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Abortion laws and women's health☆

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ABSTRACT

We examine the impact of progressive and regressive abortion legislation on women's health in Mexico. Following a 2007 reform in the Federal District of Mexico which decriminalised and subsidised early-term elective abortion, multiple other Mexican states increased sanctions on illegal abortion. We observe that the original legalisation resulted in a sharp decline in maternal morbidity, particularly morbidity due to haemorrhage early in pregnancy. We observe small or null impacts on women's health from increasing sanctions on illegal abortion. These results quantify the considerable improvements in non-mortal health outcomes flowing from legal access to abortion.

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1. Introduction

Appeals to women's health are frequently made when debating the merits of abortion legislation. These calls are made by both advocates of legal abortion as well as those advocating for abortion to become, or remain, illegal. The arguments backing up such claims are drawn from a range of sources, which are often correlational or based on small or non-representative samples of women.[1]

In this study, we present the first population-level evidence of the impact of sub-national variation in abortion laws on maternal *morbidity*, as well as mortality, using the universe of administrative health records from Mexico.[2] We focus on a period in which considerable within-country reform of abortion policy was undertaken, with both a sweeping legalisation in the Federal District of Mexico (Mexico DF) and increasing sanctions on (already illegal) abortion in other regions. In this context, we are able to determine to what extent change in abortion laws, absent other major contraceptive revolutions, impact health indicators for the population of affected women. We combine the state-level variation over time resulting from legislative changes in abortion law with high-quality vital statistics data recording over 30 million births, 18.4 thousand maternal deaths and 46 million inpatient cases for causes related to maternal health. To seek to understand the observed impact of abortion laws on health outcomes, we also take advantage of rich administrative and survey data to identify the reform's wider impact on birth rates, judicial sentencing, and on sexual behaviour, and explore potential channels of health impacts, including improvements in the quality of abortion care and changes in the composition of women giving birth.

The environment under study provides a unique opportunity to examine simultaneous expansions and contractions of abortion policies.[3] While much of the existing literature on the impact of abortion—and contraceptive policies more generally—focuses on expansion in access, there are a number of papers which focus on contractions in policies. These include historical restrictions in Romania (Pop-Eleches, 2010), the impact of parental consent or notification laws targeted at adolescents in the US (Bitler and Zavodny, 2001; Joyce and Kaestner, 1996), and a recent hollowing out in the availability of providers due to state-specific legislation in the US (Fischer et al., 2018; Lindo et al., 2019). However, the legalisation of abortion in Mexico DF, and the resulting spate of constitutional changes increasing the harshness of sentencing of illegal abortion, provides the opportunity to examine the impact of a contemporaneous series of restrictive and permissive abortion policies in a single country and time.

Reproductive health rights have been documented to be of considerable economic significance. This fact has been emphasised by a growing body of economic literature which has—empirically and theoretically—demonstrated how access to elective abortion and contraceptive methods has shaped fertility patterns, marital markets, crime, education, the labour market and female empowerment (e.g., Ananat et al., 2007; Bailey and Lindo, 2017; Chiappori and Oreffice, 2008; Guldi, 2008; Mitrut and Wolff, 2011; Myers, 2017; Pop-Eleches, 2010). The impact of abortion laws on women's health has received less attention, and the causal relationship is yet to be established. To the best of our knowledge, this is the first study to provide well identified population-level evidence of the impact of abortion legalisation on maternal morbidity and mortality based on within-country variation in abortion availability. While an association between abortion legalisation and lower abortion-related complications has been documented in previous studies, comprehensively capturing the impact of the passage of abortion law on abortion-related morbidity is a considerable challenge, especially in clandestine settings, where under-reporting may occur (Singh et al., 2010).[4]

Using two-way fixed effect (FE) models and panel event study methods (and synthetic controls as a robustness check), as well as recent advances in a literature examining causal estimation with time varying policy reform (Rambachan and Roth, 2019; Goodman-Bacon, 2018), we observe consistent evidence to suggest that the legalisation of abortion in Mexico DF brought about a sharp reduction in fertility (by 8%), haemorrhage in early pregnancy (by 35%) and abortion-related morbidity (by 20%). Event study estimates examining increases in sanctions on (already illegal) abortion point to much smaller effects on these variables, which are generally not statistically significant. In general, we observe impacts on maternal *mortality*, which are hard to consistently sign given that they are considerably less precise, suggesting that when only examining impacts of

[1] The use of such arguments even when based on weak evidence is not isolated to non-governmental organisations. Similar arguments are also made by politicians. One example is a fact sheet published on the website of the United States (US) Government's National Cancer Institute by the Bush administration positing an (unfounded) link between abortion and breast cancer (Special Investigations Division, Committee of Government Reform, House of Representatives, 2003).

[2] Associations between abortion legalisation and maternal mortality or morbidity have been documented in the medical and public health literature for multiple countries (Benson et al., 2011), including Albania (Sahatci, 1993), Bangladesh (Chowdhury et al., 2007), Nepal (Henderson et al., 2013), Romania (Serbanescu et al., 1995; Stephenson et al., 1992), Singapore (Singh and Ratnam, 2015) and South Africa (Rees et al., 1997). Existing studies are mainly based on reviews of medical charts at selected hospitals, for example in the US (Goldstein and Stewart, 1972; Stewart and Goldstein, 1971; Seward et al., 1973; Kahan et al., 1975), Guyana (Nunes and Delph, 1997), Nepal (Henderson et al., 2013) and South Africa (Mbele et al., 2006; Jewkes et al., 2002). We are aware of no prior studies which are based on population-level data, and based on within-country variation in abortion reforms.

[3] As we discuss at greater length in Sections 2.2–2.3 of this paper, the change due to the initial legalisation of abortion in Mexico DF was considerably larger than subsequent legislative tightenings in other Mexican states. In the case of the constitutional changes issued by states, in each case abortion was already illegal and any changes owe to an increased threat of prosecution or sanction. Using the universe of legal decisions in the country, we document evidence suggesting that these reforms increase the average length of sentences handed down to women.

[4] Maternal mortality is considered the "tip of the iceberg", where the mass consists of maternal morbidity (Loudon, 1992). In many settings, analyses of the impact of abortion on population health focuses only on maternal mortality due to a lack of universal health records measuring maternal morbidity.

abortion law on maternal mortality, analyses fail to account for the full weight of abortion reform on women's health.

