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The consequences of limited access to the health care and educational system

The cases of abortion provision and school closure

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Introduction

The research questions that I try to address in this dissertation have recently gained a unique position within the public debate. When I was about to finish the first two Chapters of this work – both dealing with the consequences of supply-side restrictions on abortion access – the U.S. Supreme Court decided to take away the constitutional right to abortion, giving individual states the full power to regulate the matter. A similar story can be told about the last Chapter, which was written right after the first wave of the Covid-19 pandemic, in the attempt to evaluate the consequence of the Italian national lockdown on pupils' school achievement. At that time, we thought that the interest in our work would have been limited, as the emergency was contained and we were about to come back to normal times. Two years later, after many new lockdown measures and almost two years of school disruption, we find our study to be more relevant than ever.

In the first Chapter, I document the effect of supply-side restrictions on abortion access on violence against women. Leveraging on a law that in 2013 caused the closure of nearly half of all abortion clinics in Texas, I estimated a positive relationship between distance to the nearest abortion clinic and violence against women. Chapter 2 deals with the role of conscientious objection among Italian gynecologists in explaining the raising phenomenon of illegal abortions, finding a positive and significant relationship between the share of objecting doctors and the probability of illegal abortion. The conclusive study has been conducted shortly after the first wave of the Covid-19 pandemic of Spring 2020 with the aim of evaluating the impact of the national lockdown on primary school achievements. The analysis reveals that the pandemic had a large negative impact on pupils' performance in mathematics.

The implications of these results are sizable and economically relevant. Women who have been victims of violence experience a disruption in employment (in terms of work time reduction and job loss), job performance (Showalter, 2016), academic achievements (Brewer et al., 2018, Jordan et al., 2014), and physical and mental health (Dutton et al., 2006, Gerlock, 1999). The high share of objectors in Italian hospitals is burdensome both from an economic and health perspective. Evidence shows that many Italian hospitals need to hire external gynecologists to assure abortion provision. Although the efforts to maintain the service, abortion is not easily accessible in many regions (Lalli and Montegiove, 2022), and this reflects in higher rates of abortion outside hospitals as well as in longer waiting times to obtain a termination of pregnancy (Bo et al., 2015). Abortion performed outside the legal setting may be less safe and late-pregnancy abortions have a higher probability of complications (Buehler et al., 1985, Ferris et al., 1996). In addition, anecdotical evidence[1] reveals a misallocation of resources within society created by a health system that hires and promotes doctors on the basis of their choice to object instead of on their talent and productivity. Finally, the sizable negative educational outcome reported for Italian primary school pupils after the Covid-19 lockdown and the consequent increase in inequalities in school performance suggests the possibility of long-term effects on future educational choices and employment paths, as well as, on increasing inequalities in the labor market due to differences in the family of origin backgrounds.

One of the major takeouts from these studies concerns the differential impact that every single policy has across the socioeconomic ladder. The effect of travel distance on violence against women appears particularly strong among Black women. Similarly, the impact of the share of objecting doctors on illegal abortion is driven by the effect for the subpopulation of immigrant women. Regarding the effect of the Covid-19 pandemic on school performances, although the disruption was sizable for all students, the learning loss was larger for the children of low-educated parents who are either best-performing or female.

Restricting access to abortion provision might affect the most those women who don't have the means to obtain an abortion elsewhere, and who have the least social and economic support to handle the consequences of unintended pregnancy. Similarly, limiting time at school would impact the most those kids whose home environment is less conducive to learning: kids with low-educated parents who are not able to effectively help them at home, and pupils from low-income families who don't have a good internet con-

[1] https://www.opendemocracy.net/en/5050/abortion-italy-conscientious-objection/

nection, a computer and a safe and quiet place to concentrate and study.

Policies' side effects may affect population groups differently, in accordance with the capacity of each individual to compensate and adapt to change using his/her own means. In addition, different social categories – such as gender, race, and class – may interact with each other creating additional layers of inequalities in the process of adaptation and reaction to new policies. This is why, in the act of building, implementing, and evaluating new policies, agents should adopt an intersectional lens[2] to prevent these policies from contributing in enlarging existing inequalities both across the socioeconomic and gender scale.

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[2] The term intersectionality derives from the work of Crenshaw (1989) who sought to draw attention to how treatment of African American women within the law needed to be analyzed and understood through the dual lenses of gender and race discrimination.

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Chapter 1

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

The paper documents the effect of supply-side restrictions to abortion access on violence against women. Limiting access to abortion implies higher rates of unintended pregnancies and subsequent lower bargaining power for women. Starting from the evidence of a sharp reduction in the abortion rate and an increase in fertility after the implementation of state laws regulating abortion in the U.S., I evaluate the impact of these restrictive policies on violence against women of reproductive age by implementing a generalized difference-in-differences model. A 25-mile increase in distance to reach the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality up to 2.6%. This negative impact decreases as the initial distance from a clinic rises. The effects of the policies were persistent at least up to one year after they were implemented. This effect is particularly strong among Black women. For them, a 25-mile increase in distance is estimated to increase the number of reported cases of gender violence per municipality up to 6.6% .

1.1 Introduction

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

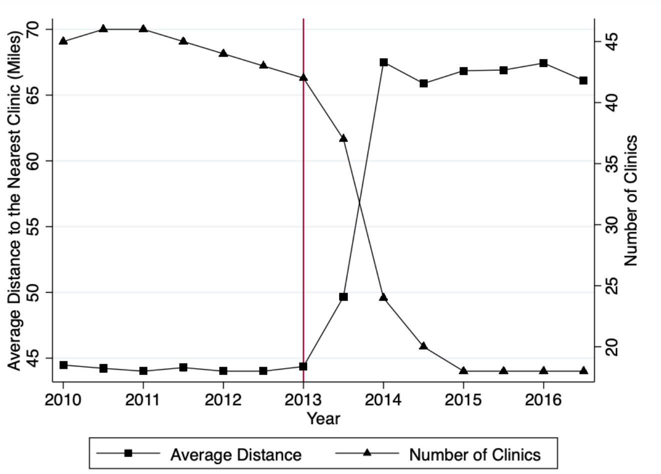
The right to abortion gives women the possibility to decide whether and when to have children. I claim that the lack of choice in this domain, caused by the loss of the right to abortion, decreases women's bargaining power in the private and public spheres and particularly among low-income individuals. This study addresses the question of whether part of the aftermath of lower access to the abortion services, with consequent decrease in bargaining power, is an increase in the likelihood of women to be victims of violence. The arrival of a child lowers women's socio-economic status, making them more vulnerable and hence raising their probability of suffering abuse.[2] An unintended pregnancy may especially increase women's likelihood to suffer from intimate partner violence (IPV), as it also has a direct effect on the ability of a woman to leave a relationship (Roberts et al., 2014). Analyzing data from the Turnaway Study, a cohort study of women seeking abortions at 30 facilities across the U.S., Chibber et al. (2014) find that eight percent of women who mentioned partners as a reason for abortion identified having abusive partners as the main reason. Some of them explained that having a baby would be a deterrent to ending the abusive relationship.

I use a generalized difference-in-differences design with two-way fixed effects, exploiting Texas as a natural experiment. In July 2013, Texas House Bill 2 (HB-2) took effect, which caused the closure of nearly half of the state's abortion clinics within the subsequent year. The change in clinics' accessibility started between the first and the sec-

[1]During the last due decades, many U.S. states have imposed additional regulations for abortion providers, targeted specifically at abortion clinics. These laws are referred to as Targeted Regulation of Abortion Providers (TRAP laws), and their primary purpose is to limit access to abortion.

[2]This mechanism is investigated more thoroughly in Section 1.3.

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



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

ond half of 2013, when the first major requirement[3] of the bill went into effect (Figure 1).

I evaluate the effect of Texas House Bill 2 on violence against women of reproductive age, which I call for simplicity *gender violence*. The assumption underlying my identification strategy is that variations in the distance from a municipality to its nearest abortion clinic are exogenous, since they are a consequence of the fact that some clinics randomly[4] met the standards imposed by H-B2, while others did not and had to shut down.

The present study contributes to two strands of the literature. First, it adds to the literature on abortion and IPV an empirical estimation of the causal impact of abortion access on IPV. To the extent of my knowledge, this is the first study that finds a causal relationship between abortion access and violence, shedding light on a dramatic implication of anti-abortion policies. Some studies have tried to measure the impact on domestic violence of the impossibility to terminate a pregnancy through survey analysis. Several authors reported a higher prevalence of domestic violence among women seeking abortion services, finding that women who seek for abortions experience domestic violence and sexual assault at up to three times the rate of those who want to continue with their pregnancies. (Aston and Bewley, 2009, Evins and Chescheir, 1996, García-Moreno et al., 2013, Hall et al., 2014, Pinton et al., 2017, Taft and Watson, 2007). In addition, domestic violence tends to increase during pregnancy (Ellsberg et al., 2008). Using information

[3]The first provision required physicians at abortion clinics to have admitting privileges at a hospital within 30 miles of the facility. This and the other three requirements are described in Section 1.2.

[4]The randomness of clinic closure is investigated in Section 1.6

from the Turnaway Study, Roberts et al. (2014) find that having an abortion was associated with a reduction over time in physical violence from the man involved in the pregnancy, compared with carrying the pregnancy to term. They conclude that having a baby with an abusive man, compared to terminating the unwanted pregnancy, makes it harder to leave the abusive relationship. With respect to these studies, I also enlarge the definition of the dependent variable to include types of violence other than IPV (*gender violence*). Second, the analysis contributes to the literature on the impact of TRAP laws on abortions and births that exploits the same setting and identification strategy used here. This contribution lies in having added several empirical tests on the randomness of treatment, repeatedly treated units, and multiple periods of treatment. This research has significant policy relevance as it sheds lights on the role of reproductive rights in the process of women's empowerment.

I find that, depending on the initial distance, a 25-mile increase in the distance to the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality up to 2.6%. This impact persisted after one year, although mainly driven by violence outside the family. The relationship is non-linear, in the sense that the effect of distance on violence is lower for municipalities already far from their nearest abortion clinic, while it is larger for women living relatively close to a clinic before the closure[5]. The impact of an increase in distance is particularly strong among Black women, who experience an increase in violence against them up to 6.6%.

The paper is organized as follows: Section 2 describes the juridical and economic background and provides details of HB-2. Section 3 explains the mechanism through which abortion access affects violence against women. The data are presented in Section 4, and Section 5 describes the empirical strategy. Section 6 explores the identification of the model and the main results are reported in Section 7. Section 8 is dedicated to sensitivity analysis, while Section 9 shows results from a placebo test. Some preliminary evidence on self-induced abortion in Texas is reported in Section 10. The last section concludes.

1.2 Background

Even if abortion in the U.S. has been legal since the *Roe v. Wade* (1973) decision of the U.S. Supreme Court, people seeking abortions may still encounter substantial financial barriers. The Hyde Amendment (1976) currently bans the use of federal dollars for abortion coverage for people enrolled in Medicaid, the nation's main public health insurance program for low-income individuals. Similar restrictions apply to other federal

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

programs and operate to deny abortion care or coverage to people with disabilities, Native Americans, prison inmates, poor and low-income individuals in the District of Columbia, military personnel, and federal employees.[6] The lack of insurance coverage for abortion for low-income individuals is worsened by the fact that poor people have lower access to contraception (Kavanaugh, Jones and Finer, 2011). This, in turn, implies a higher likelihood of experiencing unwanted pregnancies. According to the Guttmacher Institute[7] 75% of abortion patients in 2014 were poor or low-income.[8] Thus, most abortions (95%) are performed in specialized abortion clinics, rather than private physicians' offices or hospitals (Jones and Jerman, 2014) where the procedure is expensive. These clinics have been the main target of recent regulations introduced to limit abortion availability.

Early strategies to restrict abortion access were primarily directed toward patients (demand-side policies) and include, for example, parental involvement requirements for a minor's decision to terminate a pregnancy, and mandating 24-hour waiting periods between receiving information on abortion risks and having the abortion procedure.

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

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

Provisions (1), (3), and (4) went into effect on November 1, 2013, causing the first wave of abortion clinic closures. Obtaining admitting privileges can take time since hospitals have to review a doctor's education, licensure, training, board certification and

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

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

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

history of malpractice, and many hospitals require admitting doctors to meet a quota of admissions. The implementation of this provision caused nearly half of Texas abortion clinics to close (Figure 1).

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

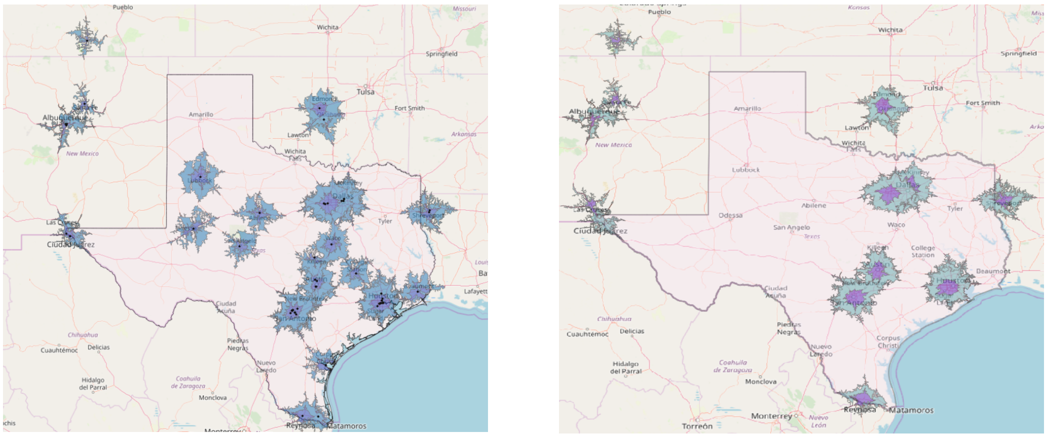
In April 2013, after the introduction of HB-2, eight of the 41 Texas abortion clinics closed or stopped providing abortion services. Eleven more facilities closed or stopped providing abortions when HB-2 was enforced, mainly because physicians experienced barriers to obtaining hospital admitting privileges.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

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

Lindo et al. (2020*a*) estimate that, on average, clinics' closure due to HB-2 doubled the distance from a Texas resident to her nearest clinic. They estimate that, relative to having the nearest abortion provider within 50 miles, having the nearest abortion provider 50-100, 100-150, 150-200 and more than 200 miles away reduces abortions by 16%, 28%, 38%, and 44%, respectively. These results are consistent with Grossman et al. (2017), who find that in Texas an increase in distance to the closest facility providing abortion services was associated with a decline in abortions between 2012 and 2014. Fischer, Royer and White (2018) estimate that abortion amongst Texas residents fell 16.7% and births rose 1.3% in counties that no longer had an abortion provider within 50 miles,

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

(a) Abortion clinics in 2009 (b) Abortion clinics in 2016



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

after the implementation of policies restricting abortion access. Similarly, Venator and Fletcher (2020) analyze the effects of Wisconsin's restrictions on abortion access introduced between 2011 and 2013. They find that a 100-mile increase in distance to the nearest clinic is associated with 30.7% fewer abortions and 3.2% more births. Finally, two recent studies adopt a broader approach. Using data for 1,178 counties in 18 U.S. states, Brown et al. (2020) find that each additional mile to a provider was associated with a decrease of 0.011 in the abortion rate. Myers (2021) exploits a new dataset for the entire nation, finding that an increase in travel distance from 0 to 100 miles is estimated to prevent 20.5% of women seeking an abortion from reaching a provider, and in turn to increase births by 2.4%.[9]

The difference between the decrease in the abortion rate and the increase in the fertility rate is consistent with women who could not terminate their pregnancy from a local provider, but who could decide to travel outside of Texas to have an abortion or to illegally self-induce an abortion (Grossman et al., 2010).

The impact of restrictions on abortion access is particularly heavy in the American context, given the prevalence of unintended pregnancies.[10] The Guttmacher Institute es-

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

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

timates that in 2011, there were 45 unintended pregnancies for every 1,000 women aged 15-44 in the United States (i.e., nearly 5% of reproductive-age women have an unintended pregnancy each year) and that nearly half (45%) of the 6.1 million pregnancies in the United States were unintended. The unintended pregnancy rate is significantly higher in the United States than in many other developed countries.[11]

For poor and vulnerable women, the burden of an unintended child is particularly heavy. First, these women constitute the group that experiences the highest rate of unintended pregnancies: they cannot afford to turn to hospitals or private physicians' offices for an abortion (a very expensive procedure) or to travel far away from home to reach the nearest abortion clinic, losing days of work and spending money on travel and hotels; in addition, they represent the group with the least access to contraception. This is especially true for Texas, wherein in 2011 a huge cut to public funds to family clinics, which provide free contraceptives to poor women and young girls, was implemented. Lu and Slusky (2019) estimate the effects of this budget cut, that caused 53 clinics to close by 2012, the vast majority of which only provided non-abortion family planning services. They estimate that an increase of 100 miles to the nearest clinic results in a 2.4% increase in the fertility rate for unmarried women. Packham (2017) finds that reducing funding for family planning services in Texas increased teen birth rates by approximately 3.4% over four years. Second, lower socioeconomic conditions are reported among IPV risk factors (Aizer, 2010, Capaldi et al., 2012), thus on average starting these women at a disadvantage.

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

1.3 The channels of causality

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

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

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

Several studies have estimated the positive relationship between abortion access and women's socioeconomic conditions. Increased legal access to the abortion procedure is associated with an increase in high school completion, employment rates, earnings, and labor force participation rates (Abboud, 2019, Angrist and Evans, 1999, Jones et al., 2021, Kalist, 2004, Lindo et al., 2020*b*); a decreased likelihood of needing public assistance, living under the federal poverty line and working full time one year later (Foster et al., 2018, Jones et al., 2021); and a higher probability of women moving between occupations and into higher-paying occupations (Bahn et al., 2020). Miller, Wherry and Foster (2020) estimate that women who were denied an abortion experience a significant increase in financial distress during the year that they give birth, compared to women who received a wanted abortion. These effects were particularly strong among Black women (Jones et al., 2021, Kalist, 2004, Lindo et al., 2020*b*). Moreover, teenage pregnancies may prevent girls from finishing high school or going to college. Schulkind and Sandler (2019) find that mothers who gave birth during the school year are 5.4 percentage points less likely to complete their high school education. Estimates show that women with medium or high levels of education face less exposure to sexual, physical, or psychological abuse from partners or non-partners compared to less educated women (Bettio and Ticci, 2017). A lower economic position has an impact on the bargaining power of women both in the private and public spheres (Agarwal, 1997).

Concerning the job market, lower economic standing decreases women's capacity to avoid violence in the workplace because of the lack of outside options in the case of job loss. According to a review by McDonald (2012), women with irregular, contingent, or precarious employment contracts are particularly vulnerable to sexual harassment. In addition, a lower economic status forces women to accept more dangerous job positions that may be associated with a higher likelihood of suffering abuse. For example, occupations that involve night shifts may expose them to a higher probability of being victims of violence by strangers. One interesting case is the one of sex work. Selling sex may be a viable option for women who need money and flexible working hours to support for their children. Several studies indicate that the majority of prostitutes report having been raped and physically assaulted during the course of their activities and they are also disproportionately represented among female murder victims (Church et al., 2001, Farley and Barkan, 1998, Lowman, 2016).

The decrease in women's economic status resulting from the arrival of a child is worsened by the fact that women, but not men, are likely to suffer a penalty in the workplace for parenthood (Blau and Kahn, 2017, Budig and England, 2001, Correll, Benard and Paik, 2007, Kleven, Landais and Søgaard, 2019). Additionally, given the unequal division of housework between partners, an increase in housework responsibility due to the arrival of a child will weigh more on the shoulders of women (for a review on housework see Coltrane, 2000), limiting their employment opportunities. Bertrand, Kamenica and Pan (2015) estimate how, after controlling for outside work, the majority of caring responsibilities still belong to women. A piece of the significant part of the gender wage gap that cannot be explained by the usual explanatory factors is likely to be caused by women taking career breaks following childbirth (Andersen and Andersen, 2017, Costa Dias, Joyce and Parodi, 2020, Hersch and Stratton, 1994, Rege and Solli, 2013).

In the household, women's decrease in bargaining power, with the consequent rise in their likelihood of suffering from intimate partner violence, has a double determinant (Roberts et al., 2014). First, an unwanted child has a direct effect on the ability of a woman to leave a relationship for economic and emotional reasons (Bettio and Ticci, 2017, Biggs, Gould and Foster, 2013, Chibber et al., 2014, Sanders, 2007). Studies on underreporting of IPV testify to this fact. Even if domestic violence and sexual assault are a major burden for the global female population[12] (Ellsberg et al., 2008), a relevant issue to address when studying IPV is still underreporting. The problem of underreporting with IPV is so serious that reported cases of domestic violence represent only a very small part of the problem when compared with prevalence data, so that they constitute the so-called "iceberg" of domestic violence.[13] Evidence shows that the rate of reporting of IPV is lower for women in the early postpartum period (Keeling and Mason, 2011, Rubertsson, Hildingsson and Rådestad, 2010). This may be because with the arrival of a child a woman becomes less likely to leave a relationship and more likely to protect the partner. Fugate et al. (2005) analyzed data from the Chicago Women's Health Risk Study, in which 491 abused women were interviewed in public health centers and a hos-

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

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

pital. They find that many women believe that to get help from the police, they must be prepared to end the relationship. Furthermore, they find that 10% of the interviewed women stated they did not call the police in order to "protect [their] partner and preserve [the] relationship" (Fugate et al., 2005). These reasonings also apply to the workplace setting, where the fear of losing their job may push women to underreport sexual harassment. These findings on underreporting are relevant to my empirical analysis since they exclude the possibility that an increase in the number of reported cases of violence may be due to a possible increase in the level of reporting (e.g., concerning IPV, one could assume that the arrival of a child makes women more likely to denounce violence to protect their children).

The second way an unwanted child may decrease women's bargaining power within the household is indirect and works through a decrease in their economic status. Women's bargaining power within the household is strictly related to their economic independence, which is significantly reduced by the arrival of a child. In the original bargaining models of marriage (e.g., Manser and Brown, 1980, McElroy and Horney, 1981) the threat point and the reservation utilities coincide with each other and correspond to the utility of divorce. The threat of divorce (or break up) becomes far less credible when a child arrives, for economic and emotional reasons. The premise here is that the greater a women's ability to physically survive outside the family, the greater her bargaining power within the family (Gelles, 1976, Montero et al., 2012). Moreover, in the marriage market, mothers are typically less "eligible" than fathers, and this further decreases their willingness to leave a relationship (Agarwal, 1997). Hence, a woman's outside options decrease as a child arrives, and this, in turn, lowers her bargaining power within the couple and increases the risk of IPV. Results from a Finnish survey show that women who were unemployed, self-employed, or on maternity leave reported experiencing IPV more often (Heiskanen, Piispa and Aromaa, 1998). Aizer (2010) estimates that decreases in the wage gap reduce violence against women within the family, and Anderberg et al. (2016) estimate a positive relationship between female unemployment and domestic abuse. In contrast to the bargaining model, there are models of male backlash that predict that a wife's improved relative economic position increases violence, as it violates traditional gender norms and redefines the power relationship between the spouses, which could trigger a violent response from the husband (Macmillan and Gartner, 1999).[14] According to this theory, the negative effect of increased female empowerment on IPV may be attenuated by a backlash effect.

To conclude, as underscored by Agarwal (1997), economic factors, together with lower bargaining power within the household, impact the bargaining power of a woman

[14]For updated empirical literature on the topic, see Cools and Kotsadam (2017), Bhalotra et al. (2018), Ericsson et al. (2019) and Guarnieri and Rainer (2021)

within the community through a lower capacity for mobility and a higher need for social support. This may have an impact on women's likelihood to avoid violence perpetrated by community members.

1.4 Data

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

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

	Before HB-2		After HB-2	
	Mean	Standard dev.	Mean	Standard dev.
Municipality level variables				
Cases of gender violence	478.10	791.80	573.05	934.56
Distance to the nearest clinic	23.83	30.19	51.79	80.31
Population	565,261.40	547,667.20	528,644.80	543,985.10
County level variables				
Population	1,850,896.00	1,113,336.00	1,841,534.00	1,194,853.00
Hispanic share	0.31	0.11	0.35	0.17
Black share	0.16	0.06	0.15	0.07
Share of females (15-49)	0.25	0.01	0.25	0.01
Income per capita	44,922.59	5,462.60	49,374.22	7,514.16
Unemployment rate	7.25	0.96	4.72	1.14
Number of Observations	389		512	

Note: Population-weighted summary statistics calculated for 105 Texas municipalities for the pre-HB-2 period (2010-2012) and post-HB-2 period (second half of 2013-2016).
Source: Abortion clinics opening and closing dates are taken from Lindo et al. (2020*a*). The average distance is calculated by the author for all the municipalities in the sample. County-level demographic controls are taken from the National Institute of Health Surveillance, Epidemiology and End Results, while county-level income per capita estimates are from the U.S. Bureau of Economic Activity. The unemployment rate by county is taken from the U.S. Bureau of Labor Statistics, and municipal population data is from the U.S. Bureau of Labor Statistics.

To measure violence, I use information on reported cases of violence against women for 105 Texas municipalities,[15] taken from the Uniform Crime Reporting (UCR) Program Data. Uniform Crime Reporting is a city, county, state, and federal law enforcement program. It provides a nationwide view of crime based on the submission of crime information by law enforcement agencies. Within this program, each city law enforcement agency reports offenses that occur within its municipal boundaries. Since the data collection is based on the voluntary submission of crime information by law enforcement

[15]The list of the municipalities used for the analysis can be found in Appendix 1.11.

agencies, data are completely missing or strongly imbalanced during my sample period for many municipalities, hence my dataset includes a subsample of Texas municipalities.

I include in the analysis all cases where the victim is a female of reproductive age (15-49) and the offender is male, and the types of offense considered include assault, homicide, human trafficking, kidnapping, and sexual offenses.[16] For simplicity, I will refer to these multiple forms of violence as gender violence. As you can see in Table 1, the mean of the number of reported cases of gender violence increases after HB-2 implementation. In the second part of the analysis, I will only consider reported cases of intimate partner violence, i.e., the offender is a male partner/ex-partner of the victim.

Data on clinics' opening and closing dates in Texas and neighboring states (Colorado, Louisiana, New Mexico, and Oklahoma) are taken from Lindo et al. (2020*a*). The inclusion of clinics in Colorado, Louisiana, New Mexico, and Oklahoma needs to account for potential travel to clinics in neighboring states. A clinic is considered open (or closed) in a six-month period if it has been opened (or closed) for at least three months.

I geocoded each abortion clinic in every six-month period of every year for the period 2010-2016. Then, I used the Stata command *georoute* to calculate the travel distance (miles) between each municipality's geographic centroid that reports crimes to the Uniform Crime Reporting Program and the nearest clinic. Municipalities' centroid coordinates are taken from the Texas open data portal.[17] Table 1 shows how the average distance to the nearest clinic has more than doubled after the implementation of HB-2.

Distance from the nearest clinic has changed differently across counties after HB-2 implementation. Representing the sample's municipalities on a map that shows the magnitude of the variation in distance for each county, I checked whether my sample includes municipalities in every kind of county, including those where distance increased the most, i.e., more than 100 miles (Figure 3).

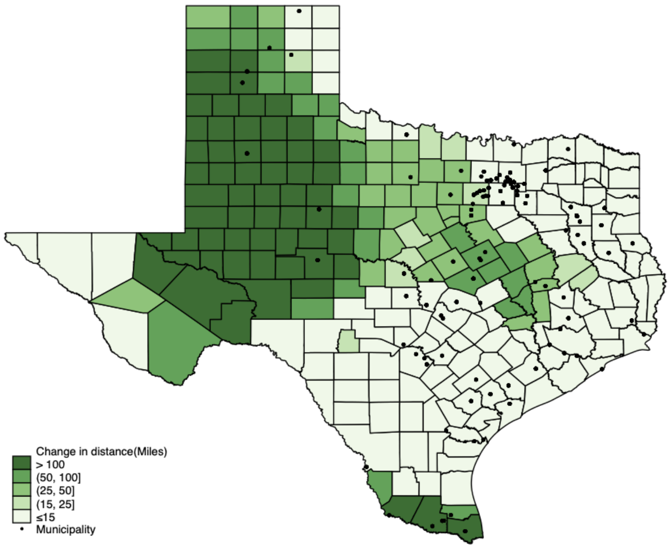
In the main specification I use the distance to the nearest clinic at the same time the case of violence occurs. I then check if the effect of clinics' closure persists after a year, when the baby has actually been born. I choose a one-year lag, and not a shorter lag, to be sure to capture the effect of the actual birth of an unintended child, avoiding the possibility that some of the women in the sample could still be pregnant when evaluating the impact of variation in distance on violence; in fact, these women could have tried to end their pregnancy at the end of the six-month period and so they could be still pregnant after six months.

