are not sensitive to different imputing rules.

I also test the sensitivity of the estimates to the start of the sample period. As mentioned briefly in the previous section when presenting results using the balanced panel, there are a few clinics that open in between 2006 and 2010 before HB2 was passed. While the closings of the clinics are plausibly exogenous, these openings are not exogenous. By narrowing the sample period to after the openings occur, the model prevents the openings from being treated in the same way as the closings. Table 10 displays the estimates from this analysis where column 1, column 2, and column 3 display estimates from the full sample, 2010 to 2014, and 2012 to 2014, respectively.[18] Each estimate is from a different regression. With the sample period narrowed around the time when HB2 was passed and began being enforced in column 3, the precision increases despite losing over 60% of observations. The coefficient for the 25-mile estimate is negative and significant at the 10% level in the second column. The

[18] Because the Poisson model drops any group that has all zero outcomes, the number of agencies decreases as the time period is narrowed. To test the sensitivity of the estimates to the sample of agencies, I use the smallest sample of 764 agencies in column 3 of Table 10 and conduct the analysis for the entire sample period (2006 to 2014) and the 2010 to 2014 time period. The results for both of these time periods are not sensitive to the change in the sample. Results are available upon request.

coefficient for the 25-mile estimate in the third column is significant at the 5% level and indicates that no longer having a clinic within 25 miles results in a 12.9% decrease in a law enforcement agency's reported number of dating partner victims. The estimates for the 50-mile and 100-mile measures are not significant for both narrowed sample periods.

Each of these sensitivity checks are important in suggesting that dating partners are affected by changes in access to abortion services. Because the estimates in all of these analyses are negative and oftentimes significant, the evidence is suggesting that a decrease in access to abortion services causes either a decrease in violence against dating partners or a decrease in the reporting of this same violence. Further analysis is necessary in order to better understand this relationship and evaluate the policies that reduce access to these services.

Table 8: Estimated effects on dating partner victims with multiple distance bins

	(1)	(2)	(3)
Clinic 50 to 100 mi	-0.097	-0.077	-0.053
	(0.086)	(0.090)	(0.077)
Clinic 100 to 150 mi	-0.095	-0.095	-0.124*
	(0.088)	(0.091)	(0.070)
Clinic > 150 mi	-0.040	-0.028	-0.171*
	(0.115)	(0.123)	(0.104)
Agencies	848	848	848
N	91,322	91,322	91,322
Exposure variable	-	X	X
County-by-year controls	-	-	X

Notes: Each column represents a different regression based on a Fixed Effects Poisson model, following a similar approach to Lindo et al. (2019). The analysis uses Texas law enforcement agency-level data over all months between 2006 and 2014. All models have law enforcement agency fixed effects and year-by-month fixed effects and have the same exposure variable (law enforcement agency's population covered) and county-by-year level controls as the main specification. Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Effects on dating partner victims by different imputing rules

	2	4	6	8	10
No clinic 25 mi	-0.101	-0.101	-0.101	-0.101	-0.101
	(0.063)	(0.063)	(0.063)	(0.063)	(0.063)
No clinic 50 mi	-0.108	-0.109	-0.109	-0.109	-0.109
	(0.069)	(0.069)	(0.069)	(0.069)	(0.069)
No clinic 100 mi	-0.137*	-0.138*	-0.138*	-0.138*	-0.140*
	(0.077)	(0.077)	(0.077)	(0.077)	(0.077)
Agencies	848	848	848	848	848
N	85,103	90,744	91,322	91,407	91,455

Notes: Each estimate is from a different regression based on a Fixed Effects Poisson model and measures the effect of access to abortion on number of victims using different rules for imputing zeros in the data. Because a law enforcement agency only appears in the Texas DPS Family Violence data in a given month if they have at least one victim that period but it is unclear whether the agency failed to report that month or had zero victims of family violence, I impute zeros for agencies based on that agency's mean number of family violence victims. If an agency's mean number of all victims of family violence is less than a particular number (the rule indicated on the top of each column), I impute zeros for that agency in all missing months. The main specification uses six as the imputing rule. Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 10: Estimated effects on dating partner victims using smaller sample windows

	(1) Full sample	(2) 2010-2014	(3) 2012-2014
No clinic within 25 miles	-0.101	-0.088*	-0.129**
	(0.063)	(0.052)	(0.054)
No clinic within 50 miles	-0.109	-0.060	-0.082
	(0.069)	(0.055)	(0.054)
No clinic within 100 miles	-0.138*	-0.071	-0.057
	(0.077)	(0.069)	(0.064)
Agencies	848	816	764
N	91,322	48,764	27,368