Previous studies on Mexico's abortion reform laws include legal and ethical overviews (Johnson, 2013; Madrazo, 2009), qualitative studies on abortion provision (Contreras et al., 2011; Schiavon et al., 2010), quantitative studies on abortion services and patient characteristics using data from selected hospitals or surveys (Mondragón y Kalb et al., 2011; Becker et al., 2013), and fertility trends using vital statistics (Gutierrez-Vazquez and Parrado, 2015).[5] This paper contributes to previous studies by examining the causal impacts of abortion legalisation, as well as regressive law changes, on women's health and well-being using the full power of vital statistics data, including administrative microdata on births, maternal morbidity, maternal mortality, and judicial statistics on penal matters. In addition, we examine heterogeneous effects of the reform as well as potential mechanisms such as usage and knowledge of contraceptive methods.

There is a large unmet need for family planning in low- and middle-income countries (Sully et al., 2019). Yet, the evidence on the impacts of reproductive health rights including safe and legal abortions in these settings, especially building on microdata, is very scarce. In light of this, our study provides strong evidence that abortion legalisation in an emerging economy leads to rapid and discernible changes in political behaviour, aggregate fertility rates, and (significant improvements in) maternal health. These results provide a number of important policy lessons for jurisdictions considering changes in abortion laws.

2. Background

2.1. Unsafe abortions and maternal mortality in Mexico

Globally, maternal mortality has declined from 385 maternal deaths per 100,000 live births in 1990 to 211 deaths per 100,000 live births in 2017. The overwhelming majority of these deaths occurred in low-income countries. During the same period in Mexico, there has been a similar decline in maternal mortality from 88 to 33 deaths per 100,000 live births (Rodríguez-Aguilar, 2018; Bongaarts, 2016). However, Mexico has still not achieved its 2015 United Nations Millennium Development Goal: a ratio of below 22 deaths per 100,000 live births. In Mexico, the highest prevalence of maternal mortality is found in metropolitan areas and among women aged 20–34. Rates of maternal mortality are especially high in socially vulnerable populations across Mexico. Areas with the highest rates of maternal mortality exhibit some of the lowest levels of government expenditure on maternal health (Pérez-Pérez et al., 2019).

Abortion-related mortality represents a substantial proportion of all maternal deaths in Mexico, accounting for 7.2% of all maternal deaths between 2000–2008 (Schiavon et al., 2012). While there has been a downward trend in maternal mortality in Mexico between 1990–2008, maternal mortality attributed to abortion-related causes has not exhibited similar declines during this period. The majority of these deaths occur in women without health insurance (Schiavon et al., 2012).

The rate of abortion-related mortality varies substantially across regions in Mexico. The highest rates can be found in some of the poorest states, such as Chiapas and Guerrero with as many as 90 and 140 deaths per 100,000 hospitalisations, respectively. This can be compared to Baja California Sur with only 9 deaths per 100,000 hospitalisations. Yet, high rates of abortion-related mortality are not exclusively observed in poor states across Mexico. In Mexico DF, for example, the abortion-related mortality rate was 38 per 100,000 hospitalisations between 2000–2008 (Schiavon et al., 2012).

Unsafe abortion procedures account for most abortion-related mortality (Schiavon et al., 2012). Rates of unsafe abortion are particularly high in the Latin America and Caribbean region with an estimated 4.2 million unsafe induced abortions performed each year (World Health Organization, 2011). This region also exhibits some of the world's most conservative laws on abortion (United Nations, 2014). However, restrictive laws on abortion do not translate to lower rates of induced abortion but are instead associated with higher rates of unsafe abortion and correspondingly higher rates of abortion-related morbidity and mortality, compared to settings with more liberal access policies (Guttmacher Institute, 2012).

Indeed, the rate of induced abortion in Mexico is considered high internationally (Becker, 2013). Despite high rates of contraceptive use, the estimated rate of induced abortions increased between 1990, with 25 abortions per 1000 fertile-aged women, and 2006, with 33 abortions per 1000 women, (Juarez et al., 2008). Many of these induced abortions were performed in clandestine conditions and often in unsafe settings (Guttmacher Institute, 2012). Medical records from public hospitals in Mexico show high rates of abortion-related complications with an estimated 150,000 women treated for complications in 2006 alone (Juarez et al., 2008).

2.2. The 2007 legal interruption of pregnancy reform in Mexico DF

Since the 1970s, women's rights advocates in Mexico have been promoting women's health rights, including access to safe abortions (Kulczycki, 2011). To address the issue of unsafe abortions as a preventable cause of maternal morbidity and its huge burden on public health system, a National Pro-choice Alliance in Mexico was established in 2000 to promote women's sexual and reproductive health rights. This movement was supported by a wide range of groups, including healthcare professionals, women's rights groups and activists, politicians, academics, and Catholics

[5] In examining the abortion reform and fertility outcomes, Gutierrez-Vazquez and Parrado (2015) use national vital statistics to examine the effect on fertility across ages. Due to the use of a limited amount of data and limitations inherent in the empirical design, one cannot assign a causal interpretation to the results with confidence. More specifically, only three different years of data are used (1990, 2000 and 2010). In a study by Koch et al. (2015) maternal mortality is found to increase in areas with more liberal abortion laws. Koch et al. (2015), however, has received strong criticism for highly misleading and inaccurate data selection (Darney et al., 2017).

with pro-choice views (Johnson, 2013; Madrazo, 2009; Kulczycki, 2011; Blanco-Mancilla, 2011).[6]

With the support of the leftist Party of the Democratic Revolution, Mexico DF's legislative assembly voted to legalise elective abortion (termed legal interruption of pregnancy or ILE for its acronym in Spanish) on April 24, 2007 (Kulczycki, 2011). The ILE reform was signed into law the following day and published in the Official Gazette of the Federal District on April 26, 2007 (Ciudad de México, 2007). This immediately permitted women above the age of 18 to request an abortion at up to 12 weeks of gestation without restriction and free of charge. Access for minors requires the consent of a parent or guardian. Under this law, induced abortion was made legal in both the public and private healthcare sectors (requiring payment in private clinics).

Immediate implementation was made possible by collaboration between the Ministry of Health of Mexico DF (MOH-DF), members of the health department and international NGOs, which had designed a comprehensive program for public provision of abortion services called the "The ILE Program" and its implementation even before the law was passed (Singh et al., 2012). As such, abortion services were made available via public clinics immediately after the law was passed in April 2007, although with lower capacity and efficiency compared to current conditions. Abortion services were also quickly available in the private healthcare sector (Blanco-Mancilla, 2011). Additionally, under this law sexual education in schools was improved and post-abortion contraceptives were made freely available directly from the health clinics that provided abortions (Contreras et al., 2011). On August 29, 2008 the decision to pass the ILE law was ratified by the Supreme Court of Mexico, making Mexico DF, together with Cuba and Uruguay, the most liberal jurisdiction in the Latin American and Caribbean region in terms of abortion legislation (Fraser, 2015).