I add to the model some time-varying control variables. First, I include in each regression the logarithm of the municipality's population; this information is taken from the U.S. Bureau of Labor Statistics (BLS).

[16] See Appendix 1.11 for a description of the type of offenses considered.

[17] data.texas.gov.

Figure 3: Yearly change in distance from each Texas county to the nearest abortion clinic and municipalities in the sample



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

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

Given the absence of data at the municipality level, I also take some information collected at the county level: the estimated income per capita from the U.S. Bureau of Economic Activity (BEA), the unemployment rate from the U.S. Bureau of Labor Statistics, and the share of women of reproductive age from the National Institute of Health Surveillance, Epidemiology and End Results (SEER). The summary statistics of these variables are reported in Table 1.

Including covariates for racial composition in each county may result in a problem of perfect collinearity with the municipality fixed effects, as the trends in the average share of Blacks, Hispanics, and Whites are flat during the time period considered for the analysis. A similar multicollinearity issue may arise using the absolute number of Hispanics, Blacks, and Whites, due to the common trends in all these variables[18].

Finally, both shares and absolute numbers may be bad controls. I assume abortion access to have an effect on gender violence through the variation in the probability of a woman to experience an unintended pregnancy. Given that this probability is very likely to be much higher among disadvantaged groups, access to abortion is likely to affect the racial composition of the population. Thus, I choose to be conservative and not to include these racial composition control variables in the main specifications. In Table D.1 I will

[18]Look at Figure D.1 a and b of the Appendix for a plot of these trends

confirm the robustness of the results to the inclusion of such controls.

1.5 Empirical strategy

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

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

I estimate the following model:

$$\begin{aligned} E[GV_{i,c,t,y}|dist_{i,c,t,y}, X_{c,y}, \Gamma_{i,y}, \alpha_i, \delta_t] = exp(&\beta_1 dist_{i,c,t,y} + X'_{c,y}\beta_2 + \\ &+\Gamma'_{i,y}\beta_3 + \alpha_i + \delta_t) \end{aligned} \tag{1}$$

$GV_{i,c,t,y}$ (gender violence) is the number of reported cases of violence against women of reproductive age for municipality i in county c, in period (six-month) t of year y. $dist_{i,c,t,y}$ is a set of measures of access from each municipality i to the nearest abortion clinic in the six-month period t or $t-2$ of year y (a one-year lag is equivalent to a lag of two six-month periods). This set includes a linear measure of distance and a quadratic measure of distance, both measured in miles. α_i is the municipality fixed effect and δ_t is the six-month fixed effect. The inclusion of municipality fixed effects should greatly reduce overdispersion, which is mainly due to differences in cities' characteristics. $X'_{c,y}$ is the vector of county controls and $\Gamma'_{i,y}$ is the vector of municipality controls. In all models, the logarithm of the municipal population is included as the exposure variable to account for the fact that municipalities vary widely in size and therefore have a different potential for offenses.

1.6 Identification

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

Given the centrality of random assignment of treatment, this assumption needs a deeper discussion. Recall that provision (1) of HB-2 required all abortion providers to have admitting privileges at a hospital located within 30 miles of the abortion clinic. I verified that each clinic's municipality has a hospital inside its boundaries,[19] i.e., within 30 miles. However, it could be the case that hospitals in more conservative areas are less likely to grant admitting privileges. I can demonstrate that this is not the case by looking at the distribution of clinics' closure within Texas state borders, since there are no clusters of closures, which are instead spread across the entire state. The geographic representation of clinics' closures illustrates this hypothesis. A superficial look at the post-policy distribution of clinics (see Figure E.1 in Appendix 1.11) may suggest a cluster of closures in the western part of Texas. But the geographic distribution of clinics closed after HB-2 reveals that clinics have been shut down across the entire state and the western portion remained unserved after 2013 only because it already had a very low number of clinics before the intervention.

Given the centrality of such an assumption, additional tests are needed to confirm its validity. I check whether some controls could have an impact on clinics' closures, resulting in failure of the randomness assumption. Results are reported in Appendix 1.11. In the first test, Poisson two-way fixed effect regression is used to estimate the impact of distance from each municipality to the nearest abortion clinic on the portion of cases of gender violence predicted by the control variables (Table E.1). First, the dependent variable is reported cases of gender violence and the independent variables are all con-

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

trols. Then the predicted cases are regressed on the variable of interest (distance to the nearest clinic), including six-month and municipality fixed effects. The coefficient is non-significant, confirming the hypothesis of random assignment of treatment.

To further investigate the issue, several OLS two-way fixed effect regressions are used to estimate the impact of distance on all the control variables (Table E.2). For the OLS models, all the control variables are logarithmic, to avoid non-normal distributions. The estimation is made at the year level because all controls are collected on a yearly basis. None of the estimated coefficients is statistically significant.

Finally, I check whether some controls have an impact on the clinic's probability of being closed in each period (Table E.3). All regressions include year and municipality fixed effects. None of the coefficients is statistically significant. This gives credit to the assumption of randomness of the treatment and excludes the hypothesis of a reverse causality problem.

Provision (2) of HB-2 states that all abortion facilities must meet the requirements of an ambulatory surgical center. The ability to meet these standards may depend on a clinic's size, which, in turn, might be a consequence of the economic well-being of the municipality to which it belongs. In any case, this provision does not create a problem for the random assignment assumption since its enforcement was blocked two weeks after its implementation by the U.S. Supreme Court.

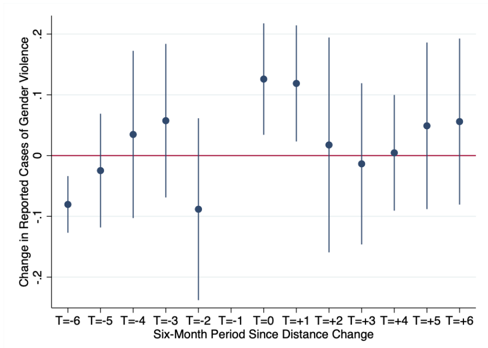
The identifying assumption underlying the generalized difference-in-differences strategy is that the only change at the exact time of the clinics' closures that impacted gender violence was the distance to the nearest abortion clinic, i.e., trends in gender violence would have been the same for treatment and control group in the absence of treatment (parallel trend assumption). I test this assumption by estimating an event study, where I define the event in question as a closure that causes an increase in distance to the nearest clinic higher than a given threshold; I choose the two reasonable thresholds of 15 and 25 miles. I estimate Equation 1 with the measure of distance replaced by an indicator variable equal to 1 if the change in distance since the last period exceeds 15 and 25 miles.[20] The regression includes leads and lags for the six-month periods surrounding the reference period, T. The indicator for period $T-1$ is omitted, meaning that the coefficients can be interpreted as the effect of a clinic closure that increases distance by more than 15/25 miles on gender violence cases relative to gender violence cases in the period prior to the clinic closure. Using data for the three years prior to the closure and for the three years following the closure (six six-month periods), I observe no significant difference in pre-closure reporting of cases of gender violence for municipalities that experience a closure relative to those that do not, except for a significant decrease in violence three years

[20] I filled the dataset with the missing observations and their distances to the nearest clinic, then created the indicator variable on this new balanced sample.

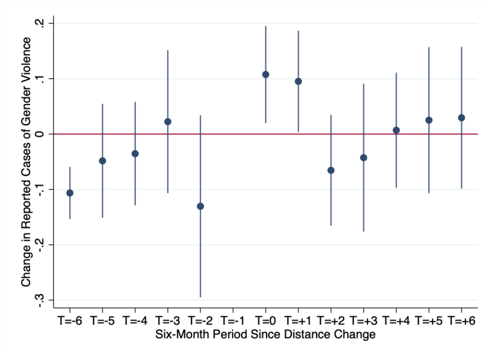
prior to the event (Figure 4[21] (a) and (b)). This effect is in the opposite direction with respect to my findings. I also see a significant increase in violence both in the treatment period and six months later. In Figure 4 (c) and (d), I restrict the sample to a two-year period (four six-month periods) on either side of the event. Again, I see no significant difference in pre-closure trends for municipalities that experience a closure relative to those that do not and a significant increase in gender violence both in the treatment period and six months later. Interestingly, three periods after the event (one year later), gender violence cases decrease again. This may be due to the key reopening of the Whole Woman's Health clinic in McAllen in the second half of 2014, which greatly reduces distances for several municipalities.

Figure 4: Event studies: Effect of an increase in distance by more than 15 or 25 miles on gender violence

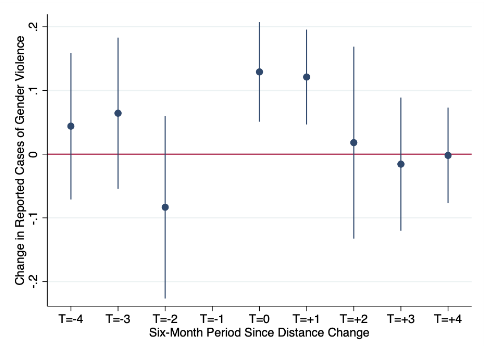
(a) 3 Years-15 miles



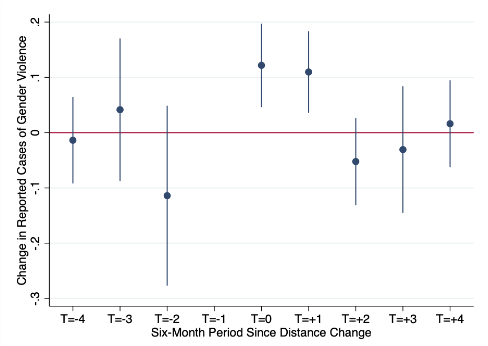
(b) 3 Years-25 miles



(c) 2 Years-15 miles



(d) 2 Years-25 miles



Note: The event studies are estimated through a two-way fixed effects Poisson model. This is equivalent to the model used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to six-month periods relative to the event. The event is defined as the first period in which a municipality switched from having a clinic to not having a clinic within the corresponding distance. The six-month period prior to the event is omitted as it is the reference group.

To further investigate the parallel trend assumption I test whether changes in dis-

[21] Regression coefficients can be found in Table F.1 of the Appendix.

tance faced by municipalities following the closures are predictive of pre-policy trends in reported cases of gender violence. I regress the change in cases between 2010 and 2013 on the change in distance between 2013 and 2016:

$$GV_{i,2013} - GV_{i,2010} = \beta_0 + \beta_1 (dist_{i,2016} - dist_{i,2013}) + \varepsilon_i \tag{1.1}$$

Table F.2 of the Appendix shows the results. There is no significant effect of distance changes in the post-policy period on trends in cases in the pre-policy period.

1.7 Results

1.7.1 The effect of abortion access on gender violence

First, I estimate the impact of restricted access to abortion on gender violence. Table 2 reports the coefficients for the estimated effect of distance to the nearest abortion clinic on gender violence,[22] with distance to the nearest clinic measured in miles.[23] In each regression, standard errors are clustered at the commuting zone level to account for both serial correlation in the outcome and overdispersion.

As indicated by Table 2, column (1), the effect of an increase in distance the nearest abortion clinic is not statistically different from zero. Following the literature (Fischer, Royer and White, 2018, Lindo et al., 2020*a*, Myers, 2021, Venator and Fletcher, 2020), I assume that this relationship is non-linear, meaning that the effect is higher for municipalities relatively close to an abortion clinic before the implementation of the policy. Hence, I add a quadratic measure of distance. The quadratic version of distance shows the non-linear relationship: an additional mile increases the cost at a diminishing rate.

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

As shown by Table 2, columns (3) – which includes time-varying controls – if the

[22] Average marginal effects are reported in Table I.1 and the estimated coefficients for all the covariates are shown in Table H.1 of the Appendix.

[23] Coefficients are consistent with the use of travel time (minutes) as a measure of distance. Results are available upon request.

Table 2: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_t$ (25 *miles*)	.008	.028***	.026***
	(.006)	(.007)	(.006)
$Distance_t^2$ (25 *miles*)		$-.0017^{***}$	$-.0017^{***}$
		(.0006)	(.0005)
Municipality & six-month FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	871[a]	871	871

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

[a] The estimation procedure employed (STATA command *ppmlhdfe*) identifies separated observations and then restricts the sample in a way that guarantees the existence of a meaningful Maximum Likelihood estimator (Correia, Guimarães and Zylkin, 2019). Hence, the number of observations used to estimate the coefficients is lower than the total sample.

closest clinic is 0 miles away, a 25-mile increase in distance to the nearest abortion clinic is associated with a 2.6% increase[24] in the number of reported cases of gender violence per municipality in the same period, with coefficients significant at the one percent level.

The effect of a 25-mile increase reduces as the starting distance increases, according to the coefficient of the squared measure of distance. Figure G.1 of the Appendix plots the estimated marginal effects by starting distance from the nearest clinic.

Table 3 shows the impact of abortion access on gender violence one year after closure, confirming the existence of a lagged effect with respect to the contemporaneous one.

This is consistent with the fact that the economic vulnerability of a woman is likely to increase when the child is actually born, causing a further increase in the likelihood of suffering abuse. A 25-mile increase in the distance to the nearest clinic is associated with a 2.3% increase in the number of reported cases of gender violence per municipality the following year, if the closest clinic is 0 miles away. The effect of a 25-mile increase reduces according to the initial distance, as shown in Figure G.2.[25]

[24] Since the model is a Poisson, the percentage effect of a one-unit change in the regressor on the dependent variable is computed using the transformation $(e^{\beta} - 1) \cdot 100$.
I estimate the effect of a 25-mile variation to show more interpretable results. The effect of a one-mile increase is 0.10%.

[25] Average marginal effects are reported in Table I.2 and the estimated coefficients for all the covariates are shown in Table H.2 of the Appendix.

Table 3: Estimated lagged effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_{t-2}$ (25 *miles*)	$-.0008$	$.024^{**}$	$.023^{***}$
	(.008)	(.010)	(.008)
$Distance^2_{t-2}$ (25 *miles*)		$-.002^{***}$	$-.002^{***}$
		(.0008)	(.0006)
Municipality & six-month FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	871	871	871

Note: Estimated lagged effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

All these results are consistent with the use of year fixed effects instead of six-month fixed effects (see Appendix 1.11, Table K.1).

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

In this section, I disentangle the impact of abortion access on intimate partner violence, by including as dependent variable only reported cases of intimate partner violence, i.e., where the victim is a female of reproductive age and the offender is a male partner or spouse/ex-spouse of the victim.

Table 4 shows the estimated coefficients. If the closest clinic is 0 miles away, a 25-mile increase in the distance to the nearest clinic is associated with a 2.3% increase in the number of reported cases of intimate partner violence per municipality at the time and a 1.4% increase after a year. The effect of a 25-mile increase reduces as the initial distance increases, as shown in Figures G.3 and G.4.[26]

When looking at the contemporaneous coefficient, the results provide evidence of the fact that a pregnancy traps some women in violent relationships the moment they realize they are pregnant[27] (Ellsberg et al., 2008). The results are also consistent with the evidence about intimate partner violence as a persistent phenomenon within a couple, as showed by the lagged effect.

[26]Table I.3 and Table H.3 of the Appendix show respectively the average marginal effects and the estimated coefficients for all the covariates.

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

Table 4: Estimated effect of a 25-mile increase in distance on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)	(5) (IPV)	(6) (IPV)
$Distance_t$ (25 $miles$)	.006	.025***	.023***			
	(.006)	(.007)	(.007)			
$Distance_t^2$ (25 $miles$)		−.0016**	−.0016***			
		(.0007)	(.0006)			
$Distance_{(t-2)}$ (25 $miles$)				−.004	.016*	.014**
				(.008)	(.009)	(.007)
$Distance_{(t-2)}^2$ (25 $miles$)					−.0018**	−.0018***
					(.0007)	(.0005)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	826	826	826	826	826	826

Note: Estimated effect of distance to the nearest abortion clinic on intimate partner violence (IPV) for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Overall, looking at the size of the coefficient, it may seem that the effect of restrictions on abortion access on gender violence could be driven largely by the impact on IPV, especially when looking at the contemporaneous effect. To further investigate the role that IPV plays in explaining the effect of abortion access on gender violence, I look at the impact of distance on all forms of violence against women except for IPV, i.e., I exclude from the main analysis all the cases where the offender is a spouse, ex-spouse, or boyfriend of the victim.

Looking at Table 5,[28] I can conclude that abortion access has a similar instantaneous impact on all forms of violence against women while, one year later, the effect appears larger for forms of violence other than IPV. This could suggest that some women could have been able to leave the abusive partner after the birth of their child.

1.7.3 The effect of restrictions to abortion access for Black women

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

[28] Marginal effects can be found in Table I.4 while the estimated coefficients for all the covariates are in Table H.4 of the Appendix.

Table 5: Estimated effect of a 25-mile increase in distance on all forms of gender violence except for intimate partner violence

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (IPV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 *miles*)	.016***	.025**	.025***			
	(.005)	(.010)	(.009)			
$Distance_t^2$ (25 *miles*)		−.0008	−.0009			
		(.0007)	(.0006)			
$Distance_{(t-2)}$ (25 *miles*)				.012	.029**	.030**
				(.008)	(.015)	(.013)
$Distance_{(t-2)}^2$ (25 *miles*)					−.0015	−.0017*
					(.001)	(.0009)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	820	820	820	820	820	820

Note: Estimated effect of distance to the nearest abortion clinic on forms of gender violence other that intimate partner violence (GV) for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

women.

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

I exploit the fact that the Uniform Crime Reporting Program Data collects information on the race of the victim. Thus, I restrict the analysis to all the offenses where the victim is *Black or African American* since this is one of the most economically and socially disadvantaged groups in the society (in 2016, the median household income of Black Americans was $39,500, compared with $65,000 for non-Hispanic white Americans[29]). Results in Table 6 confirm the hypothesis made at the beginning of this paper: when the nearest clinic is 0 miles away, a 25-miles increase in distance is associated with a contemporaneous 6.6% rise in gender violence cases against Black women. After one

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

year the impact lowers to 4.8%. The effect of an increase in the distance to the closest clinic offering abortion for black and African American women has more than doubled with respect to the entire female population.

Table 6: Estimated effect of a 25-mile increase in distance on violence against Black women.

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (IPV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 *miles*)	0.017^{*}	0.063^{***}	0.066^{***}			
	(0.009)	(0.012)	(0.021)			
$Distance_t^2$ (25 *miles*)		-0.0035^{***}	-0.0045^{***}			
		(0.000671)	(0.00127)			
$Distance_{(t-2)}$ (25 *miles*)				0.0053	0.045	0.048^{**}
				(0.014)	(0.027)	(0.022)
$Distance_{(t-2)}^2$ (25 *miles*)					-0.003^{*}	-0.0043^{***}
					(0.0016)	(0.0014)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	730	730	730	730	730	730

Note: Estimated effect of distance to the nearest abortion clinic on violence against Black women for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

1.8 Sensitivity analysis

1.8.1 Non-simultaneous distance variation and repeatedly treated observations

Although HB-2 was enforced on the exact same date for all clinics in Texas, not all distances to the nearest clinic changed in the same period, although most did. Three considerations should be noted: (1) the first wave of closures happened in April 2013 after the introduction of HB-2, while the second wave occurred after the enforcement of the law in November 2013; (2) requirement two of HB-2 went into effect one year after the first requirement (on October 3, 2014), and even if its enforcement was blocked only two weeks later by the Supreme Court, some clinics did temporarily shut down; (3) some time after the closures, certain other clinics managed to reopen because they were able to comply with the law. Figure J.1 of Appendix 1.11 shows this situation. Each panel represents the yearly change in distance from every Texas county to the nearest abortion clinic, starting from 2013. Black dots represent the municipalities included in the sample.

Instead of relying on the recent literature[30] on the difference-in-differences estimators that correct for multiple periods of treatment, I account for this issue by checking the validity of my results on two different specifications. In fact, most of these new estimators apply only to linear models, and are not consistent with several features of my setting, meaning repeatedly treated observations and missing values. First, I remove all periods after 2013, so that I include only municipalities treated in the second half of this year. Even if the law was enforced on November 1, 2013, it was signed into law at the end of July 2013. Some clinics anticipated its enforcement and closed during the summer. Second, I restrict my sample to include only municipalities treated after the enforcement of the law; according to my specification this corresponds to municipalities whose distance to the nearest clinic changes in first half of 2014.[31] Thus I remove all periods starting from the second half of 2014 and all municipalities treated during 2013.

Results are presented in Table J.1 of Appendix 1.11. Columns (1) and (2) show the estimates for the sample of municipalities treated in the second half of 2013, while columns (3) and (4) present the results for the sample of municipalities treated in the first half of 2014. With these new samples, the quadratic of the distance loses all its significance and the relationship between distance and violence seems to be linear. This could be related to the composition of these two samples in terms of the original distance from the closest clinic. The effect is negative and significant, although standard errors have increased. The reduced number of observations could be responsible for the lower significance of the coefficients, since small sample size issues may determine increasing standard errors.

For the reasons explained above, some municipalities happened to be treated more than once. There is only one municipality in the sample treated three times, while the others are treated at most twice. Thus, I verify whether repeatedly treated observations could have created any sort of bias in the results. Two kinds of checks are performed. First, for all repeatedly treated municipality, I remove the first periods during which distance has changed, leaving the values as missing, so that each of these cities appears as if it was treated only once (Table J.2, columns 1 and 2). Then, to the first periods in which there is a variation in distance, I impute the value of distance that resulted from the subsequent change. Municipalities whose distance changed greatly across subsequent periods were simply dropped (Table J.2 of Appendix 1.11, column 3 and 4). Repeatedly treated observations do not create any bias in my results, as coefficients of Table J.2 remain consistent both in sign, magnitude and level of significance.

[30]For a discussion on the issue and a review of the most recent contributions to the literature, see the dedicated sections in Callaway and Sant'Anna (2021).

[31]Each clinic is considered open in a period if it has been open for at least three months. Since the policy went into effect on November 1, 2013, clinics closed after that date were considered open for the remaining part of the year and closed starting from January 1, 2014.

1.8.2 Subsample checks

Because of missing observations in the UCR dataset, the sample used is quite unbalanced. Thus, I first check the validity of my results on a balanced subsample of municipalities. I keep municipalities that have observations for the entire sample period. Then, because the effect might be driven by the largest municipalities in the sample, I drop all the large cities, i.e., municipalities with a population of 250,000 or more[32]

Next, two different tests are performed to exclude the possibility that the municipalities whose distance has changed the most may drive results. First, all the municipalities for which distance has increased more than 100 miles are excluded from the sample; then, since the western portion of the state remains with zero clinics after HB-2 implementation, I consider the subsample of municipalities located in the eastern part of Texas (as shown in Figure J.2), thereby excluding the most affected ones. Initial distance was similar and small for municipalities located in an area with a higher density of clinics before implementation of the bill. Thus, for these last two subsamples, the relationship is linear and the distance to the second-nearest clinic is added to the model to account for the higher density of clinics also in the post-policy period.

All these results are reported in Table J.3. Coefficients remain consistent for the the balanced subsample and when excluding largest cities. On the contrary, the effect appears higher when excluding the most affected cities, although the level of significance is lower. This may be due to the fact that the most affected cities are located in the western part of Texas, so many of them were already poorly served before HB-2 implementation. Municipalities whose initial distance to the closest clinic was already large may bias coefficients downward.

1.9 Placebo test: The effect of distance on other crimes

To investigate the validity of the results, I perform a placebo test by estimating the effect of distance to the nearest abortion clinic on other crimes. To limit the analysis to crimes where the decrease in women's bargaining power is not involved, I consider only offenses where the victim, if any, is male. An unintended child may also have a negative effect on the economic situation of a couple, so lower access to abortion would generally increase the level of crime because of the consequently lower average socioeconomic conditions of the population. To account for this, I choose a list of crimes that are likely to be unrelated to a sudden decrease in socioeconomic status, at least when conducting a contemporaneous analysis that does not consider long-term scenarios. The list of crimes

[32]Following the classification made by the National Center for Education Statistics (NCES): https://nces.ed.gov/programs/edge/docs/locale_classifications.pdf.

considered is reported in Appendix 1.11 and includes sex-related offenses, weapon law violation, bribery, and purchasing prostitution. I estimate the baseline model 1. As expected, coefficients are not statistically significant and appear with the opposite sign. All coefficients are reported in Table 7.

Table 7: Estimated effect of distance on other crimes

	(1) (OC)	(2) (OC)	(3) (OC)	(4) (OC)
$Distance_t$ (25 miles)	$-.013^{**}$	-.006	-.016	-.006
	(.006)	(.008)	(.015)	(.015)
$Distance_t^2$ (25 miles)			.0003	.000004
			(.001)	(.0009)
Municipality & six-month FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	708	708	708	708

Note: Estimated effect of distance to the nearest abortion clinic on other crimes (OC) for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are Hispanics and Blacks, income per capita, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

1.10 The substitution of self-induced abortion in Mexican-bordering counties

According to a recent study, between 2011 and 2015 the number of Google searches using terms related to self-induced abortion increased from 119,000 to 700,000, and these searches were more common in states with the highest number of abortion restrictions (Stephens-Davidowitz, 2016). Another study estimated that at least 100,000 Texas residents had at some point attempted to end a pregnancy on their own, although the methods they used remain unknown (Grossman et al., 2015).

The drug most commonly used to self-induce an abortion is misoprostol, mostly known by the brand name Cytotec; it is sold for the treatment of gastric ulcers, but it also induces uterine contractions. Misoprostol and mifepristone are the two drugs approved by the U.S. Food and Drug Administration (FDA) to perform a medication abortion[33]. Misoprostol alone is effective and safe for medical abortion in the first trimester (Raymond, Harrison and Weaver, 2019).

[33] https://www.guttmacher.org/evidence-you-can-use/medication-abortion

Cytotec is only available by prescription in the United States, but it can be obtained over the counter at pharmacies in some countries, including Mexico. Jones (2011) estimates that, during the period 2008-2009, 1.2% of abortion clinic patients reported that they have self-induced abortion on their own using misoprostol.

Texas is a particularly interesting case both because of its restrictions to abortion access and because it shares a border with Mexico. In 2012, 7% of abortion patients in Texas reported having tried to end their pregnancy on their own (Grossman et al., 2014). Therefore, I test whether women living near the Mexican border are more likely to substitute abortion at clinics with self-induced abortion, experiencing a lower increase in unintended pregnancies and a subsequent lower increase in gender violence. The inclusion of such a category of women should bias the results downward, so I examine the coefficient of the effect of distance on violence when excluding counties close to Mexico (the list of excluded counties is reported in Appendix 1.11).

The effect is 0.2 percentage points larger for the contemporaneous analysis and 0.3 percentage points larger for the lagged one.[34] This provides some evidence for the hypothesis of a substitution effect between abortion in clinics and self-induced abortion for women near the Mexican border. When access to abortion is restricted, women, especially those living close to Mexico, can decide to self induce an abortion, avoiding the burden of unintended pregnancies and decreasing the likelihood of suffering abuse.

