NBER WORKING PAPER SERIES

AFTERSHOCKS: THE IMPACT OF CLINIC VIOLENCE ON ABORTION SERVICES

Mireille Jacobson
Heather Royer

Working Paper 16603
http://www.nber.org/papers/w16603

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2010

We thank Doug Almond, Kelly Bedard, Tom Chang, Damon Clark, David Clingingsmith, John DiNardo, Claudia Goldin, David A. Grimes, Bob Kaestner, Peter Kuhn, Phil Levine, Stephen L. Ross, Justin Sydnor, Linda Waite, Madeline Zavodny and various seminar attendees for many helpful comments. We also thank the Robert Wood Johnson Foundation for financial support through the Health Policy Scholars program during this project's infancy. Taylor Bishop, Ezra Golberstein, Feng Pan, Sarada Pyda, Stephen Roll, George Tam and Andrew Zhang provided excellent research assistance. Many people graciously shared the data that made this project possible. Brian Heidt, from the Bureau of Alcohol, Tobacco and Firearms made available and explained much of the data on clinic violence, Sharon Lau, at the National Abortion Federation, provided details on the data available on the NAF website, Ted Joyce shared his coding of parental consent and waiting period requirements, Becky Blank shared her data on Medicaid funding bans as well as other legislative barriers, Marianne Bitler and Madeline Zavodny shared their data on the timing of abortions, and Laurence Finer and Stanley K. Henshaw, from the Alan Guttmacher Institute, helped us access and understand the Institute's census of abortions and abortion providers. All mistakes are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Aftershocks: The Impact of Clinic Violence on Abortion Services
Mireille Jacobson and Heather Royer
NBER Working Paper No. 16603
December 2010
JEL No. D74,I18,J13

ABSTRACT

Between 1973 and 2003, abortion providers in the United States were the targets of over 300 acts of extreme violence. Using unique data on attacks and on abortions, abortion providers, and births, we examine how anti-abortion violence has affected providers' decisions to perform abortions and women's decisions about whether and where to terminate a pregnancy. We find that clinic violence reduces abortion services in targeted areas. Once travel is taken into account, however, the overall effect of the violence is much smaller.

Mireille Jacobson
RAND
1776 Main Street
Santa Monica, CA 90407-2138
and NBER
mjacobso@rand.org

Heather Royer
Department of Economics
University of California, Santa Barbara
2127 North Hall
Santa Barbara, CA 93106
and NBER
royer@econ.ucsb.edu

The May 2009 murder of Dr. George Tiller, the sole abortion provider in Wichita, Kansas, has brought attention back to the extreme elements of the anti-abortion movement. His murder adds to a thirty-year history of violence. Between 1973 and 2003, anti-abortion activists carried out over 300 attacks (arsons, bombings, and butyric acid attacks at abortion facilities and the murder of abortion providers) on abortion clinics in the United States.[1,2] The frequency of these attacks makes abortion clinic violence one of the most common form of domestic terrorism in this country.[3] Anti-abortion violence was declining in more recent years. But Tiller's murder and recent reports of increased harassment at reproductive health clinics have raised fears of a return to more violent times.[4]

In this paper, we examine how anti-abortion violence has affected providers' decisions to perform abortions and women's decisions about whether and where to terminate a pregnancy. We study the impact of abortion clinic violence with two motivations in mind. First, we are interested in the direct effect of violence on the abortion market. The rise of anti-abortion violence in the 1980s and 1990s coincides with a sustained decline in both abortions and abortion providers (see Appendix Figures 1 and 2). While some speculate that these phenomena are related (e.g., see Stanley K. Henshaw and Lawrence B. Finer (2003)), the evidence is based largely on descriptive data. In contrast, we exploit variation in the timing of the acts of violence to identify their effects. Second, and more broadly, we use the violence to assess the success of terrorism. Evidence on how well terrorist attacks achieve their intended goals is relatively thin.[5] Anti-abortion violence provides a useful case because the goals of these attacks are clear – to eliminate the market for abortions.

Anti-abortion violence has caused considerable property damage and some loss of life, increased the need for security at abortion clinics and increased the fear and stress of workers at abortion facilities and of women seeking abortion services.[6] For instance, in 1993, a period of

---

[1] This classification is consistent with the National Abortion Federation, the organization of abortion providers in the United States. See http://www.prochoice.org/about_abortion/violence/index.html.

[2] Butyric acid is a relatively new medium for abortion clinic violence. It has a putrid smell and can cause some respiratory harm if inhaled.

[3] The FBI considers anti-abortion violence domestic terrorism. See, for example, page 19 of this document; http://www.fbi.gov/publications/terror/terroris.pdf.

[4] See http://www.rhrealitycheck.org/blog/2009/03/02/under-a-prochoice-president-clinics-ready-uptick-violence.

[5] David A. Jaeger and Daniele M. Paserman (2008), Gary S. Becker and Yona Rubinstein (2010) and Eric D. Gould and Esteban F. Klor (2009) are among the few studies in economics that address this issue.

[6] According to statistics from the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF), median damages were over $10,000 and as much as $2.4 million per incident (in 2000 dollars). Damage estimates are not reported

heightened anti-abortion activity, 50 percent of clinics reported being the targets of violence and harassment (Feminist Majority 2006).[7] Such violence weighs heavily on abortion providers. More recently, in 2005, a low violence year, 23 percent of facilities reported that at least one of their staff members had resigned in that year due to anti-abortion violence, harassment or intimidation.[8] In the 1990's, this violence prompted Bill Clinton to sign the Freedom of Access to Clinic Entrances (FACE) Act in 1994, making it a federal crime to prevent access to reproductive health care.

While this evidence is highly suggestive, it is also largely anecdotal. More systematic evidence on the effects of violence is sparse despite the size of the market for abortions and the considerable physical and psychological damage caused by anti-abortion extremists. The few in-depth studies of anti-abortion activities (Alesha E. Doan 2007; Catherine Cozzarelli and Brenda Major 1994) concentrate on the influence of picketing and protests as opposed to the more discrete and extreme events that are our focus. Moreover, they suffer from the usual endogeneity problems inherent to cross-sectional studies – leaving open the question of whether and how abortion clinic violence affects the market for abortions. Understanding the effects of violence on this market is important given its size and breadth. According to current statistics, 1 out of every 3 women in the United States will seek an abortion in her lifetime and 1 pregnancy is terminated for every 4 births.[9] Moreover, a diverse population utilizes this market: in 2000, the percent of pregnancies ending in abortion was 27 for women with a high school degree or less, 38 for women with some college and 21 for women with a college degree (Rachel K. Jones et al. 2002).

To our knowledge, this work is the first to systematically characterize the effect of abortion-related terrorism on provider's decisions to offer abortions and individual women's decisions about whether and where to terminate their pregnancies. To quantify the effect of abortion clinic violence on health care providers' decisions to offer abortions and women's decisions about whether to terminate a pregnancy, we combine detailed violence data with pre-existing county-level abortion and birth data. We compare within-county provider rates, abortion rates, and births before and after an act of extreme violence. We also investigate whether and

---

for all incidents. See Kevin M. Fitzpatrick and Michele Wilson (1999) on the psychological effects of violence on abortion clinic workers.

[7]Severe violence here includes blockades, invasions, arsons, bombings, chemical attacks, stalking, gunfire, physical assaults, and threats of death, bomb, or arson.

[8] See http://www.feminist.org/research/cvsurveys/clinic_survey2005.pdf.

[9] See http://www.guttmacher.org/in-the-know/index.html.

how the violence impacts abortion services in counties adjacent to but not directly targeted by an attack.

This collection of data enables us to paint a clear picture of the effect of clinic violence on the market for abortions. We find that in targeted areas, abortion violence modestly reduces the availability of providers by 6-9 percent and leads to declines in abortion rates of 8-9 percent. Clinic-based providers experience the bulk of the impact; non-hospital-based abortion and provider rates drop by 10-14 percent. The response varies by type of attack – e.g., damage-related acts (typically arson) versus murders of abortion providers. In areas where a murder has occurred, we observe declines in abortions and providers nearly 10 times the size of the average effect, albeit imprecisely measured due to the infrequency of murders. Because murder generates substantially less property damage than arson and bombing, one might infer that the effects of violence we measure are not purely mechanical (i.e., effects due to clinic closings).[10] Additionally, the effect size is not correlated with the dollar value of damage, suggesting that our estimates capture some behavioral response, rather than an incapacitation response, to terror. The reductions in abortions and providers persist for several years after an attack.

An analysis limited to abortion and provider rates in *affected counties* captures only part of the picture as providers may relocate their services and women may travel elsewhere to obtain abortions. Indeed our analysis of births suggests substantial displacement of abortions. In the 7-11 months following an anti-abortion attack, births increase by about 1 percent among women residing in targeted areas. In the long-run births are unaffected. The short-run rise in births accounts for only 10 percent of the decline in abortions in affected areas. We look more explicitly for displacement effects by analyzing the abortion market in counties neighboring the violence. Abortion and provider rates rise in counties nearest to the violence. Although the rate of abortion offset is estimated at about two-thirds, statistically we cannot reject a full offset. Together with the birth analysis, these findings indicate that the primary effect of anti-abortion violence is a change in the location of abortions. In other words, consistent with much of the literature on local abortion policies, women respond to abortion restrictions but not necessarily in

[10] In response to the recent killing of Tiller, his family announced that they were permanently closing his clinic. However, none of the prior murders led to permanent clinic closures, perhaps because they were not private practices and employed several physicians.

the way intended.[11] Demand for abortions is quite inelastic. Instead of reducing abortions, barriers – both formal and informal – prompt women to travel to less abortion-restrictive areas when possible.

The credibility of these estimates relies on the assumption that the timing of these acts of violence is random. To maximize the impact of the violence, perpetrators may want to surprise providers, leaving them unprepared to prevent or avoid an attack. However, we take potential threats to identification seriously and offer several pieces of evidence that bias may be minimal.

First, our results are robust to including a range of different controls – state abortion policy variables, economic and demographic covariates, state-by-year fixed effects, and county linear trends. Second, the estimates are similar, albeit slightly smaller, using different sets of control counties, including only counties that ever experienced violence or counties matched based on the propensity to experience violence. Third, event study graphs reveal that the timing of the changes in abortions and births is consistent with the timing of attacks. This is particularly evident for the monthly birth data, which show a sharp and pronounced rise following an attack. The close correspondence between the timing of changes in outcomes and attacks rules out the possibility that smoothly-evolving anti-abortion sentiments drive our results. And the robustness of our results to county-specific trends further rules out a role for differential, smooth local trends in sentiments in explaining our results. Fourth, and further suggesting the violence is unlikely the product of changes in local anti-abortion sentiment or activity, we observe no systematic run-up in local abortion-related news coverage prior to the violence. Furthermore, controlling for a county's Democratic vote share in the most recent prior presidential election has little effect on our estimates. Any remaining correlation between violence and unobserved determinants of abortion may bias our estimates towards zero, since the qualitative evidence suggests that the violence is often a response to pro-abortion activities (Faye Ginsburg 1998). In other words, anti-abortion activists may resort to violence in an effort to undo pro-abortion activities, as evidenced by the recent increase in violence.[12]

---

[11] A sample of these studies include Marianne Bitler and Madeleine Zavodny 2001; Rebecca Blank et al. 1996; Janet Currie et al. 1997; Theodore Joyce et al. 1997; Joyce et al. 2001; Joyce et al. 2006; Thomas J. Kane and Douglas Staiger 1996; William A. Pridemore and Joshua D Frielich 2007; James L. Rogers et al. 1991.

[12] Recent reports of an uptick in harassment at abortion clinics following the election of Barack H. Obama, a firmly pro-choice candidate, further corroborate this view. See, for example the June 10th, 2009 episode of The Rachel Maddow Show: http://www.msnbc.msn.com/id/26315908/#31177077 as well as numerous local news reports such as http://www.whnt.com/news/sns-ap-al--tillershooting-alaclinic,0,4717850.story. In addition, Cox proportional

Our work contributes to the burgeoning literature on the economics of terrorism, which has focused largely on understanding the characteristics of terrorists (Eli Berman and David A. Laitin 2008; Alan B. Krueger 2007; Krueger and Kitka Maleckova 2003) and the aggregate effects of terrorism on specific industries or on the economy as a whole (Alberto Abadie 2006; Abadie and Javier Gardeazabal 2003; Claude Berrebi and Klor 2006; Zvi Eckstein and Daniel Tsiddon 2004; Peter Eisinger 2004; Edward L. Glaeser and Jesse Shapiro 2002; Robert T. Greenbaum and Andy Hultquist 2006). Comparatively little work assesses the impact of terrorism on individual decisions or opinions. Moreover, because the intentions of terrorists are often vague, as in the case of September 11th, we know remarkably little about the "success" of these acts.[13]

Our findings imply that anti-abortion terrorists are ultimately unsuccessful in obstructing the market for abortions services. But the terrorism does impose a cost. Where terrorists have effectively reduced abortion services or instilled significant fear and where alternative abortion facilities exist, many women respond by traveling elsewhere to terminate their pregnancies. This finding is consistent with Becker and Rubinstein (2010), who study bus-riding behavior in Israel following suicide-bombings. Becker and Rubinstein demonstrate that individuals with relatively inelastic demand, as measured by the frequency of their bus-riding, do not alter their behavior in the long-run. The terrorism does affect the mode of transportation as some users substitute taxis for buses. Together, these results suggest that terrorist attacks on markets for an inelastically-demanded category of goods (e.g., abortions) for which there are close substitutes (e.g., an abortion at facility X versus an abortion at facility Y), will have only limited effects on consumption.

I. Background on Abortion Clinic Violence

The Development and Dispersion of the Abortion Clinic Violence Movement

Over the last 35 years, nearly 14 percent of counties with abortion providers have

---

hazard models indicate that the timing of a local clinic attack is predicted by greater support for abortion services in the region (based on measures from the General Social Survey).

[13] An important exception is Gould and Klor (2009), which assesses the effects of Palestinian terrorist attacks on Israeli political opinions. They find that individuals in targeted areas become more supportive of making territorial concessions to Palestinians and are more likely to support Palestinian statehood. They are also more likely to identify as right-wing, although this party has become increasingly liberal over time, a finding the authors attribute to terrorist attacks.

experienced some form of extreme violence defined as an arson attack, bombing, bomb threat, hoax device, murder, attempted murder, butyric acid attack, or vandalism. We discuss the source of these violence data in the subsequent data section. We detail the history of these acts in Table 1.

The violence began in March 1976 with an arson attack of Planned Parenthood in Eugene, Oregon. Thirteen other attacks occurred in the late 1970s, causing several million dollars worth of damage. Individuals acting alone largely committed these early attacks. The organization and frequency of attacks increased considerably in the 1980s (see Figure 1, the time series pattern of acts). Abortion clinic violence peaked in 1984, 1992, and 1998. Current data since the inauguration of Obama indicates that such violence may be on the rise. The National Abortion Federation reports that anti-abortion activity is at highest level in the past decade (Reuters, 2009).

The growth in violence in the 1980s observed in Figure 1 coincides with the development of organized anti-abortion groups. Many scholars posit that extremist anti-abortion groups developed out of frustration over the failure to overturn *Roe* (Ginsburg 1998). Among the first groups dedicated to disrupting the market for abortions was the Pro-Life Action League, which started in Chicago in 1980. Joe Scheidler, the head of the League, advocated a range of disruptive activities, including "sidewalk counseling," sit-ins, jamming sewer lines, and blowing up water meters. His 1985 book, "Closed: 99 Ways to Stop Abortion," sought to disseminate these tactics nationwide.

In 1988, Operation Rescue, another "direct-action" pro-life group, began under the leadership of Randall Terry, a Scheidler "trainee." Relative to Scheidler, Terry advocated a more confrontational form of protest – including clinic blockades and invasions (Ginsburg 1998). Although Operation Rescue never formally endorsed acts of violence, the group made a militant call to action with its slogan: "If you think abortion is murder, act like it." It organized several highly-publicized, large-scale, multi-week clinic protests and blockades, the first of which, the "Siege of Atlanta," coincided with the 1988 Democratic National Convention in Atlanta. In 1991 in Wichita, Kansas, the group initiated the "Summer of Mercy," which blockaded 3 abortion clinics for 6 weeks.