Any abortions conducted in publicly-run clinics are provided free of charge to residents of Mexico DF. Women with residency outside Mexico DF can also access the public provision of abortion through the MOH-DF but are charged based on a sliding fee scale depending on the woman's socioeconomic background. In 2010, 74% of all women who received an abortion through the public healthcare sector were women living in Mexico DF, 24% were living in the state of Mexico (which shares a border with Mexico DF) and 2% were living in other states (Mondragón y Kalb et al., 2011). The age profile of women seeking abortions largely mirrors the age-profile of births, occurring at slightly greater rates among younger (under 25 years) and older (above 36 years) groups (Appendix Figure A1).

Records from public hospitals show that in 2007, when the reform was implemented, more than 7000 abortion procedures were performed at 14 selected MOH-DF clinics. Over the years, the MOH-DF abortion program expanded its services and became more efficient in meeting the high demand for elective abortion. The MOH-DF program offers both surgical and medical abortion procedures and is the main provider of medical abortion in Mexico DF (Winikoff and Sheldon, 2012). The large shift from 25% of all abortion procedures being medical in 2007 to as much as 74% in 2011 has played a key part in meeting the demand and reducing complications and side-effects (Becker, 2013).[7] As of 2015, approximately 150,000 abortions were carried out at MOH-DF clinics.[8] Information regarding the private provision of abortion services is limited due to a lack of supervision of the private market for legal abortion services (Becker, 2013). Despite the fact that safe abortion, at no or low cost, is provided by the public health system in Mexico DF, women do seek abortion services within the private sector. A descriptive study by Schiavon et al. (2012) suggests that private abortion services are provided at a high cost (USD 157–505) and quality of care is inferior to that in the public sector, given that the less safe and efficient "dilation and curettage" is used as the main method in the private sector (in 71% of cases).

2.3. Regressive law changes following the ILE Reform

Following Mexico DF's ILE reform, a number of states almost immediately began a series of counter legislation to change the respective sections of their constitutions or penal codes, defining the beginning of human life as occurring at conception. These legal responses often directly referenced Mexico DF's ILE reform.[9] And even in cases where the ILE reform was not directly referred to in counter legislation it seems highly likely that it was a defining factor. For example, in the 20 years prior to the ILE reform there had been only two constitutionally defined changes to the articles relating to abortion in the penal codes of all states in Mexico (Gamboa Montejano and Valdés Robledo, 2014), compared to 18 changes between June 21, 2008 and November 17, 2009. Importantly, these reforms resulted in constitutional changes which recognised life as beginning at conception, opening the door for potential homicide charges. In order to understand the precise nature of law changes, we conducted a side-by-side reading of penal codes from pre- and post-reform for each state undertaking

[6] The alliance was successful in rapidly moving the women's health agenda forward in the historically conservative setting of Mexico City using a number of different strategies including the involvement of healthcare officials, hospitals and other healthcare providers. This was not only important for building support for the reform, but also to facilitate a smooth implementation of the abortion program. The alliance was also successful in establishing a strong presence in the public media for promoting women's health rights (Kulczycki, 2011; Johnson, 2013).

[7] Misoprostol alone was the main regimen for medical abortions performed in MOH-DF clinics until 2011 when Mifepristone (combined with Misoprostol) was introduced. This change made the medical abortion procedures provided by the ILE program more efficient and safe.

[8] To put this in context, note that the quantity of abortions per year (adjusted for population) is similar to the quantity in the US. In 2010, for example, 16,945 abortions were provided by the ILE program. In 2010 Mexico DF had approximately 8.55 million inhabitants. Thus, adjusted to the US population in 2010 (308.7 million), this would imply 611,803 abortions. In reality, 765,651 abortions were reported in the US in 2010 (Pazol et al., 2014).

[9] For example, the constitutional decree issued by the state of Nayarit when changing their penal code explicitly refers to the changes in the penal and civil code of Mexico DF (Gobierno de Nayarit, 2009, p. 14).

an abortion reform. The nature of changes implied by the reform are documented in Appendix Table A1. As we display there, of the states undertaking a regressive reform all but six formally altered their penal codes to change sanctions in the case of proven abortions, while the remaining six only altered their state constitution to recognise life as beginning at conception.

In Appendix Figure A2, we display the geographical distribution of law changes (progressive, regressive or neutral) over the period under study. The only progressive reform refers to Mexico DF's ILE reform, while 18 states made regressive changes (i.e., legal tightenings) after the initial reform. We compiled the exact dates the reforms were passed into law on a state-by-state basis, and these are displayed in Appendix Table A2. To the best of our knowledge, no centralised record of the dates and laws that were altered in the post ILE era exists, and as such we compiled these records from our reading of legal source documents. In Section 4 of this paper we return to how we use the geographic and temporal variation in the passage of laws in our identification strategy.

2.4. The potential channels for impacts of abortion reform

Legislative reforms to abortion policies could potentially impact health though a number of channels. We lay these out conceptually below, without yet seeking to assess their relative importance or relevance in explaining observed outcomes. We then move on to assess the likelihood that particular channels can explain observed impacts of abortion reform on maternal health (in the context of this paper) in Section 5.3.

1. Unsafe abortions are shifted to safe legal conditions. This channel is relevant for individuals who would abort when abortion is illegal, and also would abort when abortion is legal. As discussed in Section 2.1 unsafe abortion is thought to be a significant determinant of maternal death and hospitalisation, with evidence that a lack of access to safe abortion shifts the demand for abortion towards clandestine and unsafe conditions (Haddad and Nour, 2009). To the degree that legalising abortion shifts clandestine abortions at risk of complications to sanitary conditions with lower risk, there exists a hypothesised direct channel through which abortion reform may impact women's health. In this channel, holding all else constant, abortions which would have occurred whether abortion was legal or not should imply a direct improvement in women's health when abortion is legalised. We refer to this as the "quality of care channel".