1.11 Conclusion

Results from the present analysis show that access to abortion services has a sizable effect on the incidence of violence against women of reproductive age, both in the private and public spheres. I find that, depending on the initial distance, a 25-mile increase in distance to the nearest abortion clinic is estimated to increase the number of reported cases of gender violence per municipality up to 2.6%, and the effect persists after one year. In accordance with the literature that finds the effect of distance on abortions and births being a decreasing function of distance, the relationship of interest is non-linear, meaning that the effect is higher for municipalities relatively close to an abortion clinic before the implementation of the policy. When I restrict the analysis to intimate partner violence, I conclude that restrictions on abortion access have an impact on all forms of violence, not only IPV. The analysis on Black women, for whom the effect more than doubles, confirms the hypothesis on the key role of socio-economic conditions in explaining the mechanisms underlying the present paper. Most disadvantaged women suffer the most from restrictions to abortion access, as they are more likely to experience unintended pregnancies in the first place, they have less means to obtain an abortion despite the lim-

[34]Estimated coefficients are reported in Table M.1, and marginal effects in Table M.2, of Appendix 1.11.

itations in access caused by clinics' closure, and they are more vulnerable to adverse socio-economic shocks. In light of the evidence on the underreporting of violence, a phenomenon that tends to increase after the birth of a child, these results are likely to largely underestimate the effect of abortion access on violence.

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

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Appendices

A Summary statistics for the pooled sample period

Table A.1: Population-weighted summary statistics, 2010-2016

	2010-2016				
	Mean	Standard dev.	Min.	Max.	N
Municipality level variables					
Cases of gender violence	524.95	866.27	0.00	2,856.00	883[a]
Distance to the nearest clinic (miles)	37.85	62.26	1.77	306.97	883
Population	549,951.30	547,528.90	530.00	1,464,531.00	883
County level variables					
Population	1,845,914.00	1,156,790.00	818.00	4,617,041.00	893
Hispanic share	0.33	0.15	0.04	0.96	893
Black share	0.16	0.06	0.004	0.35	893
Share of females (15-49)	0.25	0.01	0.14	0.27	893
Income per capita ($)	47,291.60	6,992.91	23,728.00	77,002.00	893
Unemployment rate	5.90	1.64	2.77	13.65	893

Note: Population-weighted summary statistics calculated for 105 Texas municipalities for the period 2010 – 2016.
Source: Abortion clinics' opening and closing dates are taken from Lindo et al. (2020*a*). The average distance is calculated by the author for all the municipalities in the sample. County-level demographic controls are taken from the National Institute of Health Surveillance, Epidemiology, and End Results, while county-level income per capita estimates are from the U.S. Bureau of Economic Activity. The unemployment rate by county is taken from the U.S. Bureau of Labor Statistics, and municipal population data is from the U.S. Bureau of Labor Statistics.

[a]Municipality level variables have fewer observations since they are weighted by municipality population, which presents some missing values

B Municipalities in the sample

1. Allen
2. Amarillo
3. Aransas Pass
4. Argyle
5. Arlington

6. Bedford
7. Bee Cave
8. Boerne
9. Borger
10. Canyon
11. Center
12. Cleburne
13. Conroe
14. Converse
15. Crowley
16. Cuero
17. Dallas-Fort Worth Int Ap
18. Decatur
19. Denison
20. Denton
21. Edcouch
22. Edna
23. Flower Mound
24. Forney
25. Fort Worth
26. Frankston
27. Frisco
28. Galveston
29. Gatesville
30. Georgetown
31. Goliad
32. Graham
33. Haltom City
34. Heath
35. Henderson
36. Hewitt
37. Highland Park
38. Hollywood Park
39. Hos Dist: Tarrant County
40. Huntsville
41. Indian Lake

42. Iowa Park
43. Isd: East Central
44. Jacksonville
45. Joshua
46. Katy
47. Kaufman
48. La Villa
49. Lacy-Lakeview
50. Lake City
51. Lakeway
52. Lampasas
53. Lancaster
54. Lewisville
55. Lindale
56. Llano
57. Longview
58. Lubbock
59. Lumberton
60. Lyford
61. Madisonville
62. Marble Falls
63. McKinney
64. Missouri City
65. Murphy
66. Nacogdoches
67. Normangee
68. North Richland Hills
69. Pampa
70. Pantego
71. Parker
72. Pearland
73. Perryton
74. Plano
75. Port Lavaca
76. Port Neches
77. Portland

78. Reno
79. Richardson
80. Rio Bravo
81. Rockwall
82. Roma
83. Rowlett
84. Royse City
85. Rusk
86. Sachse
87. San Angelo
88. San Juan
89. San Saba
90. Seguin
91. Sweetwater
92. Temple
93. Terrell
94. Texas A&M Univ: Commerce
95. The Colony
96. Thorndale
97. Tomball
98. Tyler
99. Tyler Junior College
100. Victoria
101. Weatherford
102. Wharton
103. Whitehouse
104. Woodway
105. Wylie

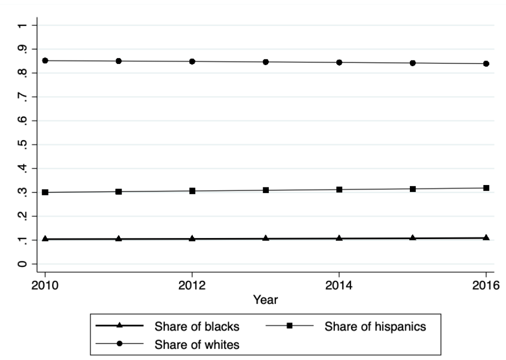
C Type of offense

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

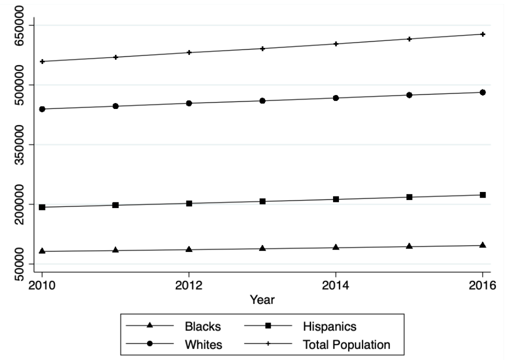
D Racial composition

Figure D.1: Trends in racial composition

(a) Trends in the average county share of Hispanics, Blacks and whites



(b) Trends in the average county absolute number of Hispanics, Blacks and whites



Note: Information on the demographic composition of each county is taken from the National Institute of Health Surveillance, Epidemiology and End Results.

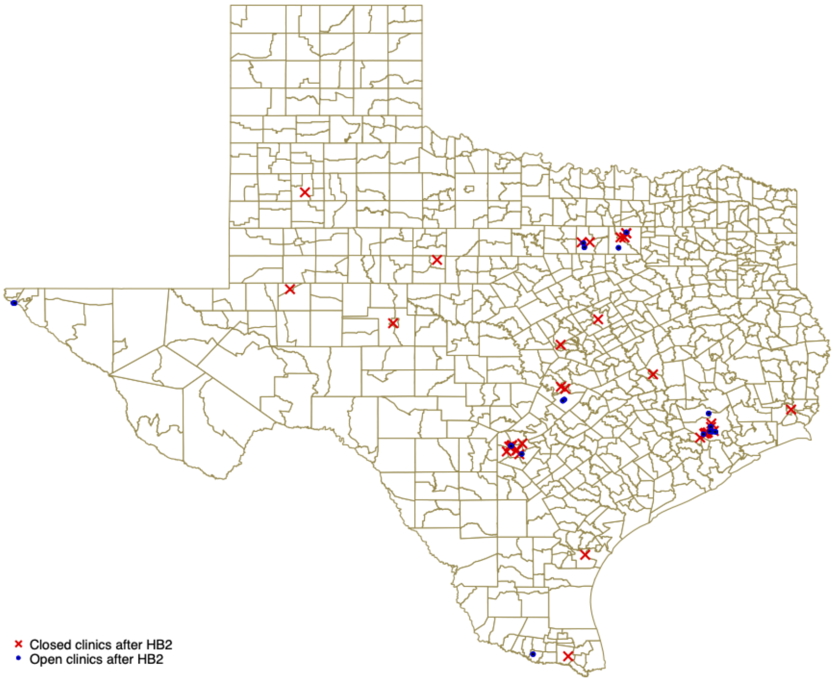
Table D.1: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence, accounting for county racial composition

	(1) (Gender violence)	(2) (Gender violence)
$Distance_t$ (25 $miles$)	.026***	
	(.007)	
$Distance_t^2$ (25 $miles$)	−.0017***	
	(.001)	
$Distance_{(t-2)}$ (25 $miles$)		.022***
		(.008)
$Distance_{(t-2)}^2$ (25 $miles$)		−.0023***
		(.0007)
Municipality & six-month FE	Yes	Yes
Time-varying controls	Yes	Yes
Number of observations	871	871

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county, Black and Hispanic residents per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

E Tests for random assignment of treatment

Figure E.1: Open and closed abortion clinics in Texas after House Bill 2



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

Table E.1: The effect of distance on the predicted level of gender violence

	(1) (GV)	(2) (Predicted GV)
Distance (miles)		$-.000005$
		(.00007)
Municipality population (log)	.37	
	(.24)	
Unemployment rate by county	.04*	
	(.02)	
Income per capita by county (log)	.93*	
	(.56)	
Share of female aged 15-49 by county	-5.14	
	(8.5)	
Municipality & six-month FE	Yes	Yes
Number of observations	871	871

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

Table E.2: The effect of distance on covariates

	(1) (Pop.)	(2) (Income)	(3) (Unemp. rate)	(4) (Fem. 15-49)
Distance (miles)	$-.00015$			
	(.00009)			
Distance (miles)		$-.00004$		
		(.00005)		
Distance (miles)			.00013	
			(.0002)	
Distance (miles)				.000014
				(.00002)
Municipality & year FE	Yes	Yes	Yes	Yes
Number of observations	442[a]	451	451	451

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

[a]The number of observations has decreased since the estimation is no longer based on a six-month period but rather on a one-year period. As the municipal population data is missing for some municipalities, the model in column (1) has fewer observations than the other models considered here.

Table E.3: The effect of covariates on the clinics' probability of closure

	(1) (Probability of closure)
Municipality population (log)	-1.6
	(2.1)
Per capita income by county (log)	1.01
	(.86)
Unemployment rate by county (log)	.66
	(.41)
Share of females aged 15-44 by county (log)	-4.95
	(4.8)
Municipality & six-month FE	Yes
Number of observations	812

Note: Estimated effect of covariates on the clinic's probability of closure in each period. Coefficients are estimated through a linear probability model, and the analysis is at the six-month municipality period level. All explanatory variables are logarithms. Robust standard errors are reported in parentheses and are clustered at the municipality level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

F Parallel trend assumption

Table F.1: Event study: Effect of an increase in distance by more than 15 or 25 miles on gender violence

	(1) (3 years-15 miles)	(2) (3 years-25 miles)	(3) (2 years-15 miles)	(4) (2 years-15 miles)
$T=-6$	$-.08^{***}$	$-.11^{***}$		
	(.024)	(.024)		
$T=-5$	-.024	-.05		
	(.048)	(.052)		
$T=-4$	0.035	-.035	.044	-.014
	(.07)	(.048)	(.059)	(.04)
$T=-3$	.057	.022	.064	.041
	(.064)	(.066)	(.06)	(.066)
$T=-2$	-.088	-.13	-.083	-.11
	(.076)	(.083)	(.073)	(.083)
Event ($T=0$)	$.126^{***}$	$.108^{**}$	$.13^{***}$	$.12^{***}$
	(.047)	(.045)	(.04)	(.04)
$T=1$	$.119^{**}$	$.095^{**}$	$.12^{***}$	$.11^{***}$
	(.049)	(.047)	(.038)	(.038)
$T=2$	.018	-.065	.018	-.052
	(.09)	(.051)	(.077)	(.04)
$T=3$	-.013	-.042	-.016	-.030
	(.068)	(.068)	(.053)	(.058)
$T=4$	.005	.007	-.002	.016
	(.048)	(.053)	(.04)	(.04)
$T=5$	.05	.026		
	(.07)	(.067)		
$T=6$	.055	.029		
	(.07)	(.065)		
Unemployment rate	$.034^{*}$	$.036^{**}$	$.039^{*}$	$.039^{*}$
	(.018)	(.018)	(.022)	(.021)
Income per capita	$.98^{**}$	1.02^{**}	$.95^{**}$	$.98^{**}$
	(.47)	(.47)	(.48)	(.49)
Share of females (15-49)	-5.65	-3.73	-6.7	-5.37
	(9.23)	(10.63)	(8.6)	(9.33)
Number of observations	871	871	871	871

Note: Estimated effect of an increase in distance by more than 15 or 25 miles on gender violence for 105 Texas cities from 2010 to 2016. Estimates are based on a two-way fixed effects Poisson model and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. This is equivalent to the model used to produce the main estimates, except that instead of a single treatment variable, there are multiple treatment variables corresponding to six-month periods relative to the event. The event is defined as the first period in which a municipality switched from having a clinic to not having a clinic within the corresponding distance. The six-month period prior to the event is omitted as it is the reference group. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

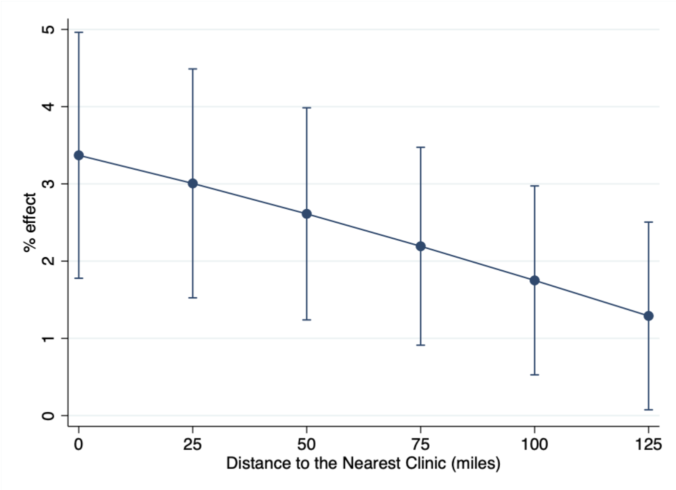
Table F.2: The effect of distance changes after clinics' closure on trends in gender violence prior to closure

	(Δ *Distance*, 2013-2016)
Δ *GV*, 2010-2013	$-.17$
	(.17)
Number of observations	39

Note: Estimated effect of changes in distance to the nearest abortion clinic between 2013 and 2016 on annual cases of gender violence between 2010 and 2013. Robust standard errors are reported in parentheses. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

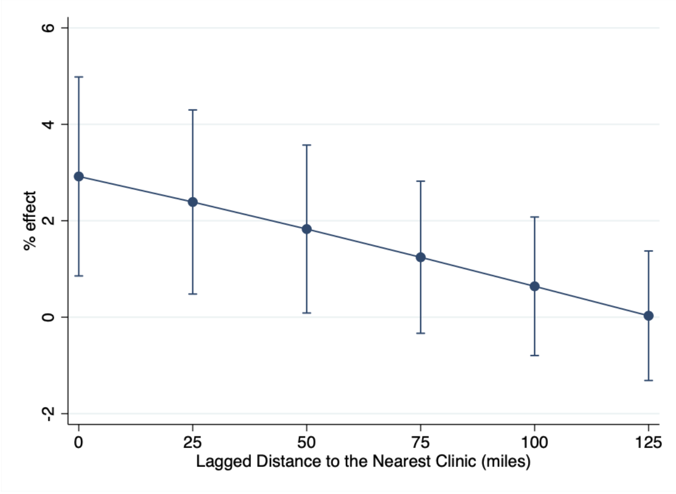
G Marginal effect of distance by initial distance

Figure G.1: Average marginal effect of a 25-mile increase in distance on gender violence by starting level



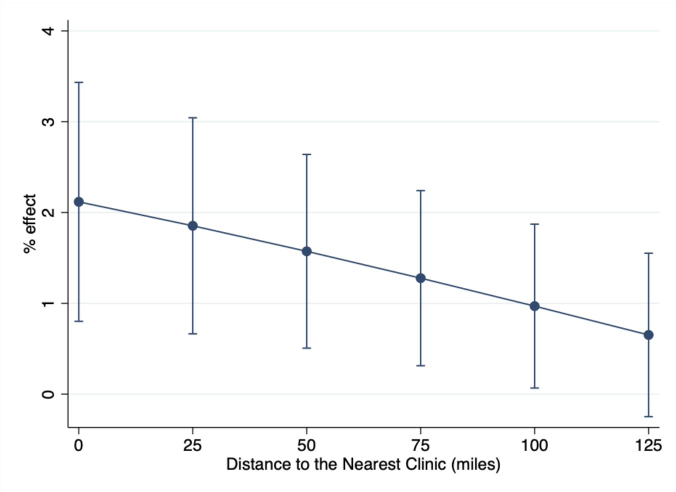
Note: Plot of estimated marginal effects and 95% confidence intervals based on results in Column 3 of Table I.1.

Figure G.2: Lagged average marginal effect of a 25-mile increase in distance on gender violence by starting level



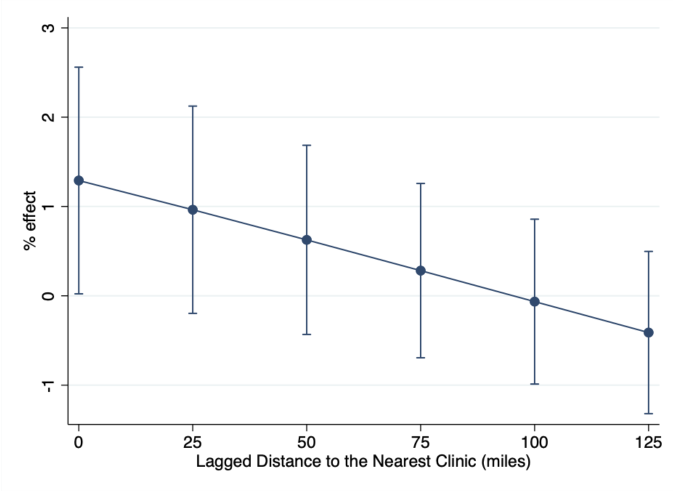
Note: Plot of estimated marginal effects and 95% confidence intervals based on results in Column 3 of Table I.2.

Figure G.3: Average marginal effect of a 25-mile increase in distance on intimate partner violence by starting level



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

Figure G.4: Lagged average marginal effect of a 25-mile increase in distance on intimate partner violence by starting level



Note: Plot of estimated marginal effects and 95% confidence intervals based on results in Column 6 of Table I.3.

H Regression tables with all control variables estimates

Table H.1: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_t$ (25 $miles$)	.008	.028***	.026***
	(.006)	(.007)	(.006)
$Distance_t^2$ (25 $miles$)		−.0017***	−.0017***
		(.0006)	(.0005)
Unemployment rate			.034*
			(.017)
Income per capita			0.941*
			(.485)
Share of females (15-49)			-5.534
			(8.655)
Municipality & six-month FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	871	871	871

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table H.2: Estimated lagged effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence

	(1) (Gender violence)	(2) (Gender violence)	(3) (Gender violence)
$Distance_{t-2}$ $(25\ miles)$	-.0008	.024**	.023***
	(.008)	(.011)	(.0084)
$Distance^2_{t-2}$ $(25\ miles)$		$-.00216$***	$-.00225$***
		(.00078)	(.0006)
Unemployment rate			.0387*
			(.0212)
Income per capita			.99**
			(.494)
Share of females (15-49)			-6.209
			(8.331)
Number of observations	871	871	871

Note: Estimated lagged effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table H.3: Estimated effect of a 25-mile increase in distance on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)	(5) (IPV)	(6) (IPV)
$Distance_t$ (25 *miles*)	.00630	.025***	.023***			
	(.006)	(.007)	(.007)			
$Distance_t^2$ (25 *miles*)		−.0016**	−.0016**			
		(.0007)	(.0006)			
$Distance_{(t-2)}$ (25 *miles*)				-.004	.017*	.014**
				(.00760)	(.00939)	(.00696)
$Distance_{(t-2)}^2$ (25 *miles*)					−.0018**	−.0018***
					(.0007)	(.0005)
Unemployment rate			.044**			.0496**
			(.0178)			(.0219)
Income per capita			.962*			1.009*
			(.520)			(.534)
Share of females (15-49)			-8.551			-9.185
			(10.55)			(10.25)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	826	826	826	826	826	826

Note: Estimated effect of distance to the nearest abortion clinic on intimate partner violence (IPV) for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table H.4: Estimated effect of a 25-mile increase in distance on all forms of gender violence except for intimate partner violence

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (IPV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 *miles*)	.016***	.025**	.025***			
	(.005)	(.010)	(.009)			
$Distance_t^2$ (25 *miles*)		-.0008	-.0009			
		(.0007)	(.0006)			
$Distance_{(t-2)}$ (25 *miles*)				.012	.029*	.03**
				(.008)	(.015)	(.013)
$Distance_{(t-2)}^2$ (25 *miles*)					-.0015	$-.0017^{*}$
					(.00105)	(.000926)
Unemployment rate			.0188			.022
			(.026)			(.026)
Income per capita			.823			.855
			(.573)			(.574)
Share of females (15-49)			-4.860			-5.162
			(10.12)			(10.2)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	820	820	820	820	820	820

Note: Estimated effect of distance to the nearest abortion clinic on forms of gender violence other that intimate partner violence (GV) for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

I Average marginal effects

Table I.1: Estimated average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on reported cases of gender violence

	(1) (Gender Violence)	(2) (Gender Violence)	(3) (Gender Violence)
$Distance_t$ (25 miles)	1.09	3.67^{***}	3.49^{***}
	(.80)	(.94)	(.87)
$Distance_t^2$ (25 miles)		$-.21^{***}$	$-.22^{***}$
		(.08)	(.07)
Municipality & six-month FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	871	871	871

Note: Average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table I.2: Estimated average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence during the following year

	(1) (Gender Violence)	(2) (Gender Violence)	(3) (Gender Violence)
$Distance_{t-2}$ (25 miles)	$-.11$	3.17^{**}	2.98^{***}
	(1.09)	(1.4)	(1.1)
$Distance_{t-2}^2$ (25 miles)		$-.28^{***}$	$-.29^{***}$
		(.10)	(.08)
Municipality & six-month FE	Yes	Yes	Yes
Time-varying controls	No	No	Yes
Number of observations	871	871	871

Note: Average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence during the following year for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipality population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table I.3: Estimated average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on intimate partner violence

	(1) (IPV)	(2) (IPV)	(3) (IPV)	(4) (IPV)	(5) (IPV)	(6) (IPV)
$Distance_t$ (25 miles)	.60	2.4^{***}	2.2^{***}			
	(.62)	(.69)	(.71)			
$Distance_t^2$ (25 miles)		$-.15^{**}$	$-.15^{**}$			
		(.06)	(.06)			
$Distance_{(t-2)}$ (25 miles)				-.38	1.59^{*}	1.3^{*}
				(.72)	(.90)	(.66)
$Distance_{(t-2)}^2$ (25 miles)					$-.17^{**}$	$-.16^{***}$
					(.07)	(.05)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	826	826	826	826	826	826

Note: Average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on intimate partner violence (IPV) for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table I.4: Estimated average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on all forms of gender violence except for intimate partner violence

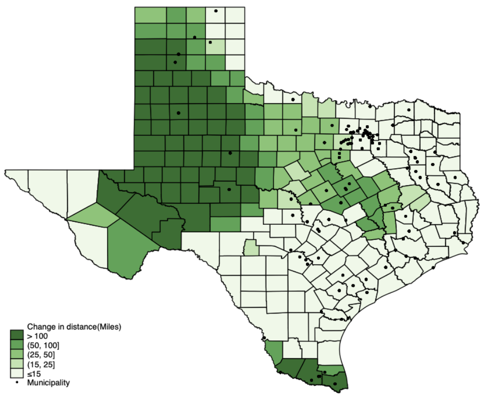
	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)	(5) (GV)	(6) (GV)
$Distance_t$ (25 miles)	$.74^{***}$	1.15^{**}	1.16^{***}			
	(.23)	(.45)	(.42)			
$Distance_t^2$ (25 miles)		$-.03$	$-.04$			
		(.03)	(.03)			
$Distance_{(t-2)}$ (25 miles)				-.53	1.34^{**}	1.37^{**}
				(.38)	(.68)	(.66)
$Distance_{(t-2)}^2$ (25 miles)					$-.07$	$-.078^{*}$
					(.05)	(.04)
Municipality & six-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	Yes	No	No	Yes
Number of observations	820	820	820	820	820	820

Note: Average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on all forms of gender violence except for IPV for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

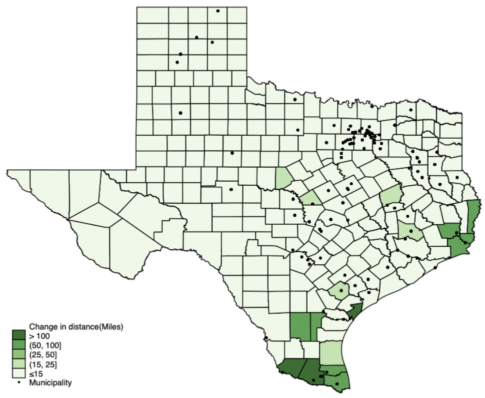
J Sensitivity analysis

Figure J.1: Yearly change in distance from each Texas county to the nearest abortion clinic and municipalities in the sample

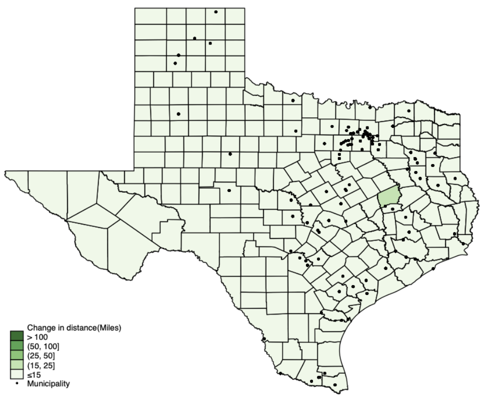
(a) Yearly county change in distance to the nearest abortion clinic from January 2013 to December 2013



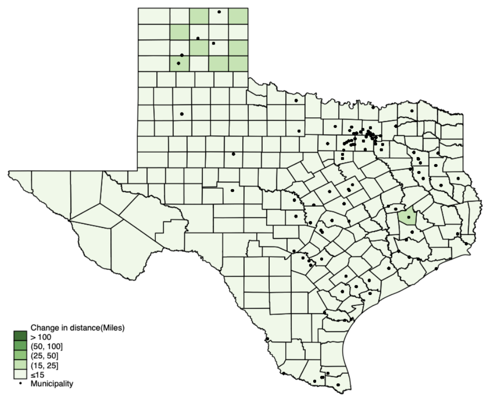
(b) Yearly county change in distance to the nearest abortion clinic from January 2014 to December 2014



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



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



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

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

Table J.1: Estimated effect of a 25-mile increase in distance on gender violence, simultaneous variation in distance

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
Time of distance change	2013	2013	2014	2014
$Distance_t$ (25 $miles$)	.011***	.011**	.019*	.018*
	(.004)	(.005)	(.009)	(.009)
Municipality & six-month FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	432	432	427	427

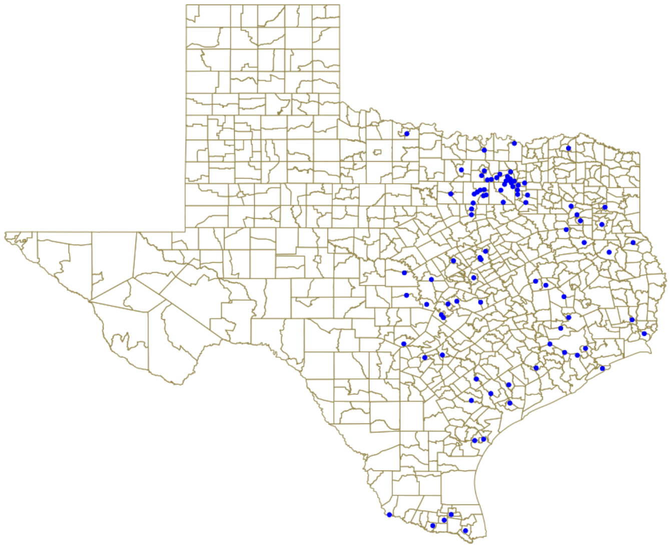
Note: Estimated effect of distance to the nearest abortion clinic on gender violence for a restricted sample of municipalities. Columns (1) and (2) show the estimates for the sample of municipalities treated in the second half of 2013, while columns (3) and (4) present the results for the sample of municipalities treated in the first half of 2014. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Table J.2: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence, accounting for repeatedly treated observations

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
$Distance_t$ (25 $miles$)	.035***	.034***	.026***	.022***
	(.008)	(.007)	(.006)	(.006)
$Distance_t^2$ (25 $miles$)	−.0025***	−.0026***	−.0014**	−.0012*
	(.0006)	(.0005)	(.0006)	(.0006)
Municipality & six-month FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	861	861	839	839

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Figure J.2: Eastern municipalities



Note: Municipalities from the sample located in the eastern part of Texas.