In contrast to the Pro-Life Action League and Operation Rescue, the third main organization responsible for many anti-abortion acts, the Army of God (AOG) explicitly advocates violence as a means to end abortion. The AOG is an informal network of anti-abortion extremists that first received public mention in 1982. The group produces a manual that, according to the National Abortion Federation (NAF), is "a 'how to' for abortion clinic violence," detailing methods for butyric acid attacks, arson, bomb-making, and similar activities.[14] An occasional AOG spokesman, Neal Horsely, hosted the infamous "Nuremberg Files" website, which had a wanted-style list of over 200 abortion providers showing their photographs and listing their addresses, license plate numbers, and family members. This website was finally removed from the internet in 2002 as a result of lawsuits from Planned Parenthood and targeted doctors.

Appendix Figure 3 displays the spatial distribution of the violence (the red dots) overlaid on a map of the 1981 distribution of abortion providers by county before the first national wave of extreme violence. There is substantial geographic dispersion in clinic attacks, suggesting that anti-abortion extremism occurs well beyond the boundaries of any organization's headquarters.[15]

Figure 2 looks further at the intensity of attacks across counties. Most (of the 1043) counties with abortion providers have never experienced an act of violence. Among the roughly 14 percent of counties experiencing any violence, over half experienced one event and nearly another quarter suffered two to three attacks. At the other extreme, one county – Harris County in Texas, which contains the city of Houston – experienced 10 acts of violence. Not surprisingly, anti-abortion activists targeted counties with multiple providers across the country.

Parallel to this rise in violence, we observe a decline in the number of abortion providers and abortions per capita (Appendix Figures 1 & 2). In the early 1980's and late 1970's, abortion and provider rates were at their highest levels and have been declining since. These decreases are large; abortion rates earlier in this decade were down over 25 percent compared to their peak whereas provider rates experienced a larger than 40 percent fall. Whether the violence caused the fall is ultimately an empirical question.

Prior Literature on the Effects of Extreme Anti-Abortion Activity

Despite the frequency and salience of anti-abortion extremism, little is known about its

[14] See http://www.prochoice.org/about_abortion/violence/army_god.html

[15] Well-known anti-abortion extremist Rachelle "Shelley" Shannon plead guilty to setting fires at several abortion clinics in Oregon, California, Idaho and Nevada. She was also convicted of attempted murder in the shooting of Tiller at his Wichita, KS clinic in 1993.

effects on the decisions of pregnant women or abortion providers. Some qualitative evidence suggests that anti-abortion violence has met its objective. Henshaw and Finer (2003) speculate that a primary source of the recent decline in abortion providers in the United States is clinic violence, particularly the murder of abortion doctors. However, this claim remains to be quantitatively evaluated. The surveys cited in the introduction corroborate the view that the effects of anti-abortion violence are widespread. And, as the recent killing of Tiller made clear, a relative lull in extreme violence does not mean the threat to providers has disappeared.

The prior literature on violence is of limited use for understanding the impact of extreme violence for two reasons. First, much of the existing literature on anti-abortion activities looks at the effect of clinic picketing rather than more extreme events. For example, Doan (2007) studies the cross-sectional impact of picketing and bomb threats on abortion rates and the number of providers. She finds only weak evidence of a relationship between protests and abortion rates but stronger evidence of protest-related and bomb-threat-related declines in providers. In a more qualitative study, Cozzarelli and Major (1994) look at the response of women approached by pro-life protesters at a Buffalo, New York abortion clinic. Although many of the 300 women approached by protesters reported being upset, none changed their decision to obtain an abortion. Second, while these studies suggest that acts of violence may have affected the market for abortion services, the cross-sectional nature of this work makes the results difficult to interpret. As shown later, the counties experiencing violence differ along many dimensions compared to the counties not experiencing such violence.

II. Data and Some Descriptive Statistics

Abortion Clinic Violence Data

The bulk of our data on clinic attacks comes from the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) records. For all attacks involving arson, bombings or firearms, the ATF is required to investigate. We cross-validate and supplement these data with reports from several independent sources, most importantly the National Abortion Federation, which provides information on butyric acid attacks (see Data Appendix)**.**

Our data on extreme violence exclude protests. Although protests likely affect the market for abortions – imposing potentially large psychic costs on women contemplating abortions, they occur more regularly than the violence and are more difficult to measure. Protests tend to neither receive extensive media attention nor precipitate federal investigation or other systematic monitoring. Protests are largely anticipated by clinics. Clinics adopt regular policies to deal with picketing – such as using volunteers to shepherd clients safely past demonstrators – but do not typically maintain good measures of these activities.[16] Our hope is that county fixed effects and time fixed effects control adequately for protests. Any variation not captured by these effects, however, may be important confounders. In robustness checks, we control explicitly for the three major protests– the 1988 "Siege of Atlanta," the 1991 "Summer of Mercy," and the April 1992 "Spring of Life" – that were clear deviations from the norm and generated considerable media attention. Our results are impervious to these controls, suggesting that these protests are unrelated to extreme clinic violence.

Abortion Data

To estimate the effect of violence, we compare trends in abortion rates (the number of abortions per 1,000 women aged 15-44), abortion provider rates (the number of abortion facilities per 100,000 women aged 15-44), and monthly births before and after the acts of violence.

As direct measures of the market for abortions, we use confidential county*year data from the Alan Guttmacher Institute (AGI) on abortions and providers by setting (hospital or non-hospital) and volume of the provider (<30 abortions performed, 30-399 abortions, 400-999 abortions, 1000+ abortions).[17] We include in our estimation all counties that appear at least once in our data, roughly one-third of all U.S. counties. These data, intended to be a census of all abortion providers in the United States, are only available for the years 1973-1982, 1984, 1985, 1987, 1988, 1991, 1992, 1995, 1996, 1999, and 2000 but are considered the most comprehensive source of abortion data in the United States (Blank et al. 1996). Our data cover mainly surgical abortions as medical abortions, a technological innovation that uses medications to terminate a pregnancy, did not receive FDA approval until September 2000, the last year of our analysis. We

[16] According to one former volunteer at an abortion clinic in Boston, protesters outside that clinic had such regular schedules that clinic staff knew which protesters to expect on any given day of the week.
[17] In an earlier version of the paper, we also used data from the Centers for Disease Control. However, while yearly, these data are state-level and thus, reduce considerably the variation in attacks.

exclude 1973 data from our analysis because of the poorer coverage of providers in that year.[18] However, our results are not sensitive to this exclusion.

Due to their controversial nature, abortions are almost certainly undercounted. Anti-abortion violence may exacerbate the measurement error in abortions. For example, facilities may be fearful to report abortions or may be more difficult to survey because they have either temporarily closed or relocated in response to violence. In the rare cases that clinics are unable or unwilling to respond, however, AGI uses abortion figures from the previous survey.[19] Assuming targeted clinics are less likely to report, this correction should bias our estimates towards zero.

Birth Data

We supplement the abortion data with confidential monthly natality counts by county of residence for the years 1973-2000. These data cover all counties, irrespective of county size.[20] Data on births enable us to discern the overall effect of the violence (i.e., the effect after taking into account travel to more hospitable areas for abortions). The frequency of these data allows us to precisely pinpoint the potentially-affected cohorts - reducing the potential for omitted variable bias.

Birth data have several advantages over the AGI abortion data. First, most, if not all, births result in a birth certificate and thus are less likely to be undercounted than abortions, especially during times of violence. Second, we use monthly data, allowing us to take advantage of the precise timing of an attack. Third, the reporting of births by county of residence rather than by county of occurrence enables us to ascertain the total effect of violence. If a woman obtains an abortion outside of a county where she would have otherwise gone to terminate a pregnancy, this action will appear as a decline in abortions in that county in the AGI data. But, this woman's pregnancy will *not* appear as a birth in the natality data.

Descriptive Statistics

Table 2 provides sample means of our data. We provide means for three groups: (1) counties with abortion providers who were not attacked between 1974 and 2000, (2) counties

[18] This information comes from personal communication with Henshaw, a senior fellow at AGI, on October 28, 2008.
[19] This information also comes from personal communication with Henshaw on October 28, 2008.
[20] Beginning in 1989, the public-use natality data only include county identifiers for larger counties.

with abortion providers who were attacked at some point during this period and (3) counties within 50 miles of an attack.[21] These data indicate that the targets of anti-abortion violence are not random.

The mean number of abortion providers per 100,000 women ages 15-44 differs little across non-violence and violence counties. But this aggregate figure masks an important difference in the type of providers across areas. In non-violence counties, most of the providers are hospital-based (4.6 out of 6.6) and perform fewer than 30 abortions per year (3.7 out of 6.6). In contrast, most providers in violence counties (4.1 out of 6.2) are based in non-hospital settings (primarily abortion clinics) and about three-quarters of them perform more than 30 abortions per year. Areas surrounding the violence have slightly lower provider rates.

As might be expected, violence counties and their neighbors have significantly higher abortion rates than the non-violence counties. The abortion rate in violence counties is nearly 4 times that in non-violence counties. In nearby counties, the abortion rate is almost 1.5 times higher than that in non-violence counties. Violence counties also tend to be more populous, with on average 155,000 women of child-bearing age compared to only about 27,000 in non-violence counties. Residents of violence counties and the surrounding areas are also richer than those in non-violence counties, with a real per capita income over $21,000 compared to less than $19,000 in the non-violence counties.

Because counties that have and have not experienced violence are so different, one might worry about using non-violent counties to estimate counterfactual trends. While our identification strategy (discussed in detail below) does not rest on the direct comparability of non-violent and violent counties, it does lean on the assumption that trends in abortion services are comparable across the two groups in the absence of violence. Consequently, we will test the sensitivity of our results to the use of alternate samples and comparison groups.

III. Empirical Analysis

Event Studies and Model Specification

Patterns for Abortions and Providers

To motivate our regression analysis, we begin with a graphical display of the data using

[21] Distance is based on the geographic-population centroid of each county.

an event study analysis. Specifically, we plot estimated year-relative-to-event fixed effects along with 95 percent confidence intervals from a regression of non-hospital abortion provider and abortion rates. The year-relative-to-event fixed effects are the set of coefficients δ from the following regression:

$$Y_{ct} = \beta_0 + X_{ct}\gamma + \mu_c + \delta_{t-t*} + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is either the abortion provider rate ((number of providers/number of women 15-44 years old)*10000) or the abortion rate ((number of abortions/number of women 15-44 years old)*1000) in county c in year t, X_{ct} are time-varying covariates such as state laws governing abortion access and county-level time-varying characteristics, μ_c are county fixed effects, δ_{t-t*} are year-relative-to-event fixed effects (e.g., t-t*=0 at the time of the event), and ε_{ct} is an error term.[22] t* denotes the year of the first violent event in the county. The county covariates included are the log per-capita income, log employment, the share of employment in construction, the share of income paid in unemployment income, and the share of females that are non-white. As the appropriate set of covariates is unclear a priori, we follow the existing literature and use the same set of covariates as Kane and Staiger (1996). See the Data Appendix for more details on the state abortion laws.

The display of these event study fixed effects serves several purposes: (1) to guide our modeling of the violence effect and (2) to verify that the time-series patterns are consistent with a causal impact of violence on abortion services. We focus on a window of 15 years prior to and 15 years post an anti-abortion attack so that observations very distant in time from an attack are not heavily influencing our estimates.[23] As will be discussed further below, the results are similar for shorter windows (e.g., 7 years prior to and 7 years post an anti-abortion attack).

Figure 3 displays the year-relative-to-event fixed effect estimates for our non-hospital abortion provider rate regressions. We focus on non-hospital providers since these are the type of facilities that have been attacked. Panel A displays the pattern for the full sample and Panel B restricts the sample to counties that have a predicted propensity for a violent attack in the range of [0.1, 0.9] following Richard Crump et al. (2009). The predicted propensity is based on a logit

[22] We have also used log rates as dependent variables but we prefer rates rather than log rates due to the presence of abortion and provider rates of 0.

[23] This selection rule results in the dropping of observations only for violent counties. For non-violent counties, all years of data are used.

of the probability that a county ever experienced violence during our sample period as a function of the 1974 values of the county covariates (i.e., pre-violence covariates) included in equation (1).[24] This restriction eliminates from the sample those counties that have virtually no probability of experiencing violence and those that will almost certainly face it.

The patterns in Panel A of Figure 3 show that prior to attacks, abortion provider rates are modestly rising. The pre-violence rise in the provider rate is an artifact of the composition of the counterfactual sample. In comparison, the pre-attack trends are relatively flat in Panel B, which eliminates violent and non-violent counties that that have either very low or very high probabilities of attack.[25] Despite the differences in pre-violence trends, the conclusions from both figures are similar. Non-hospital provider rates fall for the first two years after an attack (time 0) and return to trend thereafter. They do, however, appear to remain lower than what they would have been in absence of the violence.

Figure 4 shows an analogous event-study figure for non-hospital abortion rates. As for non-hospital provider rates, pre-attack abortion rates are rising for the full sample but are quite flat for the restricted sample. Also similar to the provider rates, abortion rates fall precipitously after a violent attack. Relative to the pre-violence trend, abortion rates are lower in the post-violence era – indicating permanence of the violence effect. The abortion rates in the post-violence period are noisy, possibly reflecting the fact that additional acts may have occurred. Nonetheless, the figure provides the first piece of evidence that counties that were the targets of violence experienced large and persistent declines in abortion services post attack.

Specification for Abortion and Provider Analysis

Based on these event-study graphs, we model the effects of violence on abortion and provider rates as follows:

$$Y_{ct} = \beta_0 + \beta_1 f(t - t^*) + \beta_2 1(t > t^*) + X_{ct}\gamma + \mu_c + \delta_t + \varepsilon_{ct} \qquad (2)$$

---

[24] These covariates include log per-capita income, log employment, the share of employment in construction, the share of income paid in unemployment income, the share of females that are non-white, the log of the population of women ages 15 to 44 in 1974 and an indicator for whether the state had a mandatory waiting period or a TRAP law. In 1974, none of the other state policies were in effect. This selection rule effectively drops roughly 75 percent of the non-violence sample and roughly 10 percent of the violence sample.

[25] Both panels show a spike in provider rates at time 0. This spike is driven in part by the fact that counties can only be attacked if they have at least one provider. Thus, attacked counties have more providers at time 0. Focusing on counties with many providers (e.g., in the top quartile of pre-attack provider rates), we observe virtually no spike at time 0.

where Y_{ct} is an outcome such as the abortion rate in county c at time t, $f(t-t^*)$ is a quadratic function of t-t* used to control for smooth trends in our outcomes around the time of the violence,[26] $1(\cdot)$ is an indicator function that equals 1 in the post-violence period and 0 otherwise.[27, 28] The other terms in this equation are identical to those in equation (1). The year fixed effects control for nationwide changes in abortion services (e.g., the enactment of the Freedom of Access to Clinic Entrances Act in 1994 that increased the penalty for obstructing access to abortion clinics). We cluster all standard errors at the county-level. Our parameter of interest is β_2.

The specification laid out in equation (2) has several features worthy of discussion. Importantly, it (a) assumes that the effects of violence are permanent in the post-attack period,(b) implicitly ignores the effect of acts of violence occurring after the first event and (c) nets out the national effect of violence.

Our first main assumption, the permanence of the violence effect is motivated by our plots of the time series patterns in Figures 3 and 4, which show a shift in the trend in abortion and provider rates downward in the year of the violence. We have conducted sensitivity checks that confirm this choice.[29] Moreover, as we show below, results are similar as we narrow the window around an attack, providing further support that the effect is permanent. Regarding (b), our primary reasoning for excluding subsequent events is that the timing of these further attacks may be endogenous. For example, an anti-abortion activist who feels unsuccessful may decide to

---

[26] If we include separate polynomials for the pre- and post-attack periods, the regression results are similar.

[27] In particular, the post dummy variable equals one for years after the violence (i.e., for the violent year, post equals 0). We do this for several reasons. First, in the year of the violence, sometimes AGI denotes the number of abortions performed in that year as equal to the abortions performed in the previous year, as reported to us by Henshaw, a senior fellow at AGI, on October 28, 2008. Second, the violence usually does not occur at the beginning of the year – effectively meaning that only a fraction of the year's abortions could be affected by the violence. Also, recall that since the data are not available for all years, only a fraction of counties will report data in the year of violence. In results not shown here, we find that our estimates are qualitatively unchanged if we a) set this violence variable equal to 1 for the violence year and all years after or b) make the violence variable equal to a fraction reflective of the fraction of the year following the violence and 1 for all years after (e.g., the violence variable equals 1/12 for 1993 if the violence occurred in December 1993 and 1 for all years after 1993).