2. Undesired pregnancies are avoided. This channel is relevant for individuals who would not abort when abortion is illegal, but would abort when abortion is legalised. To the degree that abortion being illegal discourages women who would otherwise have sought to discontinue their pregnancy by seeking an abortion, changes in laws will shift the demand for abortion. This channel, which we refer to as the "demand" channel could, theoretically at least, have varying impacts on women's health. For (a) undesired pregnancies which would have resulted in poor maternal health outcomes, the legalisation of abortion and resulting avoidance of risky pregnancies will, all else constant, improve maternal health. However, for (b) undesired pregnancies which would not have resulted in poor maternal health outcomes, the legalisation of abortion may shift maternal health outcomes if abortion—even in safe legal conditions—implies some risk of complications.[10]

3. The composition of women becoming pregnant changes. This channel is relevant for individuals who would not abort when abortion is illegal, and would also not abort when abortion is legal. This channel is relevant only to the degree that the population of women giving birth following abortion reform will consist of more women who desire to take their pregnancy to term, regardless of whether abortion is legal. As women's health complications arise throughout pregnancy, changes in the composition of women becoming pregnant and taking a pregnancy to term may result in changes in rates of health complications observed at the population level. Ex ante, this selection process has ambiguous impacts on maternal health as it depends on the nature the selectivity. We will refer to this as the "selection channel".

3. Data

We construct a balanced panel recording morbidity and mortality outcomes, birth rates and legal outcomes for each of Mexico's 32 states between 2001–2015. In principal models we consider a monthly panel; however, we also generate more temporally aggregate data (by year and by trimester) to include the universe of cases where monthly records are not available, as laid out below. We construct this panel from a large number of administrative microdata bases covering hundreds of millions of records, which are described at more length in Appendix B and summarised below. Along with outcomes generated from administrative data, we collect a number of measures of sexual behaviour from survey data, time varying controls, and the exposure to the ILE reform or subsequent legal changes.

Health outcomes. We observe all morbidity events resulting in a hospital inpatient visit to any public hospital in Mexico. Depending on the hospital type (state run or social security run),[11] the microdata format varies and as a result we can consistently group records only for the years 2004–2015, and only with a yearly total. Given this, we provide two sets of results: one based only on visits to state-run hospitals, as in this case we can calculate monthly

[10] As we discuss later in the paper, this second channel appears to be quite unlikely given that abortion in safe settings has very low rates of associated morbidity and mortality. In fact, induced abortions in safe settings are considered to be "one of the safest procedures in contemporary practice" with a mortality rate below 1 per 100,000 procedures (Grimes, 2005).

[11] The Mexican health system consists of public and private hospitals. Among public hospitals, there are two broad classes: those administered by social security providers for workers in the formal sector and those administered directly by the federal or state governments for individuals without social security coverage. Hospitals in the latter class are principally administered by the Secretariat of Health of federal or state governments and, as such, below we refer to these as Secretariat of Health hospitals.

aggregates for the longer period of 2001–2015, and another based on yearly records for the shorter 2004–2015 period when all hospitalisation data is observed. We similarly consider leakage to the private system using administrative records from all private hospitals.

Our principal measures of interest are maternal morbidity outcomes, classified according to International Classification of Diseases tenth revision (ICD-10) codes,[12] plausibly impacted by abortion. Specifically, these are (a) "abortion-related morbidity" (ICD-10 codes O02–O08) and (b) haemorrhage in early pregnancy (ICD code O20).[13] The remainder of the ICD codes are not examined as it is unlikely that they are sequelae of abortion (e.g., eclampsia or pre-eclampsia), or they are morbidities occurring in the puerperium period, and so are unable to be sequelae of abortion. The two codes are not chosen arbitrarily by the authors but rather in line with the fact that haemorrhage and incomplete abortion are the two most common complications of unsafe abortion (World Health Organization, 2018). We provide a full breakdown of all ICD codes related to pregnancy, childbirth and the puerperium (the ICD-10 O codes) in Appendix Table A3, which documents both the considerable frequency of the two chosen classes and the implausibility of impacts on other variables. Finally, as an exploratory analysis, we aim to measure mental health outcomes of women. In practice, the only way that we can consistently measure this in our data is via the code F53 which captures post-partum depression.[14]

We consider a number of placebo outcomes and do so in a number of ways. A first placebo test consists of considering a (presumably) unaffected morbidity cause *within* the same ICD class (the O codes), namely late term obstetric complications (ICD codes O70–O75). While this is not a perfect placebo insofar as it may be impacted by a changing composition of mothers (via the "selection" channel indicated earlier), it is unlikely to be mechanically related to abortion, so provides a useful comparison to our main morbidity outcomes. Additionally, and to completely isolate the placebo tests from potential changes in the composition of mothers, we consider a larger number of placebo outcomes coming from *other* ICD classifications.[15]

While our main interest here is to document the impacts of abortion reform on the much larger pool of morbidity outcomes, we additionally consider maternal *mortality* as recorded in Mexico's complete vital statistics registers. We thus calculate the number of all maternal deaths in each state and time period and, additionally, only those maternal deaths classified as owing to abortion. Additional discussion of the generation of these data, as well as the recording of microdata bases, is provided in Appendix B.

Births. In order to benchmark the Mexican abortion reforms' impact on birth rates with respect to the wider literature, we also require aggregate data on birth rates by state. We generate these state-level measures from publicly available microdata on births provided by the National Institute of Statistics and Geography, known by its Spanish acronym INEGI. We use each birth register for women aged 15–49 over the time period between 2001–2013, a sample of 30,341,376 births. As discussed in more detail in the Data Appendix, we can only observe birth records up to 2013 as we follow the procedure suggested by the National Population Council (known by its Spanish acronym CONAPO) of using birth registers up to four years following each birth date to ensure that we capture births even if they are registered with a considerable delay. We use these same data to measure average characteristics of mothers and fathers (where registered) to examine any changes in the composition of parents following abortion reforms. In both the case of births and the case of health outcomes, in order to calculate rates of occurrence we record the state-level population in each year for all women aged 15–49 provided by CONAPO.

Additional measures. We additionally collect a number of other state- and time-varying measures to examine potential mechanisms of action of the reform or to dismiss alternative possible explanations of observed impacts. These measures consist of (a) all first legal decisions made by the Mexican judiciary related to abortion compiled from microdata collected by INEGI and released as Mexico's Judicial Statistics on Penal Matters; (b) information on contraceptive use and sexual behaviour from the Mexican Family Life Survey (MxFLS); and (c) time-varying controls to capture possible confounders of abortion policy, namely education, health investment and access to formal healthcare among the state population, access to the public insurance program *Seguro Popular*, economic development, and women's social inclusion. These controls are fully described in Data Appendix B, as well as in the discussion of summary statistics below.