Table J.3: Estimated effect of a 25-mile increase in distance on gender violence, using different samples.

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
Sample	Balanced	Pop. $\leq$ 250,000	Change $\leq$ 100 miles	East
$Distance_t$ $(25\ miles)$	.024***	.026***	.081*	.084*
	(.006)	(.006)	(.044)	(.047)
$Distance_t^2$ $(25\ miles)$	−.0014**	−.0018*		
	(.0007)	(.0005)		
Municipality & six-month FE	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes
Number of observations	532	827	795	758

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

K Year fixed effect

Table K.1: Estimated effect of a 25-mile increase in distance to the nearest abortion clinic on number of cases of gender violence

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
$Distance_t$ (25 miles)	.029***	.028***		
	(.006)	(.006)		
$Distance_t^2$ (25 miles)	−.0018***	−.0019***		
	(.0005)	(.0005)		
$Distance_{t-2}$ (25 miles)			.028***	.028***
			(.010)	(.008)
$Distance_{t-2}^2$ (25 miles)			−.002***	−.003***
			(.0008)	(.0006)
Municipality & Year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	871	871	871	871

Note: Estimated effect of distance to the nearest abortion clinic on gender violence for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

L Placebo test

Type of offense

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

M Evidence on self-induced abortions

Counties near the Mexican border excluded from the analysis

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

Table M.1: Estimated effect of a 25-mile increase in distance on gender violence, excluding municipalities in counties on the Mexican border

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
$Distance_t$ $(min.)$	$.028^{***}$	$.028^{***}$		
	(.007)	(.006)		
$Distance^2_t$ $(min.)$	$-.0017^{***}$	$-.0018^{***}$		
	(.0006)	(.0006)		
$Distance_{(t-2)}$ $(min.)$			$.026^{***}$	$.026^{***}$
			(.01)	(.007)
$Distance^2_{(t-2)}$ $(min.)$			$-.0022^{***}$	$-.0024^{***}$
			(.0007)	(.0006)
Municipality & six-month FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	837	837	837	837

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

Table M.2: Estimated average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on gender violence, excluding municipalities in counties on the Mexican border

	(1) (GV)	(2) (GV)	(3) (GV)	(4) (GV)
$Distance_t$ (25 miles)	3.89^{***}	3.81^{***}		
	(.97)	(.94)		
$Distance_t^2$ (25 miles)	$-.23^{***}$	$-.24^{***}$		
	(.08)	(.03)		
$Distance_{(t-2)}$ (25 miles)			3.61^{***}	3.6^{***}
			(1.3)	(1.01)
$Distance_{(t-2)}^2$ (25 miles)			$-.30^{***}$	$-.33^{***}$
			(.05)	(.04)
Municipality & six-month FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	837	837	837	837

Note: Average marginal effect of a 25-mile increase in distance to the nearest abortion clinic on all forms of gender violence except for IPV for 105 Texas municipalities from 2010 to 2016. Estimates are based on a Poisson model, and the analysis is at the six-month municipality level. All regressions include municipality and six-month fixed effects. The exposure variable included in all regressions is municipal population. Time-varying controls are share of females of reproductive age (15-49) per county, income per capita per county, and unemployment rate per county. Robust standard errors are reported in parentheses and are clustered at the commuting zone level. *, **, and *** indicate statistical significance at ten, five, and one percent levels respectively.

Chapter 2

Conscientious objection among gynecologists and illegal abortions

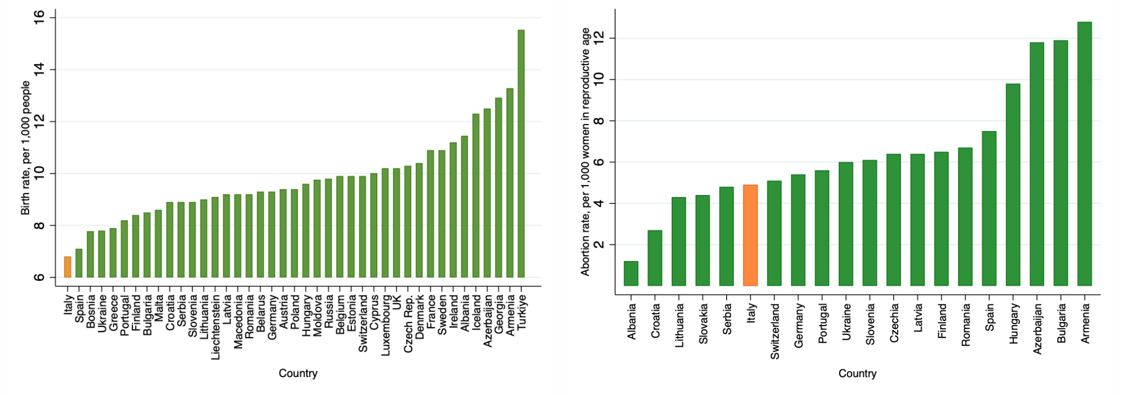
Among European countries, Italy has one of the highest percentages of doctors in public hospitals who deny performing abortions on the basis of conscientious objection. This situation pushes many women to resort to illegal abortion which can end up in complications that require hospitalization.
I evaluate the impact of objection on the individual probability of illegal abortion in Italy, finding that a 10% increase in the share of objecting gynecologists is associated with a 4 to 5% increase in the individual probability of self-inducing an abortion. The effect seems to be driven by immigrant women, showing that are the most disadvantaged social categories to suffer the most from restrictions on abortion access in public facilities.

1 Introduction

Among European countries, Italy has one of the highest shares of gynecologists in public hospitals who deny performing abortions on the basis of conscientious objection. This may result in many women being denied services that they legally qualify for, a situation that pushes the most disadvantaged social categories to resort to homemade practices or clandestine abortion to terminate the unwanted pregnancy. In the present study, I try to answer the question of whether the high number of objectors in Italian public hospitals pushes many women to complete the abortion procedure outside the legal setting.

Some preliminary evidence of this mechanism is given by the trends in the abortion rate, fertility rate, and contraception. The abortion rate in Italy has dramatically decreased in the last decades, but this trend has not been accompanied by a rise in the fertility rate, which instead is also decreasing over time (see Figure A.1 of the Appendix). The rates of abortions and births are also particularly low from a comparative perspective, as shown in Figure 1. The two panels plot the information on birth and abortion rates for the European countries[1] for which the information is available. Among them, Italy shows the lowest birth rate and ranks low also for the abortion rate. In addition, Italy has poor access to contraception when compared to the other European countries.[2]

Figure 1: Comparative statistics: birth rate and abortion rate by European country, year 2020



Note: The abortion rate is the number of abortions per 1,000 women in reproductive ages in a given year. Crude birth rate indicates the number of live births per 1,000 midyear population.
Source: Data on birth rates are from The World Bank database and data on abortion rates are from the Eurostat database.

The discrepancy among these trends may be partly due to the increasing phenomenon of illegal abortions. In 2016, the Italian National Institute of Statistics (Istat), in collaboration with the Italian Institute of Health, estimated the number of clandestine abortions in Italy for the years 2014, 2015, and 2016, as around 10.000-13.000 cases per year. For this estimate, they use the positive difference between the expected births and reported births, minus the registered voluntary terminations of pregnancy (VTP) (Salute, 2017).

In this paper, I evaluate the impact of the share of gynecologists who declare objection on the individual probability of illegal abortion. I use a simple regression model that includes three types of fixed effects: province of abortion, province of birth, and year fixed effects. The identification strategy relies on these fixed effects that should capture all the time-invariant characteristics of the place of the abortion and the place a woman comes from (especially due to the short time frame), plus possible time shocks. In addition, the richness of the data allows the researcher to include many individual control variables.

[1]I chose to use European-only countries as comparison group since Italy shares with them cultural traits. This may imply more similar reproductive behaviors, making these trends more comparable.

[2]See the Contraception Atlas at https://www.epfweb.org/node/89

The paper is organized as follows. Sections 2 and 3 describe the Italian situation regarding abortion accessibility and the phenomenon of self-induced abortions. Section 4 is dedicated to the data and Section 5 contains the empirical analysis. The last Sections are dedicated to several robustness checks, and the discussion and conclusion.

2 Conscientious objection

Italian law (*law 194 of May 22, 1978*[3] *on the adoption of social protection of motherhood and the voluntary termination of pregnancy*) guarantees the right for women to terminate a pregnancy on request during the first 90 days. Abortions are performed free of charge in public hospitals or private structures authorized by the regional health authorities. The law also allows termination in the second trimester of the pregnancy only when the life of the woman would be at risk if the pregnancy is carried to term or the fetus carries genetic or other serious malformations which would put the mother at risk of serious psychological or physical consequences.

Likewise most European legislations, the law gives the option for health professionals to claim the right to refuse to perform abortion (unless the personal intervention is essential to save the life of a woman in imminent danger), i.e. to declare conscientious objection:

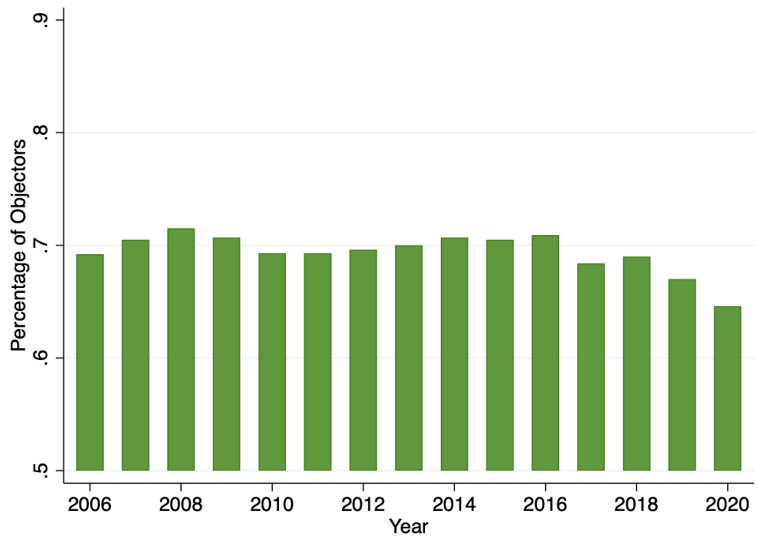
> *"Conscientious objection is the refusal to participate in an activity that an individual considers incompatible with his/her religious, moral, philosophical, or ethical beliefs.*[4]"

The prevalence of conscientious objection varies widely across countries. Italy has one of the highest percentages of objecting gynecologists with respect to the other countries: according to data from the Ministry of Health, between 1997 and 2016 there was a 12.9% increase in the number of gynecologists who refuse to perform abortions, from 62.8% to 70.9%, the highest percentage ever recorded (Figure 2).

[3] https://www.trovanorme.salute.gov.it/norme/dettaglioAtto?id=22302

[4] International Covenant on Civil and Political Rights (1966)

Figure 2: Trends in the percentage of objecting gynecologists



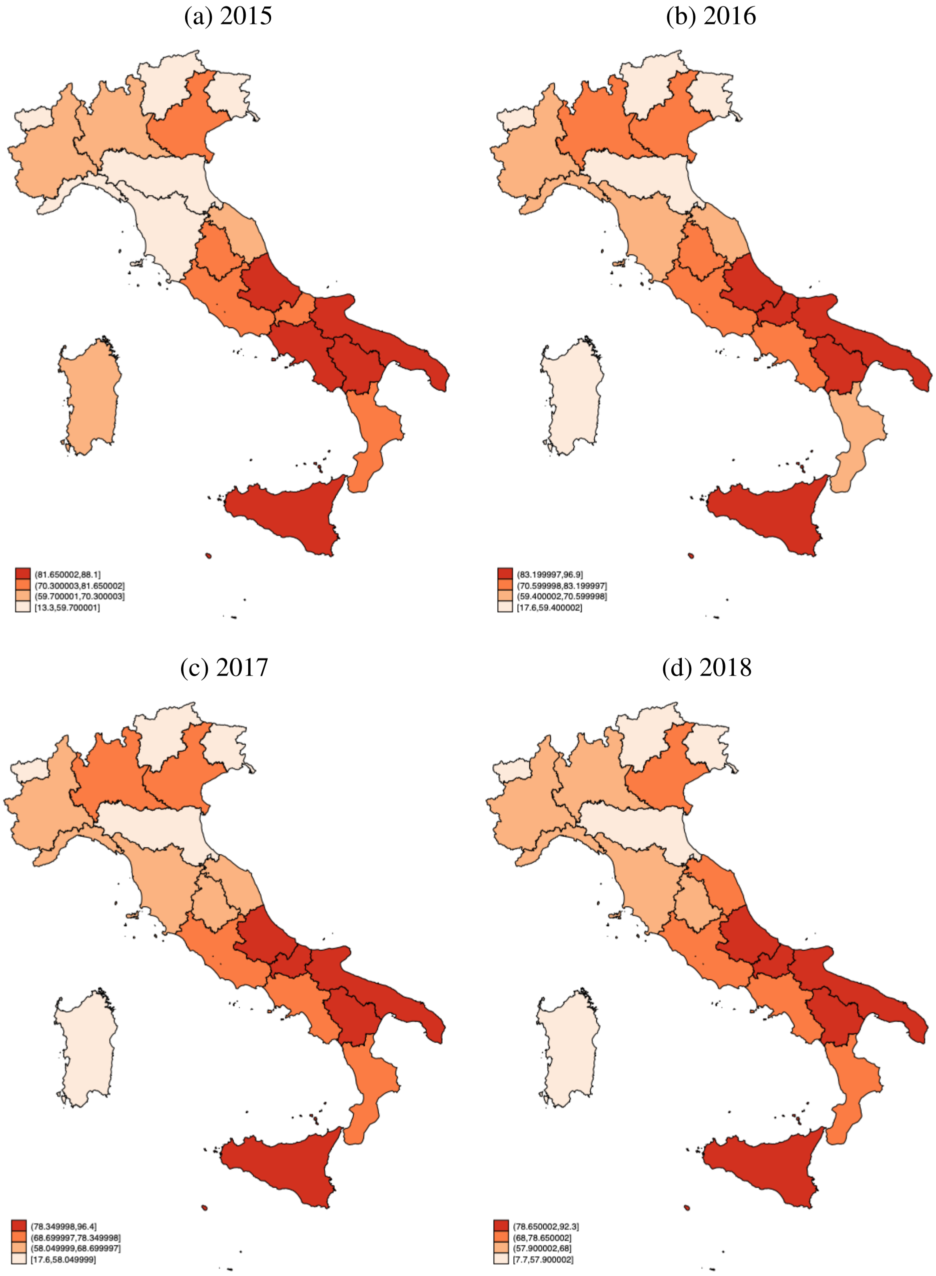
Note: Percentage of gynecologists who declare conscientious objection in Italy. Years 2006-2020.[5]
Source: Ministry of Health

This percentage varies widely across regions. Figure 3 shows the distribution of objecting gynecologists across Italian regions for the four years of my sample (2015-2018). As of 2016, for example, the percentage was higher than the national average in Southern Italy (83.5%) and Sicily and Sardinia (77.7%), and lower in Central (70.1%) and Northern Italy (63.9%). As a result, voluntary abortion was performed only in 60% of the hospitals in the country that have a gynecologist department.

The latest annual relation of the Ministry of Health (2020) confirms that the number of objectors does not create a problem for the supply of the service. Despite the Ministry's consideration, in a 2013 decision, the European Council established that the Italian situation was discriminatory and violated the right to health. In 2016 the Council of Europe verified that Italy was violating the European Social Charter at two main levels. On one hand, it was violating the right to protection of the health of women seeking an abortion, and on the other, it was violating the right to work and to dignity at work of non-objecting medical practitioners, because of different treatment and moral harassment. Again on January 24, 2019, the European Committee of Social Rights of the Council of Europe reaffirmed that there was "disparity of access" to abortion in Italy. Nowadays, Italy is a special observed by the European Council.

[5]There are publicly available data on conscientious objectors before 2006, but they present a lot of missing values, resulting in misleading national averages.

Figure 3: Distribution of objection across Italian regions, 2015-2018.



Note: Percentage of gynecologists who declare conscientious objection in Italian regions. Years 2015-2018.
Source: Ministry of Health.

There is a very little empirical evidence on the impact of conscientious objection on abortion access, partly because of the limited data availability in most countries. For Italy, Bo et al. (2015) find a correlation at the regional level between the workload of non-objecting gynecologists and the waiting times needed to obtain an abortion. Meier et al. (1996), analyzing how twenty-three different U.S. state-level abortion restrictions affected abortion rates, found the conscience clause that allows physicians to refuse to perform abortions to be irrelevant. However, the model incorporated only a dummy variable indicating the existence of this clause, and not a measure of physicians' actual invocation of the clause. Autorino et al. (2020) use regional and individual data on abortion in Italy between 2002 and 2016 and find the share of objecting gynecologists per region to be a significant driver of a woman's decision of having an abortion out of the region of residence and to increase largely the waiting times to obtain one.

Even if there is very little piece of evidence on the consequences of objection, lots of authors have studied the impact of restricted or denied access to abortion on women's reproductive outcomes and socioeconomic conditions, especially with respect to the case of Texas. Between 2004 and 2014 Texas enacted several pieces of legislation regulating abortion, which restrict access to the service through the closure of about half of Texas' abortion clinics. The negative relation between increasing distance to the nearest abortion clinic and the abortion rate in Texas has been extensively studied (Lindo, Myers, Schlosser and Cunningham (2020), Venator and Fletcher (2020), Quast et al. (2017), Fischer et al. (2018), Grossman et al. (2017) and Colman and Joyce (2011). Fischer et al. (2018) and Lu and Slusky (2019) report a positive effect of increasing distance to the nearest clinic on the birth rate. On the opposite, Lindo, Myers, Schlosser and Cunningham (2020) find no evidence of this phenomenon and explain this also through the higher accessibility, safety, and lower cost of modern methods to self-induce abortion.

Religious justification is usually accepted without argument as the primary motivation behind conscientious objection and, not surprisingly, higher levels of self-described religiosity are associated with higher levels of disapproval and objection regarding the provision of certain procedures (Fonnest et al., 2000). Several empirical studies confirm self-reported religiosity to be associated with unwillingness to perform abortion (Aiyer et al., 1999, Hammarstedt et al., 2005). Looking at the existing literature (for a review see (Chavkin et al., 2013, De Zordo and Mishtal, 2011, Fiala and Arthur, 2014) prevalent causes of objection include:

- Lack of economic incentive
- Stigmatization, i.e. non-objecting doctors suffer discrimination and stigmatization.
- Career considerations. Silvana Agatone, a gynecologist and founder of the LAIGA (Libera Associazione Italiana Ginecologi per l'applicazione della legge 194/78) association of non-conscientious objectors, suggests that widespread conscientious objection in Italy has little to do with religious or moral beliefs and more to do with doctor's careers: *"Non-objector gynecologists are often seen as the "dirty" ones, sometimes colleagues isolate them. [...] Moreover, they have more difficulties in advancing their career. The reason is simple: the majority of hospital directors are conscientious objectors, and they often come from religious schools. So in turn*

they tend to prefer doctors who are objectors[6]."

- Inadequate medical training.
- Religion and moral beliefs.
- The excessive workload for non-objectors: because of the very low number of doctors who perform abortions, non-objecting gynecologists are forced to spend all their work hours delivering such service, without accessing the other gynecological specialties.
- Abortion is seen as an uninteresting medical procedure.

Another important reason behind objection is the incorrect idea that facilitating access to safe and legal abortion services promotes abortions. Many practitioners feel uncomfortable with the notion of increasing the number of abortions and following this reasoning, lots of states around the world have restricted access to the service in the past decade (Joyce, 2011). Despite this idea, empirical pieces of evidence show that making legal abortion more broadly available does not increase the abortion rate but reduces maternal mortality and morbidity (Joyce, 2011). On the opposite, countries with the most restrictive abortion laws have the highest rates of abortion, as reported by a study by the Guttmacher Institute of March 2018[7]. The report also found that it is instead the easiest access to birth control that drives down abortion rates. Sedgh et al. (2012) estimate that in 2008, the abortion rate was lower in subregions where larger proportions of the female population lived under liberal laws than in subregions where restrictive abortion laws prevailed.

Abuse of conscientious objection can result in inequities in access, creating disproportionate risks for poor women, young women, ethnic minorities, and other particularly vulnerable groups of women who have fewer alternatives for obtaining services. In Italy, the right of performing abortions is extended to physicians working in private clinics, towards which wealthy women can appeal in case of limited public access.

3 Illegal abortions

As Chavkin et al. (2013) point out, where access to legal abortion services is restricted, women seek services under other circumstances. The drug most commonly used to self-induce an abortion is Misoprostol, mostly known under the brand name Cytotec, sold

[6]https://www.opendemocracy.net/en/5050/abortion-italy-conscientious-objection/
[7]http://www.guttmacher.org/fact-sheet/induced-abortion-worldwide

for the treatment of gastric ulcer, but inducing uterine contractions in 90% of the cases[8]. Misoprostol together with Mifepristone are the two drugs approved by the U.S. Food and Drug Administration (FDA) to perform a medication abortion[9]. Misoprostol alone is effective and safe for medical abortion in the first trimester (Raymond et al., 2019). Jones (2011) estimates that, during the period 2008-2009, 1.2 percent of abortion clinics patients reported that they have self-induced abortion on their own using Misoprostol.

Misoprostol is only available by prescription in Italy but, in addition to being sold on the black market, it is also provided by some international organizations fighting for women's reproductive rights. Among them, *Women on Web* is a non-profit organization providing support for the right to access safe abortion for all pregnant women around the world. In countries where abortion is legal, they provide medical prescriptions for Misoprostol to women less than 10 weeks pregnant. I requested access to their data to gain some evidence about the existence of the phenomenon. The dataset for the period 2015-2019 shows how the requests for medical prescription of Misoprostol in Italy are growing, with a big jump after the translation of the website in Italian in 2018 (Figure 4). This trend also proves how the demand for illegal abortion depends mainly on the accessibility of the supply and it increases as the provision of the service increases. These data need to give empirical evidence of the existence of the phenomenon but should be underlined that they dramatically underestimate the phenomenon for several reasons. First of all, there exist many websites that freely provide or sell abortion pills online, as well as, medical staff or sellers in the black market, for which I cannot have information. Moreover, a lot of women could resort to abortion techniques that do not involve the use of pills (Grossman et al., 2010). Despite these limitations, these data constitute one of the unique empirical proof of the re-emergence of the practice of illegal abortion in Italy.

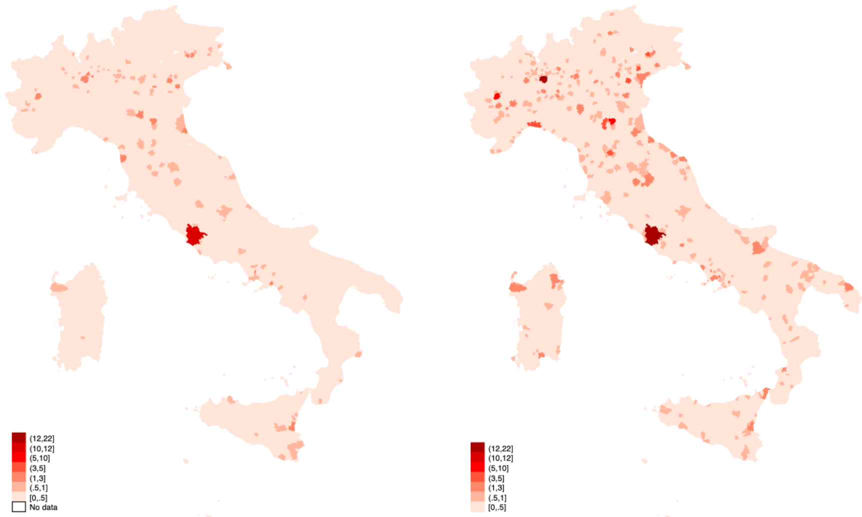
Even if Misoprostol is the most commonly used technique to self-induce an abortion in Western countries, other methods should be mentioned. It is in fact very likely that some disadvantaged and poor women may not have access to the web or may not possess the necessary knowledge to find the pills online and buy them. These other types of abortion are less safe than the medical one and expose women to more serious risks. In my definition of illegal abortion I do not distinguish between "traditional" approaches that rely on herbs, tisanes, massage, etc..., and approaches that rely on allopathic medication (e.g., Mifepristone and Misoprostol) used outside the confines of clinical supervision. Given the widespread use of abortion pills for abortions outside the legal setting in Western countries, I use indistinctly throughout the paper the terms illegal abortion or self-induced/self-managed abortion.

Moseson et al. (2020) collect the methods reported to self-induce an abortion into

[8] Medical abortion within hospital facilities involves the use of Misoprostol together with Mifepristone

[9] https://www.guttmacher.org/evidence-you-can-use/medication-abortion

Figure 4: Delivery of medical prescriptions for Misoprostol. Years 2017 and 2019.



Note: The left figure plots 2017 data and right figure represents 2019 data.
Source: Women On Web (https://www.womenonweb.org/en/)

eight categories: (1) plants/herbs (ingestion), (2) toxic substances (ingestion), (3) intrauterine trauma, (4) physical trauma, (5) a combination of Mifepristone and Misoprostol, (6) Misoprostol only (7) alcohol and drug abuse, and (8) other drugs, substances, and mixtures. The use of Misoprostol and Mifepristone or Misoprostol alone cover 71% of the studies published during or after 2000 contained in the review. The authors report that the studies described people obtaining these pills through online telemedicine services, online vendors, telephone vendors, their social networks, over-the-counter pharmacies, friends, relatives, accompaniment groups, doctors, nurses, and community health workers.

Moseson et al. (2020) cite seven studies that reported on the occurrence of heavy bleeding after a self-managed abortion. Among those who self-managed their abortions using medications after receiving evidence-based guidelines on how to administer Mifepristone and Misoprostol, or Misoprostol alone, the proportion with heavy bleeding ranged from 5.2% up to 13%. They also find that eight studies reported on participants seeking care at a health facility following a self-managed abortion. The percentage of women who visited a doctor or hospital after self-managing an abortion varies between 0.3% and 29%, depending on the geographical area and the type of procedure used. Concerning the occurrence of surgical intervention following self-managed abortion to complete the abortion, the percentage of women varies again across studies, and by method of self-managed abortion, from 2% up to 56%.

As underlined in the review by Chemlal and Russo (2019) and by the Guttmacher Institute, self-managed abortion occurs across settings, including where abortion is legally available on request and accessible. Among the reasons why women in settings where abortion is legal decide to self-manage an abortion, there is the ”staff unwillingness to provide abortion or make a referral” on the ground of personal, religious and cultural reasons (Chemlal and Russo, 2019). According to a recent study, between 2011 and 2015 the number of Google searches using terms related to self-abortion increased from 119,000 to 700,000 and these searches were more common in states with the highest number of abortion restrictions (Stephens-Davidowitz, 2016). In Texas, another study estimated that at least 100,000 Texas residents had ever attempted to end a pregnancy on their own, though it is unknown what methods they used (Grossman et al., 2015).