[28] Specifying the model in this way, 766 of 18187 or 4 percent of county-year cells are "treated." Results using a propensity-score matched sample or only counties that were attacked are similar. In the matched sample, 676 of 5027 or 12 percent are treated. In the sample of attacked counties, 766 of 2194 or 35 percent of county-year cells are treated.

[29] In results not shown here, we have estimated models that break out the post event dummy into three separate dummies for the (1) 1st year post attack, (2) 2nd year post-attack, and (3) 3rd and all subsequent years post-attack. This specification confirms the permanence of the violence effects for abortions and indicates a 1-year lag and then permanent provider response. These results are available upon request.

strike again. Earlier we observed that roughly half of the attacked county experience more than one violent attack. However, the elapsed time between events in most cases is short. Over 60 percent of second acts of violence occur in the same year as the first event or one year later, making (b) a relatively innocuous feature of our model. Furthermore, the results are qualitatively unchanged when we specify the treatment effect in a range of other ways.[30] Finally, with year fixed effects in our specification, we interpret our estimates as relative effects – effects in the targeted county relative to other counties. It is plausible that the non-targeted counties also experienced impacts from the violence. In this case, our estimates are lower bounds of the total effect of the violence. We will explicitly test this assumption and adjust our estimates by analyzing geographic spillovers (discussed in detail below).

Natality Data Analysis

Our analysis of the natality data closely resembles that for abortions. Nevertheless, there are some important differences because of differences in the expected timing of the violence effect on births. Specifically, any effect of violence on births should occur with some delay. Women close to giving birth today are not candidates for abortions because they are too far along in their pregnancy. Instead women between 0 and 12 weeks pregnant (roughly 0 to 3 months pregnant) are the primary group weighing the decision to have an abortion.[31] Therefore, to the extent that births are affected, we should detect this with a roughly 7 to 10 month lag since the usual gestational length is 40 weeks (slightly less than 10 months). In practice, we observe a rise in births 7 to 11 months post-violence, although our results are not sensitive to the exclusion of month 11 from the treatment cohort.

[30] We have estimated models that specify the treatment as either (1) the cumulative number of attacks, (2) the number of attacks during the previous 5 years, 5 to 10 years, and 11 years plus or (3) five separate indicators for the first through the fifth attack. Only 15 of 140 counties experienced more than five attacks so we think this cutoff is reasonable. Estimates from (1) are quite imprecise and difficult to interpret given that the regression imposes a linear relationship between the number of attacks and abortion and provider rates. Based on (2), the number of attacks in the last 5 years is the most salient. This does not necessarily imply that the violence effect is temporary; the standard errors on the coefficient on the number of events more than 10 years prior are quite large and would not allow us to rule out long-run effects of the size we estimate in our main regressions. Based on (3), we find that the estimated effects for first attacks are quite similar to our main results and the effects of subsequent attacks are generally imprecise. This is not surprising since most affected counties (68 of 140) were attacked only once and most second attacks occur "close" to the first one. Results from these various specifications are available upon request.

[31] In 2004, almost 90 percent of abortions were obtained before 13 weeks of gestation (CDC, 2007).

In the spirit of Figures 3 and 4, Figure 5 shows our event-study analysis for log births.[32,33] In both panels, the size of the cohort born 7 to 11 months after the violence, represented by 0 on the x-axis, is a deviation from trend. This aberration is on the order of 1 percent; the corresponding fall in abortions seen earlier easily trumps this effect size.

Besides affecting the behavior of already pregnant women, the violence could also affect the fertility behaviors and the abortion decisions of women at risk of becoming pregnant in the post-violence period. Since births quickly return to their pre-violence trend after a 5-month post-violence deviation, any decrease in conceptions would have to fully counterbalance the decrease in abortions. In other words, those who would have otherwise sought abortions must, post-violence, decrease conceptions. In the long-run, violence did not affect total abortions, once factoring in travel to nearby areas, or total births much. Thus, we interpret these findings as suggesting that the violence affects the abortion decisions of already-pregnant women and women who soon become pregnant but has negligible effects on fertility.

Specification for the Natality Data Analysis

Given the patterns in Figure 5, a natural regression specification for detecting the effect of violence on births:

$$Y_{ct} = \beta_0 + \beta_1 f(t - \tilde{t}) + \beta_2 1(\tilde{t} + 4 \geq t \geq \tilde{t}) + \mu_c + \delta_t + \varepsilon_{ct} \qquad (3)$$

where Y_{ct} denotes log births in county c for month t,[34] $f(.)$ is a linear function of time relative to event, $\tilde{t}$ is the first month in which births could be possibly affected (i.e., t^* (the month of violence) + 7 months), $1(.)$ is an indicator for whether the birth was 7 to 11 months after the violence, μ_c are county fixed effects, and δ_t are month*year fixed effects. Note that we exclude county x time covariates since they are not available at the monthly level. Since we are exploiting a sharp change in time, these variables would also need to vary sharply for omitted variables bias to be problematic. To estimate equation (3), we use data close to the time of the violence – from 29 months prior to the violence to 43 months following the violence to insure a

[32] Due to the inherent noise in the monthly data, we group births into 5-month intervals for the purposes of this figure but in the regressions to follow, each observation represents births in a county in a particular month.
[33] We do not have population data by month to create birth rates.
[34] We adjust the birth data such that each month of data covers essentially the same number of days by dividing the monthly birth counts by the number of days in the month and multiplying that by 30.

balanced window of potentially-affected cohorts around the violence.[35] Results are similar using narrower windows, as discussed later.

Our interest is in β_2, the effect of the violence, in percent change terms, on the size of the cohort born 7 to 11 months. This coefficient will capture the total effect of the violence on abortions – both the change in abortions performed in targeted counties and any dislocation of abortions to surrounding areas.

Identification

The goal of estimating equations (2) and (3) is that β_2 will identify the causal effect of abortion clinic violence. However, identification of the violence effect rests on the assumption that the timing of the extreme clinic violence, conditional on the set of covariates, is effectively random. Given that acts of terror rely on an element of shock, a surprise event should induce the largest impact as providers will be unprepared. However, we take the threat to identification seriously and try to address it in several ways.

First, we measure the intensity of the abortion discussion around the time of the violence using newspaper articles. Analyzing this pattern helps us address the concern that violent anti-abortion activists respond to changes in local abortion funding or are spurred on by local demonstrations against clinics. The hope for identification purposes is that the violence is unrelated to local events or feelings, which might be captured by newspaper article counts. In Appendix Figure 4, we display month-relative-to-event fixed effects from a regression of the number of articles in a month with the word "abortion" in the headline. See the Data Appendix for more details on the construction of this data source. In this Figure, there are no pre-existing trends in local abortion-related news prior to an attack. We take this as prima facie evidence that the estimates presented later capture changes in the market for abortion services rather than broader local changes in anti-abortion sentiment leading to anti-abortion violence.

Second, we include X_{ct}, time-varying state-related abortion policies and county-level economic and demographic variables in the regressions. If the events are unpredictable, then the inclusion of these variables should not impact our estimates of β_2. As we show later, our

[35] That is, we consider 3 years of cohorts potentially unaffected by the violence and 3 years of cohorts potentially affected by the violence.

estimates of the violence effect are relatively insensitive to the addition of these controls.[36]

Third, we test the sensitivity of our estimates to the inclusion of co-existing trends either through the inclusion of state-by-year fixed effects or county time trends. The specifications with state-by-year fixed effects rely on within-state variation in the timing of violent attacks across counties, allowing us to control in an relatively unrestricted way for any contemporaneous changes (e.g., in state policies and economic factors) at the state-year level that affect the market for abortions.[37] The county time trends allow for a more flexible specification of $f(t-t^*)$. In both sets of specifications, we find that our estimates are quite similar.

Fourth, for births, the frequency of the data allows us to more finely pinpoint the effect of violence. We can compare the number of births immediately prior and immediately following the violence. In order for omitted variables bias to be problematic, the omitted variables would also need to be discontinuous at the time of the violence. Thus, this analysis provides a reasonably clean approach to estimating the causal impact of abortion clinic violence.

Fifth, we perform a set of sensitivity tests to further rule out the possibility that omitted factors drive our results. Specifically, we test the sensitivity of our estimates to two alternative control groups: (1) a sample of counties that have a predicted propensity for violent attacks based on 1974 characteristics in the range of [0.1, 0.9] and (2) areas that ever experienced violence. As will be discussed below, these results are also quite similar.

While together these specification checks provide some credibility to the identification, omitted variables bias could remain. Where year fixed effects do not control adequately for violence-inducing pro-abortion activities, we may have estimates of the effect of the violence on the market for abortions that are biased towards zero since some evidence suggests that anti-abortion violence responds to pro-abortion activities (Ginsburg 1998), as evidenced today.

IV. **Results**

AGI County-Level Analysis

---

[36] We have also estimated Cox proportional hazard models of the timing of a first anti-abortion attack. With the exception of the log employment and the share of employment in construction, both lagged one year, these covariates are generally poor predictors of the timing of an anti-abortion attack.

[37] If women travel to other areas in their state in response to the violence, the inclusion of state-by-year fixed effects could lead us to overestimate the effect of the violence.

In Table 3, we present our first set of regression estimates of equation (2) for 6 different dependent variables: provider rates, hospital-based provider rates, and non-hospital (i.e., clinic or physician office-based) provider rates, abortion rates, hospital-based abortion rates, and non-hospital based abortion rates. For each dependent variable, we present estimates from five separate regressions, each including a varying number of control variables. In the first column, we present an estimate of the violence's impact (i.e., an estimate of β_2) excluding county fixed effects, a naïve regression. The positive effect on provider rates from this regression, which suggests that violence increases provider rates, is not surprising in light of the patterns in Table 2. This specification does not exploit exclusively the variation in violence we argue might be exogenous – the variation in the timing of violence within a county.

In contrast, the second estimate takes into account the inherent differences between areas with and without violence by including county fixed effects. The sign of the estimated effect flips as expected. The estimate implies that abortion provider rates fall by 0.56 providers per 100,000 women or about 8 percent following an attack of violence. The estimate is only significant at the 10 percent level.

The next three columns of estimates include a successive number of covariates to assess the degree to which the regression estimates are biased. In the third column, we add county-level economic and demographic covariates such as log per-capita income and log employment. In the fourth column, we include the state-level abortion-related policies described earlier. In the fifth column, we include state-by-year fixed effects. The hope is that the estimates in the second through fifth columns are similar, and that is indeed what we find.

Table 3 assesses our *a priori* expectation that non-hospital and hospital facilities will respond differently since anti-abortion violence almost exclusively targets clinics. As expected, we observe a decline in non-hospital provider rates. In the period following an attack, the number of non-hospital based providers falls by between 0.40 and 0.55 per 100,000 women of child-bearing age. With the exception of the specification with state-by-year fixed effects, these estimates are statistically significant at the 5 percent level. Off a base rate of roughly 4, the estimates imply a 10 to 13 percent decline in non-hospital provider rates. We might expect some substitution to hospital providers, but there is no clear positive impact on hospital provider rates.

Table 3 also assesses the impact of violence on abortion rates. Controlling for fixed county differences, we find a statistically-significant decline of 3.2 (8 percent) in the abortion rate within a county in the years following an act of extreme violence. This estimate is insensitive to the inclusion of county covariates and state policy variables or state-by-year fixed effects. As was the case for providers, the decline in abortion rates is concentrated entirely in non-hospital based settings with a 3.4-3.7 fall in non-hospital abortion rates (a 10 to 11 percent drop). Abortion rates appear to increase in hospital settings, although these estimates are very imprecise.

As a first gauge of the plausibility of our findings, Table 4 decomposes provider and abortion rates by the volume of the facility. Results are broken down according to the categories made available by AGI -- providers performing fewer than 30 abortions, 30-399 abortions, 400-999 abortions and 1000 or more abortions in a county-year. Since violence targets clinics, we would expect the effects to be concentrated among the more sizable providers. To reduce the number of columns, we only report results from our preferred regression specification with county fixed effects, county covariates and state policy variables. Results from other county fixed effects specifications are virtually identical.

Panel A presents results for providers. The largest violence effects are for large providers – those performing 400-999 abortions per year – but are essentially zero for the very largest providers. Provider rates decline by almost 0.3 per 100,000 or about 33 percent among those performing 400 to 999 abortions. This result suggests that the medium-sized clinics (or possibly "large" clinics in less populous areas) are less able to absorb the cost of an attack and thus more likely to shut down. In contrast, the largest clinics, which are often part of a national chain such as Planned Parenthood, may have the resources to repair their infrastructure and hire extra security. Changes in abortion rates for the four volume categories, in panel B, are analogous to the provider results. In particular, the bulk of the reduction in abortion rates is among clinics performing 400-999 abortions per year. Abortion rates performed at these providers fall by about 2 or 32 percent following an attack.

In principle, since a clinic can change categories from year to year and because our data are at the county not clinic level, this result could reflect closures in other categories along with downsizing or upsizing in the medium clinic group. For example, this result is consistent with the largest clinic in a county closing as a result of violence and the medium-sized clinic in that county absorbing enough of their clients to move into the large category the next year. Regardless of the composition, the net effect is the loss of a medium-size clinic.

Sensitivity of Main Abortion Results

Our results thus far indicate that non-hospital provider and abortion rates decline in response to the violence. The estimates in Table 3 use data from a 15-year window around a violent attack. One might worry that 15 years is too long and is not in the spirit of an event-study analysis. Such a long window might capture events that are correlated with but occurred very distantly from an attack or create a very unbalanced sample with earlier events contributing most to post-event patterns and later events contributing most to pre-event patterns. Appendix Table 1 demonstrates that this is not the case: progressively narrowing the window to 12 (12 years before and 12 years after the first event), 10 and 7 years leads to similar estimates of the relative effect of violence on provider and abortion rates. Non-hospital provider rates fall by between 0.42 and 0.48 per 100,000 women or 10 to 12 percent compared to an average decline of about 0.50 per 100,000 or 11 percent in Table 3. The abortion rate declines are slightly smaller but still quite similar: non-hospital abortion rates decline by between 2.56 and 3.27 per 1000 or 7 to 10 percent compared to an average decline of about 3.50 per 1000 or 11 percent in Table 3.

Another concern, typical in any difference-in-difference analysis, is whether we have chosen an appropriate control group. Without a good control group we cannot fully capture unobservable, co-occurring factors, such as changes in anti-abortion sentiment, that may affect outcomes. To test the sensitivity of our results to the choice of control group, we consider two alternative samples. First, we confine the sample to those counties with propensities of violence between 0.1 and 0.9 as we did for the sample in Panel B of Figures 3-5.

We display the results from the propensity-matched sample in Appendix Table 2. The basic pattern of results is consistent with the main regression results, although the abortion estimates are slightly smaller than the full sample estimates in Table 3. For example, we find a reduction in non-hospital-based providers of roughly 0.44 per 100,000 or 11 percent in the matched sample. Similarly, non-hospital-based abortions decline by roughly 2.99 per 1000 women or about 9 percent in the matched sample. These results, which are similar to estimates using the full sample but smaller windows around a violent attack, suggest that the magnitude of our main within-county abortion estimates may be slightly high but not markedly so.

Appendix Table 3 further tests the sensitivity of our results to the choice of control group by restricting the sample to counties that ever experienced violence. In this way, our control group in any given year consists of counties that experience violence in a different year. The results here are akin to our main results, if somewhat smaller and less precise. After an attack, non-hospital-based providers decline by 0.3-0.44 per 100,000 women and abortions decline by roughly 2-3 per 1000 women in the violence only sample. Thus, as was the case for estimates from the matched sample and the narrower windows, the results here suggest that our full-sample, within-county estimates are unlikely to vastly overstate the true impact of violence on abortion services in targeted counties.