Summary statistics. Summary statistics of principal outcomes (maternal morbidity, mortality and births) are provided in Table 1. The total number of cases of each morbidity class are described in Panel A, and mortality

[12] It is important to note that the procedure of assigning ICD codes for a hospitalisation is typically implemented by a particular person or persons in charge of coding diagnoses in hospitals (such as an epidemiologist or a hospital employee). This coding is made based on their reading of the treating team's charts; it is *not* determined by the team actually treating the patient (Velasco Sustaita, Undated). Moreover, the ICD coding is typically processed after the patient is discharged from the hospital. Anecdotal evidence suggests that the prosecution of women for the crime of abortion usually occurs during the hospital stay, after either a clinician, nurse or social worker have raised their "suspicion" to the police. There is no ICD code for induced illegal abortion in the ICD-10 classifications and indeed evidence from other contexts suggests that, if anything, providers may seek to record abortion-related morbidity within sub-codes of ICD-10 classifications (such as recording abortions as spontaneous instead of induced) rather than classifying it as an alternative code (Suh, 2014). The authors are grateful to Dr Raffaela Schiavon for providing very useful background details in correspondence.

[13] For a longer discussion on classification of abortion-related morbidity see Schiavon et al. (2012) and World Health Organization (2018).

[14] We explored using external causes of morbidity to capture suicide attempts; however, the inpatient data from Mexico only consistently provides a single ICD code capturing the principal cause of hospitalisation, while external causes are classified as a secondary ICD code. Thus, these external causes are not recorded consistently in administrative data.

[15] Specifically, these are diseases of the ear and mastoid processes; neoplasms; endocrine, nutritional and metabolic diseases; diseases of the blood and blood-forming organs; diseases of the nervous system; and diseases of the skin and subcutaneous tissue. We discuss these at greater length in Section 5.3.

Table 1
Summary statistics for month by year by state specifications

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Panel A: Morbidity outcomes					
Total Number of Deliveries in Public Hospitals	5760	1455.9	1282.8	143	8496
Total Inpatient Cases for ICD O codes, except births	5760	1276.3	1002.8	139	6271
Total Inpatient Cases for Abortion-Related Causes	5760	268.7	232.9	27	1573
Total Inpatient Cases for Haemorrhage Early in Pregnancy	5760	31.4	27.2	0	207
Total Inpatient Days for Abortion-Related Causes	5760	361.3	327.4	28	2168
Total Inpatient Days for Haemorrhage Early in Pregnancy	5760	64.7	63.0	0	463
Total Inpatient Cases for Obstetric Complications	5760	36.0	51.1	0	375
Total Inpatient Cases for Post-Partum Depression	5760	0.1	0.3	0	6
Panel B: Mortality outcomes					
Total Number of Maternal Deaths	6144	2.98	3.26	0	25
Total Number of Maternal Deaths due to Abortion	6144	0.21	0.50	0	4
Panel C: Demographic outcomes					
Population of 15–49 Year-old Women	6144	864691	743706	116430	4228223
Total Number of Births	4992	6078	4904	719	28546
Birth rate per 1000 women	4992	7	1	5	13

Each observation is a state × year × month cell. Mexico is composed of 32 States. The number of cells varies due to the number of years and months of data available. In Panel A, morbidity data is displayed for 12 months in 12 years (2004–2015). Values are generated from all inpatient cases as classified from microdata from the primary care (hospital) records from all public hospitals administered by the Secretariat of Health (social security system hospitals do not report month of hospitalisation). Each type of morbidity is classified by ICD-10 codes. In Panel B, mortality outcomes are displayed for 12 months in 16 years (2001–2016). In Panel C, data on population is displayed for 12 months in 16 years (2001–2016), and data on births is displayed for 12 months in 13 years (2001–2013). Following CONAPO, the last four years of birth outcomes are suppressed to account for reporting outside of the period of birth. State × year summary statistics including social security system hospitals are provided in Appendix Table A4.

outcomes, both for all maternal deaths and those only classified as owing to abortion, are provided in Panel B. State by year × month averages of the number of births and births per 1000 fertile-aged women are displayed in Panel C of Table 1. On average, morbidity outcomes are various orders of magnitude higher than mortality outcomes. For example, the average quantity of hospitalisations for abortion-related causes was 269 per state and month versus 3 maternal deaths on average, or 0.2 maternal deaths when considering only those classified as owing to abortive causes. In this table, morbidity data is recorded only based on public hospital data where the month of the visit is recorded. As we will discuss at greater length below, we will also consider variation by state and year allowing us to capture the full universe of health outcomes from both state run and social security run hospitals (see Appendix Table A4 for summary statistics), and control variables are summarised in Appendix Table A5.

Raw trends of principal maternal health outcomes are provided in Fig. 1, pointing to important shifts in inpatient visits related to maternal health following the ILE reform. Fig. 1(a) and (b) document the quantity of monthly inpatient cases for abortion-related morbidity and haemorrhage in early pregnancy in Mexico DF for states that adopted a subsequent legal tightening and for states with no subsequent legal changes. We observe reductions in the absolute quantity of cases in Mexico DF in both cases following the ILE reform in April 2007 (indicated as a vertical red line). Panels (a) and (b) are based only on hospitalisation data from Secretary of Health hospitals. When we additionally extend to include social security hospitals in panels (c)-(f), observing yearly variation only, we see a similar pattern with reductions in total hospitalisations (panels (c) and (d)), and rates of hospitalisations for fertile-aged women (panels (e) and (f)).