4 Data and descriptive statistics

Unique dataset on objectors. The first great challenge of the present study concerns the collection of information on the number of gynecologists per hospital who declare and do not declare conscientious objection. The only available dataset on the subject is the one published every year by the Ministry of Health. After the Law on the voluntary termination of pregnancy came into force in 1978, the Surveillance System on Induced Abortion was launched. Within this framework, the National Institute of Statistics started to collect data on conscientious objectors among gynecologists, anesthetists, and non-medical personnel, in coordination with the Italian Regions, the Italian Ministry of Health, and the Italian Institute of Health. Statistics on objection are published every year in a ministerial report that provides information at the regional level. Beyond the high level of aggregation that is problematic in my setting, these public data on objection present a huge measurement error that will be discussed in depth later in this Section. Hence, I collect new data by contacting every regional contact person for the Italian Institute of Health. I obtained information for 322 facilities distributed in 92 provinces of 19 Italian regions (N=964).

Given the reluctance of several regions to transmit such information, the collection lasted for almost a year and data are missing for two regions (Sardinia and Apulia). There are many missing values also in the other regions so, for each year, I exclude from the dataset all the provinces where at least one hospital presents some missing values, for a total of 6 provinces dropped.[10] This check is done at the provincial level, since the independent variable of interest is the number of gynecologists who declare conscientious objection over the number of gynecologists, per province. The denominator also includes the external gynecologists temporarily hired each year by the hospitals to perform abor-

[10]To keep more observations, when a province presented missing values for only one year and the share of objectors is stable over time, I imputed the number of gynecologists, objectors, and non-objectors from the previous/following year.

tions in case of scarcity of non-objecting doctors. I choose to use a provincial measure of access to the procedure to account for the fact that a woman may be willing to move at least within her province to get an abortion. Using a municipal aggregation level was unfeasible since several municipalities don't have a hospital; at the same time, using the hospital level was unrealistic, since large municipalities have more than a single facility. Before aggregation, 24 private facilities are also excluded from the dataset, assuming that women who can afford to be hospitalized in a private hospital are also very likely to be able to pay for abortions in private facilities or to travel outside the province to get an abortion. The final province-level dataset contains 281 observations distributed in 86 provinces during the period 2015 to 2018.

As mentioned earlier, there are issues related to the right of objection that concern legality and that collaborate to generate measurement problems in the public dataset on objection published by the Ministry of Health. The right to objection is illegally applied to entire hospitals generating a huge problem in terms of data collection.[11] Gynecologists working in objecting structures don't have to declare objection and thus they are all registered as non-objecting doctors, even if, in practice, they do not perform abortions. This bias in the data is worsened by the fact that some facilities do not have a VTP point, even if they have an obstetrics and gynecology department.[12] This can be a problem if gynecologists in these facilities are registered as non-objectors. To account for these, when I collected the data I explicitly asked the number of non-objectors who perform abortions. Figure 5 shows data misreporting for Sardinia[13], where stable and relevant differences between reported non-objectors and the real number of doctors who perform VTP can be observed. Since not all Regions specified how many non-objecting gynecologists perform an abortion, I also inspect whether every hospital in my dataset with a positive number of objectors registered at some point in time with at least one VTP, thus identifying all the facilities where no one performs abortions.

By looking at the facilities reported in the Istat dataset on VTP, I find 61 hospitals in my dataset registering zero VTP in every period[14]. Of these facilities, 22 reported a positive number of non-objectors: I control for this by imputing them 100% of objectors.

Survey on hospital discharge after miscarriages. As argued in the previous Section, a share of the women who self-manage their abortion ends up in the hospital, both because of complications and/or heavy bleeding and to complete the procedure. Usually,

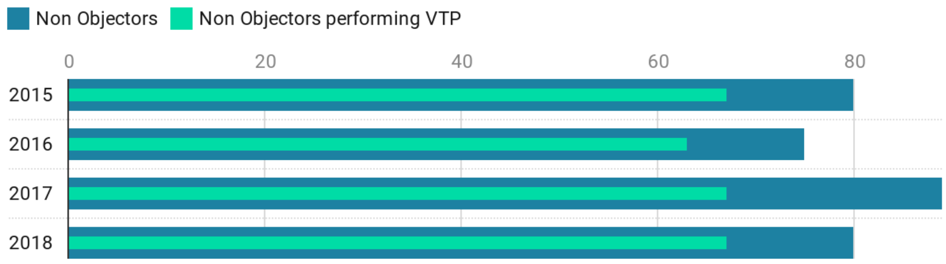
[11]Conscientious objection is also often applied improperly and illegally to hospitals and to emergency contraception with the lack of medical prescriptions by the doctors or with the refusal of pharmacists to sell the day-after pill.

[12]For the whole population, I compare the miscarriage dataset with the VTP dataset identifying more than 100 Italian facilities having an obstetrics and gynecology department, but not a VTP point.

[13]Sardinia didn't send me data disaggregated by hospital or province, so it is excluded from the analysis.

[14]This information is obviously limited to my restricted sample. It does not give a complete picture of the Italian situation in terms of VTP access.

Figure 5: The size of the measurement error: the example of Sardinia.



Note: Gynecologists in Sardinia, 2015-2018.
Source: Data have been collected by the author.

these abortions are registered as miscarriages, since it is very difficult to distinguish a medication abortion from a miscarriage. In addition, physicians may decide to not register the episode as an induced abortion to protect the woman, since inducing an abortion outside a hospital or a private authorized facility is illegal in Italy.

I required access to the dataset *Survey on hospital discharge after miscarriages* compiled by the Italian National Institute of Statistics and comprising anonymized information on all the miscarriages recorded in Italy between 2015 and 2018.[15] After the Law on the voluntary termination of pregnancy came into force in 1978, Istat started collecting detailed information about each episode of miscarriage taking place in any Italian healthcare facility. Detailed characteristics are gathered through an individual and anonymous form filled out by the physician who treats the miscarriage. From this dataset, I built the outcome variable of the present study, which is the individual probability to have tried to self-induce an abortion. To build this variable, I submitted a brief online survey to some gynecologists around the country. I asked them which ones of the 36 causes that appear on the discharge form for miscarriages are most likely connected with the suspect of an illegal abortion. Among the complete list of possible causes for miscarriages reported on the discharge form, the interviewed gynecologists agreed upon only 9 causes as likely to be attributed to a self-induced abortion. These causes constitute the references of the main dependent variables, in the sense that it takes value one if the cause of the miscarriage is one of the followings:

- Professional physical trauma
- Other physical trauma
- Psychic trauma

[15]Data analysis for this work was conducted at the Laboratory for Elementary Data Analysis of Istat and was carried out in compliance with the law concerning the protection of statistical secrecy and personal data. Results and opinions reported in this study are the exclusive responsibility of the author and do not constitute official statistics.

- Other infectious and parasitic diseases
- Cervix lacerations and inflammation
- Cervical insufficiency
- Endometritis
- Inflammatory diseases of the appendages
- Rh incompatibility

Another possible cause is “Other or not determined”, but since it covers too many possible cases, it is not considered as taking value 1 in the construction of the dependent variable. Hence, the individual probability of self-induced abortion takes value 1 for the 0.8% of observations. To check my results on a different definition of the dependent variable, I build another measure of the probability of self-induced abortion, similar to the first one but less strict. I include as causes related to self-induced abortion, all the causes indicated cumulatively by all gynecologists interviewed (not only the ones upon which everyone agrees). The new causes, that add to the previous ones, are:

- Syphilis and its consequences
- Influenza and other viroses
- Uterine fibroids

Hence, for this second definition - from now on *wider definition* - the individual probability of self-induced abortion takes value 1 for the 0.9% of observations.

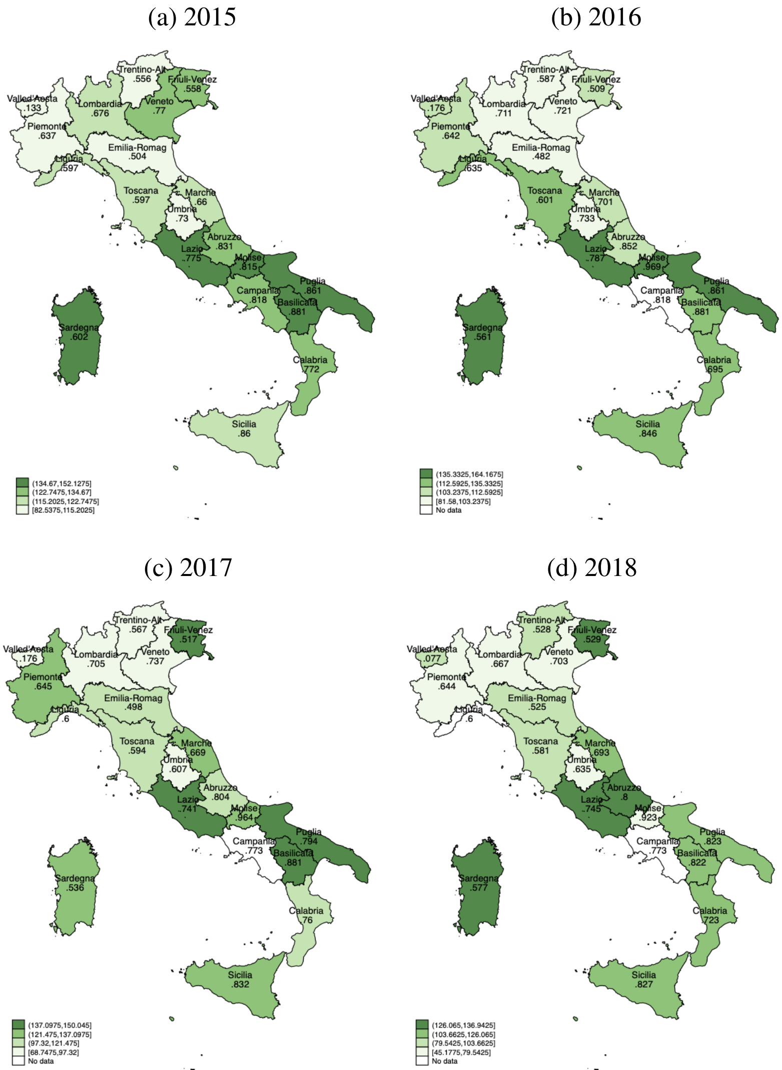
The fact that the category “Other or not determined” includes both miscarriages and self-induced abortions and covers many cases creates a measurement error in both these specifications of the dependent variable. This error is very likely to be random, hence creating an issue only in terms of significance level, not of endogeneity bias. To account for this issue, I use a third specification for the dependent variable that does not suffer from this particular form of error. Then, following the 2016 analysis of Istat on clandestine abortions (included in Salute (2016), pp. 95-104), I build a third definition for the dependent variable: the individual probability of miscarriage in the first 9 weeks of amenorrhea[16], since a self-induced abortion is usually performed in the early stages of the pregnancy. Early-stage miscarriages are very frequent so this variable may suffer from an even larger measurement error but of a different nature. Thus, the fact that the main results are confirmed using this definition is reassuring of the validity of the estimates.

[16] The Istat analysis uses the gestational weeks, but I only have information on weeks of amenorrhea. Using weeks of amenorrhea is slightly more restrictive with respect to the official definition.

Throughout the analysis, the first definition will be taken as the preferred specification. It presents a smaller error when compared to the probability of early-stage miscarriage and it is more strict when compared to the *wider definition*.

To give a first look at the association between miscarriages and objection level, Figure 6 shows the distribution of the miscarriage rate across the Italian regions (in green shades) and the percentage of objectors in each region, using data from the Ministry of Health.

Figure 6: Miscarriage rate and percentage of objectors, years 2015-2018.



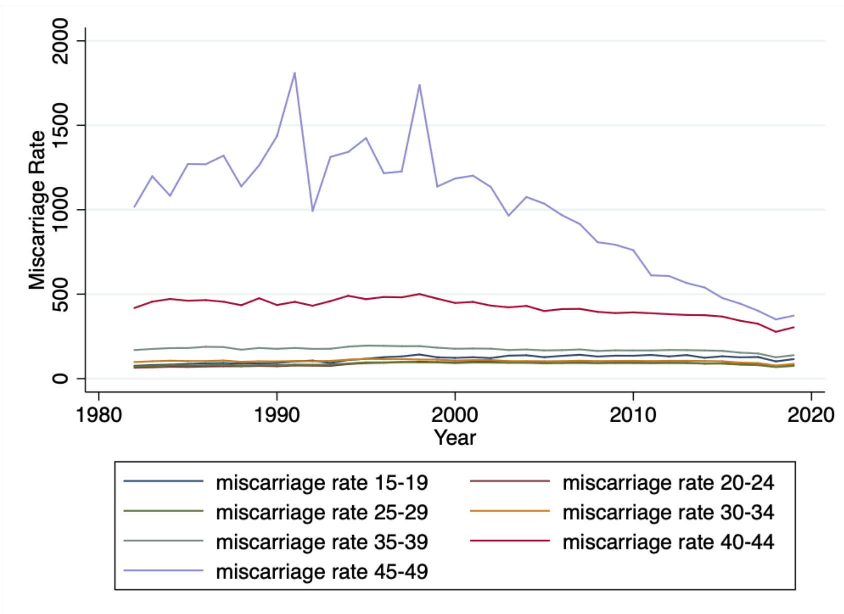
Note: The average miscarriage rate by region is plotted in green shades while percentage of objectors is overwritten, 2015-2018.
Source: Data on miscarriages by region are from Health For All, Istat. The percentage of objectors per region is taken from the relations on the application of the abortion law made every year by the Ministry of Health.

The data on miscarriages dramatically underestimate the phenomenon of self-induced abortions since a consistent part of them does not require hospitalization. Due to the introduction of medical termination of pregnancy, the rate of complications from unsafe abortion has hugely decreased. Studies[17] report a 6-8% curettage/vacuum aspiration rate for incomplete termination of pregnancy using medical abortion. Piffer et al. (2014), in a study in the Province of Trento, estimate that on average, 46% of the cases registered in the emergency room are reported by Istat data.

The use of this dataset presented one main challenge. Each facility may have several identification codes and, often, these codes do not perfectly match the ones reported by the Ministry of Health[18]. Hence, to identify facilities, I manually check each code over more than 300,000 observations.

I consider only miscarriages in public hospitals for the provinces for which I have information on objectors (N=154,792). I restrict the sample to the subpopulation of women aged less than 40 years - since the risk of miscarriage increases dramatically for women older than 40, as shown by Figure 7 - and more or equal to 13 years, the age around which most women become fertile. I also exclude women who became pregnant through the use of artificial reproductive techniques, who are very unlikely to desire an abortion after opting for this procedure.

Figure 7: Miscarriage rate by age category, 2015-2018.



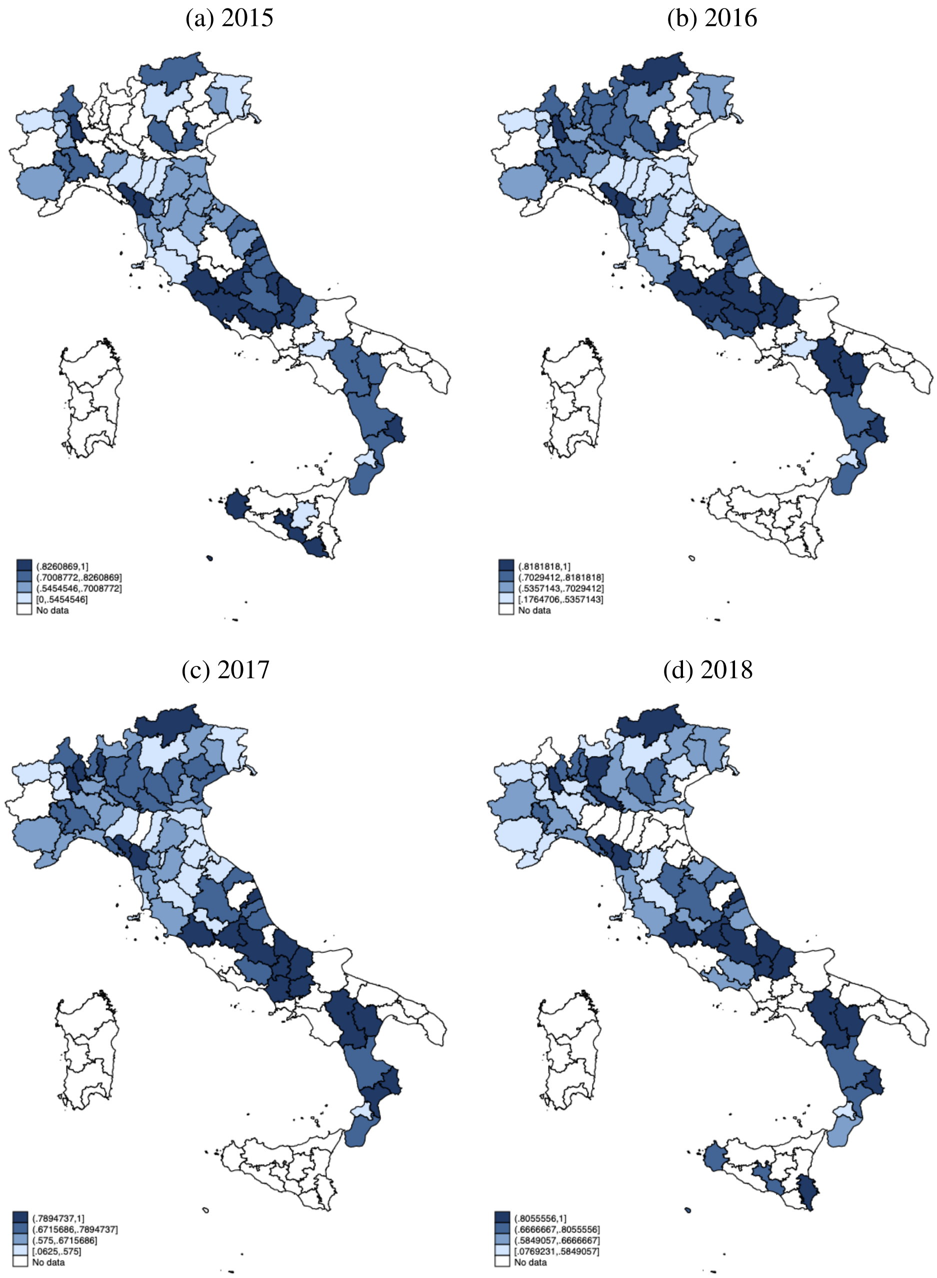
Source: Data are from the project Health-for-All-Italy, by the Italian National Institute of Statistics and the Ministry of Health

Figure 8 gives a graphical representation of the percentage of objecting gynecologists per province. White areas represent provinces with some missing values so that the Figure shows the unbalancedness of the sample.

[17] Gomperts et al. (2008), Faucher et al. (2005) and Ravn et al. (2005)

[18] http://www.dati.salute.gov.it/dati/homeDataset.jsp

Figure 8: Percentage of objectors by province, 2015-2018.



Note: The percentage of objectors is calculated as the ratio between the number of gynecologists who declare conscientious objection and the total number of gynecologists. Every year, only provinces without missing values are considered.
Source: Data have been collected by the author.

Data for the South and Islands have a lot of missing values, both because of the high number of missing observations and the fact that Apulia and Sardinia did not transmit the data. For this geographical area, I have a total of 20 provinces out of 42. Thus, I decided to cut the sample and conduct the analysis only in the North and the Center of Italy. Coefficients from regressions run first on the entire sample and then on the South and Islands alone are shown in Section 6 and suggest that results are manly driven by the North and the Center of Italy.

I end up with a pooled cross-section for the period 2015-2018, composed of 76,743 individual observations, for 426 facilities distributed in 67 provinces[19]. The dataset on miscarriages also includes several individual information on women's socio-economic characteristics and reproductive history, that are included in the main model as individual controls.

Additional datasets from the National Institute of Statistics. To better account for possible source of endogeneity, robustness of the estimates are checked to the inclusion of a time-varying measure of religiosity: the share of religious marriages over the total number of marriages. This is calculated from the dataset *Marriages* by the National Institute of Statistics.

Table 1 reports summary statistics for all the variables used for the analysis.

Table 1: Summary statistics. Years 205-2018

	Mean	Standard dev.	Min.	Max.	N
Probability of self-induced abortion					
Probability of self-induced abortion	0.008	0.088	0	1	76,743
Prob. of self-induced abortion (wider definition)	0.009	0.097	0	1	76,743
Prob. of self-induced abortion (early-stage misc.)	0.577	0.494	0	1	76,743
Complications					
None	0.985	0.123	0	1	76,743
Haemorrhage	0.008	0.091	0	1	76,743
Infection	0.002	0.042	0	1	76,743
Death	0.005	0.072	0	1	76,743
Marital status					
Unmarried	0.409	0.492	0	1	76,743
Married	0.566	0.496	0	1	76,743
Divorced	0.023	0.151	0	1	76,743
Widow	0.002	0.040	0	1	76,743
Educational attainment					
None or primary school diploma	0.056	0.231	0	1	76,743
Middle school diploma	0.246	0.431	0	1	76,743
High school diploma	0.459	0.498	0	1	76,743
University degree	0.239	0.426	0	1	76,743
Nationality					
Italy	0.718	0.450	0	1	76,743
Africa	0.079	0.270	0	1	76,743

continued

[19]Sample selection is described in Table A.1 of the Appendix.

Table 1: Summary statistics. Years 205-2018

	Mean	Standard dev.	Min.	Max.	N
Europe	0.124	0.329	0	1	76,743
Asia	0.053	0.225	0	1	76,743
America	0.008	0.088	0	1	76,743
South America	0.018	0.131	0	1	76,743
Oceania	0.0003	0.017	0	1	76,743
Antarctica	0.0005	0.021	0	1	76,743
Employment position					
Unemployed	0.340	0.490	0	1	76,743
Entrepreneur or freelance professional	0.054	0.225	0	1	76,743
Other autonomous worker	0.037	0.189	0	1	76,743
Employee: managing	0.032	0.176	0	1	76,743
Employee: office worker	0.286	0.452	0	1	76,743
Employee: office or factory worker	0.134	0.340	0	1	76,743
Other employee	0.057	0.232	0	1	76,743
Other individual characteristics					
Age	32.284	5.079	13	39	76,743
Number of previous miscarriages	0.360	0.749	0	14	76,743
Number of previous births	0.706	0.889	0	12	76,743
Number of previous abortions	0.125	0.455	0	15	76,743
Weeks of amenorrhea	9.611	2.936	1	25	76,743
Provincial indicators					
Share of objecting gynecologists	0.648	0.193	0.063	1	218
Share of religious marriages	40.525	7.955	23.7	80.3	210

Note: Summary statistics calculated for the period 2015-2018.
Source: Individual variables are taken from the *Survey on hospital discharge after miscarriages* of the National Institute of Statistics – ADELE. The share of religious marriages is calculated from the dataset *Marriages* by the National Institute of Statistics. Data on objection have been collected by the author.

5 Empirics

5.1 The model

I estimate the following model:

$$y_{ipt} = \alpha_{ipt} + \beta_1\ Objectors_{p_it} + \beta_2\ \Delta_{obj_{p_i,t} - obj_{p_j,t}} + X'_{it}\Gamma + \gamma_{pm} + \zeta_{pb} + \eta_t + \varepsilon_{ipt} \qquad (1)$$

where i indicates the individual, p the province, and t the year. y_{ipt} is the individual probability of self-inducing an abortion that has been described above. $Objectors_{p_it}$ is the share of gynecologists who declare conscientious objection to abortion per province (i.e. number of objectors over the total number of gynecologists, that includes gynecologists temporarily hired by the hospital to perform abortions). X is a vector of individual controls which includes: complications (during the intervention), citizenship, marital status, educational level, age, number of previous live births, number of previous abortions, number of previous miscarriages, position in the profession, and weeks of amenorrhea. γ_{pm}, ζ_{pb}, and η_t are respectively province of miscarriages, province of birth, and year fixed effects.

I assume that women can move to different provinces to get an abortion in the case they cannot obtain one in their own province, before trying to self-induce an abortion, i.e. I consider the possibility of spillover effects. To take into account this issue, I include in the main model the difference between the share of objectors in province i and the share of objectors in neighboring province j ($\Delta_{obj_{p_i,t}-obj_{p_j,t}}$). I do not directly use the spatial lag of the share objectors per province, since there could be a high correlation among neighboring provinces in the percentage of objecting doctors. Hence, the spatial lag may absorb part of the effect of the main independent variable[20].

5.2 Identifying assumption

Regarding the validity of the identifying assumption, a concern could be that the share of objecting gynecologists may be correlated with underlying determinants of women's decision to terminate their pregnancy outside the legal setting. To assess this concern, I follow Card et al. (2019) and implement a series of OLS models for a set of observed individual characteristics, looking for evidence of correlation with my measure of objection. Every regression includes year, province of birth, and province of abortion fixed effects, and errors are always clustered at the provincial level. Table B.1 of Appendix 7 summarizes these results. None of the coefficients is statistically significant, providing evidence of the exogeneity of the regressor.

It is reasonable to assume that fixed effects are able to capture cultural and religious traits that may be correlated with the share of objectors and the woman's probability to self-induce an abortion. Given the centrality of the religious justification in explaining the decision to object, I check the validity of my results to the inclusion of a time-varying measure of religiosity, i.e. the share of religious marriage over the total number of marriages by province. Section 6, which contains the robustness checks, reports these results. The check is done for every definition of the dependent variable, given the importance to assess exogeneity in every regression. As expected, coefficients remain almost unchanged.

[20] As robustness check, I show in Section 6 coefficients from a regression that does not account for spatial dependency.

5.3 Results

Results for the main specification are presented in Table 2, top panel. Errors are clustered at the provincial level.

I estimate the model through OLS, Probit, and Logit. The linear probability model gives non-significant coefficients probably due to the distribution of the dependent variable, i.e. it takes value one for almost 1% of the observations. Thus, I focus on the Maximum Likelihood estimates.

Table 2: Self-induced abortions and conscientious objection. Marginal effects.

	(1) (LPM)	(2) (LPM)	(3) (Probit)	(4) (Probit)	(5) (Logit)	(6) (Logit)
Narrow definition of self-induced abortion						
Share of objectors	.0016	.018	.437***	.384***	.435***	.399***
	(.074)	(.073)	(.108)	(.104)	(.113)	(.109)
Number of observations	76,743	76,743	66,459	66,341	66,459	66,341
Wider definition of self-induced abortion						
Share of objectors	.010	.011	.506***	.447***	.486***	.447***
	(.075)	(.074)	(.126)	(.118)	(.127)	(.120)
Number of observations	76,743	76,743	67,669	67,550	67,669	67,550
Early-stage miscarriages						
Share of objectors	.468**	.440**	.475**	.438**	.476**	.444**
	(.217)	(.215)	(.217)	(.215)	(.216)	(.215)
Number of observations	76,743	76,743	76,743	76,736	76,743	76,736
Provinces FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes	No	Yes

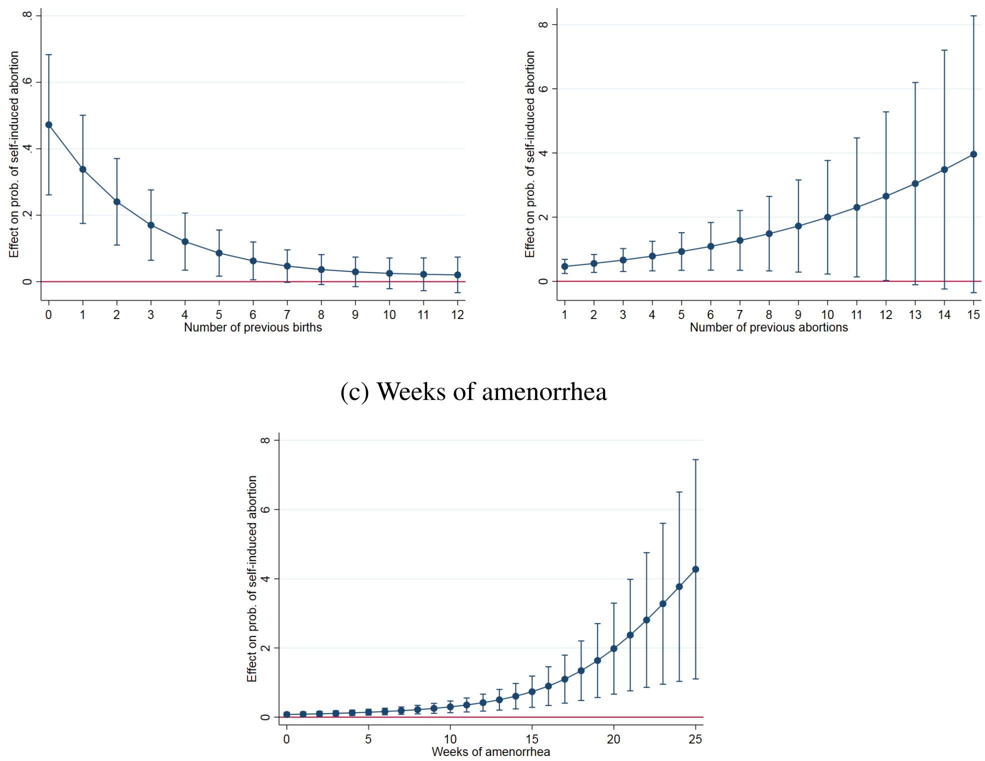
Note: Estimated effect of the share of objecting gynecologists on the individual probability of self-induced abortion, from 2015 to 2018. Estimates are based on a Linear probability model, and Logit and Probit models and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

The marginal effects show that a 10% increase in the share of objecting gynecologists is associated with a 3.8-5.1% increase in the individual probability of self-inducing an abortion. The effect is sizable and consistent across all the estimations. This result shows that the high percentage of objectors within Italian public hospitals creates a problem of access that pushes many women to resort to abortion outside the legal setting.