Despite the stability of our results, we may still worry that our control groups do not adequately capture factors that evolve differently over time in violence areas. By including linear county-specific time trends in our models, we can control for factors that, like local anti-abortion sentiment, may evolve smoothly but differentially across areas. Panel A of Appendix Table 4 presents results from our preferred regression specification (with county fixed effects, county covariates and state policy variables) augmented by county-specific linear trends for provider rates, hospital provider rates and non-hospital provider rates. For each dependent variable, we show estimates using the full sample (column 1), the propensity score matched sample (column 2) and the sample of attacked counties (column 3). Panel B shows the same breakdown for abortions rates, hospital abortion rates and non-hospital abortion rates

Based on the full sample, results for provider rates and non-hospital provider rates are broadly analogous to our main specification estimates, though a bit smaller and less precisely estimated. By restricting to the matched and violence-only samples, we improve the precision. We find, for example, that non-hospital provider rates decline by 0.37 per 100,000; this estimate is statistically distinguishable from zero in both samples. The same basic pattern holds for abortion rates: the magnitude and precision of our full sample results decrease a bit. Results from the matched and violence-only sample are statistically distinguishable from zero and imply declines of 2.3 to 2.6 non-hospital abortions per 1000 women of childbearing age. Although our results with county-specific trends are slightly smaller than our main results, they still are consistent with sizeable declines of 9 to 10 percent for non-hospital providers and 7 to 8 percent for non-hospital abortion rates.

Behavioral versus Mechanical Response

Our results thus far indicate that in response to the violence non-hospital abortion and provider rates fell in targeted communities. While the results are clear, the causes of the declines could be multi-faceted. The fall could be demand or supply driven.[38] Affected facilities could close or be temporarily shut down, reducing abortion access. Since violent counties have on average 8 providers (median is 4), closures of attacked clinics are unlikely to explain the total decline in abortion rates unless the remaining clinics are already at capacity. Closures of unaffected clinics could further contribute to the declines. On the demand side, after an attack, women could fear visiting any abortion facilities in the same area. This feeling may not be unfounded because anti-abortion activists often target the same area on the same day or within a few days or weeks of the first event.[39]

The loss of infrastructure is a supply response that has a crucial impact on the interpretation of our results. That is, if the only impact of clinic violence is the direct reduction of clinic capacity, then the effects of terrorism may be limited. Clinic capacity may be relatively easy to restore. And terrorism should have larger effects when it has an impact beyond the direct damage it causes to physical or human assets. We take several approaches to determining whether our results are purely mechanical – i.e., due to the destruction of infrastructure – or reflect some degree of behavioral response to terror.

As a first approach, we study violence effects by the pre-attack availability of providers. In places with few providers, there is more scope for a mechanical response. If one provider closes, people must go elsewhere. In Table 5 we classify attacked counties into quartiles based on their pre-violence provider rates. Counties with a provider rate in the lowest quartile of this distribution are denoted by the [0, 25] group in this table, counties with a provider rate in the interquartile range are represented by the (25, 75) group, and counties with a provider rate in the highest quartile are classified into the [75, 100] group. Non-attacked counties are included in all regressions. One should be somewhat careful about interpreting these estimates since the response to violence may be heterogeneous with respect to provider availability for other reasons besides provider density.

---

[38] To distinguish between relative shifts in supply versus demand for abortions in a targeted county, we would need some information on the market price of abortion services. While some price data are available, they are quite crude – with very poor geographic and time series coverage. Moreover, the actual market price is difficult to measure because of the existence of insurance and subsidies from organizations such as Planned Parenthood.

[39] For example, in Houston, TX two abortion clinics were bombed on September 7, 1984. John Salvi open fired at two clinics on December 30, 1994 in Brookline, MA and five clinics in Miami, FL faced butyric acid attacks on May 16, 1998. Similarly, in Baton Rouge, LA two clinics were set on fire within five days of each other in October 1985 and in Columbus, OH two clinics were bombed within 10 days of each other in February 1991.

With this caveat in mind, Table 5 indicates that declines in provider rates are only detectable in areas with many providers relative to the population of women of childbearing age. On the other hand, the effects of attacks on abortion rates are felt throughout the provider rate distribution. While the relative effect of attacks on abortion rates is largest in areas in the lowest quartile of the provider rate distribution, areas in the top quartile also experience sizeable declines in abortion rates. Total abortion rates decline by 3.85 per 1000 or about 15 percent in the bottom quartile and by 4.65 or 8.5 percent in the top quartile. Drops also appear in the middle of the distribution. Thus, while some of the response to violence may be mechanical – due to a loss of infrastructure – the response in areas with many providers suggests we are also capturing a real behavioral response to terror.

To shed further light on whether our results reflect some behavioral response to violence, we categorize the attacks into 3 groups: (1) damage attacks: arson, butyric acid, bombing (134 acts), (2) murder attacks: murder and attempted murder (3 acts), and (3) other attacks: hoax devices, major threats, and kidnappings (3 acts). The damage acts should incur the largest incapacitation effects; at least in the short run, the clinic would need to close. The murder attacks, in contrast, may impose large psychic costs on potential clients who fear for their safety. We report these results in Table 6.

Since the damage attacks are the most common extreme act of violence, their effects largely mirror the pattern and size of results in Tables 3 and 4; damage attacks lead to an 11 percent decline in non-hospital-based provider rates and a 9 percent decline in non-hospital based abortion rates. Murders, while rare, have much more sizeable negative effects. An abortion-related murder in a county is associated with 3 fewer non-hospital providers per 100,000 women or over a 75 percent decline in these facilities.[40] Given how few murders

[40] This result is not driven by murders occurring in areas with more providers. Prior to the violence, non-hospital provider rates are 4.37 per 100,000 women of childbearing age in the areas where murders occurred compared to 4.43 per 100,000 in areas that had arson attacks. However, murders did target areas with significantly higher abortion rates. Pre-violence abortion rates are roughly twice as high in counties experiencing murders than counties experiencing other forms of violence. But even after accounting for this difference, the effects of murders are relatively large in percentage terms. Interestingly, relatively small clinics account for much of the decline in provider rates; facilities performing 30-399 procedures a year experience a drop of 2.11 providers per 100,000 women or 72 percent – even though murders targeted high volume providers. This is consistent with these murders provoking fear. In contrast, the highest volume providers decline by 0.33 per 100,000 women or about 26 percent. Temporary closures to accommodate a criminal investigation, improve security, or move to a new location (e.g., in the Brookline murders) may drive some of the decrease. But the effect on smaller clinics is almost certainly behavioral.

occurred during our sample, however, we caution against inferring too much from these results.[41] For the other violence category, none of the effects are statistically different from zero.

As an alternative approach to assessing whether the estimates are purely mechanical or reflect some behavioral response to terror, we estimate regressions analogous to those in Table 3 with an interaction of the damage cost of an attack. Since the dollar damages capture losses due to physical damage alone, they provide a marker for whether a facility is unusable, at least in the short-run. We observe no heterogeneity in the impacts on provider or abortion rates by the amount of damage, suggesting something more than the destruction of physical infrastructure may be at play.

On net, the results in Tables 3-6 indicate that acts of extreme anti-abortion violence lead to declines in clinic-based providers and abortions. If, in response to the violence, providers move to nearby counties and/or women seek services elsewhere, we may overstate its true impact. On the other hand, if violence experienced in one county also discourages providers and women in counties "close" to the attack from participating in this market, we will understate the true impact of violence. Said differently, if our "control" counties experience a change in providers and abortions as a result of the violence, then our earlier estimates will be biased. Ultimately, we care about the full effect of the violence on abortion activity in the areas near an attack. Since the reporting of births is by county of residence rather than by county of occurrence, natality data provide one way for us to ascertain the total effect of violence.

County-Level Natality Analysis

The event studies patterns displayed in Figure 5 clearly indicate that births to women residing in targeted counties rise in the 7 to 11 month period after the attack. In Table 7, we quantify this effect. This table presents a series of estimates of equation (3). We display estimates of the violence dummy for two samples: (1) counties appearing in the AGI data and (2) all counties, including those that have no abortion providers. The first column of estimates excludes county fixed effects; the second column adds county fixed effects. Thus, the preferred specification is the second column of estimates.

The estimates in Table 7 corroborate Figure 5. Once county fixed-effects are included,

[41] In this table, we test multiple hypotheses simultaneously without making strong *a priori* predictions on the size and magnitudes of the effects. As such, the standard errors are likely to be too small. However, we think the results are suggestive of heterogeneity in the effects.

regardless of the sample used, the regression estimates imply that births were 0.9-1 percent higher in the 7 to 11 months following the violence.[42] The stability of the natality estimates gives us some confidence that omitted variables bias is not a huge concern. Furthermore, Appendix Table 5, which presents estimates using progressively narrower windows of months around an attack (30, 24, 18 and 12 months), demonstrates that the results are insensitive to this choice. We also find that the addition of county linear trends does not affect the estimates (not shown).[43]

As with our abortion estimates, we may still worry that these estimates are sensitive to the choice of control group. In Appendix Table 6, we perform the same type of propensity score analysis for births as discussed above for AGI abortions. The estimates are virtually identical to those in Table 7 – 0.009 compared to 0.009-0.010 in panel A of Table 7. Limiting the sample to counties that ever experienced violence (Appendix Table 7) also yields comparable results – point estimates of 0.008.[44] Consequently, we are not worried that the choice of comparison group drives our main conclusions.

With 140 counties experiencing at least one act of violence, anti-abortion violence led to an additional 5,694 births that would have been otherwise aborted.[45] In contrast, the implied drop in abortions in the attacked county for the year immediately following the violence is between roughly 45,650 and 69,760.[46] Thus, only between 8 to 13 percent of the women deterred from

[42] In results not shown we explored heterogeneity in the impact of the violence on births. As in Table 6, we estimated regressions where we categorize acts according to their type. Not surprisingly, the effect of the damage acts on births is roughly equivalent to the overall effect in Table 7. There is some indication of a very strong response of births to the murder of an abortion clinic worker. Because these events are quite rare, however, we hesitate to draw strong conclusions. In addition, as we have in principle, individual-level birth data, we studied whether the violence affected different subpopulations differently. We find that the effects are nearly constant across subgroups – the exception being across maternal age with older mothers being the most responsive.

[43] For example, for counties appearing in the abortion data, the estimated effect of violence from a model that includes county-specific linear trends is 0.010 with standard error of 0.004.

[44] There are 4 counties that had attacks of violence but do not appear in the AGI abortion data. These counties (Crow Wing County Minnesota, Marion County Missouri, Shelby County Ohio, and Eastland County Texas) are small with an average population size of 40,000. As such, it is likely that they only had or have one abortion facility. Finer and Henshaw (2003) mention that the AGI data exclude a small fraction of suspected abortion facilities (because of non-response). However, the exclusion of these small counties has no impact on our birth estimates, so we do not worry about the lack of data for these counties.

[45] This calculation comes from multiplying 0.010 (the point estimate), 852 (the average number of monthly births for affected counties prior to the violence), 140 (the number of affected counties during the time span of the analyzed data) and 5 (because the point estimate is based on a five-month dummy but births are in months). Note we report that there are 150 counties that had at least one attack of violence between 1976 and 2005, but recall that our analysis ends in 2000.

[46] The 69760 figure is based on the product of -3.22 (the point estimate from Table 3), 154.75 (the number of thousands of females ages 15 to 44 violent counties from Table 2), and 140 (the number of affected counties during the time span of our analyzed data). The 45,650 is from our smallest point estimate of the violence effects, 1.98,

receiving an abortion in a violence county end up giving birth in the short run. It should be noted that an alternative interpretation of the size of the short-run abortion effect relative to that for births is that abortions at the time of violence are severely misreported.[47] In the long run, however, our results imply a true behavioral response to violence. Abortion rates decline precipitously in targeted counties. In principle, this decline could also reflect a decrease in conceptions post-violence. However, an alternative hypothesis that we find evidence to support is that violence displaces abortion activity. In other words, some providers and women seeking to terminate their pregnancies may avoid attacked areas and "relocate" their abortions to other nearby non-violent areas. In this way, we can observe large and sustained declines in abortions in targeted areas but only small, temporary increases in births.

An analysis of births based on the availability of providers prior to an attack offers the first suggestive evidence that such relocation may occur. Appendix Table 8 presents estimates from regressions that, like Table 5, split the sample by the pre-attack provider rate distribution. These results indicate that in counties in the top quartile of the distribution of pre-attack provider rates, births to women do not increase. In sharp contrast, women living in counties in the bottom and in the middle of the pre-attack provider distribution have 1.3 to 1.4 percent more births in the 7 to 11 months after an extreme act of anti-abortion violence. Since abortions decline in counties in all quartiles of the provider-rate distribution, according to Table 5, and since the availability of providers in neighboring counties is lowest for the bottom and highest for the top quartile, these results suggest that where possible, women may fully adjust to attacks by traveling elsewhere to terminate their pregnancies.[48] The increase in births among counties in the bottom and middle quartile of the provider distribution may reflect the relatively low provider rates in neighboring counties. Women in these counties, in contrast to those in high provider-rate counties, may simply have fewer nearby alternative options, creating a barrier in the short run. In the long run, however, births return to trend in all counties.

---

which is the estimate in Appendix Table 4 based on attacked counties and including linear county-specific time trends.

[47] As noted earlier, Stanley Henshaw, a senior official at AGI, argues that attacked facilities are not less likely to report to AGI after they are attacked.

[48] The median provider rates for counties within 50 miles of attacked counties in the bottom quartile and middle of the pre-attack provider distribution are, respectively, about 1.7 and 3.4 per 100,000. The median for counties neighboring attacked counties in the top quartile is about 6 per 100,000.

V. Displacement Effects of Violence: Reconciling the Abortion and Natality Results

We next try to more explicitly reconcile the sustained declines in abortion rates and the much smaller, temporary increases in births in counties experiencing clinic violence. Specifically, we assess whether most women wanting to terminate their pregnancies but fearful because of the violence respond by traveling elsewhere to do so.

To test for geographical spillovers, we supplement equation (2) with a population-weighted indicator for counties within 50 miles of an attack in the years following the attack. The weight is that county's attack year share of the population of all neighbors within 50 miles (e.g., two neighboring counties of size 50,000 will have weights of 0.5). We adopt this weighting scheme so as to estimate the total effect of violence on all neighbors instead of the average effect across them. We choose 50 miles as the treatment area because this provides good coverage of a woman's potential market for an abortion. In 1992, for example, 76 percent of abortions occurred within 50 miles of the mother's place of residence (Henshaw 1995). Analogous to the specification of our post-attack dummy, our neighboring-county attack "dummy" is equal to 1 after the earliest attack and is not affected by any additional attacks within the area.[49]

In panel A of Table 8, we present the results from these regressions. We report coefficients and standard errors from the neighbor effects that are standardized to the pre-violence means of the female population in the attacked county (i.e., multiplied by the ratio of the pre-violence mean of females ages 15 to 44 in neighboring counties relative to attacked counties, which is 0.409). In this way, the effects on attacked counties and their neighbors are directly comparable; adding them provides a standardized estimate of the total effect of violence on the targeted counties and their neighbors.

The results in Table 8 indicate that an anti-abortion attack in one county increases both the number of hospital-based and non-hospital-based providers in counties within 50 miles. Following an attack, hospital-based providers increase by 0.53 per 100,000 women and non-hospital-based providers increase by 0.22 per 100,000 women. Based on the point estimates, the increase in non-hospital based providers does not fully offset the decline in this type of provider

[49] In a few cases, neighbors were also attacked. Specifically, 30 of 364 counties within 50 miles also experienced anti-abortion violence. Results are similar if we limit neighbors to counties that never directly experienced violence.

in attacked counties. However, the full effect of violence is not statistically distinguishable from zero for either non-hospital or hospital-based providers. These findings suggest that some and possibly all of the previously-reported declines in provider rates in attacked counties are offset by increases in neighboring counties.

Turning to the abortion rate estimates, we find that following an attack neighboring counties experience an increase in non-hospital-based abortion rates of about 2.5 per 1000 women of childbearing age. With an average decline in attacked counties of 3.85 per 1000, the increase among neighbors implies an offset of almost 65 percent. Given the standard errors, we also cannot reject full offsetting through an increase in non-hospital abortion rates in neighboring counties. In sum, the sign and magnitude of the estimated effects in Table 8 suggest that surrounding counties absorb most, if not all, the drop in abortion services in the violence counties. The effects of violence do not appear to spread as abortions in nearby counties do not fall. Most women travel elsewhere to terminate a pregnancy rather than being deterred from obtaining an abortion altogether. Thus, the net effect of violence on abortions (and thereby births) is small and temporary. In contrast, the displacement of abortions to nearby counties is quite considerable.

Spillovers in the Natality Results

We also test for spillover effects of the violence on births. The nature of the spillover in the context of births should be quite different than for abortions. Since births are reported by place of residence rather than occurrence, the idea of displacement is not relevant. However, like women in targeted counties, women residing in neighboring counties who want to terminate their pregnancies may be discouraged from seeking abortions in the short run. To measure whether this is the case, we supplement our main regression specification with an indicator that equals 1 for births that occur 7 to 11 months after an attack for counties within 50 miles of the violence and 0 otherwise.