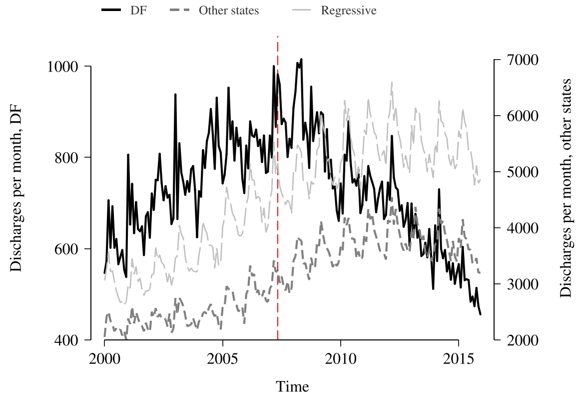
4. Methodology

We aim to examine the impact of the ILE reform and regressive law changes, compared with outcomes in states in which no reform was implemented. We thus begin by estimating the following two "two-way fixed effect" specifications[16]:

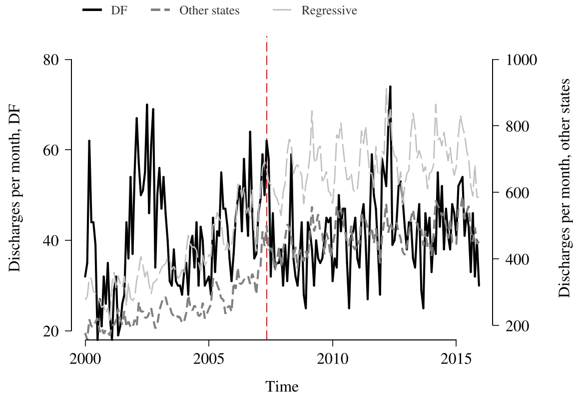
$$Health_{st} = \alpha_0 + \alpha_1 \mathrm{ILE}_{st} + X'_{st}\Gamma_1 + \phi_s + \mu_t + (\phi_s \times month) + \varepsilon_{st} \quad (1)$$

$$Health_{st} = \beta_0 + \beta_1 \mathrm{Regressive}_{st} + X'_{st}\Gamma_2 + \phi_s + \mu_t + (\phi_s \times month) + \eta_{st}. \quad (2)$$

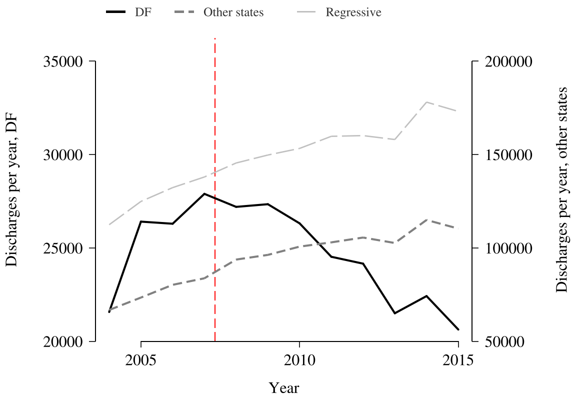
[16] We estimate these models separately; however, all results documented in this paper are robust to estimating a single model including both the ILE and regressive variables. Full results in this set-up are documented in Clarke and Mühlrad (2018).



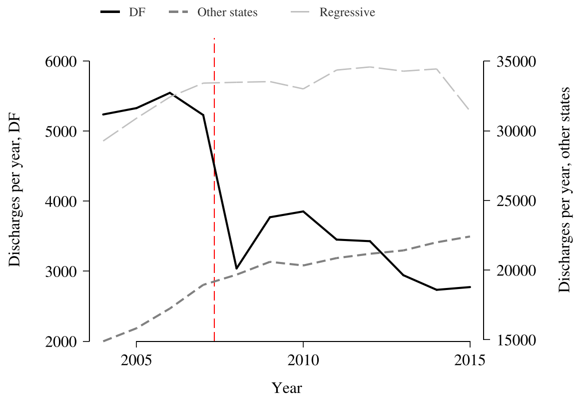
(a) Abortion-related Morbidity (Public hospitals)



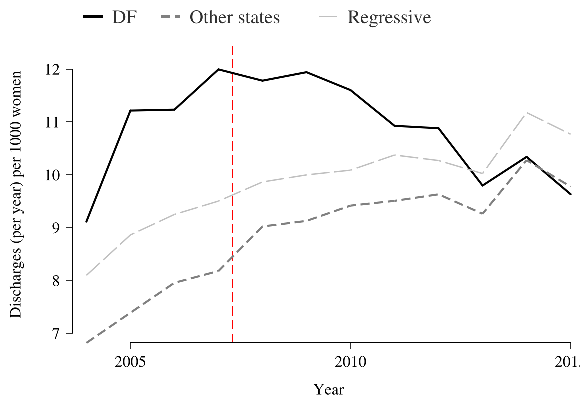
(b) Haemorrhage (Public hospitals)



(c) Abortion-related Morbidity (Full sample)



(d) Haemorrhage (Full sample)



(e) Abortion-related Morbidity (Full sample, per 1000 women)

(f) Haemorrhage (Full sample, per 1000 women)

Fig. 1. Raw trends in abortion-related morbidity and haemorrhage in early pregnancy. Notes: Figures present the total number of discharges due to abortion-related morbidity (left-hand panels) and haemorrhage in early pregnancy (right-hand panels) in Mexico DF (which adopted the ILE reform), states which had a later tightening of abortion laws, and all other states. Panels (a) and (b) plot monthly trends based on all discharges from Secretariat of Health hospitals given that these are the only registers which record month and year of discharge. Panels (c) and (d) plot yearly trends based on all public hospital discharges (both Secretariat of Health hospitals and social security hospitals which do not report month of discharge), and panels (e) and (f) plot yearly discharges based on all public hospital data expressed per 1000 women of fertile age. The dotted vertical line is plotted in April 2007, the date of passage of the ILE abortion reform, and wide-scale rollout of available abortions. Residualised plots and longer yearly plots based on panels (a) and (b) are provided as Appendix Figures A3 and A4, respectively.

Here *Health* refers to average rates of morbidity or mortality in state s at time t, and ILE and regressive refer to the post-ILE and post-regressive law changes in affected states. Our parameters of interest are α_1 from Eq. (1) and β_1 from Eq. (2). In the case of specification (1), the estimation sample consists of Mexico DF (which adopts ILE) and all non-reforming states. In the case of specification (2), the sample consists of all states which adopt regressive abortion laws and all non-reforming states. We include state and year fixed effects as ϕ_s and μ_t, respectively, as well as state-specific monthly fixed effects ($\phi_s \times month$) to capture any potential state-specific seasonality, and examine stability to the inclusion of the time-varying controls X_{st} listed in Section B.2. In initial specifications, time t refers to monthly measures. This allows us to examine fine-grained temporal variation in outcomes in all hospitals run by the Mexican Secretariat of Health. However, to ensure that these results are also observed in the full universe of public health records, we also consider specifications where time t refers to yearly cells including all data.