Notes: Each estimate is from a different regression based on a Fixed Effects Poisson model and measures the effect of access to abortion clinics on number of victims in the other category, which includes dating partners. The full sample period uses Texas law enforcement agency-level data over all months between 2006 and 2014 and columns two and three narrow the sample period around Texas HB2, which was passed in 2013. Each regression includes law enforcement agency fixed effects, year-by-month fixed effects, an exposure variable (law enforcement agency population covered), and county-by-year controls. Each column has a different number of agencies because the Poisson model drops any group that has all zero outcomes. Standard errors are in parentheses and allow errors to be correlated within law enforcement agencies overtime. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

7. CONCLUSION

In this paper, I exploit variation in access to abortion resulting from Texas HB2 to estimate the effects on domestic violence – one of the largest health issues that women currently face. The results of the analysis suggest that a decrease in access to abortion causes a decrease in the reported number of intimate partner victims, but it is ambiguous whether this is due to a decrease in reporting or a decrease in the actual violence. The main specification indicates that a law enforcement agency that no longer has a clinic within 50 and 100 miles reports 6.6% and 8.7% fewer intimate partner victims, respectively, though the estimates are weakly significant. When I stratify the sample by the victim's relationship to their offender, I find that dating partner victims drive the main estimates, where a law enforcement agency that no longer has a clinic within 100 miles reports 13.8% fewer victims classified as other, which I argue is comprised mostly of dating partners. The estimates for spouses, common-law spouses, and ex-spouses have large standard errors, so we cannot conclude how the changes in abortion access affect these groups using this methodology. This analysis provides evidence that dating partners experience the most apparent effects from this policy. One reason for this could be that dating partners are affected more homogenously by changes in access to abortion compared to other types of relationships. Because dating partners are the most vulnerable to domestic violence and also the most likely to seek abortion services compared to other intimate partner relationships, the evidence further suggests that analysis on this particular group is vital as more research is conducted on this topic.

After an analysis of female homicides, it is unclear whether the decrease in the number of reported dating partner victims is due to a decrease in the true violence or a decrease in the reporting of the crime because the estimates using homicides as an outcome variable are imprecise. Because the mechanism is unknown, it is risky to use this study for any policy implications regarding access to abortion services. If the result is due to a decrease in the crime itself, HB2 and similar policies may be assisting victims in a way that is unanticipated. However, if the relationship that has been found is due to a decrease in reporting, this would suggest that abortion clinics and the information they provide about domestic violence are important to victims, and thus, policies that limit access to abortion could further harm female victims. Although nothing can be said about whether changes in access to abortion are helpful or hurtful for victims of domestic violence, the results do suggest that abortion and family planning clinics should screen for domestic violence because there are likely very few costs of doing so, while the benefits may be great.

It is often subjective whether some impacts of HB2 and similar policies are good, bad, or neutral, such as decrease in abortion rates and increase in birth rates. However, domestic violence is objectively harmful to victims, their health, and those around them, so this outcome as it relates to policies that limit access to abortion is a vital outcome to evaluate. This paper provides the first causal estimates on how access to abortion affects domestic violence, and although the mechanism is unclear, the analysis does provide evidence that there is an important link between these family planning resources and violence against dating partners. Domestic violence is a serious issue that policy makers

continually attempt to mitigate, and this topic begs further exploration of the existing mechanisms to better guide policy and gain a deeper understanding of the dynamics of abuse.

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APPENDICES

APPENDIX A

ADDITIONAL TABLES

Table A1: Fraction of victims of family violence by relationship

	Mean	Std. Dev.
Spouse	0.229	0.139
Common-law spouse	0.168	0.148
Ex-spouse	0.043	0.056
Parent	0.081	0.081
Sibling	0.066	0.069
Child	0.056	0.067
Grandparent	0.006	0.024
Grandchild	0.004	0.017
In-law	0.021	0.038
Step-parent	0.011	0.029
Step-child	0.016	0.037
Step-sibling	0.003	0.015
Roommate	0.083	0.123
Foster parent	0.001	0.007
Foster child	<0.001	0.005
Other relationship	0.213	0.165
Unknown relationship	<0.001	<0.001
N	67367	

Notes: This table displays the fraction of victims in all relationship types included in Texas DPS Family Violence data, which are at the law enforcement agency by month level. Other includes dating partners. The statistics are weighted by law enforcement agencies' population covered.