5.4 Heterogeneous effects

The richness of the dataset allows the researcher to conduct many heterogeneous and subsample analyses. After analyzing all possible heterogeneous specifications, four main results emerge. First, women who already have other kids are less likely to self-induce an

Figure 9: Heterogeneous effects



(a) Previous births

(b) Previous induced abortions

(c) Weeks of amenorrhea

Note: Coefficients are estimated using a Logit model. The figures plot marginal effects.

abortion (Figure 9, panel (a)[21]). Women who already have many children may be more likely to have a partner and/or a net of support. These factors are particularly important because, as already mentioned, poor and disadvantaged women are likely to suffer the most from restrictions on access to abortion in public hospitals. Moreover, women with large families may be less likely to have an abortion for cultural and religious reasons, or simply because of their preferences. As shown by Figure 9 panel (a), the effect decreases with the number of previous births, and for women with 7 or more children, it disappears. This may be due to the very low number of women with 7 or more children[22], while the general decreasing trend in the estimated coefficients is confirmed from the beginning of the distribution.

Similarly, the probability of illegal abortion increases with the number of previous induced abortions. This is in line with women who already had an abortion being more likely to self-induce one. The same reasoning discussed earlier on the insufficient number

[21] In the Appendix are reported average marginal effects of the share of objectors on the probability of self induced abortion with respect to weeks of amenorrhea, number of previous abortions, and number of previous births, estimated both through Probit and Logit models (Tables C.1, C.2 and C.3).

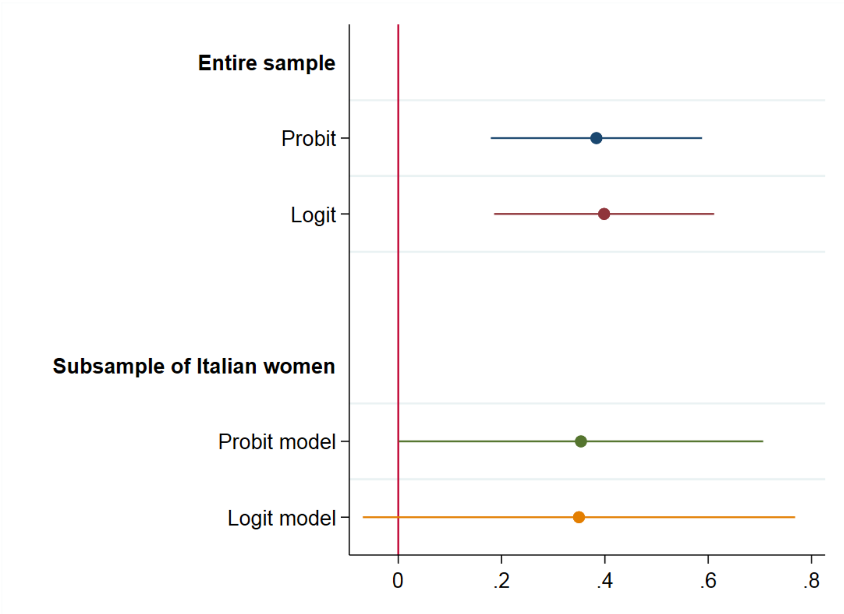
[22] To clarify how many women belong to each category, Table C.4 in the Appendix collects information of the frequency for number of previous births, previous abortions, and weeks of amenorrhea.

of observations for women with more than 4 past abortions suggests looking at the trend for women with few previous abortions. Even restricting the analysis to the left part of the distribution, the increasing trend persists.

On the contrary, women in their initial weeks of amenorrhea show a lower probability of self-inducing an abortion (Figure 9, panel (c)). It is implausible that this is an indicator of women in the first stage of their pregnancy being less like to self-induce an abortion, and more in accordance with illegal abortions in the first weeks of pregnancy being undistinguishable from real miscarriages for which the doctor is not able to determine a specific cause.

Finally, the magnitude and the significance of the effect decrease when I run the regression on the subsample of Italian women, as shown in Figure 10.[23]

Figure 10: Self-induced abortions and conscientious objection: entire sample vs. Italian women



Note: The regression includes all covariates and it is estimated using a Probit and a Logit model.

This is in line with poorer immigrant women suffering more from restrictions on public access to abortion, hence being the main driver of the results.

6 Robustness checks

The robustness checks included in this Section are all performed on the preferred definition of the dependent variable, except for the first one, which plays a role in confirming

[23]Estimated coefficients are reported in Table C.5 of the Appendix.

the exogenity of the model. For all tests, coefficients remain consistent to the use of alternative definitions of the probability of illegal abortion.[24]

As anticipated during the discussion on possible sources of endogeneity, a time-varying measure of religiosity – share of religious marriages – is included in all specifications, given the relevance of such a justification in explaining the objection decision. As shown in Table 3, coefficients remain stable in size and significance, confirming the validity of fixed effects in capturing cultural and religious traits in a short time span.

Table 3: Self-induced abortions and conscientious objection, controlling for a time-varying measure of religiosity. Marginal effects.

	(1) (LPM)	(2) (LPM)	(3) (Probit)	(4) (Probit)	(5) (Logit)	(6) (Logit)
Narrow definition of self-induced abortion						
Share of objectors	-.00191	-.0156	.376***	.343***	.370***	.346***
	(.0720)	(.0722)	(.122)	(.117)	(.123)	(.111)
Number of observations	73,068	73,068	62,533	62,420	62,533	62,420
Wider definition of self-induced abortion						
Share of objectors	-.0228	-.0198	.488***	.444***	.463***	.417***
	(.0734)	(.0738)	(.153)	(.143)	(.151)	(.134)
Number of observations	73,068	73,068	68,831	63,717	68,831	63,717
Early-stage miscarriages						
Share of objectors	.478**	.295**	.481**	.514**	.482**	.517**
	(.229)	(.121)	(.229)	(.230)	(.228)	(.229)
Number of observations	73,068	73,068	73,068	73,068	73,068	73,068
Provinces FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Religiosity	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes	No	Yes

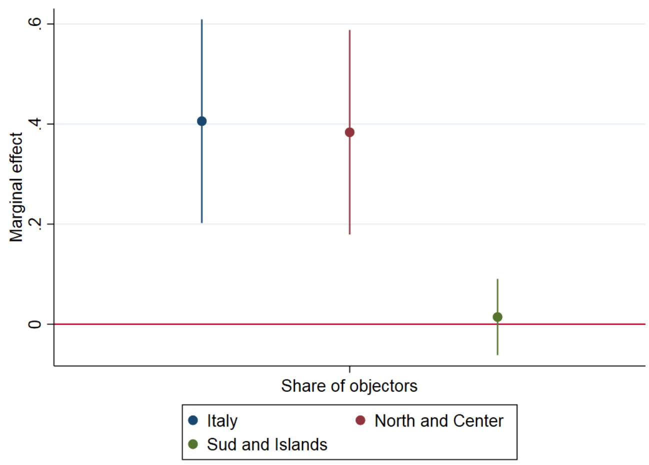
Note: Estimated effect of the share of objecting gynecologists on the individual probability of self-induced abortion, from 2015 to 2018. Estimates are based on a Linear probability model, and Logit and Probit models and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Since the analysis is restricted to the North and Center of the country, I look at the effect for two other specifications of the geographic area - Italy as a whole, and the South and Island. Figure 11 compares point estimates obtained from the main regression runs over these two samples. The effect appears to be driven by the impact in the North and the Center, while it disappears in the South and Islands[25]. This can both be caused by women in the North and Center of Italy being more likely to substitute an abortion at the hospital with a self-induced abortion – for cultural reasons as well as for different levels

[24]Results are available under request.
[25]Coefficients are reported in Table D.1 of the Appendix

of access to the necessary information – and by the huge amount of missing observations for the southern regions.

Figure 11: Impact of objection on the individual probability of self-inducing an abortion for other geographic specifications.



Finally, to confirm the validity of the analysis, I perform a set of robustness checks, reported in Table 4. In columns (1) and (2), standard errors are clustered by region, instead of province; in columns (3) and (4), I insert a fourth type of fixed effects, i.e. province of residence fixed effects. This should account for women moving across provinces to get an abortion, for whom the province of abortion differs from the province of residence. Finally, I run a regression that does not account for spillover effects among provinces. Results are reported in the last two columns. Coefficients remain consistent in magnitude and significance for all specifications except the last one. This result confirms of the presence of spillover effects across provinces.

7 Discussion and conclusion

My research inserts into the policy debate about abortion by considering the effect of the limited applicability of the Italian law that regulates the voluntary termination of pregnancy. I find a positive and significant relationship between the number of objectors in a province and the women's probability to self-induce an abortion in the same province. The empirical analysis also highlights inequalities in access to abortion among women from different socioeconomic backgrounds, as Italian women suffer less from restrictions on abortion in public hospitals. This study contributes to the very small literature on conscientious objection to abortion, by providing evidence on the practical limits poses

Table 4: Robustness checks. Marginal effects.

	(1)	(2)	(3)	(4)	(5)	(6)
	Regional cluster		Province of residence FE		No spillover effects	
	(Probit)	(Logit)	(Probit)	(Logit)	(Probit)	(Logit)
Share of objectors	.384***	.399***	.394***	.400***	.0831**	.0937**
	(.127)	(.136)	(.111)	(.115)	(.0389)	(.0372)
Provinces and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	66,341	66,341	64,538	64,538	66,341	66,341

Note: Robustness checks. Estimated effect of the share of objecting gynecologists on the individual probability of self-induced abortion, from 2015 to 2018. In columns (1) and (2) errors are clustered by region, in columns (3) and (4) province of residence fixed effect is included in the model, and in columns (5) and (6) spillover effects are dropped fro the regression. Estimates are based on a Probit and Logit estimation and the analysis is at the individual-year level. Robust standard errors are reported in parentheses. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

by the high number of objectors and its impact on the growing phenomenon of illegal abortions.

The analysis suggests several policy-relevant points that need to be further discussed. The first important implication of restrictions on abortion access in the public sector is its impact on inequalities. Limiting access to abortion outside the private sector has its largest effect on disadvantaged categories who cannot travel to find a provider and cannot pay for the procedure privately (Harris et al., 2018). This is in line with the heterogeneous analysis conducted in this paper, which shows that the effect of restrictions on abortion access decreases when non-Italian women are excluded from the sample. The fact that the negative consequences of limiting abortion are mainly experienced by the most disadvantaged social categories has not only social justice implications - inequalities in reproductive rights between rich and poor individuals - but it also worsens the economic and social situation of poorer women, who, when abortion is restricted, find themselves more likely to have unwanted children to provide for (the additional costs associated with raising a child typically exceed \$9,000 in annual expenses (Lino et al., 2017)) when compared to more advantaged women. In line with that, many studies have estimated the positive relationship between abortion access and women's socioeconomic conditions. Increased legal access to the abortion procedure is associated with an increase in high school completion, employment rates, earnings, and labor force participation rates (Abboud, 2019, Angrist and Evans, 1999, Jones et al., 2021, Kalist, 2004, Lindo, Pineda-Torres, Pritchard and Tajali, 2020); a decreased likelihood of needing public assistance, living under the federal poverty line and working full time one year later (Foster et al., 2018, Jones et al., 2021); and a higher probability of women moving between occupations and into higher-paying occupations (Bahn et al., 2020). Miller et al. (2020) estimate that women who were denied an abortion experience a significant increase in financial distress during the year that they give birth, compared to women who received a wanted abortion. These effects were particularly strong among Black women (Jones et al., 2021, Kalist, 2004, Lindo, Pineda-Torres, Pritchard and Tajali, 2020), confirming the hypothesis on the unequal impact of abortion access across the economic ladder.

A second relevant point to underline concerns the economic costs related to this situation. Many hospitals need to use external gynecologists to perform VTP, in the case that the small number of non-objectors hired by the hospital is not enough to assure the service. The amount of doctors temporarily hired by each hospital varies greatly among facilities. Here I show the example of Lazio which reports a large use of external doctors. In 2016 - a year for which I do not have missing provinces for this region - the percentage of objectors in Lazio was around 80%. In the same year, about 10% of all gynecologists performing VTP in Lazio were externally hired just to do VTPs. Of the 24 public facilities for which I got the data, 23 use external gynecologists for at least one year of the dataset,[26] Figure 12 shows the shares of objectors and external gynecologists, calculated as the number of objectors (external gynecologists) over the total number of gynecologists performing abortions in each public facility for which I have data.

Figure 12: Shares of objectors and external gynecologists. Lazio, 2015-2018.

Source: Data have been collected by the author.

The high number of objectors in Italian hospitals has also economic implications in

[26]The remaining hospital (n=11 in Figure 12) has missing information for 3 out of 4 years of the panel.

terms of lower career opportunities for non-objecting gynecologists. As argued in this paper, many doctors who decide to perform abortions see their career opportunities vanish, because of the prevalence of objectors among gynecology departments' directors. When a category of individuals is hired and promoted more than another not based on merits and talent but some job-irrelevant attribute, talents end up being misallocated in the job market and the economy loses efficiency.

From a policy perspective, many solutions have been proposed. Some studies suggest changing the law to allow general practitioners to perform early-term abortions, as currently only gynecologists and obstetricians can, thereby significantly increasing the number of willing participants (Gold and Nash, 2013, Minerva, 2015). Minerva (2015) also proposes to provide financial incentives or additional annual leave to non-conscientious objectors and, in accordance with Gold and Nash (2013), to require hospitals to maintain a sufficient ratio of conscientious objectors and non-conscientious objectors to ensure that abortion services are not compromised. Minerva (2015) suggests that each hospital maintains a 50/50 split until empirical studies have been conducted to demonstrate an ideal ratio. Harris et al. (2018) stress the relevance of making the regulations themselves clearer and pursuing complementary strategies to address the environment of stigma. One of these complementary strategies would be to include training on abortion and conscientious objection in pre-clinical education and in-service training for appropriate clinicians that robustly prepare these clinicians to perform abortions. Finally, Fiala and Arthur (2014) argue that there is no place in a democracy for conscientious objection: health systems and institutions that prohibit staff from providing abortion services are being discriminatory by systematically denying healthcare services to a vulnerable population and disregarding conscience rights for abortion providers. They also claim that by manipulating women into continuing an unwanted pregnancy against their best interests, the exercise of conscientious objection undermines women's self-determination and liberty and risks their health and lives.

Given the abundance of policy proposals on the matter, I won't give my preferred solution to the issue, but I conclude the present paper by informing policymakers with additional policy-relevant insights that can guide them in building reforms.

First, restrictions on abortion due to conscientious objection are worsened by the wide spread of surgical instead of medical abortion. Even if medical abortion is legal in Italy since 2009, most gynecologists keep performing the surgical procedure. During the period 2015-2018, the percentage of abortions induced by the abortion pill was around 20% (National Institute of Statistics). Using surgery is not only unnecessarily invasive - making abortion more traumatic than needed - but it also implies the use of more medical personnel and more time, when the staff available is already missing because of objection. Moving toward the common use of medication abortions would allow general practitioners to perform early-term abortions, as proposed by Minerva (2015) and Gold and Nash (2013).

Second, the proper application of the law - that does not permit an entire health facility to completely deny the service because of objection - would change the level of access. A recent investigation (Lalli and Montegiove, 2022) reveals that, in 2022, 24 Italian public hospitals with a VTP point have 100% of objectors among gynecologists, anesthesiologists, nurses, and health workers. As proved by the literature (Lindo, Myers, Schlosser and Cunningham (2020), Venator and Fletcher (2020), Quast et al. (2017), Fischer et al. (2018), Grossman et al. (2017) and Colman and Joyce (2011)), distance to the nearest abortion provider impacts significantly a woman's chance to legally get an abortion. If any authorized facility would provide a minimum level of access, VTP points would be more equally distributed across provinces.

The present analysis has two main limitations that point to the need for further research on the matter. The dependent variable - in all its specifications - suffers from some measurement problems related to the unavailability of information of self-induced abortions (see Section 4). In addition, data on objection are unbalanced. Researchers interested in going deep into the subject should mainly focus on building a complete and updated dataset on objection, paying attention to which doctors effectively perform VTP - not only to the ones who declare to not object. Anyway, the real turning point in the field would be to find a way to better measure illegal abortions.

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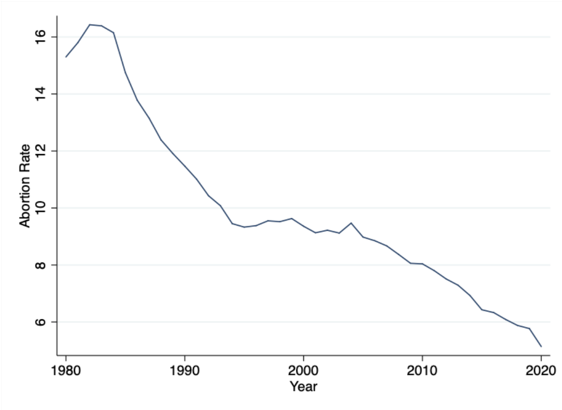
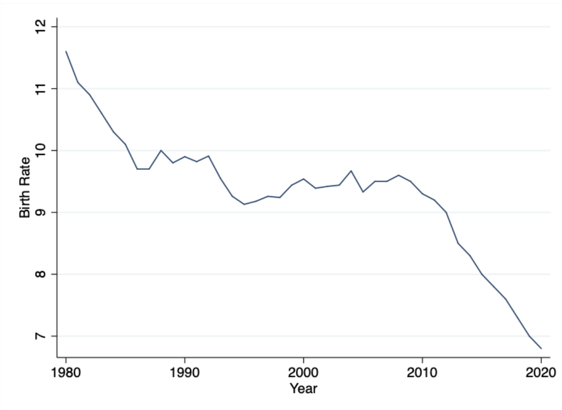
Appendices

A Additional materials

A Stylized facts

Figure A.1: Trends in abortion rate and birth rate. Italy, 1980-2018





Source: National Institute of Statistics

Table A.1: Sample selection

Initial sample	154,792
Miscarriage in private facilities	12,727
Women who use artificial reproductive techniques	2,688
Women younger than 13 or older than 39 years	32,751
Miscarriages in South and Islands	18,937
Observations with missing information	10,946
Final sample	76,743

B Balance test

Table B.1: Orthogonality of share of objectors and individual characteristics

	(1) Share of objectors
Complications	
None	0.013
	(0.013)
Haemorrhage	-0.010
	(0.010)
Infection	-0.034
	(0.003)
Death	0.0004
	(0.008)
Marital status	
Unmarried	-0.040
	(0.040)
Married	0.048
	(0.043)
Divorced	-0.011
	(0.012)
Widow	0.003
	(0.0035)
Educational attainment	

continued

Table B.1: Orthogonality of share of objectors and individual characteristics

	(1) Share of objectors
None or primary school diploma	0.070
	(0.044)
Middle school diploma	0.002
	(0.028)
High school diploma	-0.032
	(0.041)
University degree	0.023
	(0.035)
Nationality	
Italy	0.008
	(0.023)
Africa	-0.004
	(0.024)
Europe	0.001
	(0.016)
Asia	-0.024
	(0.024)
America	0.009
	(0.009)
South America	0.010
	(0.009)
Oceania	0.0001
	(0.0009)
Antarctica	0.0006
	(0.002)
Employment position	
Unemployed	-0.011
	(0.047)
Entrepreneur or freelance professional	-0.017
	(0.024)

continued

Table B.1: Orthogonality of share of objectors and individual characteristics

	(1) Share of objectors
Other autonomous worker	0.019
	(0.024)
Employee: managing	0.008
	(0.012)
Employee: office	0.007
	(0.036)
Employee: factory worker	0.013
	(0.038)
Other employee	-0.019
	(0.018)
Other individual characteristics	
Age	0.357
	(0.298)
Number of previous miscarriages	0.019
	(0.082)
Number of previous births	-0.005
	(0.076)
Number of previous abortions	0.046
	(0.037)
Weeks of amenorrhea	-0.217
	(0.257)
Provinces FE and year FE	Yes
Number of observations	76,743

Note: Estimated coefficients of the impact of individual and miscarriage characteristics on the share of objecting gynecologists. Each row indicates a separate regression. Estimates are based on an OLS model and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

C Heterogeneous effects

Table C.1: Self-induced abortions and conscientious objection with respect to previous births. Marginal effects.

	(1) (Probit)	(2) (Logit)	(3) (Probit)	(4) (Logit)
# of previous births				
1	0.499***	0.457***	.495***	.472***
	(.121)	(.121)	(.125)	(.128)
2	.384***	.334***	.371***	.338***
	(.101)	(.0964)	(0.102)	(0.0990)
3	.296***	.242***	.278***	.240***
	(.087)	(.0789)	(.0871)	(.0792)
4	.230***	.175***	.210***	.170***
	(.0761)	(.0654)	(.0751)	(.0644)
5	.181***	.126**	.161**	.120**
	(.0674)	(.0544)	(.0645)	(.0522)
6	.146**	.0929**	.126**	.0860**
	(.0611)	(.0457)	(.0558)	(.0422)
7	.123**	.0700*	.102**	.0625*
	(.0578)	(.0395)	(.0505)	(.0346)
8	.107*	.0548	.0864*	.0468
	(.0579)	(.0360)	(.0498)	(.0297)
9	.0979	.0451	.0769	.0364
	(.0608)	(.0350)	(.0539)	(.0274)
10	.0932	.0392	.0719	.0295
	(.0655)	(.0359)	(.0619)	(.0270)
11	.0916	.0358	.0703	.0250
	(.0708)	(.0379)	(.0725)	(.0280)
12	.0919	.0341	.0711	.0221
	(.0758)	(.0404)	(.0839)	(.0299)
13	.0932	.0335	.0737	.0204
	(.0798)	(.0429)	(.0939)	(.0325)
Provinces FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	66,459	66,363	66,459	66,363

Note: Estimated marginal effect of the share of objecting gynecologists on the individual probability of self-induced abortion, by number of previous births. Estimates are based on Logit and Probit models and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table C.2: Self-induced abortions and conscientious objection with respect to previous induced abortions. Marginal effects.

	(1) (Probit)	(2) (Logit)	(1) (Probit)	(2) (Logit)
# of previous abortions				
1	0.442***	0.432***	.462***	.464***
	(.123)	(.122)	(.134)	(.134)
2	.499***	.499***	.534***	.556***
	(.152)	(.148)	(.168)	(169)
3	.563***	.575***	.616***	.663***
	(.192)	(.183)	(.214)	(.217)
4	.633***	.660***	.707***	.786***
	(.241)	(.228)	(.272)	(.279)
5	.710**	.755**	.809**	.928**
	(.301)	(.285)	(.342)	(.357)
6	.795**	.861**	.924**	.090**
	(.372)	(.352)	(.426)	(.452)
7	.887*	.980**	1.050**	1.275**
	(.454)	(.433)	(.525)	(.567)
8	.989*	1.111**	1.191*	1.485**
	(.548)	(.526)	(.640)	(.706)
9	1.099*	1.255**	1.346*	1.724**
	(.654)	(.633)	(.774)	(.874)
10	.1.218	1.413*	1.517	1.995*
	(.774)	(.754)	(0.930)	(1.076)
11	1.348	1.586*	1.707	2.304*
	(.906)	(.889)	(1.110)	(1.317)
12	1.488	1.773*	1.916	2.653*
	(1.053)	(1.038)	(1.319)	(1.597)
13	1.638	1.976*	2.147	3.046
	(1.213)	(1.199)	(1.560)	(1.916)
14	1.798	2.193	2.403	3.482
	(1.386)	(1.373)	(1.834)	(2.262)
15	1.970	2.426	2.684	3.960
	(1.572)	(1.557)	(2.144)	(2.623)
Provinces FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	66,459	66,363	66,459	66,363

Note: Estimated marginal effect of the share of objecting gynecologists on the individual probability of self-induced abortion, by number of previous abortions. Estimates are based on Logit and Probit models and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table C.3: Self-induced abortions and conscientious objection with respect to weeks of amenorrhea. Marginal effects.

	(1) (Probit)	(2) (Logit)	(3) (Probit)	(4) (Logit)
Weeks of amenorrhea				
1	.0574**	.0619**	.0703**	0.0781**
	(.0263)	(.0280)	(.0357)	(.0384)
2	.0666**	.0711**	.0795**	.0874**
	(.0282)	(.0299)	(.0375)	(.0401)
3	.0775**	.0820**	.0902**	.0982**
	(.0305)	(.0322)	(.0397)	(.0422)
4	.0908***	.0950***	.103**	.111**
	(.0334)	(.0352)	(.0424)	(.0448)
5	.107***	.111***	.118**	.126***
	(.0370)	(.0389)	(.0459)	(.0481)
6	.127***	.130***	.136***	.143***
	(.0418)	(.0437)	(.0503)	(.0524)
7	.151***	.153***	.157***	.164***
	(.0480)	(.0499)	(.0561)	(.0579)
8	.181***	.182***	.183***	.189***
	(.0561)	(.0580)	(.0637)	(.0651)
9	.218***	0.216***	.214***	.218***
	(.0665)	(.0683)	(.0736)	(.0746)
10	.263***	.259***	.251***	.255***
	(.0800)	(.0814)	(.0867)	(.0868)
11	.320***	.311***	.298***	.299***
	(.0972)	(.0981)	(.104)	(.103)
12	.389***	0.375***	.355***	.353***
	(.119)	(.119)	(.126)	(.123)
13	.474***	.453***	.426***	.420***
	(.146)	(.145)	(.154)	(.149)
14	.577***	.547***	.515***	.503***
	(.180)	(.177)	(.190)	(.182)
15	.702***	.659***	.627***	.606***
	(.221)	(.216)	(.235)	(.223)
16	.851***	.792***	.768***	.736***

continued

Table C.3: Self-induced abortions and conscientious objection with respect to weeks of amenorrhea. Marginal effects.