In this two-way FE specification, the ILE variable in Eq. (1) is an indicator which switches from 0 to 1 at a particular point in time; however, the regressive indicator in Eq. (2) switches from 0 to 1 at varying times depending on the state. In the case of heterogeneous treatment effects and time-varying adoption, this single-coefficient model can considerably mis-estimate the average treatment effect on the treated (ATT), given that already treated units act as controls in future periods (Goodman-Bacon, 2018). While this is not a concern for our estimate of α_1, it is for our estimate of β_1. Accordingly, we will (a) document the parameter decomposition derived in Goodman-Bacon (2018) and (b) estimate event study specifications which avoid these potential biases. There are 32 states in Mexico (including the Mexico DF), and these laws are defined at the level of the state. In order to account for the possibility of unobserved correlations of outcomes for women within a state, standard errors are clustered by state using a clustered wild bootstrap.

Our outcomes of interest for this procedure are the measures of maternal morbidity and mortality discussed in Section 3, as well as birth rates. We additionally consider exploratory analyses examining post-partum depression. We thus implement the procedure for a measure of all abortion-related morbidity, morbidity due to haemorrhage in early pregnancy, post-partum depression, total maternal mortality, and maternal mortality due to abortion. In each case in the main outcomes, we focus on rates of morbidity and mortality per the population of fertile-aged women. We express our outcomes in this way for two reasons. The first is that it allows us to capture the full effect of the reform. As we will show that the abortion reform reduces fertility, if we express our outcomes as morbidity or mortality per live birth this is equivalent to a partial impact, removing any impact of the reform which flows from the ability to avoid undesired, and potentially risky, births.[17] In practice, we are interested in the total impact of the reform, which consists of the reduction in morbidity and mortality due to fewer births, as well as any direct impact the reform may have on the composition of mothers giving birth. The second reason is that this focus allows us to ignore any challenges arising from the endogenous decision of whether or not to engage in legal abortion. If we instead report the impact of the law on rates of morbidity and mortality per live birth, we will be confounding our estimates due to the fact that a non-random group of women choose to proceed with births following the reform, and this group may be selectively more or less healthy than the women who elect to abort. We address changes in the composition of mothers explicitly in Section 5.3 of this paper.

For two-way FE estimates to capture the causal effects of abortion laws, we require a parallel trend assumption to hold, or that outcomes in each of the "Regressive", "ILE" and untreated states would have evolved similarly in the absence of abortion reforms. We provide a partial test of this, and additionally quantify any dynamic reform effects, by estimating the following panel event study specifications:

$$Health_{st} = \kappa_0 + \sum_{j=-36}^{36} \delta_{-j} \Delta \text{ILE}_{s,t+j} + X'_{st}\Gamma_1 + \phi_s + \mu_t + (\phi_s \times month) + \nu_{st} \tag{3}$$

$$Health_{st} = \kappa_1 + \sum_{k=-36}^{36} \gamma_{-k} \Delta \text{Regressive}_{s,t+k} + X'_{st}\Gamma_2 + \phi_s + \mu_t + (\phi_s \times month) + \upsilon_{st}. \tag{4}$$

We normalise both δ and γ setting $\delta_{-1} = 0$ and $\gamma_{-1} = 0$. These event study specifications are increasingly common in panel settings, and here we adopt the notation of Freyaldenhoven et al. (2018), using Δ to denote the precise moment in which treatment status changes. In this specification, we are interested in the leads and the lags of the policy changes, where leads capture any prevailing trends prior to the reform and lags show the change in health outcomes following the reform's implementation. In the main specifications (3) and (4), we present the model for morbidity data available between 2001 to 2015, which allows us to consider a large number of monthly lags and leads. We use 36 monthly lags and leads, where the final lag and lead indicates all periods beyond this time. As was the case with two-way FE models, we also examine robustness to using yearly data with all hospitalisations. In this case we are able to use data exclusively from between 2004–2015, aggregated to a yearly-level, and as such we estimate three leads and eight lags of the ILE reform, and five leads and seven lags of regressive law changes. We can estimate additional lags for regressive reforms in this case given that there is greater variation in treatment timing.[18] In the case of

[17] It is also important to note that the ILE reform included the option of accessing free contraceptives after undergoing an abortion procedure, which could also impact birth rates. We discuss this at greater length in Section 5.3.

[18] Practical details are discussed in Clarke and Tapia Schythe (2020).

mortality or fertility where longer periods of data are available, lags and leads are modified in yearly event studies to provide a fully saturated model in each case. In one case where parallel pre-trends appear less convincing, we conduct a newly described estimation and inference procedure known as "Honest DD" to examine possible implications of violations of the parallel trend assumption (Rambachan and Roth, 2019).

It is important to note here that unbiased estimation of reform impacts hinges, pivotally, on the assumption of (conditional) parallel trends, and on a stable unit treatment value assumption (SUTVA) implying no spillovers to untreated states. We address the SUTVA explicitly in Section 5.3 where we consider whether we observe appreciable policy spillovers. As always, the parallel trends assumption is something that cannot be tested formally given the unobserved counterfactual state for treated areas in the post-treatment period. Thus, the appearance of any simultaneous reforms *only* in treatment areas would result in biased estimates of reform impacts. Nevertheless, as discussed above, we aim to reduce the likelihood of such factors by controlling for time-varying factors (including the *Seguro Popular* rollout), and we additionally consider a range of placebo tests to determine whether we observe similar patterns in variables which plausibly should *not* respond to abortion reforms.

5. Results

5.1. Abortion laws and maternal morbidity and mortality

5.1.1. Estimated impacts on maternal morbidity

Two-way FE estimates (which for simplicity hereafter we will refer to as "DD" estimates given the similarity to double difference models) of the impact of the legal reforms on morbidity are presented in Table 2. All coefficients are cast as the effect of law changes on morbidity per 1000 women. Columns 1–2 are baseline DD models including only time and state fixed effects, while columns 3–4 add in time-varying controls described previously. We present results for Eq. (1) in Panel A, and for Eq. (2) in Panel B. On average, compared with non-reform states, the ILE reform resulted in a reduction in morbidity by approximately 0.06 to 0.08 cases per 1000 women (per month) when considering all abortion-related morbidity and 0.013 to 0.016 cases per 1000 women (per month) when considering the incidence of haemorrhage in early pregnancy. When compared to average rates of morbidities, this is approximately a 20% reduction in abortion-related morbidity and a 35% reduction in rates of haemorrhage in early pregnancy. Note that these large effects were notable even in raw trends displayed in Fig. 1.