The results in panel B of Table 8 indicate that the effects of the violence on births are felt solely in targeted counties. That is, while births to women in attacked counties increase by about 1 percent in the short-run, births to women who *reside* in neighboring counties do not increase, even in the short-run, after an attack. Although these women may increasingly avoid seeking abortions in attacked counties, women from neighboring counties adjust to the violence right away by availing themselves of abortion services in either their own county of residence or in other counties that were not attacked.

Overall our results indicate that clinic violence had only modest short-term effects on the market for abortions. One important caveat to this conclusion, however, is that our analysis considers changes in provider, abortion and birth rates net of any national trends. We do this in order to control for common factors affecting the market for abortions that could confound our estimates of the effects of violence. The cost of this approach, however, is that our estimates do not capture any effect that local terrorist attacks have across all abortion markets. Although beyond the scope of the current paper, the effect of local clinic violence across all abortion markets is an important area for future research.

VI. Conclusions

Past work on the consequences of terrorism focuses mainly on its effects on financial markets (Berrebi and Klor, 2006), the broader economy (Eckstein and Tsiddon, 2004), or both (Abadie and Gardeazabal, 2003). In this study, we try to isolate the effects of terror on the very market it is trying to disrupt – the market for abortions. This is a large and important market, with 1 in 3 women in the United States having an abortion in her lifetime and women from vastly differing demographic groups, whether measured by education, race or age, utilizing this market. These women and the providers who perform abortions have been the targets of repeated acts of terrorism in the United States.

In the 1980s and 1990s, radical anti-abortion activists unleashed a storm of violent attacks against abortion clinics and providers. Clinic arsons, bombings and even staff murders became widely-publicized tools in the anti-abortion effort to limit access to abortion services. Although these acts are less common today, the uncertainty and fear they instilled in the past may have had long-term impacts on the market for abortions. Moreover, survey evidence indicates that abortion facility workers are still fearful of the violence today. The recent murder of Tiller has likely compounded this fear. Additionally, extreme anti-abortion activists have warned that today's political climate may be the impetus for violence in the future.

To our knowledge, this study is the first to systematically assess the impact of anti-abortion violence on the market for abortion services. Our results suggest that extreme acts of clinic violence cause a small but significant decline in the number of clinic-based providers per 100,000 women of childbearing age in targeted areas. These acts also reduce the rate of abortions in these areas, specifically those performed in non-hospital settings and in relatively high-volume facilities. Considering only abortion and provider rates in affected counties captures only part of the picture. Abortion rates rise in counties neighboring the violence. The decline in abortions in attacked counties is almost completely offset by increases in counties within 50 miles of the attack.

Our findings indicate that violence affects the market for abortions in much the same way as legislative abortion restrictions – both in terms of the size of the effect and the response of women who might be seeking an abortion. For example, parental involvement laws appear to cause sizeable declines in abortion rates, typically on the order of 15 to 25 percent, when measured by state of occurrence (Charlotte Ellerston, 1997; Deborah Haas-Wilson, 1996; Rogers et al., 1991). But birth rates increase only modestly, if at all, in response to these laws, implying that the decline in abortions in impacted states is counterbalanced with a rise in abortions in neighboring states (Virginia G. Cartoof and Lorraine V. Klerman, 1986; Ellertson, 1997; Haas-Wilson, 1996).

Based on this literature on legislative barriers as well as our own findings on anti-abortion violence, we conclude that the demand for abortions is extremely inelastic. Locational choice elasticities are much higher. When the "costs" of abortions rise and there are substitute facilities in nearby areas, many women seeking to terminate their pregnancies simply travel to less restrictive or safer, more hospitable environments to do so.

Although violence led to declines in abortion rates in targeted counties that persist for many years, these attacks did not have the intended effect of grinding the market for abortions to a halt. Once travel is taken into account, the net effect of violence on abortion rates is not long-lived. This finding is made clearest when considering births. In response to anti-abortion violence, birth rates increase only modestly, on the order of 1 percent, in the short run with a return to normal in the long run.

Interestingly, we also show that the effects of the terrorism are not widespread. The violence does not impact the decisions of women in counties near the targeted counties. If we interpret this as a behavioral rather than a mechanical response, this means that a person's fear is based on local experiences. This finding is consistent with evidence of the effects of September 11th on psychological stress: individuals living farther from New York City were less likely to report symptoms of post-traumatic stress (Krueger 2007). More generally, our results suggest that where consumers are mobile and their demand inelastic, terrorist attacks will have only limited effects on the markets they are trying to disrupt.

We conclude that anti-abortion terrorists were ultimately unsuccessful in obstructing the market for abortions services. The net effect of anti-abortion violence is a geographic expansion in the market for abortion services not a reduction in abortions themselves. Whether it is appropriate to characterize these effects as small or large in magnitude is up for debate. According to Krueger (2007), an appropriate evaluation of this question depends on the existence of substitutes. In the case of abortion violence, in areas less dense with providers where the availability of substitute facilities may be limited, we observe the largest effects of violence on short-run births. But, over time, the market adapts as in the case of office space in New York after 9/11 (Krueger 2007). Ultimately, while the impact of terrorism on net abortions is small, such violence imposes high costs on the women receiving and the providers performing these abortions.

References

Abadie, Alberto. 2006. "Poverty, Political Freedom, and the Roots of Terrorism." *The American Economic Review,* 96(2): 50-56.

Abadie, Alberto and Javier Gardeazabal. 2003. "The Economic Costs of Conflict: A Case Study of the Basque Country." *The American Economic Review,* Vol. 93(1): 113-132.

Becker, Gary, and Yona Rubinstein, 2010. "Fear and the Response to Terrorism: An Economic Analysis," University of Chicago mimeo.

Berman, Eli and David D. Laitin. 2008. "Religion, Terrorism and Public Goods: Testing the Club Model." *Journal of Public Economics,* 92(10-11): 1942-1967.

Berrebi, Claude, and Esteban F. Klor. 2006. "On Terrorism and Electoral Outcomes: Theory and Evidence from the Israeli-Palestinian Conflict." *Journal of Conflict Resolution*, 50(6): 899-925.

Bitler, Marianne and Madeleine Zavodny. 2001. "The Effect of Abortion Restrictions on the Timing of Abortions." *Journal of Health Economics*, 20(6):1011-1032.

Blank, Rebecca, Christine C. George, and Rebecca A. London. 1996. "State Abortion Rates: The Impact of Policies, Providers, Politics, Demographics, and Economic Environment." *Journal of Health Economics*, 15: 513-553.

Cartoof, Virginia G. and Lorraine V. Klerman. 1986. "Parental Consent for Abortion: Impact of the Massachusetts Law." *American Journal of Public Health*, 76(4): 397-400.

Cozzarelli, Catherine and Brenda Major. 1994. "The Effects of Anti-Abortion Protesters and Pro-Choice Escorts on Women's Psychological Responses to Abortion." *Journal of Social and Clinical Psychology,* 13: 404-427.

Crump, Richard, Joseph Hotz, Guido Imbens, and Oscar Mitnik. 2009. "Dealing with Limited Overlap in Estimation of Average Treatment Effects." *Biometrika*, 96(1):187-199.

Currie, Janet, Lucia Nixon, and Nancy Cole. 1997. "Restrictions on Medicaid Funding of Abortions: Effects on Birthweight and Pregnancy Resolutions." *The Journal of Human Resources*, 31(11): 159-188.

Doan, Alesha E., 2007. *Opposition and Intimidation The Abortion Wars and Strategies of Political Harassment*. Ann Arbor: University of Michigan Press.

Ellertson, Charlotte. 1997. "Mandatory parental involvement in minors' abortions: Effects of the laws in Minnesota, Missouri, and Indiana." *American Journal of Public Health*, 87(8): 1367–1374.

Eckstein, Zvi and Daniel Tsiddon. 2004. "Macroeconomic Consequences of Terror: Theory and the Case of Israel." *Journal of Monetary Economics*, 51(5): 971-1002.

Eisinger, Peter. 2004. "The American City in the Age of Terror - A Preliminary Assessment of the Effects of September 11." *Urban Affairs Review*, 40: 115-130.

Finer, Lawrence B. and Stanley K. Henshaw. 2003. "Abortion Incidence and Services in the United States in 2000." *Perspectives on Sexual and Reproductive Health*, 35(1): 6–15.

Fitzpatrick, Kevin M., and Michele Wilson. 1999. "Exposure to Violence and Posttraumatic Stress Symptomatology among Abortion Clinic Workers." *Journal of Traumatic Stress*, 12(2): 227-242.

Ginsburg, Faye. 1998. "Rescuing the Nation: Operation Rescue and the Rise of Anti-Abortion Militance." in *Abortion Wars: A Half Century of Struggle*, Rickie Solinger, 227-250. Berkeley and Los Angeles: University of California Press.

Glaeser, Edward L., and Jesse Shapiro. 2002. "Cities and Warfare: The Impact of Terrorism on Urban Form." *Journal of Urban Economics*, 51: 205-24.

Gould, Eric D., and Esteban F. Klor. 2009. "Does Terrorism Work?" Unpublished.

Greenbaum, Robert T., and Andy Hultquist. 2006. "The Economic Impact of Terrorist Incidents on the Italian Hospitality Sector." *Urban Affairs Review*, 42(1): 113-130.

Haas-Wilson, Deborah. 1996. "The Impact of State Abortion Restrictions on Minors' Demand for Abortions." *The Journal of Human Resources*, 31(1): 140-158.

Henshaw, Stanley K., and Lawrence B. Finer. 2003. "The Accessibility of Abortion Services in the United States, 2001." *Perspectives on Sexual and Reproductive Health*, 35(1): 16-24.

Henshaw, Stanley K.. 1995. "Factors Hindering Access to Abortion Services." *Family Planning Perspectives*, 27(2): 54-59.

Jones, Rachel K., Jacqueline E. Darroch, and Stanley K. Henshaw. 2002. "Patterns in the Socioeconomic Characteristics of Women Obtaining Abortions in 2000-2001." *Perspectives on Sexual and Reproductive Health*, 34(5): 226-235.

Joyce, Theodore, Robert Kaestner, and Silvie Colman. 2006. "Changes in Abortions and Births

Following Texas's Parental Notification Law." *New England Journal of Medicine,* 354 (10): 1031-1038.

Joyce, Theodore, and Robert Kaestner. 2001. "The Impact of Mandatory Waiting Periods and Parental Consent Laws on the Timing of Abortion and State of Occurrence among Adolescents in Mississippi and South Carolina." *Journal of Policy Analysis and Management*, 20: 263-282.

Joyce, Theodore, Stanley K. Henshaw, and Julia DeClerque Skatrud. 1997. "The Impact of Mississippi's Mandatory Delay Law on Abortions and Births." *Journal of the American Medical Association*, 278(8): 653-658.

Kane, Thomas J., and Douglas Staiger. 1996. "Teen Motherhood and Abortion Access." *Quarterly Journal of Economics*, 111(2): 467-506.

Krueger, Alan B. 2007. *What Makes a Terrorist: Economics and the Roots of Terrorism*. Princeton: Princeton University Press.

Krueger, Alan B., and Jitka Maleckova. 2003. "Education, Poverty, and Terrorism: Is There a Causal Connection?" *The Journal of Economic Perspectives*, 17(4): 119-144.

Jaeger, David A. and Daniele M. Paserman. 2008. "The Cycle of Violence? An Empirical Analysis of Fatalities in the Palestinian-Israeli Conflict." *The American Economic Review*, 98(4): 1591-1604.

Pridemore, William A. and Joshua D. Frielich. 2007. "The Impact of State Laws Protecting Abortion Clinics and Reproductive Rights on Crimes Against Abortion Providers: Deterrence, Backlash, or Neither?" *Law and Human Behavior*, 31(6): 611-627.

Rogers, James L., Robert Boruch, George B. Stoms, and Dorthy DeMoya. 1991. "The Impact of the Minnesota Parental Notification Law on Abortion and Birth." *American Journal of Public Health*, 81(3): 294–298.

Figure 1 – Number of Abortion Clinic Violence Incidents by Year (1976-2005)

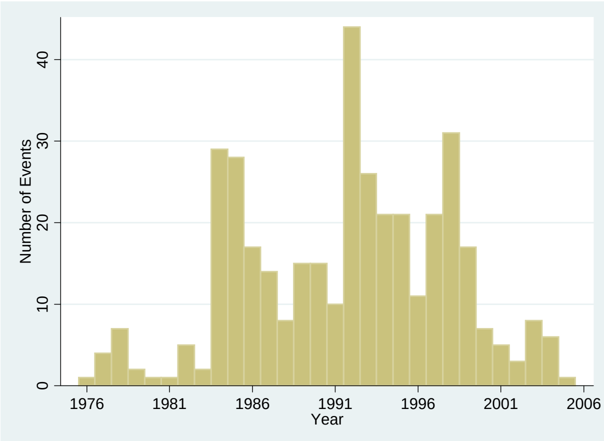
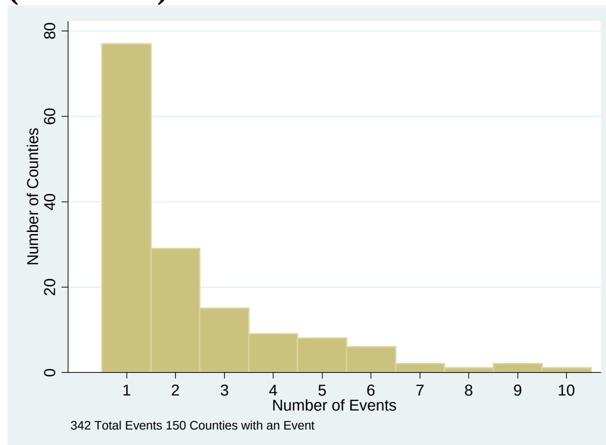


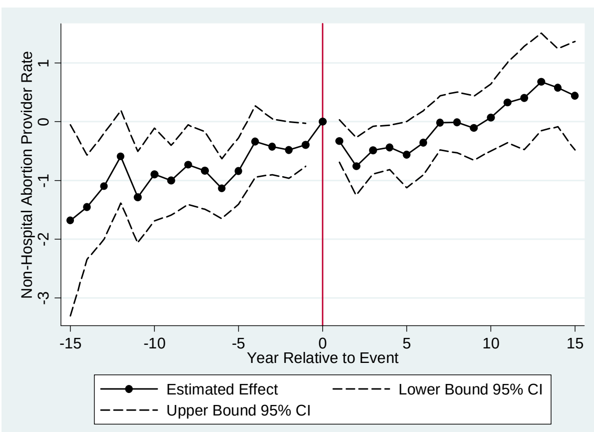
Figure 2 – Distribution of Number of Incidents among Places with Abortions (1976-2005)



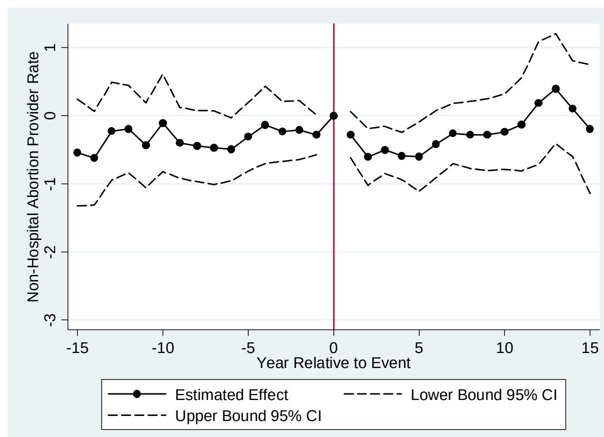
Note: Incidents occurring in the same county on the same day are considered one event in this figure. Including these incidents separately, there would be a total of 384 total events.