	(1) (Probit)	(2) (Logit)	(3) (Probit)	(4) (Logit)
	(.271)	(.263)	(.293)	(.275)
17	1.026***	0.948***	.945***	.898***
	(.332)	(.319)	(.367)	(.340)
18	1.229***	1.129***	1.166**	1.098***
	(.403)	(.385)	(.459)	(.422)
19	1.463***	1.337***	1.438**	1.343**
	(.487)	(.463)	(.575)	(.524)
20	1.726***	1.570***	1.766**	1.636**
	(.583)	(.552)	(.718)	(.649)
21	2.019***	1.829***	2.152**	1.980**
	(.693)	(.653)	(.893)	(.800)
22	2.337***	2.113***	2.593**	2.372**
	(.814)	(.766)	(.099)	(.979)
23	2.678***	2.416***	3.083**	2.807**
	(.947)	(.890)	(1.336)	(1.184)
24	3.036***	2.736***	3.612**	3.277**
	(1.090)	(1.023)	(1.600)	(1.414)
25	3.404***	3.068***	4.166**	3.771**
	(1.241)	(1.166)	(1.884)	(1.663)
26	3.774***	3.403***	4.722**	4.273**
	(1.396)	(1.314)	(2.181)	(1.926)
Provinces FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	66,459	66,363	66,459	66, 363

Note: Estimated marginal effect of the share of objecting gynecologists on the individual probability of self-induced abortion, by weeks of amenorrhea. Estimates are based on Logit and Probit models and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Table C.4: Frequency table. Number of previous births, number of previous abortions, and weeks of amenorrhea

	Frequency	Percentage
Number of previous births		
0	38,694	50.42
1	26,099	34.01
2	8,911	11.61
3	2,303	3.00
4	500	0.65
5	146	0.19
6	45	0.06
7	19	0.02
8	9	0.01
9	5	0.01
10	9	0.01
11	2	0.00
12	1	0.00
Number of previous abortions		
0	69,460	90.51
1	5,672	7.39
2	1,177	1.53
3	288	0.38
4	74	0.10
5	36	0.05
6	14	0.02
7	8	0.01
8	8	0.01
9	4	0.01
10	1	0.00
15	1	0.00
Weeks of amenorrhea		
1	32	0.04
2	18	0.02
3	27	0.04
4	293	0.38
5	1,218	1.59

continued

Table C.4: Frequency table. Number of previous births, number of previous abortions, and weeks of amenorrhea

	Frequency	Percentage
6	5,155	6.72
7	7,652	9.97
8	14,195	18.50
9	15,654	20.40
10	12,329	16.07
11	7,306	9.52
12	5,072	6.61
13	1,904	2.48
14	1,032	1.34
15	838	1.09
16	898	1.17
17	629	0.82
18	557	0.73
19	516	0.67
20	514	0.67
21	393	0.51
22	327	0.43
23	101	0.13
24	52	0.07
25	31	0.04
	76,743	100.00

Note: Absolute numbers and percentages of women in the sample for each value of the variables: number of previous births, number of previous abortions, and weeks of amenorrhea

Table C.5: Self-induced abortions and conscientious objection for the subpopulation of Italian women. Marginal effects.

	(1) (Probit)	(2) (Probit)	(3) (Logit)	(4) (Logit)
Share of objectors	.375**	.354**	.383*	.350
	(.184)	(.180)	(.218)	(.213)
Provinces FE and year FE	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes
Number of observations	43,437	43,384	43,437	43,384

Note: Estimated effect of the share of objecting gynecologists by province on the individual probability of self-induced abortion, from 2015 to 2018. The sample is restricted to include only the subpopulation of Italian women. Estimates are based on Logit and Probit models and the analysis is at the individual-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

D Robustness checks

Table D.1: Self-induced abortions and conscientious objection by geographic area. Marginal effects.

	(Italy)		(South & Islands)		(North & Center)	
	(Probit)	(Logit)	(Probit)	(Logit)	(Probit)	(Logit)
Share of objectors	.406***	.424***	.0144	−.0123	.384***	.399***
	(.104)	(.112)	(.0389)	(.0396)	(.113)	(.109)
Provinces FE and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	81,847	81,847	13,459	13,459	66,341	66,341

Note: Estimated effect of the share of objecting gynecologists by province on the individual probability of self-induced abortion, from 2015 to 2018, by geographic area. Estimates are based on Logit and Probit models and the analysis is at the provincial-year level. All regressions include province of miscarriage, province of birth and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the provincial level. *, ** and *** indicate statistical significance at ten, five and one percent levels respectively.

Chapter 3

The COVID-19 Pandemic and School Closure: Learning Loss in Mathematics in Primary Education[1]

Italy was the first Western country hit by Covid-19 in February 2020, responding with a tight lockdown and full school closure until the end of the school year. This paper estimates the effect of the pandemic and school closure on the math skills of primary school pupils in Italy. We compare the learning achievements of two cohorts of pupils, the pre-Covid and the Covid cohort. For both cohorts, we match scores on the national standardized assessment in grade 2 with scores on a standardized test delivered by the researchers at the end of grade 3. The pandemic had a large negative impact on the pupils' performance in mathematics (-0.19 standard deviations). Among children of low-educated parents, the learning loss was larger for the best-performing ones (up to -0.51 s.d.) and for girls (-0.29 s.d.).

[1]This chapter is based on a joint work with Dalit Contini, Maria Laura Di Tommaso, Daniela Piazzalunga, and Lucia Schiavon, " Who Lost the Most? Mathematics Achievement during the COVID-19 Pandemic." The BE Journal of Economic Analysis & Policy 22.2 (2022): 399-408.

1 Introduction

In a bid to contain the number of cases during the Covid-19 pandemic, most countries imposed severe lockdown measures. Schools worldwide were closed for several months starting from spring 2020. By the end of June 2020, students had experienced 7 to 19 weeks of school closure and UNESCO (2020) reported that about 1.6 billion students, more than 90% of the world's student population, did not attend in-person teaching. A year later, by the end of June 2021, the weeks of closure had risen to 60. In most countries, school closures were just one of the features of the lockdown, which also included severe measures to ensure social distance and limited contact with others. Many children's lives were also profoundly affected by the pandemic in other ways, such as in the case of parental job loss or friends' and relatives' illnesses.

These disruptions have raised concern over the human capital development of children, both in the short- and the long-term (OECD, 2021), related to learning losses, adverse socio-emotional effects, mental health issues, and increasing educational inequalities. In 2020, for the first time since the concept was developed, the United Nations Development Programme simulated a decrease in human development of about -0.025 (UNDP 2020).

The detrimental effect of Covid-19 school closures on the educational performance of primary school pupils and educational inequality has been empirically assessed in a number of studies analysing standardized test scores, mainly focused on Anglo-Saxon countries and a few Western European countries (Belgium, Germany, and the Netherlands).[2] The majority of these studies report declining student achievements both in reading/comprehension and in math, with about $0.07 - 0.10$ standard deviations in the latter for 8-10 weeks of school closure. The notable exception is Maldonado and De Witte (2022), who quantify the learning loss in Belgium to be about 0.19 standard deviations in math and 0.29 in Dutch for 9 weeks of school closure. The research also documents larger learning losses for disadvantaged children and children living in deprived areas or enrolled in low socioeconomic (SES) schools.

In this paper, we evaluate the impact of the pandemic and school closures during the spring of 2020 on the mathematics achievements of primary school pupils in Italy, in the province of Torino. We compare the progress over about one year of two cohorts: a pre-Covid cohort - pupils enrolled in grade 3 during the school year 2018-19 - and a Covid cohort - pupils enrolled in grade 3 during the school year 2019-20 who experienced

[2] Australia: Gore et al. (2021). U.S. states: Dorn et al. (2020), Kuhfeld et al. (2020), Domingue et al. (2021), Kogan and Lavertu (2021), Pier et al. (2021). United Kingdom: Blainey and Hannay (2021), Renaissance Learning (2021), Rose et al. (2021). Belgium: Maldonado and De Witte (2022). Netherlands: Engzell et al. (2021), Haelermans et al. (2021); Germany: Schult et al. (2022). Switzerland: Tomasik et al. (2021).

school closure and the pandemic. For both cohorts, we use scores from the national standardised assessment in grade 2, matched with the scores from a standardized test delivered by the researchers at the end of grade 3. Our main goal is to estimate the impact of the school closure on learning inequalities. To this aim, we focus on the heterogeneous impact of the pandemic by family background and prior level of achievement.

The effects of the school closure across countries are likely to be strongly influenced by the school system and the characteristics of the pandemic itself, such as the infection rate, the type of lockdown, and the length of school closure. To the best of our knowledge, this is the first paper looking at the impact of the pandemic on Italian children's learning, adding to preliminary descriptive evidence available from the national assessment conducted in 2021 (INVALSI, 2021).[3]

Italy is a particularly interesting case, because it was one of the first countries severely affected by the Covid-19 pandemic after China, and the first Western country to experience a widespread outbreak and rapid transmission of the virus. Italian schools were closed for almost an entire semester, for 15 weeks starting on February 24, 2020. This was one of the longest school closures in Europe during spring 2020, where the average school closure lasted less than 10 weeks. In-person instruction was replaced, whenever possible, by distance education, with teachers, pupils, and schools alike largely unprepared and left struggling to cope. Apart from school closures, the first Italian lockdown entailed the enforcement of strict social distancing measures. Public parks were closed, and people were only permitted to walk within a radius of 200 meters from home. All social venues, such as coffee shops, restaurants, museums, and libraries, as well as most business and service activities were closed, with the exception of "essential" ones. This had serious repercussions on income and employment. The staggering number of infections, which was largely underestimated in 2020, put enormous pressure on the health care system, and completely upended the lives of families and children. Champeaux et al. (2020) found that the negative effect of the lockdown on children's emotional wellbeing, estimated on the basis of parents' perception, was twice as large for Italian children than for French ones. However, due to the lack of national assessments in 2020, there is no documented evidence of the effect of the pandemic on learning losses in Italy.

It should be noted that before the outbreak of the pandemic, Italy had one of the lowest scores on the Digital Economy and Society Index in the European Union, one of the lowest shares of households with a fixed broadband subscription, and one of the lowest shares of individuals with at least basic software skills (Commission, 2020). These figures

[3]INVALSI measured children's skills in math and Italian in May 2021 and compared them with the previous assessment from May 2019. They found that learning losses in primary school were negligible in Italian and small in math (INVALSI, 2021). However, their results are based on a raw difference between the two cohorts (i.e. second grade in 2019 and 2021) and cannot be interpreted in a causal way, without controlling for prior performance and possible compositional differences.

are mirrored within school settings, with teachers usually having low ICT skills and little experience with blended and technology-enhanced teaching (OECD, 2018, Schoolnet, 2012).

Engzell et al. (2021) has defined the Netherlands as a "best-case" scenario, because of its short school closures, the low impact of the first wave of the pandemic, the country's high degree of technological preparedness, and more in general, its well organized and efficiently managed school system (Woessmann, 2016). Under the same criteria, Italy might instead be considered one of the "worst-case" pandemic scenarios in Europe.

There are at least two main reasons for evaluating the impact of the pandemic on primary school children in Italy. The first and more general one is that childhood is a crucial period for the development of an individual over the entire lifecycle, and child development is considered a dynamic and cumulative process, where early investments have the highest rate of return. Also, inequalities in children's cognitive skills and academic achievements due to family background arise early in life and increase quickly over time (Cunha and Heckman, 2008). Second, during the 2020 lockdown, only 65% of primary school pupils in Italy were provided opportunities for online lessons, as opposed to almost 100% of lower and upper secondary school students (Champeaux et al., 2020).

Our results indicate that the school closure had a large negative mean impact on the math competencies of pupils (-0.19 standard deviations), which is equivalent to about 3 months of school. Somewhat unexpectedly, on average we do not find evidence of increasing inequalities among children with different family backgrounds. Instead, we find heterogeneous patterns within the group of children with low-educated parents: the learning loss in that cohort was larger for the best-performing children (up to -0.51 s.d.) and for girls (-0.29 s.d.).

Our results suggest that the children whose performance suffered most were those who normally benefit the most from attending school. The children of low-educated parents may have had little support within the family to cope with the situation, and among them, the best-performing were those who usually gain most from school attendance.

The rest of the paper is organized as follows. Section 2 summarizes the possible channels through which the pandemic may have affected learning and the existing evidence. Section 3 presents the empirical strategy and Section 4 describes the data. In Section 5, we report our main results on the effects of the pandemic on math skills and the effects in terms of educational inequality. Section 6 discusses the limitations of the study, and Section 7 concludes.

2 Background

The effects of the pandemic on pupils' educational outcomes could be direct, as a result of school closures, or indirect, as the result of changes in the lives of the children and their families which may, in turn, have had an impact on learning.

Direct effects. Following the analysis by Agostinelli et al. (2022), we expect school closure to have a detrimental effect on pupils' educational outcomes and to widen educational inequalities owing to the different effects it had on children's development across the socioeconomic ladder.

First, the crisis was characterized by the widespread use of distance learning, but the digital tools and stable internet connection required for taking part in online lessons were not always available to children. As many as 12.3% of students in Italy between 6 and 17 years did not have access to a computer or other digital tools at home in the years 2018-19 (Istat, 2020). Students lacking a computer/tablet or a good internet connection may have been severely affected by the school closure (Gavosto and Romano, 2020). Moreover, global evidence shows that online learning is not as effective as the traditional classroom (Andrew et al., 2020). A starting point for evaluating the direct impact of the shift to distance learning is that of the existing research on the effect of time-in-school and summer learning loss. There is evidence that time spent in school reduces inequalities, particularly in math (Battistin and Meroni, 2016, Marcotte, 2007), and that long summer breaks have negative effects on educational outcomes and are a major source of learning inequality (Alexander et al., 2007, Cooper et al., 1996, Downey et al., 2004).

The second channel pointed out by Agostinelli et al. (2022) is the change in peer environment. In our scenario, the peer effect involves the psychological impact of losing contact with some friends and having a different pool of peers to socialize with. In turn, socialization with peers has a sizable impact on education (Epple and Romano, 2011, Sacerdote, 2011), and may negatively impact children's academic performance. This effect is particularly large for low-attaining children and children from disadvantaged families, for whom schools provide an opportunity to socialize with children from more privileged households. Thus, another way that Covid-19 school closures increased educational inequality was through changes in the peer groups that children had access to. One of the channels through which schools operate as an equalizer is by mixing children from different socioeconomic backgrounds.

The third channel was the parents' response to the school closure (Agostinelli et al., 2022). Distance learning places additional demands on parents, whose response depends on their level of education, time availability and financial resources: richer and better-educated parents are in a better position to meet these demands. In response to the closing of schools, those parents may have invested more in their children than poor parents, since not only did they have more financial resources to do so and higher levels of previous knowledge, but their children had on average higher human capital. Hence, parents' response to school closure added another layer of inequality to educational opportunities.

Indirect effect. Besides the direct effect of school closure on children's learning, the pandemic impacted pupils' educational outcomes by affecting several other aspects of their lives. The pandemic may have caused children to face severe changes such as parental job loss, disruptions in social ties, a lack of after-school activities, crowded dwellings, illness, and death of relatives due to Covid, isolation, and stress. Each of these changes could have affected children's learning: students whose parents experienced partial or complete earning loss would have been less likely to receive additional paid learning resources (Hupkau et al., 2020) and more likely to experience grade retention (Stevens and Schaller, 2011) than similar children whose parents did not experience a drop in earnings; paternal job loss has been found to have a negative effect on children's school performance (Rege et al., 2011, Ruiz-Valenzuela, 2020); after-school activities like sports, school-related activities, reading and caring/tidying up activities have been estimated to have a positive effect on children's cognitive and non-cognitive development (Felfe et al., 2016, Fiorini and Keane, 2014, Meroni et al., 2021) which in turn may influence learning abilities and cognitive development (Almlund et al., 2011); finally, for all the reasons mentioned above, the quarantine has been reported to have negatively affected children's mental wellbeing, and particularly their ability to concentrate (Orgilés et al., 2020), which was made even more difficult by crowded dwellings. These changes also seem to have had an unequal effect on children's educational outcomes, especially through the higher probability of parents from disadvantaged backgrounds to have experienced a partial or complete earnings loss since the onset of the pandemic (Hupkau et al., 2020) and the larger impact of parental job loss on educational outcome for disadvantaged students (Rege et al., 2011, Ruiz-Valenzuela, 2020).

3 Empirical strategy

To evaluate the effects of the Covid-19 pandemic on the math achievements of children, we adopt a difference-in-differences strategy. In our sample, there are two cohorts of children. The treated children are those who at the end of grade 3 had experienced the pandemic and school closure (for the sake of brevity, we refer to them as the "Covid cohort"). The Covid cohort were enrolled in grade 3 during the school year 2019-2020 and were provided with distance learning instead of in-person classroom lessons from February 2020 until the end of the school year. The control children were those enrolled in grade 3 during the school year 2018-2019 who participated solely in traditional classroom lessons (the pre-Covid cohort).

Due to the availability of longitudinal data at the individual level (see Section 4), we can estimate the average impact of the Covid-19 pandemic on math achievements with the following model:

$$Y_{1ikj} = \beta_0 + \beta_1 C_{kj} + \beta_2 Y_{0ikj} + \beta_3 X_{ikj} + \beta_4 D_j + e_{ikj} \tag{1}$$

where Y_{1ikj} is a standardized math test set by child i of cohort k in school j at about age 8, i.e. at the end of grade 3 (MATHGAP test, described below); C_{kj} is a dummy variable equal to 1 if the child i is in the Covid cohort k,0 otherwise; Y_{0ikj} is a vector of initial skills at about age 7, including the standardized math and Italian tests taken at the end of grade 2 (INVALSI tests, described below) and the mark in math assigned by the teachers at the end of the first term of grade 2; X_{ikj} is a vector of sociodemographic variables (age, gender, migratory background, parental education)[4]; D_j is a vector of school dummies, i.e. school fixed effects, which account for the large heterogeneity observed across schools; e_{ikj} are stochastic errors normally distributed and clustered at the class level. β_1 is the coefficient of interest: it captures the causal effect of being part of the Covid cohort rather than the pre-Covid cohort on math skills at age 8, given previous performance in math and Italian. As the outcome variable is standardized, the impact is expressed in terms of standard deviations. The identifying assumption is that conditional on grade 2 test abilities, the math performance of children in grade 3 in the Covid cohort would have been the same as the pre-Covid cohort had the pandemic not occurred. This assumption seems rather weak, given that the two cohorts are just one year apart.

Since we are not only interested in the average impact of the pandemic but also in its differential impact across children with different socio-demographic characteristics, we also estimate a similar model including a set of interactions between C_{kj} (the dummy identifying the Covid-cohort) and initial math competences, gender, migratory background, and parental education. To highlight potential differences between social backgrounds, we also estimate the coefficients of such interactions separately for the children with low- and high-educated parents.

It is necessary here to clarify an important point about the outcome variable. Y_{1ikj} was observed at the end of grade 3 (at the end of April 2019) for children in the pre-Covid cohort, but at the beginning of grade 4 (October 2020) for children in the Covid cohort (because of the Covid-related school closure in the spring of 2020). The potential consequences of this temporal misalignment will be discussed in Section 6.

[4] A definition of dependent and independent variables can be found in Table A1 in Appendix A.

4 Data and descriptive statistics

4.1 Data and math tests

We construct a unique dataset, linking the results of a standardized test administered by the research team to pupils at the end of grade 3 (named the MATHGAP test) with information coming from the Italian National Institute for the Evaluation of Education and Training System (INVALSI), which includes the INVALSI standardized tests in math and Italian administered at the end of grade 2, teacher-assigned marks, and socio-demographic variables.

INVALSI assessments in grade 2

In the Italian educational system, children enter formal schooling at age 6. Primary education lasts for five years until age 11. Curricula and learning targets are set at the national level, but teachers are completely free to choose the teaching methods they feel are best. The school year starts in early September and finishes in mid-June. In primary school, math instruction covers the domains of numeracy, relations, data and predictions, space and figures.

The INVALSI assessment tests were first administered to the entire population of Italian students in grade 8 in the school year 2007-2008. The following year, the INVALSI tests were extended to pupils in primary schools in grades 2 and 5, and over the years they have also been administered to students in grades 10 and 13.

In grade 2, pupils complete two INVALSI achievement tests: one in Italian and the other in mathematics. The Italian achievement test evaluates pupils' reading skills and degree of linguistic and metalinguistic development. The mathematics achievement test assesses pupils' math skills in different domains (numeracy, space and figures, data and predictions) and mathematical dimensions (knowing, arguing, and problem-solving) (INVALSI, 2018*a*,*b*, 2019).

In addition to scores in grade 2, INVALSI collects data about marks given by teachers in Italian and in math at the end for the first term[5] and information about parental characteristics and family background.

Both the pre-Covid and Covid cohorts sat the INVALSI national standardized assessment in math and Italian at the end of grade 2 before the pandemic and about one year before the MATHGAP test. INVALSI provided math and Italian standardized test scores in grade 2, teachers' marks in math in the first term of grade 2, child migratory status (native children versus first- and second-generation migrant children),[6] and parental ed-

[5]Teacher's marks are the marks that teachers assign to pupils at the end of the first semester, based on their overall performance during the term; they can range between 4 and 10 (6 is the pass grade).

[6]First-generation migrants are children born abroad with both parents born abroad, second-generation migrants are children born in Italy with both parents born abroad, whereas native children are born in Italy

ucation (low- or high-educated parents, where parents are labelled as high-educated if at least one holds a tertiary degree). These data have been matched to the MATHGAP test score at the individual level.

The outcome measure, MATHGAP test

We measure pupils' math skills with the use of a math test, the MATHGAP test, which was designed by scholars of mathematics education to assess math skills acquired by children in grade 3, following the same conceptual framework as the INVALSI national assessment[7]. The test focuses on the domain of numeracy and contains 20 test items.

Like the INVALSI tests, MATHGAP assesses different topics and mathematical dimensions; it contains both open and multiple choice-type answers. Each correct answer received 1 point and incorrect or missing answers received 0 points. Total possible scores could therefore be between 0 and 20 points, which was then standardised to have a zero mean and a standard deviation equal to 1.

The MATHGAP test was designed as part of a project conducted during the school year 2018/19, aimed at evaluating the impact of teaching practices based on active and cooperative learning on the gender gap in mathematics (MATHGAP project, Di Tommaso et al. (2021)).[8] The impact was evaluated in a randomized controlled trial conducted in 25 primary schools (50 classes) in the province of Torino who volunteered to take part in the project. Randomization was done at the class level: one class per school in the treatment group and one class in the control group. As part of the project, the test was first assessed during a pilot phase, through item-response-theory models and qualitative interviews with pilot-teachers; it was then administered at the end of April 2019 as a post-treatment test to approximately $1,000$ children in grade 3.

Leveraging on the data collected within the MATHGAP project, the same test was then administered to the classes participating in the present study, to measure the math skills of children in grade 3 who experienced the Covid-19 pandemic during the school year 2019-20. With the support of the Regional Board of Education[9] of Piedmont, in May 2020 we invited the 25 schools to participate in an assessment involving all of the pupils enrolled in grade 3 during the Covid-19 pandemic outbreak. Due to the school closure until the end of the 2019-20 school year, the assessment was planned for the autumn 2020, at the beginning of grade 4, when the pupils finally returned to the classroom. The schools' enrollment in the new project was on a voluntary basis. During online presentations of the project in the summer of 2020, the application procedure was explained to

with at least one parent born in Italy (see Table A1).

[7]See online Appendix B for the MATHGAP test. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4114323

[8]Project "Tackling the gender gap in mathematics in Italy". Project website: https://sites.google.com/view/mathgendergap.

[9]The Regional Board of Education is the regional institution of the Ministry of Education, which manages and monitors the schools at a regional level.

school principals and teachers. Fifty-six classes from 14 schools applied. Although not all of the invited schools ended up enrolling in the study, those who did volunteered more classes than in the previous project. Therefore, the number of classes and children in the pre- and Covid cohorts were similar, as were their average characteristics (see Section 6).

For both cohorts, external tutors administered the MATHGAP test in person, to all children, including those with special educational needs and disabilities. Tutors stayed in the classroom while pupils completed the test and collected them. For the Covid cohort, the tutors returned to the school to administer the test to students who had been absent the first time.[10] The tutors then graded the tests under the supervision of an external examiner, an expert in formulating and grading Italian national standardized tests in math.

4.2 Sample selection and descriptive statistics

The initial sample was made up of 1,044 pupils in the pre-Covid cohort and 1,144 pupils in the Covid cohort, for a total of 2,188 pupils, with a similar proportion of children with special educational needs in the two groups (approximately 14%). As summarized in Table A.2 in Appendix A, we exclude from the sample: i) children with special educational needs who did not perform the MATHGAP test even if they were in class (less than 0.6% of the sample); ii) children without parental consent for the release of INVALSI data (0.3% of the sample); iii) children whose data were not released by INVALSI, probably because of privacy concerns or a lack of records (about 5.3% of the sample); iv) children who were absent from the MATHGAP test or from one of the INVALSI assessment tests in grade 2 (respectively, 5.2% and 5.6% of the sample), and iv) children with other missing relevant information (teachers' marks for math during the first term of grade 2 or migratory background - 4.8%). Finally, we exclude girls from the pre-Covid cohort who received treatment (active learning teaching intervention) within the MATHGAP project. The reason for this is that these girls benefited from the intervention, whereas boys did not (see Di Tommaso et al. 2021). As a robustness check, we also exclude the treated boys, although we prefer to keep them in the main specifications to avoid the number of observations to reduce too much (see Section 5.2). As we will see, the results are very similar for the different analytical samples.

The final analytical sample thus contains 1,539 children, about 62% of which are in the Covid cohort. Table 1 presents the descriptive statistics for the overall sample as well as separately for the pre-Covid and Covid cohorts. It should be noted that MATHGAP test scores in grade 3 are standardised at the sample level, while the Italian and math test scores (INVALSI) in grade 2 are standardised at the national level (i.e. the mean is 0 for the whole Italian sample). The values in Italian and math test scores (INVALSI) indicate

[10]For the pre-Covid cohort, children absent during the MATHGAP test sat it at a make-up session administered by the teacher rather than the tutor. For this reason, those tests are excluded from the analysis.

Table 1: Descriptive statistics, overall and by cohort

	Overall Mean	Pre-Covid cohort Mean	Covid cohort Mean	P-value of the diff.
Math score, grade 3 (MATHGAP)	0.02	0.13	-0.05	0.00
Math score, grade 2 (INVALSI)	0.25	0.33	0.20	0.02
Italian score, grade 2	0.23	0.13	0.29	0.00
Teacher's mark in math, grade 2	8.25	8.18	8.29	0.04
Covid cohort	0.62	–	–	–
Native	0.89	0.89	0.89	0.98
High-educated parents	0.33	0.36	0.31	0.04
Observations	1,539	591	948	

Notes: T-test on the equality of means for pre-Covid and Covid cohort for each variable. Covid cohort is the proportion of pupils belonging to the Covid cohort. Native is the proportion of natives vs. first- or second-generation migrants. High-educated parents is the proportion of children with at least one parent with a tertiary degree.
Source: INVALSI data and data collected by the research team.

that our sample is positively selected with respect to the Italian population.

The proportion of natives is similar across the two cohorts, whereas the proportion of females is statistically different due to the design explained above (similar proportion before the exclusion of treated girls). Instead, the two cohorts present differences in terms of test scores and teacher's marks both in grades 2 and 3 and in the proportion of children with high-educated parents. The Covid cohort scored higher on the INVALSI Italian test (grade 2) and received higher teacher's marks in math than the pre-Covid cohort. At the same time, the pre-Covid cohort achieved higher scores on the INVALSI math test than the Covid cohort and contains a higher proportion of children with high-educated parents. The two cohorts therefore differ to some extent. However, our econometric design controls for the level of initial competences and characteristics (see the identifying assumptions in 3).

5 The effect of school closure on math skills

In this section, we present our main findings of the impact of the Covid-19 pandemic on children's math achievement and the development of learning inequalities relative to gender, parental background, migrant status, and initial abilities. We then describe the results of a few robustness checks.

5.1 Main results

Table 2 shows the effects of the Covid-19 pandemic on children's math test scores in grade 3, assessed by the MATHGAP test, reporting the impact estimates when controlling only for different prior skill measures relative to grade 2 – INVALSI math and Italian test scores and teacher assigned math marks – (column 1), and when adding the socio-demographic control variables gender, parental education, and migratory background (column 2). All specifications include school fixed effects.

These results show that the pandemic negatively affected children's math skills: the estimated loss ranges between -0.23 and -0.19 standard deviations in test scores. The magnitude of the loss is large: we could express the estimates in terms of the existing estimates of the achievement gains in a typical year. For the US, Bloom et al. (2008) estimated a gain of about 0.89 s.d. between grades 2 and 3; thus, the average impact corresponds to about 3 months of school, nearly the time that the schools remained closed in Italy. An alternative way of quantifying the magnitude of the effect is to express the impact in terms of how many percentiles of the test scores distribution the students lose on average when they have experienced school closure. In this perspective, assuming normality of the test score distribution, the average impact of the pandemic on children's test scores (-0.19 s.d.) corresponds to a downward shift in the test score distribution of about 4-5 percentile points.[11]

Moreover, since learning is a cumulative process (Cunha et al., 2006), this short-term loss may have long-run consequences. Kaffenberger (2021) simulates that a reduction of about one third of the usual learning gains during grade 3 - assuming that no remedial efforts are made when children return to school - yields a loss equivalent to a full year of school by grade 10.