Table A2: Number of agencies reporting in a given number of months each year

Months in a year	Number of agencies	Percent of all agencies
> 6 months	497	.555
> 9 months	381	.425
All 12 months	203	.227

Notes: The table shows the number of agencies appearing in the Texas family violence data in at least 6 months in all years between 2006 and 2014. The total number of agencies in the sample is 896 and includes local police departments and county sheriff's offices only. The average number of months in a year that an agency appears in the family violence data is 8.354 months out of 12.

Table A3: Summary statistics of agencies reporting at least one victim in all months

Variable	Mean	Std. Dev.
Victims per 10,000 people		
All family violence	7.57	3.69
All intimate partners	4.97	2.80
Spouse	1.59	0.93
Ex-spouse	0.35	0.31
Common-law spouse	1.29	1.18
Other	1.73	1.61
Distance measures		
2012 distance to nearest clinic	18.65	26.88
2012 no clinic within 25 miles	0.19	0.39
2012 no clinic within 50 miles	0.10	0.30
2012 no clinic within 100 miles	0.04	0.19
2014 distance to nearest clinic	41.33	62.84
2014 no clinic within 25 miles	0.34	0.47
2014 no clinic within 50 miles	0.23	0.42
2014 no clinic within 100 miles	0.13	0.34
Economic controls		
Per capita income	38191.26	10392.68
Unemployment rate	6.38	1.91
Population characteristics		
Population covered	608695.80	746975.60
Proportion white non-Hispanic	0.47	0.21
Proportion black non-Hispanic	0.10	0.07
Proportion Hispanic	0.38	0.25
Proportion of other races/ethn.	0.04	0.04
Proportion of females 15 to 19	0.04	0.00
Proportion of females 20 to 24	0.03	0.01
Proportion of females 25 to 29	0.04	0.00
Proportion of females 30 to 34	0.03	0.00
Proportion of females 35 to 39	0.04	0.00
Proportion of females 40 to 44	0.04	0.00

Notes: This table shows the summary statistics of the 203 law enforcement agencies that report at least one victim of family violence in all months between 2006 and 2014, resulting in 21,924 observations. The summary statistics for victims and distance measures are weighted by each law enforcement agency's population covered. The distance measures are the straight-line distance from a law enforcement agency to the nearest abortion clinic and are summarized in the year before HB2 (2012) and the year after HB2 (2014). All economic controls and population characteristics except for population covered are averaged over county-years and are weighted by county population. Population covered is averaged over law enforcement agency-years and weighted by the population covered.

Table A4: Estimated effects by relationship using a consistent sample

	Spouse	Common-law spouse	Ex-spouse	Other	All
No clinic 25 mi	0.001	-0.061	0.003	-0.101	-0.055
	(0.032)	(0.097)	(0.059)	(0.063)	(0.035)
No clinic 50 mi	0.011	-0.107	-0.050	-0.108	-0.066*
	(0.037)	(0.099)	(0.059)	(0.069)	(0.036)
No clinic 100 mi	-0.011	-0.136	-0.064	-0.139*	-0.088*
	(0.047)	(0.096)	(0.070)	(0.077)	(0.047)
Agencies	771	771	771	771	771
N	83,006	83,006	83,006	83,006	83,006

Notes: This table replicates Table 4, but because the Poisson model drops any law enforcement agency that has all zero outcomes causing the samples across the relationships in Table 5 to differ, this table uses the exact same sample for each relationship. I exclude any agency that has zero victims in all months categorized as spouse, ex-spouse, common-law spouse, or other, so the Poisson model does not drop any agency. The last column combines the number of victims for all categories with this same sample, and these estimates can be compared to the estimates in column 3 of Table 3. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A5: Estimated effects on all intimate partner victims using a balanced panel

	Main results	All months 2006-2014	All months 2012-2014
No clinic 25 mi	-0.055	-0.067*	-0.077**
	(0.035)	(0.039)	(0.034)
No clinic 50 mi	-0.066*	-0.079*	-0.060*
	(0.036)	(0.043)	(0.035)
No clinic 100 mi	-0.087*	-0.083	-0.033
	(0.047)	(0.055)	(0.043)
Agencies	895	203	203
N	96,398	21,924	7,308

Notes: This table replicates Table 5, but the outcome variable is all intimate partners including dating partners. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A6: Estimated effects on all intimate partner victims by agency type