Figure 3 – Event Study of the Effect of Violence on Non-Hospital Abortion Provider Rates

Panel A: Full Sample



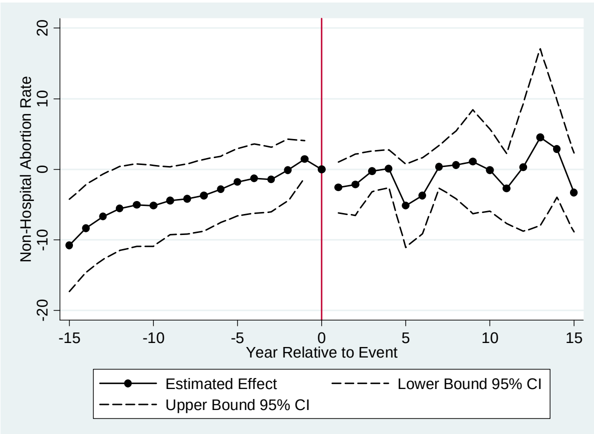
Panel B: with a Propensity of Violence Between 0.1 and 0.9



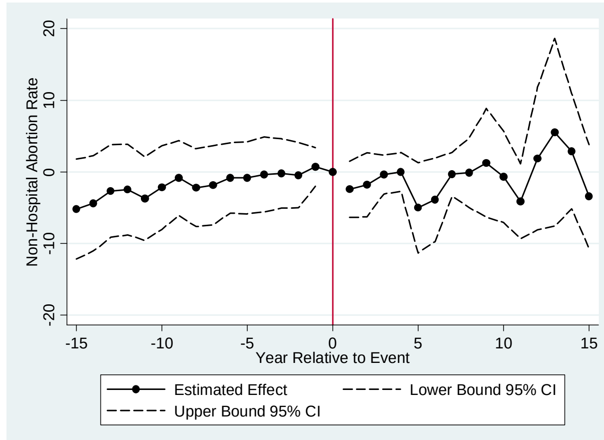
Notes: The figures above depict coefficient estimates of year-relative-to-event fixed effects from a regression of provider rates on county fixed effects, county-level covariates (see main text for a full description of these covariates), and state-level abortion policy variables. The excluded dummy is the year-relative-to-event-equal-to-0 dummy. The dashed lines represent the upper and lower bounds of the 95-percent confidence intervals of the year-relative-to-event fixed effects. The standard errors of these fixed effects are adjusted for within-county correlation. The sample includes data from 15 years prior to the violence and up to 15 years after the violence.

Figure 4 – Event Study of the Effect of Violence on Non-Hospital Abortion Rates

Panel A: Full Sample



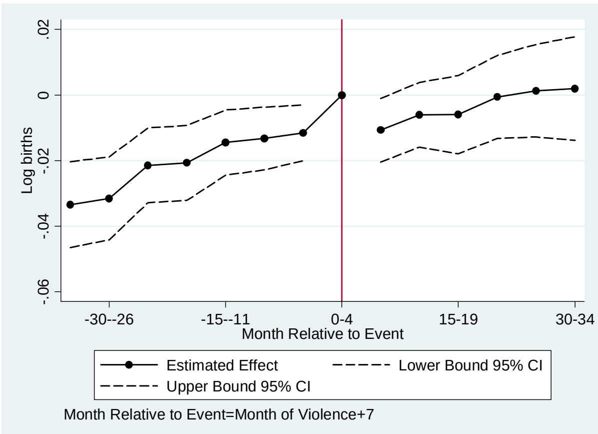
Panel B: with a Propensity of Violence Between 0.1 and 0.9



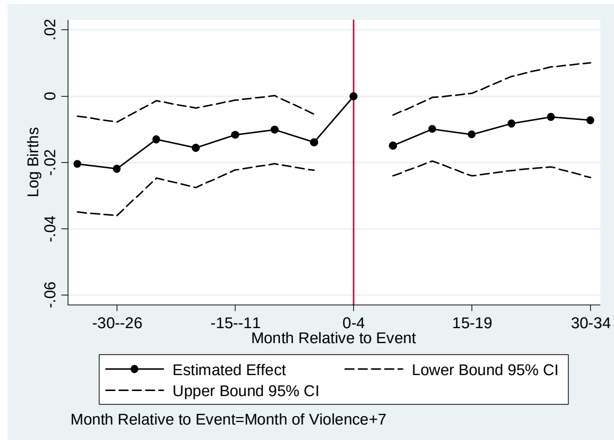
Notes: The figures above depict coefficient estimates of year-relative-to-event fixed effects from a regression of abortion rates on county fixed effects, county-level covariates (see main text for a full description of these covariates), and state-level abortion policy variables. The excluded dummy is the year-relative-to-event-equal-to-0 dummy. The dashed lines represent the upper and lower bounds of the 95-percent confidence intervals of the year-relative-to-event fixed effects. The standard errors of these fixed effects are adjusted for within-county correlation. The sample includes data from 15 years prior to the violence and up to 15 years after the violence.

Figure 5 – Event Study of the Effect of Violence on Log Births

Panel A: Full Sample



Panel B: with a Propensity of Violence Between 0.1 and 0.9



Notes: The figures above depict coefficient estimates of month-relative-to-event fixed effects (in 5-month groupings) from a regression of log births on county fixed effects and month x year fixed effects. The excluded dummy is the month-relative-to-event-equal-to-0 dummy. The dashed lines represent the upper and lower bounds of the 95-percent confidence intervals of the month-relative-to-event fixed effects. The standard errors of these fixed effects are adjusted for within-county correlation. The sample includes data from 3 years prior to the violence and up to 3 years after the violence. The regression includes only counties in the Alan Guttmacher Institute abortion data. The monthly data are adjusted for the length of the month.

Table 1 - Extreme Acts of Abortion Clinic Violence: 1976-2004

Year	Arson	Bomb	Murder	Acid	Notes and Damages (in 2000$)
1976	1				1st recorded attack on abortion provider. Planned Parenthood in OR sustains about $60,000 in damage.
1977	4				Attacks on clinics in MN, VT, NE, OH cause over $1.1 million in damage.
1978	3	4			All but 2 of the attacks are in OH. Damages are about $800,000.
1979	1	1			Arson in NY causes approximately $250,000 in damage.
1980	1				One attack on a TX clinic causes $320,000 in damage.
1981	1				Michigan clinic attack causes $57,000 in damage.
1982	4	1			Attacks in IL, FL (2) and VA result in over $1.1 million in damage. Those convicted in FL and VA attacks also responsible for the 1982 kidnapping of an IL physician and his wife.
1983	2				Attacks in VA and WA cause over $500,000 in damage.
1984	6	23			Attacks spread over 9 states and DC. Damages total about $4.3 million.
1985	17	11			Attacks spread over 10 states and DC. Damages total about $3.8 million.
1986	9	8			Total attacks decline but they are spread over 9 states and cause over $2.2 million in damage.
1987	7	7			Attacks continue to decline but still touch 8 states. Dollar loss falls to under $100,000.
1988	7	1			8 attacks spread over 5 states. Damages are over $245,000.
1989	9	6			Attacks pick up. 15 incidents cause $464,000 in damage across 11 states.
1990	9	7			16 attacks occur across 9 states causing about $140,000 in damage.
1991	7	4			11 attacks occur in 7 states, causing approximately $1.3 million in damage.
1992	16	4	1 (attempt)	31	Butyric acid, a noxious chemical with a vomit-like odor, is first used in 13 states. Providers in 8 other states face different threats. The first attempt on a provider's life is reported. Damages reach almost $1.8 million.
1993	10	7	1 and 1 (attempt)	7	Dr. David Gunn is shot outside of his office. Butyric acid attacks decline. But damages from all attacks reach almost $4.6 million.
1994	9	4	4 and 1 (attempt)	6	Employees at 2 clinics are killed. Damages from arson, bombings, and acid attacks total $1.4 million.
1995	15	6			Attacks decline in number and estimated damages fall to $285,000.
1996	6	2			Arson and bombings spread over 7 states causes about $215,000 in damage.
1997	13	7	1 (attempt)		Attacks in 13 states and DC cause over $1.7 million in damages.
1998	6	4	1	19	Butyric acid attacks occur across clinics in FL, LA and TX between May and July. We have no good estimates of damage from these attacks. Damage from other attacks is just under $100,000.
1999	8	3			Incidents decline but damages are over $173,000.
2000	4	3			Incidents continue to decline. Damages are $32,000.
2001	3	2			Incidents continue to decline. Damages are $250,000.
2002	2				Incidents remain rare. Damages are below $1,000.
2003	3	2			Damages total about $21,000.
2004	3				Damages total about $92,000.

Notes: Data are from the Bureau of Alcohol, Tobacco, Firearms and Explosives, the National Abortion Federation, refuseandresist.org and LexisNexis searches. Bomb threats, hoaxes and acts of vandalism are not counted in the table, although they are in our data and in our counts of total acts of terrorism against abortion providers. The murder tally indicates the number of people killed in anti-abortion violence in any given year. An attack that involves multiple homicides is counted as one incident in the analysis.

Table 2 - Comparison of Counties Experiencing Act of Violence and Counties Not Experiencing Act of Violence

	Non-Violence Counties	Violence Counties	Counties Within 50 Miles of Violence Counties
Number of Providers	1.40	8.66	3.27
	[3.16]	[15.34]	[9.12]
Providers per 100,000 Women Ages 15-44			
Overall	6.56	6.15	5.42
	[12.00]	[5.95]	[7.80]
Non-Hospital-Based	1.98	4.05	1.79
	[5.74]	[5.28]	[3.80]
Provider volume <30 abortions per year	3.74	1.47	2.74
	[10.42]	[2.34]	[6.57]
Abortions per 1,000 Women Ages 15-44			
Overall	9.35	38.65	13.90
	[31.60]	[31.12]	[26.75]
Non-Hospital-Based	7.51	34.01	11.18
	[30.48]	[27.20]	[23.55]
Provider volume: 400-999 abortions per year	1.71	5.77	2.03
	[9.39]	[8.42]	[6.36]
Birth rate	66.33	65.36	63.74
	[14.55]	[9.57]	[11.46]
Females Aged 15-44	27129	154746	63460
	[45000]	[230377]	[138480]
Real per capita income (2000$)	18883	23645	21048
	[4985]	[6694]	[6118]
Ratio of unemployment benefits to income	0.007	0.005	0.007
	[0.006]	[0.004]	[0.006]
Number of counties x year observations	15993	2194	7440

Notes: The numbers in this table without square brackets represent means for the listed variables. The numbers in square brackets represent the corresponding standard deviations. The per capita income is deflated by the personal consumption expenditures deflator from the Bureau of Economic Analysis. Violence county and neighboring county observations are limited to the 15 years before and after a violent attack. Neighboring counties can include counties that were ever attacked and only include those with providers.

Table 3 - The Effect of Violence on Provider and Abortion Rates Overall and by Setting

Dependent Variable:	Provider Rate				Abortion Rate			
Post-Event Dummy	-0.56	-0.57	-0.60	-0.40	-3.21	-3.39	-3.22	-3.14
	(0.31)	(0.31)	(0.33)	(0.50)	(1.44)	(1.46)	(1.45)	(1.49)
Controls								
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	NO	YES	YES	YES	NO	YES	YES	YES
Policy Variables	NO	NO	YES	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Number of Observations	18187	18187	18187	18187	18187	18187	18187	18187
Mean of Dependent Variable	6.68	6.68	6.68	6.68	38.18	38.18	38.18	38.18

Dependent Variable:	Hospital Provider Rate				Hospital Abortion Rate			
Post-Event Dummy	-0.03	-0.02	-0.09	-0.01	0.32	0.30	0.28	0.29
	(0.12)	(0.13)	(0.15)	(0.30)	(0.33)	(0.33)	(0.32)	(0.32)
Controls								
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	NO	YES	YES	YES	NO	YES	YES	YES
Policy Variables	NO	NO	YES	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Number of Observations	18187	18187	18187	18187	18187	18187	18187	18187
Mean of Dependent Variable	2.49	2.49	2.49	2.49	5.47	5.47	5.47	5.47

Dependent Variable:	Non-Hospital Provider Rate				Non-Hospital Abortion Rate			
Post-Event Dummy	-0.53	-0.55	-0.51	-0.40	-3.52	-3.69	-3.50	-3.43
	(0.25)	(0.25)	(0.25)	(0.37)	(1.45)	(1.47)	(1.46)	(1.51)
Controls								
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	NO	YES	YES	YES	NO	YES	YES	YES
Policy Variables	NO	NO	YES	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	YES	NO	NO	NO	YES
Number of Observations	18187	18187	18187	18187	18187	18187	18187	18187
Mean of Dependent Variable	4.19	4.19	4.19	4.19	32.70	32.70	32.70	32.70

Notes: Each column represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (2). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The sample includes all counties reporting some abortion activity during the 1973-2000 period. The county-level covariates include log per-capita income, log employment, share of employment in construction, share of income paid in unemployment income, and the share of females that are non-white. The policy variables are separate indicators equal to 1 if (1) a Medicaid-eligible woman in that state has no access to public funding for an abortion, (2) a teenager seeking an abortion must either notify her parents or have explicit parental consent, (3) a woman must receive counseling prior to obtaining an abortion, (4) a woman must wait a period of time (usually 24 hours) between her initial visit and the abortion procedure, (5) the state has "targeted regulations" for abortion providers, (6) the state prohibits individuals from obstructing access to an abortion clinic, (7) the state imposes civil or criminal penalties for using force civil or criminal penalties for damaging clinic property. The presented mean represents the mean for all counties experiencing violence in the years prior to violence. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44.

Table 4 - The Effect of Violence on Provider and Abortion Rates by Provider Volume

Panel A: Provider Results

	Dependent Variable: Provider Rate			
Provider Volume of	<30	30-399	400-999	1000+
Post-Event Dummy	-0.01	-0.25	-0.28	-0.05
	(0.19)	(0.23)	(0.12)	(0.09)
Controls				
County Fixed Effects	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES
Number of Observations	18187	18187	18187	18187
Mean of Dependent Variable	1.59	2.92	0.91	1.27

Panel B: Abortion Results

	Dependent Variable: Abortion Rate			
Provider Volume of	<30	30-399	400-999	1000+
Post-Event Dummy	-0.01	0.10	-1.85	-1.45
	(0.03)	(0.30)	(0.84)	(1.51)
Controls				
County Fixed Effects	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES
Number of Observations	18187	18187	18187	18187
Mean of Dependent Variable	0.20	4.04	5.93	28.01

Notes: Each column in this table represents a separate regression. In panel A, the dependent variables are for different-sized categories of providers (<30 abortions provider, 30-399 abortions provider, 400-999 abortions provider, and 1000+ abortions provider). In panel B, we classify attacked counties into quartiles based on the pre-violence provider rates; we use data from all non-attacked counties in all regressions. Counties with a provider rate in the lowest quartile are denoted by the [0, 25] group, counties with a provider rate in the interquartile range are represented by the (25, 75) group, and counties with a provider rate in the highest quarter are classified into the [75, 100] group. The post-event dummy estimates are estimates of β_2 from equation (2). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The sample includes all counties reporting some abortion activity during the 1973-2000 period. See notes to Table 3 for details of the county level covariates, policy variables and definition on the dependent variable means.

Table 5 - The Effect of Violence on Provider and Abortion Rates - Differential Effects by Pre-Attack Provider Rate

Dependent Variable:	Provider Rate			Abortion Rate		
Provider Rate Pre-Attack Distribution	[0, 25]	(25, 75)	[75,100]	[0, 25]	(25, 75)	[75,100]
Post-Event Dummy	-0.01	-0.12	-2.27	-3.85	-2.91	-4.65
	(0.36)	(0.26)	(1.10)	(2.94)	(2.01)	(3.36)
Controls						
County Fixed Effects	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES
State x Year Fixed Effects	NO	NO	NO	NO	NO	NO
Number of Observations	16513	17124	16536	16513	17124	16536
Mean Provider Rate	3.02	5.78	12.36	25.33	36.54	54.89

Dependent Variable:	Hospital Provider Rate			Hospital Abortion Rate		
Provider Rate Pre-Attack Distribution	[0, 25]	(25, 75)	[75,100]	[0, 25]	(25, 75)	[75,100]
Post-Event Dummy	-0.02	0.13	-0.70	0.35	-0.24	1.35
	(0.24)	(0.17)	(0.43)	(0.42)	(0.26)	(1.16)
Controls						
County Fixed Effects	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES
State x Year Fixed Effects	NO	NO	NO	NO	NO	NO
Number of Observations	16513	17124	16536	16513	17124	16536
Mean Provider Rate	1.42	2.35	3.91	1.09	4.15	12.76

Dependent Variable:	Non-Hospital Provider Rate			Non-Hospital Abortion Rate		
Provider Rate Pre-Attack Distribution	[0, 25]	(25, 75)	[75,100]	[0, 25]	(25, 75)	[75,100]
Post-Event Dummy	0.01	-0.25	-1.57	-4.20	-2.67	-6.00
	(0.21)	(0.17)	(0.88)	(2.85)	(2.00)	(3.42)
Controls						
County Fixed Effects	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES
State x Year Fixed Effects	NO	NO	NO	NO	NO	NO
Number of Observations	16513	17124	16536	16513	17124	16536
Mean Provider Rate	1.60	3.43	8.45	24.24	32.39	42.13

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (2). We classify attacked counties into quartiles based on their pre-violence provider rates; we use data from all non-attacked counties in all regressions. Counties with a provider rate in the lowest quartile of this distribution are denoted by the [0, 25] group in this table, counties with a provider rate in the interquartile range are represented by the (25, 75) group, and counties with a provider rate in the highest quarter are classified into the [75, 100] group. For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The sample includes all counties reporting some abortion activity during the 1973-2000 period. The county-level covariates include log per-capita income, log employment, share of employment in construction, share of income paid in unemployment income, and the share of females that are non-white. The density of women of childbearing age is the population of women ages 15 to 44 (in 1000s) per square mile of land in the county; it is measured in the year of a county was attacked.