Table 3 reports the heterogeneous effect of the pandemic on children's achievements according to initial skills, gender, parental education, and migratory background. The interaction with prior math test scores shows that the effect of school closures had a larger impact on well-performing pupils. Figure 1 (a) helps to visualize this pattern: the point estimate of the effect of school closure for a child who scored -1 s.d. in grade 2 is -0.059 (not statistically significant), for a child who scored $+1$ s.d. is -0.273, and for a child who scored $+2$ s.d. is -0.380.[12] Instead, the learning loss due to the pandemic does not differ significantly on average across family background or between girls and boys.

[11] This is the idea: assume that the test score distribution in regular times is standard Normal. What happens to a student affected by school closure? If the z-value decreases on average by 0.19 units, the percentile of the distribution decreases by: more than 7 points at the center of the distribution ($z = 0.19$ corresponds to $P(Z < Z) = 0.5753, z = 0$ corresponds to $P(Z < z) = 0.5$), nearly 4 points around $z = 1(z = 1.19$ corresponds to $P(Z < z) = 0.8810$, $z = 1$ corresponds to $P(Z < Z) = 0.8413)$, less than 1 point around $z = 2(z = 2.19$ corresponds to $P(Z < Z) = 0.9854, z = 2$ corresponds to $P(Z < z) = 0.9772)$. Very roughly, the estimate of the weighted average of the probability differences is 4-5 points.

[12] These figures can be obtained from the estimates in Table 3 : $-0.059 = -0.166 - 1 * (-0.107); -0.273 = -0.166 + 1 * (-0.107)$; $-0.380 = -0.166 + 2 * (-0.107)$

Table 2: Main effects of the pandemic on children's math achievements in grade 3

Variables	Math score (1)	Math score (2)
Covid cohort	-0.232^{***}	-0.188^{***}
	(0.053)	(0.053)
Math score, grade 2	0.400^{***}	0.386^{***}
	(0.032)	(0.031)
Italian score, grade 2	0.107^{***}	0.108^{***}
	(0.024)	(0.024)
Teacher's mark in math, grade 2	0.369^{***}	0.366^{***}
	(0.032)	(0.031)
Female		-0.226^{***}
		0.031)
High-educated parents		0.077^{*}
		(0.041)
Native	1,539	-0.055
	0.562	(0.056)
Observations	1,539	1,539
R-squared	0.562	0.575
School fixed effects	Yes	Yes

Notes: High-educated parents: at least one parent has a tertiary degree.
Clustered standard errors at class level in parentheses. $^{***}p < 0.01, **p < 0.05, ^{*}p < 0.1$.

Table 3: Heterogeneous effects of the pandemic on children's math achievements in grade 3

Variables	Math scores (1)	Math scores (2)	Math scores (3)	Math scores (4)
Covid cohort	-0.166^{***}	-0.167^{***}	-0.171^{***}	-0.184^{*}
	(0.053)	(0.056)	(0.061)	(0.111)
Covid cohort * Math score in grade 2	-0.107^{***}			
	(0.039)			
Covid cohort * Female		-0.056		
		(0.067)		
Covid cohort * High-educated parents			-0.055	
			(0.074)	
Covid cohort * Native				-0.005
				(0.108)
Observations	1,539	1,539	1,539	1,539
R-squared	0.577	0.575	0.575	0.575
Initial abilities	Yes	Yes	Yes	Yes
Socio-demographic controls	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes

Notes: Initial abilities include math and Italian test scores in grade 2, teacher-assigned marks in math in the first term of grade 2. Socio-demographic controls include gender, native, and high-educated parents (at least one parent has a tertiary degree).
Clustered standard errors at class level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

To further examine the differences between children of different socioeconomic backgrounds, we split the sample into two groups of children with low-or high-educated parents and then rerun the models with interactions. The results can be seen in Table 4. In terms of point estimates (columns 1 and 2), school closure affected the children with low-educated parents more than those with high-educated parents (-0.198 versus -0.164), but the difference is not statistically significant. The interaction with prior performance (columns 3 and 4) is significant only for the children with low-educated parents, with an impact that reaches -0.51 s.d. for the children who scored $+2$ s.d. in grade 2 (also see Figures 1(b) and 1(c)).[13] We also observe relevant gender differences: among the children with high-educated parents, girls were less affected by school closure than boys, although the difference is not significant (column 6). Instead, among the children with low-educated parents, the learning loss experienced by girls (-0.29) was much larger than that experienced by boys (-0.13) (column 5). This result is particularly alarming if we consider that even in ordinary times girls usually do worse than boys in math and math-related subjects.

Table 4: Heterogeneous effects of the pandemic on children's math achievements, by parental education

	Low-edu parents	High-edu parents	Low-edu parents	High-edu parents	Low-edu parents	High-edu parents
	Math scores	Math scores	Math scores	Math scores	Math scores	Math scores
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Covid cohort	-0.189^{***}	-0.164^{***}	-0.181^{***}	-0.161^{*}	-0.133^{*}	-0.201^{*}
	(0.065)	(0.073)	(0.062)	(0.082)	(0.070)	(0.085)
Covid cohort * Math score G2			-0.166^{***}	-0.006		
			(0.048)	(0.080)		
Covid cohort * Female					-0.164^{*}	0.110
					(0.092)	(0.126)
Observations	1,038	501	1,038	501	1,038	501
R-squared	0.585	0.523	0.590	0.523	0.586	0.524
Initial abilities	Yes	Yes	Yes	Yes	Yes	Yes
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

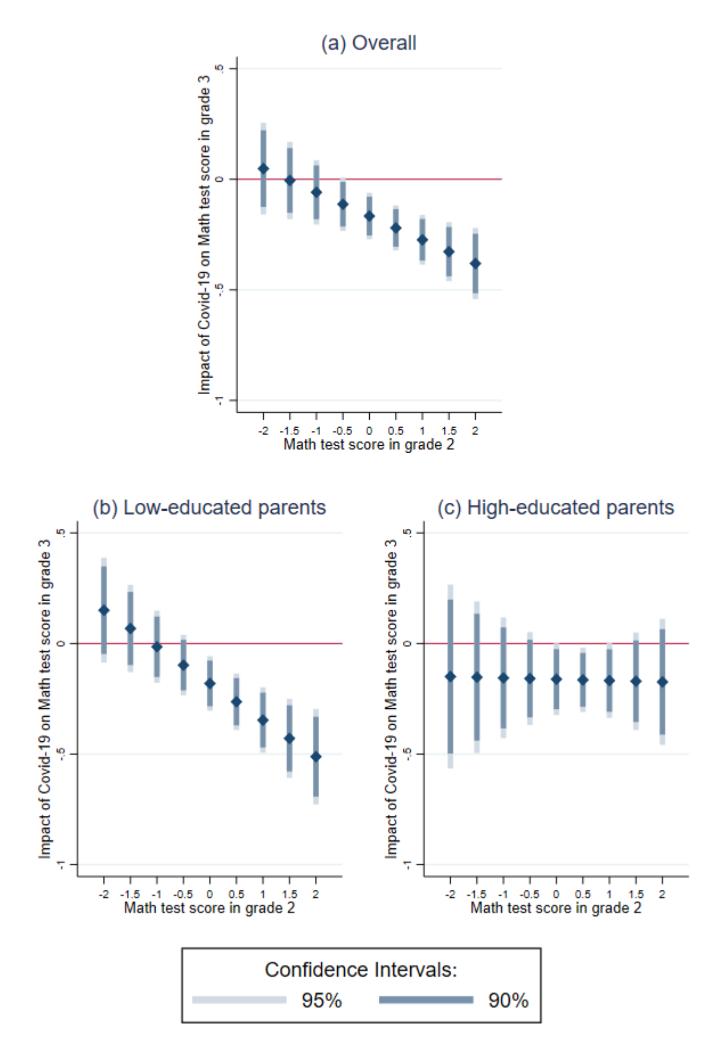
Notes: G2: grade 2. Low-educated parents: no parent has a tertiary degree. High-educated parents: at least one parent has a tertiary degree. Initial abilities include math and Italian test scores in grade 2, teacher-assigned marks in math in the first term of grade 2. Socio-demographic controls include gender and migratory background.
Clustered standard errors at class level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

The mechanisms underlying these results deserve further discussion.

Only a few contributions in the literature report results by gender. There is some evidence that the pandemic had a greater detrimental effect on boys than on girls, but those

[13] We tested the assumption of linearity but found no evidence of non-linearity.

Figure 1: Effects of Covid-19 on math skills by initial math skills, overall and by parental education



studies do not differentiate by SES (Champeaux et al., 2020, Haelermans et al., 2021). In contrast, when focusing on socio-emotional skills, Mendolia et al. (2022) point to stronger effects among girls, particularly from lower-income families. These contrasting results may be generated through different channels. Boys from disadvantaged families have more behavioral and academic problems than girls from similar families (Figlio et al., 2019). One may thus expect boys to be more negatively affected by school closure than girls. However, if parents are aware of this difference and try to compensate for it, the results could reverse. Indeed, Del Bono et al. (2021) found that, for the UK, boys spent less time on schoolwork but, at the same time, received more parental help.

Heterogeneous effects in terms of prior achievement have not been previously investigated, although they are acknowledged to be very important. How can we explain the finding that high-performing students from low-SES backgrounds suffer the strongest negative effects? High-performing children from low-SES backgrounds are presumably those who benefit most from attending school. We speculate that, even in the absence of differences in terms of parental time investment by socioeconomic background (Del Bono et al., 2021), other differences may emerge, such as the parents' ability to support their children effectively. The literature is relatively silent on this point. There exists abundant evidence on the different educational outcomes of children from different backgrounds, but less so on the relative importance of school for high and low-achieving children with similar backgrounds. One notable exception is the paper by Crawford et al. (2017). They show that, on average, initially high-achieving children from poor families quickly lose ground compared with their wealthier peers; however, the effect is largely diminished when focusing on children attending the same school. This suggests that the school system may help mitigate the impact of family background on child outcomes (OECD 2018), supporting the view of school as the "great equalizer" (Horace Mann 1848). Hence, the pandemic and consequent school closures had a greater detrimental impact on the children who could have gained most from the traditional classroom, and increased educational inequalities.

5.2 Robustness checks

To confirm the validity of our results, we perform two robustness checks. First, we replicate the analysis excluding the boys exposed to treatment in the MATHGAP project. In the previous section, the analytical sample for the pre-Covid cohort was made up of the children participating in the MATHGAP project, excluding the girls in the treatment group, because the evidence is that they had benefited from the intervention, whereas boys had not (Di Tommaso et al., 2021).[14] We now replicate the analyses by also excluding the boys in the treatment group; the reason for this is that although the average treatment effect for boys was null, we cannot exclude that none of the boys were affected by exposure to the active learning intervention. Our previous findings are largely confirmed, both in terms of the direction and magnitude of the estimates (Table 5, column 1).

Second, we estimate a model without school fixed effects, but including class-level variables (share of females, of natives, of children with high-educated parents, and average test scores) in grade 2 to control for the different contexts. Once again, the previous findings are confirmed (Table 5, column 2). We also estimate a model with interactions between the Covid cohort dummy and context variables at the school level. The interactions are not significant, suggesting that, once controlling for individual characteristics,

[14]In addition to being non-statistically significant, the point estimate of the effect of the MATHGAP intervention was practically 0 for boys, and the result was robust to different specifications. Interestingly, there is also preliminary evidence that this is also the case in the medium run.

Table 5: Robustness checks

Variables	Math scores (1)	Math scores (2)
Covid cohort	-0.200^{***}	-0.154^{**}
	(0.057)	(0.064)
Observations	1,346	1,539
R-squared	0.573	0.532
Initial abilities	Yes	Yes
Socio-demographic controls	Yes	Yes
School fixed effects	Yes	No
Context variables (class level)	No	Yes

Notes: In column 1, we exclude boys treated during the MATHGAP project; in column 2, we substitute school fixed effects with contextual variables computed at the class level. Initial abilities include math and Italian test scores in grade 2, teacher-assigned marks in math in grade 2. Socio-demographic controls include gender, native, and high-educated parents (at least one parent has a tertiary degree). Contextual variables at the class level include the proportion of females, natives, and children with high-educated parents, and the average math and Italian test scores in grade 2.
Clustered standard errors at class level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

the effect of the pandemic on math scores does not vary with school characteristics.[15]

6 Possible limitations

The present analysis has two main limitations, which are presented and discussed hereafter.

6.1 Timing of the math test in grade 3

Pupils of the pre-Covid cohort sat the MATHGAP test at the end of grade 3 (the end of April 2019), whereas the pupils of the Covid cohort sat the test at the beginning of grade 4 (October 2020). This misalignment could have two opposite effects. On the one hand, the children in the Covid cohort are a bit older and more mature, and had attended at least one and a half more months of school (May in grade 3 and September in grade 4). Thus, the estimated effect of the pandemic might be downward biased. On the other hand, the children in the Covid cohort had also gone through the summer break, potentially responsible for learning losses. In this perspective, the estimated effect would be upward biased, because the observed change would not be entirely attributable to the pandemic,

[15]Results available from the authors upon request.

but it is also due to the summer break. The two effects may cancel out, but the net effect of the two opposite forces is not known a priori. The rough existing estimates of the summer learning loss point to a reduction of about -0.10 standard deviations (McCombs et al. (2011) - estimates for the US): if we trusted these estimates and disregarded the potential opposite bias, since our ATE estimate is about 0.2 standard deviations, we would conclude that there is still evidence of a sizable negative effect of the pandemic.

6.2 Self-selection of schools and external validity

Our sample of schools might be affected by self-selection, given that only 14 out of the 25 schools invited to participate eventually took part in the post-Covid assessment. To assess the degree of self-selection, we first compare the 14 schools that agreed to participate in the project with the 11 schools that did not. To do so, we rely on INVALSI data available for both groups of schools, including the composition of classes, pupils' characteristics, math and Italian skills in grade 2. We present the mean at the school level using class averages of INVALSI data for classes in grade 2 in the school year 2017-18. We find few statistical differences between the two groups of schools: in the 14 schools who participated in the new project, the children were more likely to have attended kindergarten, but their parents were less educated; no differences emerge in terms of their math or Italian abilities (Table A.3 in Appendix A). However, we cannot rule out that the two groups of schools differ in how they coped with distance learning. Since we lack information in this regard, we cannot test for any such difference. Nonetheless, if any self-selection occurred, we would expect it to be positive, i.e. that the schools coping better with distance learning were more likely to participate. If so, our estimates would represent a lower bound of the true causal effect of school closure. To assess external validity, we compare the average characteristics at the class level of the 14 schools with the same characteristics at the regional and national level (pre-Covid cohort).[16] There is evidence that the children in our schools are more skilled on average and have a higher proportion of high-educated parents than the children in Piedmont and Italy as a whole (Table 6). Moreover, since these schools are located in the province of a large city rather than in rural or remote areas, we can expect the children and teachers to have better technological tools and broadband access at home. This means that our findings probably underestimate the effects of the pandemic on pupils' achievements at the national level.

As just mentioned, the available data contain no information about how schools responded to the pandemic. For this reason, we administered a short questionnaire on distance learning to the teachers of the Covid cohort (Table 7). The response rate was

[16]For the regional and national schools, we use INVALSI data of classes belonging to the so-called representative sample, to whom the national test is administered under external supervision, reducing the risk of cheating.

Table 6: Comparison of participating 14 schools with regional and national data

Variable	Classes in our schools' sample	Piedmont classes	P-value of the difference our sample vs. Piedmont classes	Italian classes	P-value of the difference our sample vs. Italian classes
Average number of pupils per class	20	19	0.04	18	0.00
Female	0.50	0.51	0.70	0.49	0.50
Pre-kindergarten (age 0-3)	0.37	0.32	0.16	0.38	0.82
Kindergarten (age 3+)	0.99	0.89	.0..	0.94	0.03
Migrant 1st generation	0.01	0.02	0.17	0.03	0.10
Migrant 2nd generation	0.10	0.15	0.04	0.13	0.10
INVALSI Italian score std., grade 2	0.51	0.09	0.00	0.02	0.00
INVALSI Math score std., grade 2	0.52	0.03	0.00	0.02	0.00
Mother's level of education					
Primary school	0.00	0.02	0.03	0.02	0.01
Lower secondary school[a]	0.27	0.33	0.04	0.31	0.10
Upper secondary school[b]	0.45	0.44	0.62	0.43	0.38
Tertiary degree	0.27	0.22	0.04	0.24	0.09
Father's level of education					
Primary school	0.01	0.01	0.03	0.03	0.01
Lower secondary school[a]	0.40	0.45	0.05	0.40	0.86
Upper secondary school[b]	0.41	0.38	0.15	0.41	0.80
Tertiary degree	0.19	0.16	0.16	0.16	0.10
Number of classes	81	75		1,482	

Notes: Std. = Standardised (mean 0, st.dev. 1). [a] Includes also vocational qualification. [b] Includes also Post-diploma qualification.

quite high, 71.43% (40 out of 56 teachers), although not all of them answered all the questions. Overall, 85% of the teachers reported that they provided some type of distance learning activities during the lockdown of March-June 2020. Seventy-nine percent of the teachers stated that the distance learning activities consisted mainly of streaming live lessons. By means of comparison, at the national level Champeaux et al. (2020) report that, in primary school, online classes were offered to 65% of pupils and Scarpellini et al. (2021) report a percentage of 81.6 (non-representative online surveys). These figures suggest that the share of children exposed to some distance learning - as opposed to no school at all - was not smaller in our sample than at the national level. Thus, we should not worry about our results being overestimates of the true effect of school closure because of lower exposure to some form of instruction.

7 Discussions and conclusions

The Covid-19 pandemic has caused long periods of school closure, often coupled with severe lockdowns, causing an unprecedented disruption to children's lives and their learning process. Italy was the first Western country hit by the pandemic, and the one with the longest period of school closure in spring 2020. In this paper, we present the first esti-

Table 7: Descriptive statistics of the teachers' survey

Variable	Obs.	Mean	Std. Dev.	Min	Max
Female (teacher)	40	0.98	0.16	0.00	1.00
Teacher's age	40	49.33	9.75	25	63
Full time (class)	40	0.65	0.48	0.00	1.00
Distance learning					
Distance learning[a]	39	0.85	0.37	0.00	1.00
Simultaneous distance learning[b]	33	0.79	0.42	0.00	1.00
Hours of distance learning per week	33	8.38	4.89	1.50	20.00
Teacher opinion about distance learning[c]	35	3.20	0.80	1.00	5.00

Notes: Data from the questionnaire completed by math teachers of the Covid cohort classes in the sample. Response rate 71.43%. [a] Distance learning: 1 if some distance learning was provided, 0 otherwise. [b] Simultaneous distance learning: 1 if simultaneous distance learning was provided, 0 otherwise. [c] $1-5$ scale.

mates of the effects of the pandemic on the learning losses and educational inequalities among Italian pupils enrolled in primary school.

This research estimates the effect of the pandemic on the math performance of children in grade 3 with a difference-in-difference strategy. We use a unique dataset, constructed by matching scores in a standardised math test administered at the end of grade 3 with the scores from the national standardized assessment in grade 2. The data have been collected for a sample of about 2,000 children, enrolled in primary school in the province of Torino, a large metropolitan area located in the north of Italy.

Children faced large learning losses during the spring 2020, i.e. the first Italian lockdown, with an average impact of -0.19 standard deviations. The magnitude of the loss is large, and corresponds to about 3 months of school, nearly the time that the schools remained closed in Italy. Our results end up being similar to the findings from previous research on different countries: learning loss due to school closure is about 0.01 s.d. for each week of school closure (in Italy -0.19 s.d. for 15 weeks of closure, in other countries 0.07-0.10 s.d. for about 8-10 weeks).

On average, we find no evidence of stronger effects among children from disadvantaged backgrounds. Instead, our results reveal that the school closure had a larger negative impact on well-performing pupils with low-educated parents. Moreover, among children with low-educated parents, the learning loss experienced by girls was double that of boys. We might speculate that high-performing children (and perhaps girls) from disadvantaged backgrounds are probably those who have previously benefited most from school attendance and who are more likely to benefit more in the future. If these children are the ones who were hurt the most by the school closure, the consequence is likely to be that learning inequalities increase, at least in the upper part of the performance distribution.

Indeed, the 2020 school closure had a large negative impact for many children. If we add to this the possible effects of distance learning many children also experienced during the following school year, and the cumulative effects these initial losses might develop over time, we can expect dramatic long-term consequences on an entire generation of young people.

These findings call for urgent policy actions to be taken. On the one hand, strategies need to be put in place in order to limit other school closures in the unfortunate event of a resurgence of the pandemic. On the other, remedial measures should be introduced to limit the damage already occurred, to help the pupils who might otherwise be left behind, at the same time fostering learning among the (more or less) talented children who would have been able to reach learning targets in a traditional classroom learning environment, but were disadvantaged by distance learning.

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Appendices

A Additional materials

Table A.1: Variable definition

Variable	Definition
Individual level	
Math test score, grade 3	Standardized math test score in MATHGAP test, grade 3
Math test score, grade 2	Score in Math INVALSI test, grade 2 (standardised at national level)
Italian test score, grade 2	Score in Italian INVALSI test, grade 2 (standardised at national level)
Teachers' mark, grade 2	Teachers's mark in math, first term grade 2 (mark that teachers assign to pupils at the end of the first semester, based on their overall performance during the term; it can range between 4 and 10, and 6 is the pass grade)
Covid cohort	1 if Covid cohort, 0 if pre-Covid cohort
Female	1 if female, 0 if male
Native	1 if the child is born in Italy with at least one parent born in Italy, 0 otherwise

Table A.1: Variable definition

Low-educated parents	1 if no parent has a tertiary degree, 0 otherwise
High-educated parents	1 if at least one parent has a tertiary degree, 0 otherwise
Class level	
Average number of pupils per class	Average number of pupils per class
Female	Percentage of females in class
Pre-kindergarten (age 0-3)	Percentage of pupils who attended pre-kindergarten (age 0-3)
Kindergarten (age 3+)	Percentage of pupils who attended kindergarten (age 3+)
Migrant 1st generation	Percentage of children born abroad with both parents born abroad
Migrant 2nd generation	Percentage of children born in Italy with both parents born abroad
INVALSI Italian score std., grade 2	Mean of INVALSI Italian score standardised (at national level), grade 2
INVALSI Math score std., grade 2	Mean of INVALSI Math score standardised (at national level), grade 2
Mother/father's level of education	
Primary school	Percentage of mothers/fathers with a primary school degree
Lower secondary school	Percentage of mothers/fathers with a lower secondary school degree or vocational qualification

Table A.1: Variable definition

Upper secondary school	Percentage of mothers/fathers with an upper secondary school degree (diploma or post-diploma qualification)
Tertiary degree	Percentage of mothers/fathers with a tertiary degree
Teacher questionnaire	
Distance learning	1 if some distance learning was provided, 0 otherwise
Simultaneous distance learning	1 if simultaneous distance learning was provided, 0 otherwise
Hours of distance learning per week	Number of hours of distance learning the teacher provided
Teacher's opinion on distance learning	Opinion of the teacher on distance learning (1=negative; 5= positive)
Female (teacher)	1 if the teacher is female, 0 otherwise
Teacher's age	Age of the teacher
Full time (class)	1 if the class has a full-time schedule (40 hours per week), 0 otherwise (27/30 hours per week)

Table A.2: Sample selection

Sample	Overall	Pre-Covid cohort	Covid cohort
Initial sample	2,188	1,044	1,144
Pupils with special educational needs with no math test in grade 3	12	4	8
Lacking parental consent for INVALSI data[1]	7	2	5
Not released by INVALSI[2]	115	50	65
Absent from the MATHGAP math test, grade 3	106	50	56
Absent from one of the INVALSI assessment tests, grade 2	110	67	43
Missing other relevant information[3]	89	70	19
Females in treated classes of MATGHAP project	210	0	0
Final sample	1,539	591	948

Notes:[1] Children without parental consent for the release of INVALSI data or lacking an INVALSI identification number. [2] Data were not released by INVALSI, probably because of privacy concerns (matching) or due to missing records. [3] Children without complete information about teacher-assigned marks in math in tthe first term of grade 2 and/or migratory background.

Table A.3: Comparison between the 14 schools of the Covid cohort and the 11 other schools from the original pre-Covid sample

	14 schools Applied	11 schools Not applied	P-value of the diff.
Females	0.50	0.50	0.97
Pre-kindergarten (age 0-3)	0.37	0.43	0.18
Kindergarten (age 3+)	0.99	0.90	0.02
Migrant, first generation	0.01	0.01	0.96
Migrant, second generation 0.10	0.07	0.08	
INVALSI Italian score standardized, grade 2[a]	0.51	0.40	0.25
INVALSI Math score standardized, grade 2[a]	0.52	0.47	0.68
Mother's level of education Primary school	0.00	0.01	0.89
Lower secondary school[b]	0.27	0.21	0.02
Upper secondary school[c]	0.45	0.42	0.31
Tertiary degree	0.27	0.36	0.01
Father's level of education Primary school	0.01	0.01	0.06
Lower secondary school b	0.40	0.33	0.03
Upper secondary school c	0.41	0.39	0.42
Tertiary degree	0.19	0.27	0.01
Number of classes	81	55	

Notes: a Standardized scores, with 0 mean and 1 standard deviation. b Includes also vocational qualification. c Also includes post-diploma qualification.

Conclusion

Restrictive measures on abortion access may have sizable second-order effects that go beyond the scope of these policies. Among them, I document the increase in the likelihood of women to be victims of violence and to experience illegal abortions. A 25-mile increase in distance to reach the nearest abortion clinic is estimated to increase the number of offenses reported to the police where the victim is female of reproductive age and the offender is male by up to 2.6%. For black women, this effect increases up to 6.6%. In Italian hospitals, the high percentage of gynecologists who deny performing abortion on the basis of conscientious objection leads to a rise in the phenomenon of illegal abortion. A 10% increase in the share of objecting gynecologists is associated with a 4 to 5% increase in the individual probability of having an illegal abortion. The effect seems to be driven by immigrant women.

To the extent of my knowledge, no similar studies exist in economics so far. The only work looking at the relationship between abortion and violence is the already cited survey analysis by Roberts et al. (2014), while none of the very few studies on conscientious objection to abortion (Autorino et al., 2020, Bo et al., 2015, Meier et al., 1996) looks at the relationship between objection and illegal abortions.

The lack of literature in these domains opens the door to new promising areas of research; at the same time, my results underline how little we know at this point about the consequences of restrictions on abortion access on women's health and well-being.

Moving the focus to education, the first Italian lockdown of Spring 2020 due to the Covid-19 pandemic decreased pupils' performance in mathematics by 0.19 standard deviations. Among children of low-educated parents, the learning loss was larger for the best-performing ones and for girls.

The Covid-19 pandemic is still ongoing and the most recent investigations (Betthäuser et al., 2022) report a substantial overall learning deficit, which arose early in the pandemic and persists over time. Substantial heterogeneity has been found, with forgone learning particularly large among children from low socioeconomic backgrounds. Even if we don't know how long we will live along with the virus, the literature agrees upon the dramatic negative effects that distance learning has on students' achievements and educational inequalities. Future policies aimed at addressing the spread of the virus need to prioritize in-person learning over distance learning and, when closing schools is the only possible strategy, implement remedial interventions to help the pupils who might otherwise be left behind.

The sizable long-term consequences of restrictions on access to health care and educational services – in terms of employment and job performance disruption, poorer academic achievements and employment paths, worse physical and mental health, and increasing inequalities across the socioeconomic ladder[17] – underlines how central is for economic and social development to guarantee the access to the health and educational system to everyone, with particular attention to most disadvantage social categories that may otherwise be left further behind. Expanding the provision of free education and health care may boost economic growth, by increasing overall productivity and decreasing socioeconomic inequalities.

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