	Local Police Departments	Rural	Urban
No clinic 25 mi	-0.045	-0.082	-0.047
	(0.040)	(0.051)	(0.040)
No clinic 50 mi	-0.062	-0.074	-0.063
	(0.040)	(0.058)	(0.040)
No clinic 100 mi	-0.093*	-0.045	-0.096*
	(0.055)	(0.056)	(0.056)
Agencies	641	387	508
N	69,067	41,695	54,703

Notes: This table replicates Table 6, but the outcome variable is all intimate partner victims including dating partners rather than dating partners. . *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A7: Estimated effects on all victims with multiple distance bins

	(1)	(2)	(3)
Clinic 50 to 100 mi	-0.014	0.002	-0.025
	(0.041)	(0.043)	(0.039)
Clinic 100 to 150 mi	-0.070*	-0.071*	-0.069*
	(0.039)	(0.040)	(0.039)
Clinic > 150 mi	-0.033	-0.030	-0.122*
	(0.068)	(0.074)	(0.074)
Agencies	895	895	895
N	96,398	96,398	96,398
Exposure variable	-	X	X
County-by-year controls	-	-	X

Notes: This table replicates Table 8, but the outcome variable is the number of all intimate partner victims including dating partners. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A8: Estimated effects on all victims by different imputing rules

	2	4	6	8	10
No clinic 25 mi	-0.054	-0.054	-0.055	-0.055	-0.055
	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
No clinic 50 mi	-0.066*	-0.066*	-0.066*	-0.066*	-0.066*
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
No clinic 100 mi	-0.085*	-0.086*	-0.087*	-0.087*	-0.088*
	(0.046)	(0.047)	(0.047)	(0.047)	(0.047)
Agencies	892	894	895	895	895
N	89,547	95,606	96,398	96,483	96,531

Notes: This table replicates Table 9, but the outcome variable is all intimate partners including dating partners. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A9: Effects on all intimate partner victims using smaller sample windows

	(1) Full sample	(2) 2010-2014	(3) 2012-2014
No clinic within 25 miles	-0.055	-0.072**	-0.062**
	(0.035)	(0.034)	(0.029)
No clinic within 50 miles	-0.066*	-0.058*	-0.044
	(0.036)	(0.034)	(0.028)
No clinic within 100 miles	-0.087*	-0.057	-0.037
	(0.047)	(0.044)	(0.034)
Agencies	895	880	855
N	96,398	52,604	30,644

Notes: This table replicates Table 10, but the outcome variable is all intimate partners including dating partners. The number of agencies decrease as the time period becomes smaller because the Poisson model drops any agency that has all zero outcomes. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

APPENDIX B

PARTNERS WITH NO LEGAL TIE

Because having a legal tie may be correlated with reactions to a change in access to abortion, I also use the sum of victims categorized as common-law spouse and victims categorized as other as another outcome variable. Common-law marriages in Texas require that 1) a couple has agreed to be married and live together as husband and wife and 2) the couple presents themselves as married to others (Texas Family Code Sec 2.401). This type of partnership is commonly conflated with partners who live together, and it is possible that victims living with their perpetrator are included in the common-law category of the family violence data because of this misunderstanding. For this reason, it is reasonable to aggregate these victims into one category and analyze this outcome.

Table B1 displays results using common-law spouse victims, other victims (which includes dating partners), and common-law spouse plus other victims in columns 1, 2, and 3, respectively. Because the Poisson model drops any group with all zero outcomes, I excluded any agency that has zero common-law spouse victims or zero other victims in all months in order to have the same sample across all regressions in this table (this is why the number of agencies differ slightly than the number of agencies in Table 4 and Table A4). The results in Table B1 show that combining the victims allows the estimates to be more precise, where each measurement for access to abortion is significant at the 5% level. This indicates that victims with less of a legal tie to their perpetrator experience more obvious effects with a reduction in access to abortion services.

Table B1: Estimated effects on dating partner and common-law spouse victims

	Common-law spouse	Other	Common-law spouse + other
No clinic 25 mi	-0.061	-0.101	-0.087**
	(0.096)	(0.063)	(0.042)
No clinic 50 mi	-0.108	-0.109	-0.107**
	(0.099)	(0.069)	(0.042)
No clinic 100 mi	-0.138	-0.138*	-0.131**
	(0.095)	(0.077)	(0.051)
Agencies	845	845	845
N	90,998	90,998	90,998

Notes: The category called other includes dating partner victims. In this sample, I ensure the Poisson model does not drop any agency by excluding any agency that has all zero outcomes for any of the given groups. This is why the sample sizes are different from those in Table 4. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.