See Table 3 for a description of the policy variables. The presented mean represents the mean for all counties experiencing violence in the years prior to violence. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44

Table 6 - The Effect of Violence by Type of Violence

	Provider Rates					
	Hospital	Non-hospital	<30	30-399	400-999	1000+
Post-Damage Attack Dummy	-0.08	-0.45	0.0002	-0.22	-0.27	-0.04
	(0.15)	(0.25)	(0.1953)	(0.23)	(0.12)	(0.09)
Post-Murder Dummy	0.31	-3.41	-0.10	-2.11	-0.50	-0.33
	(0.87)	(0.87)	(1.78)	(0.87)	(0.30)	(0.16)
Post-Other Violence Dummy	-1.55	-0.34	-0.86	-0.18	-0.52	-0.25
	(0.97)	(0.90)	(0.77)	(0.82)	(0.40)	(0.37)
Observations	18187	18187	18187	18187	18187	18187
Mean Abortion/Provider Rate	2.49	4.19	1.59	2.92	0.91	1.27

	Abortion Rates					
	Hospital	Non-hospital	<30	30-399	400-999	1000+
Post-Damage Attack Dummy	0.25	-2.92	-0.00002	0.09	-1.79	-0.96
	(0.32)	(1.45)	(0.03498)	(0.31)	(0.85)	(1.50)
Post-Murder Dummy	1.14	-26.58	-0.16	-1.74	-3.08	-20.46
	(1.65)	(14.60)	(0.15)	(1.27)	(2.18)	(14.62)
Post-Other Violence Dummy	1.15	-8.87	-0.12	2.80	-3.94	-6.46
	(0.71)	(7.55)	(0.13)	(2.92)	(2.89)	(8.40)
Observations	18187	18187	18187	18187	18187	18187
Mean Abortion/Provider Rate	5.47	32.70	0.20	4.04	5.93	28.01

Notes: Each column in this table represents a separate regression. The post-event dummies are similar to that in equation (2) but broken out by type of incident. Damage includes arson, butyric acid, and bombing. Murder includes murder and attempted murder. "Other violence" include hoax devices, major threats, kidnappings, and protests. All regressions control for county fixed effects in addition to the full set of policy variables and the county-level covariates in Tables 3-4. Robust standard errors clustered at the county-level are presented in parentheses. The sample includes all counties reporting some abortion activity during the 1973-2000 period. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44. The mean of abortions and provider rates by type or volume represents the mean for all counties experiencing violence in the years prior to violence.

Table 7 - The Effect of Violence on Log Births in Targeted Counties

	Sample: All Counties in Abortion Data		Sample: All Counties	
Post-Event Dummy	1.849	0.009	2.842	0.010
	(0.094)	(0.004)	(0.094)	(0.004)
Controls				
County Fixed Effects	NO	YES	NO	YES
Observations	293165	293165	958773	958773
Mean Number of Births/Month	852	852	829	829

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (3); the dummy is defined to be equal to 1 for births occurring 7 to 11 months after the violence and 0 otherwise. Robust standard errors clustered at the county-level are presented in parentheses. The presented means represent the means for all counties experiencing violence in the months prior to violence.

Table 8 - The Effect of Violence on Targeted Counties and Counties Within 50 Miles

Panel A: Provider and Abortion Rates in Targeted Counties and their Neighbors

	Provider Rates		Abortion Rates	
	Hospital	Non-hospital	Hospital	Non-hospital
Post-Event Dummy	-0.17	-0.54	0.37	-3.85
	(0.16)	(0.26)	(0.33)	(1.51)
Post-Event Dummy * Neighbor	0.53	0.22	-0.57	2.47
Population Share	(0.27)	(0.13)	(0.32)	(1.22)
Observations	18187	18187	18187	18187
Mean of Dependent Var	2.49	4.19	5.47	32.70
Standardized Mean for Neighbors	0.70	0.71	0.62	5.22

Panel B: Births in Targeted Counties and their Neighbors

	Sample: All Counties in Abortion Data		Sample: All Counties	
Post-Event Dummy	1.961	0.009	3.026	0.010
	(0.091)	(0.004)	(0.088)	(0.004)
Post-Event Dummy for Neighbor	0.581	-0.001	0.863	0.001
	(0.067)	(0.003)	(0.045)	(0.003)
Controls				
County Fixed Effects	NO	YES	NO	YES
Observations	206029	206029	745614	745614
Mean Number of Births/Month	357	357	184	184

Notes:

Panel A : Each column in this panel represents a separate regression. All regressions control for county fixed effects in addition to the full set of policy variables and the county-level covariates in Tables 3-4. We supplement our standard regression with a dummy that equals 1 for counties within 50 miles of an attack in the years following an attack, weighted by the attack-year population share of each neighbor within this area. The coefficient and standard error from the neighbor effects are standardized to the pre-violence means of the female population in the attacked county (i.e., multiplied by the ratio of the pre-violence mean of females ages 15 to 44 in neighboring counties to attacked counties (0.409)). In this way, the effects on attacked counties and their neighbors can be directly compared. Robust standard errors clustered at the county-level are presented in parentheses. The sample includes all counties reporting some abortion activity during the 1973-2000 period. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44. The mean of abortions and provider rates represents the mean in the years prior to the violence for violence counties or their neighbors, as indicated. The mean for neighbors has also been population-standardized.

Panel B : Each column in this panel represents a separate regression. The estimates are based on our standard regression supplemented with a separate post-event dummy for counties within 50 miles of an attack. Both post-event dummies are defined to be equal to 1 for births occurring 7 to 11 months after the violence and 0 otherwise. Robust standard errors clustered at the county-level are presented in parentheses. The presented means represent the means for all counties experiencing violence in the months prior to violence.

Data Appendix

County-Level Abortion and Abortion Provider Data from Alan Guttmacher Institute (AGI)

The Alan Guttmacher Institute (AGI) is a non-profit organization interested in sexual and reproductive health. On a semi-regular basis, AGI attempts to collect data on abortions performed by all abortion providers in the United States. AGI attempts to contact each suspected abortion provider many times. Although incomplete, these data represent nearly a census of all known providers, where providers are facilities that perform some abortions. For example, AGI obtained data for nearly 85 percent of the providers surveyed in 2000 (Finer and Henshaw, 2003). AGI conducted surveys in the years 1973-1982, 1984, 1985, 1987, 1988, 1991, 1992, 1995, 1996, 1999, and 2000. Finer and Henshaw (2003) provide a detailed description of the survey methodology, including the great lengths taken by surveyors to contact all providers.

We acquired confidential county-level AGI data on the number of abortion facilities (providers) and the number of abortions performed in each survey year. We have data on the number of abortion providers or facilities broken down by setting – hospital or non-hospital – and the corresponding number of abortions performed in each setting. We also have the number of providers by volume of the facility (<30 abortions performed, 30-399 abortions, 400-999 abortions, 1000+ abortions) and the total number of abortions performed by each group.

County-Level Natality Data from the National Center for Health Statistics (NCHS)

From the National Center for Health Statistics' Detailed Natality Files, we also have county-level monthly data on births from 1973 to 2002. For this period, we have counts of births for every county regardless of population size. Public-use natality data from 1989 onward exclude county identifiers for counties with fewer than 100,000 persons. Because our birth counts are from non-public-use data, this sample selection criterion does not apply.

Abortion Clinic Violence

The main source of abortion violence data is derived from the Bureau of Alcohol, Tobacco, and Firearms (ATF). All anti-abortion incidents involving explosives fall under the jurisdiction of the ATF. The types of incidents reported to the ATF include arson, bombing, bomb threats, and hoax devices. For the years 1976-2005, there were 287 abortion clinic incidents reported to the ATF.

Independently, the National Abortion Federation (NAF), the Feminist Majority Foundation's "National Clinic Access Project" and refuseandresist.org also report incidents of extreme violence, with the NAF maintaining the most comprehensive list.[1] Lexis-Nexis searches of news articles and television broadcasts helped to validate and augment these data sources.[2] Combining all five sources, there were 342 acts of extreme violence for the 1976 to 2005 period.

[1] The Feminist Majority Foundation has published regular "Anti-Abortion Violence Alerts" since June 1997. See http://www.feminist.org/rrights/antialert.html

[2] Thirteen arson and bombing attempts or attacks were reported to the NAF by member organizations between 1977 and 1982 but were not investigated by the ATF or publicized in any independent source. Including those cases in our analysis does not change the basic pattern of results or the general magnitude of the implied effects.

State-Level Policy Variables

We code up several abortion-related policy variables. Specifically, we include separate indicators equal to 1 if (1) a Medicaid-eligible woman in that state has no access to public funding for an abortion, (2) a teenager seeking an abortion must either notify her parents or have explicit parental consent, (3) a woman must receive counseling prior to obtaining an abortion, (4) a woman must wait a period of time (usually 24 hours) between her initial visit and the abortion procedure, (5) the state has "targeted regulations" for abortion providers, (6) the state prohibits individuals from obstructing access to an abortion clinic, (7) the state imposes civil or criminal penalties for using force or threatening force to prevent access to an abortion clinic, and (8) the state imposes civil or criminal penalties for damaging clinic property.

Our coding of Medicaid funding bans is based on an updating of Blank et al. (1996). Coding of the other laws is based primarily on an extensive database maintained by NARAL as part of its project, "Who Decides? The Status of Women's Reproductive Rights in the United States."[3] Although this database describes changes over time, we also check dates against other sources in the literature (e.g., Joyce et al. 2001) to make sure we are not missing laws that have been previously repealed.

County-Level Covariates

To scale the abortion and abortion provider measures, we use the number of females ages 15 to 44 from US Census Bureau Population Estimates Program. We also use data from the Bureau of Economic Analysis's Regional Economic Information System (REIS) to control for a host of county level covariates – the log per-capita income, log employment, the share of employment in construction, the share of income paid in unemployment income, and the share of females that are non-white.

Lexis-Nexis Newspaper Article Counts

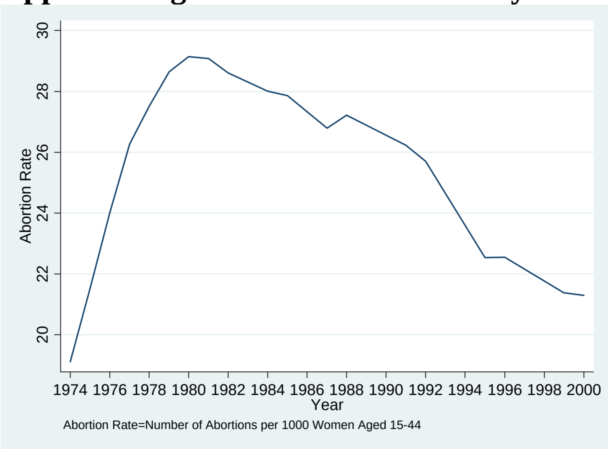
To develop a measure of underlying local abortion discussion, we conducted a systematic search of newspaper articles using the Lexis-Nexis database. Specifically, for each area experiencing an act of violence, we identified a newspaper in the area. For each newspaper outlet, we searched for all articles containing the word "abortion" in the headline.

The coverage rates of newspapers in Lexis-Nexis are quite variable. For some newspapers, many years of articles are available whereas for other newspapers, they are not. Of the 150 counties experiencing at least one event between 1976 and 2005, we were able to extract articles for 59 of them. Recall that in our main sample we have 140 counties because we consider the years 1976-2000 for that sample. For 10 of the 59 counties appearing in the Lexis-Nexis database, Lexis-Nexis had newspaper coverage 12 months prior to the event up until 12 months following the event. The covered areas are as follows: Birmingham, Alabama on 1/29/1998 (Birmingham News); San Francisco, California on 2/28/1995 (San Francisco Chronicle); Modesto, California on 3/19/2003 (Modesto Bee); Denver, Colorado on 8/26/2003 (Denver Post); Washington DC on 7/4/1984 (Washington Post); Miami, Florida on 5/16/1998 (Miami Herald); Buffalo, New York

[3] See http://www.naral.org/choice-action-center/in_your_state/who-decides/state-profiles/california.html

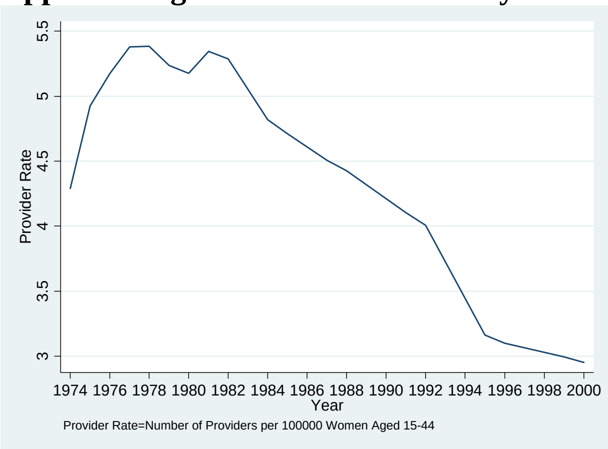
on 4/18/1992 (Buffalo News); New York City, New York on 12/10/1985 (New York Times); Syracuse, New York on 5/23/1990 (Post Standard); and Greensboro, North Carolina on 3/17/1991 (News & Record). While this list is certainly not comprehensive, estimates based on this sample should be informative about the political and social discussion concerning abortion near the time of the abortion violence.

Appendix Figure 1 – Abortion Rate by Year



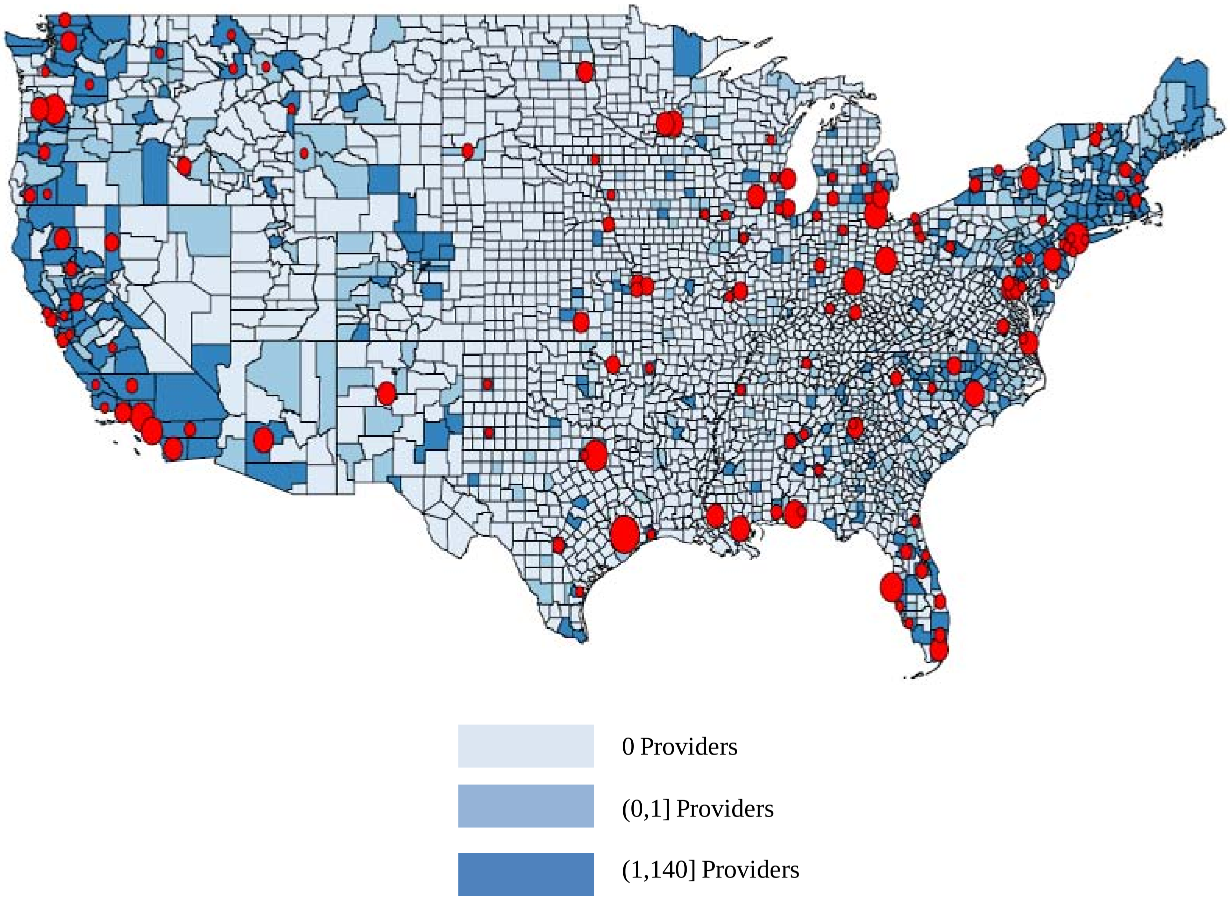
Notes: Based on county-level data from the Alan Guttmacher Institute. Data are unavailable for the years 1983, 1986, 1989, 1990, 1993, 1994, 1997, and 1998.

Appendix Figure 2 –Provider Rate by Year



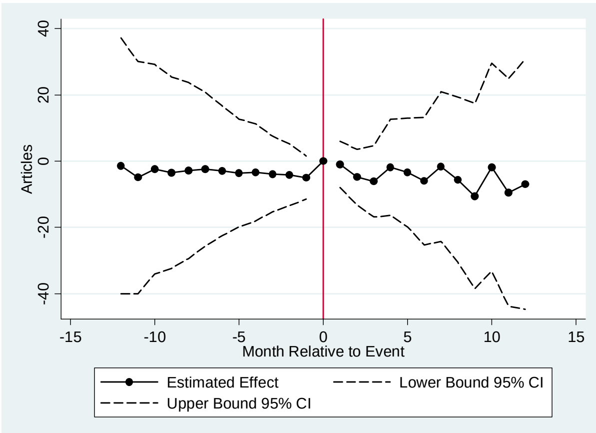
Notes: Based on county-level data from the Alan Guttmacher Institute. Data are unavailable for the years 1983, 1986, 1989, 1990, 1993, 1994, 1997, and 1998.

Appendix Figure 3 – 1981 Providers by County and the Location of Violent Attacks



Notes: The map shows the number of providers by county in 1981, a year without any recorded attacks and prior to the surge in anti-abortion violence in the United States. The dots in red show the location of anti-abortion attacks from 1973 to 2000 and are proportional to the total number of attacks over this period.

Appendix Figure 4 – Abortion Newspaper Articles Over Time



Notes: The figure above depicts coefficient estimates of month-relative-to-event fixed effects from a regression of the number of newspaper articles on month-relative-to-event fixed effects, month x year fixed effects, and county fixed effects. The excluded dummy is the month-relative-to-event-equal-to-0 dummy. The dashed lines represent the upper and lower bounds of the 95 percent confidence intervals of the month-relative-to-event fixed effects. The standard errors of these fixed effects are adjusted for within-county correlation. The counts of the number of newspaper articles come from a Lexis-Nexis search of articles with “abortion” in the headline.

Appendix Table 1 - The Effect of Violence on Provider and Abortion Rates: Estimates Using Narrower Windows Around the Violence

Panel A: Provider Results

	Dependent Variable: Provider Rate			Dependent Variable: Hospital Provider Rate			Dependent Variable: Non-Hospital Provider Rate		
Window (years)	12	10	7	12	10	7	12	10	7
Post-Event Dummy	-0.58	-0.48	-0.49	-0.14	-0.06	0.00	-0.44	-0.42	-0.48
	(0.27)	(0.25)	(0.23)	(0.15)	(0.14)	(0.14)	(0.19)	(0.18)	(0.16)
Controls									
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
State x Year Fixed Effects	NO	NO	NO	NO	NO	NO	NO	NO	NO
Number of Observations	17858	17612	17194	17858	17612	17194	17858	17612	17194
Mean Provider Rate	6.48	6.39	6.16	2.37	2.3	2.1	4.11	4.08	4.07

Panel B: Abortion Results

	Dependent Variable: Abortion Rate			Dependent Variable: Hospital Abortion Rate			Dependent Variable: Non-Hospital Abortion Rate		
Window (years)	12	10	7	12	10	7	12	10	7
Post-Event Dummy	-2.83	-2.98	-2.26	0.10	0.29	0.31	-2.93	-3.27	-2.56
	(1.45)	(1.42)	(1.51)	(0.34)	(0.39)	(0.32)	(1.42)	(1.40)	(1.46)
Controls									
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
State x Year Fixed Effects	NO	NO	NO	NO	NO	NO	NO	NO	NO
Number of Observations	17858	17612	17194	17858	17612	17194	17858	17612	17194
Mean Abortion Rate	39.24	39.85	41.02	5.42	5.34	5.02	33.82	34.5	35.99

Notes: Each column in this table represents a separate regression using a window of the specified number of years before and after an attack in counties experiencing violence and all years in counties that did not experience violence in our sample. The main results use a window of 15 years. The post-event dummy estimates are estimates of β_2 from equation (2). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The sample includes all counties reporting some abortion activity during the 1973-2000 period. The county-level covariates include log per-capita income, log employment, share of employment in construction, share of income paid in unemployment income, and the share of females that are non-white. The density of women of childbearing age is the population of women ages 15 to 44 (in 1000s) per square mile of land in the county; it is measured in the year of a county was attacked. See Table 3 for a description of the policy variables. The presented mean represents the mean for all counties experiencing violence in the years prior to violence. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44.

Appendix Table 2 - The Effect of Violence on Provider and Abortion Rate - Propensity-Score Matched Counties

Panel A: Provider Results

	Dependent Variable: Provider Rate					Dependent Variable: Hospital Provider Rate					Dependent Variable: Non-Hospital Provider Rate				
Post-Event Dummy	0.75	-0.48	-0.51	-0.52	-0.40	-0.31	-0.03	-0.06	-0.08	-0.07	1.06	-0.45	-0.45	-0.44	-0.33
	(0.54)	(0.21)	(0.21)	(0.22)	(0.22)	(0.23)	(0.09)	(0.09)	(0.10)	(0.11)	(0.44)	(0.17)	(0.17)	(0.17)	(0.17)
Controls															
County Fixed Effects	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
County-Level Covariates	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Policy Variables	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Number of Observations	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703
Mean of Dependent Variable	6.29	6.29	6.29	6.29	6.29	2.42	2.42	2.42	2.42	2.42	3.87	3.87	3.87	3.87	3.87

Panel B: Abortion Results

	Dependent Variable: Abortion Rate					Dependent Variable: Hospital Abortion Rate					Dependent Variable: Non-Hospital Abortion Rate				
Post-Event Dummy	21.64	-2.76	-2.71	-2.77	-2.75	2.77	0.33	0.30	0.22	0.11	18.87	-3.09	-3.01	-2.99	-2.86
	(3.71)	(1.33)	(1.32)	(1.33)	(1.44)	(1.45)	(0.35)	(0.34)	(0.33)	(0.31)	(3.18)	(1.36)	(1.34)	(1.35)	(1.50)
Controls															
County Fixed Effects	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
County-Level Covariates	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Policy Variables	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Number of Observations	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703	5703
Mean of Dependent Variable	39.85	39.85	39.85	39.85	39.85	5.69	5.69	5.69	5.69	5.69	34.17	34.17	34.17	34.17	34.17

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (2). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. For all counties reporting some abortion activity during the 1973-2000 period, we predicted their propensity for violence using 1973 values for their log per-capita income, log employment, share of employment in construction, share of income paid in unemployment income, and share of females that are non-white. The estimation sample includes all counties with propensity for violence between 0.1 and 0.9. The county-level covariates include log per-capita income, log employment, share of employment in construction, the share of income paid in unemployment income, and the share of females that are non-white. See Table 3 for a description of the policy variables. The presented mean represents the mean for the counties experiencing violence in the estimation sample in the years prior to violence. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44.

Appendix Table 3 - The Effect of Violence on Provider and Abortion Rate, Sample: Counties that Ever Experienced Violence

Panel A: Provider Results

	Dependent Variable: Provider Rate					Dependent Variable: Hospital Provider Rate					Dependent Variable: Non-Hospital Provider Rate				
Post-Event Dummy	-0.11	-0.37	-0.44	-0.41	-0.29	0.01	0.08	0.03	0.03	0.02	-0.12	-0.46	-0.47	-0.44	-0.30
	(0.29)	(0.28)	(0.29)	(0.29)	(0.26)	(0.12)	(0.10)	(0.11)	(0.11)	(0.16)	(0.25)	(0.24)	(0.24)	(0.24)	(0.19)
Controls															
County Fixed Effects	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
County-Level Covariates	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Policy Variables	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Number of Observations	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194
Mean of Dependent Variable	6.68	6.68	6.68	6.68	6.68	2.49	2.49	2.49	2.49	2.49	4.19	4.19	4.19	4.19	4.19

Panel B: Abortion Results

	Dependent Variable: Abortion Rate					Dependent Variable: Hospital Abortion Rate					Dependent Variable: Non-Hospital Abortion Rate				
Post-Event Dummy	-2.79	-2.09	-2.00	-2.20	-3.17	0.01	0.16	0.11	-0.02	-0.40	-2.80	-2.25	-2.10	-2.18	-2.77
	(1.97)	(1.40)	(1.36)	(1.36)	(1.68)	(0.81)	(0.30)	(0.30)	(0.31)	(0.28)	(1.79)	(1.40)	(1.37)	(1.35)	(1.70)
Controls															
County Fixed Effects	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
County-Level Covariates	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Policy Variables	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO
State x Year Fixed Effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Number of Observations	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194	2194
Mean of Dependent Variable	38.18	38.18	38.18	38.18	38.18	5.47	5.47	5.47	5.47	5.47	32.70	32.70	32.70	32.70	32.70

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (2). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The estimation sample includes only counties experiencing violence during the 1973-2000 period. The county-level covariates include log per-capita income, log employment, share of employment in construction, share of income paid in unemployment income, and the share of females that are non-white. See Table 3 for a description of the policy variables. The presented mean represents the mean for all counties experiencing violence in the years prior to violence. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44.

Appendix Table 4 - The Effect of Violence on Provider and Abortion Rates Including Linear County-Specific Trends

Panel A: Provider Results

Dependent Varable:	Provider Rate			Hospital Provider Rate			Non-Hospital Provider Rate		
Post-Event Dummy	-0.36	-0.40	-0.32	0.06	-0.03	0.05	-0.41	-0.37	-0.37
	(0.63)	(0.18)	(0.21)	(0.54)	(0.11)	(0.09)	(0.31)	(0.13)	(0.18)
Sample	Full	P-score	Violence only	Full	P-score	Violence only	Full	P-score	Violence only
Controls									
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
Linear County Trends	YES	YES	YES	NO	YES	YES	YES	YES	YES
Number of Observations	18187	5703	2194	18187	5703	2194	18187	5703	2194
Mean of Dependent Variable	6.68	6.29	6.68	2.49	2.42	2.49	4.19	3.87	4.19

Panel B: Abortion Results

Dependent Varable:	Abortion Rate			Hospital Abortion Rate			Non-Hospital Abortion Rate		
Post-Event Dummy	-2.01	-2.16	-1.98	0.17	0.31	0.34	-2.19	-2.55	-2.32
	(1.62)	(1.00)	(1.07)	(0.26)	(0.25)	(0.31)	(1.58)	(0.98)	(1.07)
Sample	Full	P-score	Violence only	Full	P-score	Violence only	Full	P-score	Violence only
Controls									
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
County-Level Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
Policy Variables	YES	YES	YES	YES	YES	YES	YES	YES	YES
Linear County Trends	YES	YES	YES	YES	YES	YES	YES	YES	YES
Number of Observations	18187	5703	2194	18187	5703	2194	18187	5703	2194
Mean of Dependent Variable	38.18	39.85	38.18	5.47	5.69	5.47	32.70	34.17	32.70

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (2). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The county-level covariates include log per-capita income, log employment, share of employment in construction, the share of income paid in unemployment income, and the share of females that are non-white. See Table 3 for a description of the policy variables. The presented mean represents the mean for the counties experiencing violence in the estimation sample in the years prior to violence. Provider rates are per 100,000 women aged 15 to 44; abortion rates are per 1,000 women aged 15 to 44.

Appendix Table 5 - The Effect of Violence on Log Birth Rate: Estimates Using Narrower Windows Around the Violence

	Sample: All Counties in Abortion Data				Sample: All Counties			
Window (months)	30	24	18	12	30	24	18	12
Post-Event Dummy	0.009	0.009	0.009	0.011	0.009	0.009	0.009	0.011
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Controls								
County Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Number of Observations	291567	289951	288310	286652	957127	955463	953774	952068
Mean Number of Births/Month	852.26	854.90	855.82	860.27	829.71	832.28	833.16	837.51

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (3) using different window widths; the dummy is defined to be equal to 1 for births occurring 7 to 11 months after the violence and 0 otherwise. Robust standard errors clustered at the county-level are presented in parentheses. The presented means represent the means for all counties experiencing violence in the months prior to violence.

Appendix Table 6 - The Effect of Violence on Log Births - Propensity-Score Matched Counties

Treatment Group = County of Violence

	Sample: Counties in Abortion Data		Sample: All Counties	
Post-Event Dummy	0.415	0.009	0.467	0.009
	(0.080)	(0.003)	(0.081)	(0.003)
Controls				
County Fixed Effects	NO	YES	NO	YES
Observations	60895	60895	65755	65755
Mean Number of Births/Month	912	912	912	912

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (3). The post-event dummy is defined to be equal to 1 for births occurring 7 to 11 months after the violence and 0 otherwise. Robust standard errors clustered at the county-level are presented in parentheses. The presented means represent the means for all counties experiencing violence in the years prior to violence.

Appendix Table 7 - The Effect of Violence on Log Births, Sample: Counties that Ever Experienced Violence

Treatment Group = County of Violence

	Sample: Counties in Abortion Data		Sample: All Counties	
Post-Event Dummy	0.002	0.008	0.008	0.008
	(0.013)	(0.004)	(0.014)	(0.004)
Controls				
County Fixed Effects	NO	YES	NO	YES
Observations	9999	9999	10291	10291
Mean Number of Births/Month	852	852	829	829

Notes: Each column in this table represents a separate regression. Estimates are based on a window of 32 months before and after the first violent attack. The post-event dummy estimates are estimates of β_2 from equation (3). The post-event dummy is defined to be equal to 1 for births occurring 7 to 11 months after the violence and 0 otherwise. Jackknife standard errors clustered at the county-level are presented in parentheses. The presented means represent the means for all counties experiencing violence in the years prior to violence.

Appendix Table 8 - The Effect of Violence on Log Birth Rate

	Sample: All Counties in Abortion Data			Sample: All Counties		
Provider Rate Pre-Attack Distribution, Attacked Counties	[0, 25]	(25, 75)	[75,100]	[0, 25]	(25, 75)	[75,100]
Post-Event Dummy	0.014	0.013	-0.003	0.013	0.014	-0.002
	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)
Controls						
County Fixed Effects	YES	YES	YES	YES	YES	YES
Number of Observations	285657	288143	285697	950973	953459	951013
Mean Number of Births/Month	869.09	966.58	603.74	869.09	966.58	603.74

Notes: Each column in this table represents a separate regression. The post-event dummy estimates are estimates of β_2 from equation (3). For all regressions, robust standard errors clustered at the county-level are presented in parentheses. The counties experiencing violence are divided into three groups based on the provider rate in the county at the time of the violence - bottom quartile denoted [0, 25] in the table, the interquartile range denoted (25,75) in the table, and the top quartile denoted [75,100] in the table. Each regression includes the full set of control counties. The presented means represent the means for counties experiencing violence in the years prior to violence.