In the case of subsequent restrictive reforms, we do not find evidence to suggest that these reforms shifted morbidity outcomes. For abortion-related morbidity and for haemorrhage in early pregnancy, we find no significant impacts across specifications reported in Table 2. When instead of the total number of cases we examine the total number of inpatient days (Appendix Table A6), we similarly observe a large reduction following Mexico's ILE reform and no significant, or consistently signed, impact in the case of regressive reforms.

Fig. 2a presents event study evidence for haemorrhage in early pregnancy, and Fig. 2b presents event study estimates for abortion-related morbidity. In both cases, the left-hand panel shows the lag and lead coefficients for Mexico DF surrounding the ILE reform (Eq. (3)), and the right-hand panel shows the coefficients for regressive states (Eq. (4)). In Fig. 2a we observe an immediate sharp decline in rates of haemorrhage in Mexico DF following the adoption of ILE. Additionally, we observe little evidence of prevailing differences in treated and untreated states *before* the reform in all leads. In the case of regressive states (Panel (b)), we observe a similar quite flat profile prior to the reform. Following the reform, we observe no similar reduction in rates of haemorrhage early in pregnancy as that observed in Mexico DF, with no lag terms being statistically distinguishable from zero at 95% significance levels. These results support evidence from the medical literature that haemorrhage is one of the major drivers of maternal morbidity and mortality following unsafe abortions (World Health Organization, 2011), as the appearance of a legal and safe alternative to clandestine abortion resulted in an immediate sharp reduction in hospitalisations resulting from haemorrhage early in pregnancy. Moreover, we observe that these reductions occur very quickly following the moment that abortion was legalised, suggesting an immediate effect of safe legal abortion on women's health outcomes.

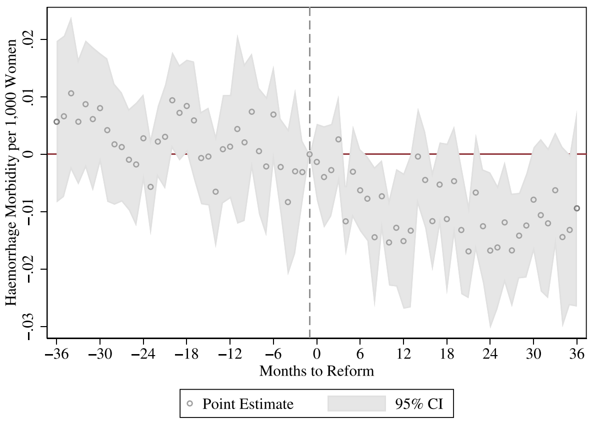
When considering rates of abortion-related morbidity, event studies document larger prevailing (pre-reform) differences between Mexico DF and untreated states, although these are largely driven by a number of monthly fluctuations greater than two years prior to the reform.[19] This agrees with simple trends in outcomes documented in Fig. 1, which suggest a number of relatively sharper jumps observed in Mexico DF prior to 2005 that were not seen in the rest of the country. However, these *do not* seem to point to radically different pre-reform trends but rather cyclical outliers. In the case of states which altered legislation in response to ILE, we observe very little evidence of an impact of these reforms on abortion-related morbidity in Fig. 2b. In both the pre- and post-reform period, all estimates are not statistically distinguishable from zero and are centred around a null impact. In the case of abortion-related morbidity, it is important to note that the procedure used for abortions realised under the auspices of ILE has changed over time, which may partially explain the delay in observed impacts on morbidity. Initially, the majority of abortions were performed by surgical procedures compared to medical abortions (25%). This gradually changed in subsequent years, with medical abortion procedures reaching 74% in 2011, and the use of dilation and curettage was eliminated entirely (in accordance with World Health Organization (WHO) recommendations for first-trimester abortions). In addition, the quality of medical abortions

[19] In Appendix Figure A5 similar results, with fewer fluctuations in pre-periods, are observed when considering trimesterly rather than monthly cells.

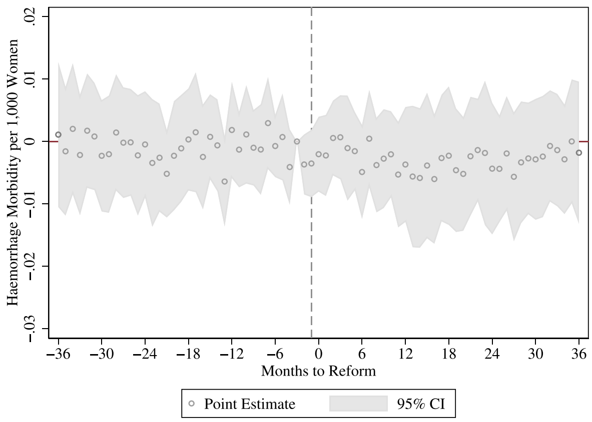
Table 2
Difference-in-differences estimates of legal reforms on morbidity

	Abortion-related morbidity				Haemorrhage early in pregnancy			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: ILE versus non-reformers								
Post-ILE Reform (DF)	−0.064***	−0.071***	−0.063***	−0.075***	−0.014***	−0.016***	−0.014XX	−0.013**
	(0.013)	(0.006)	(0.016)	(0.012)	(0.004)	(0.004)	(0.007)	(0.007)
Observations	2496	2496	2496	2496	2496	2496	2496	2496
Mean of Dependent Variable	0.338	0.338	0.338	0.338	0.044	0.044	0.044	0.044
Panel B: Regressive reforms versus non-reformers								
Post-Regressive Law Change	−0.001	0.000	−0.009	−0.007	−0.005	−0.003	−0.005	−0.002
	(0.013)	(0.011)	(0.011)	(0.011)	(0.006)	(0.004)	(0.006)	(0.004)
Observations	5952	5952	5952	5952	5952	5952	5952	5952
Mean of Dependent Variable	0.328	0.328	0.328	0.328	0.042	0.042	0.042	0.042
State and Year × Month FEs	Y	Y	Y	Y	Y	Y	Y	Y
Population Weights		Y		Y		Y		Y
Time-Varying Controls			Y	Y			Y	Y

Notes: Each column displays a difference-in-differences regression of the impact of abortion reform on rates of morbidity (inpatient cases). Each morbidity class is measured as cases per 1000 fertile-aged women each month, and average levels in the full set of data are available at the foot of the table. Each regression is estimated using states that adopt reforms (ILE in Panel A, regressive reforms in Panel B) versus other non-adopting states. All standard errors are clustered at the level of the state.
XX $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.



(a) Progressive Abortion Reform (ILE)

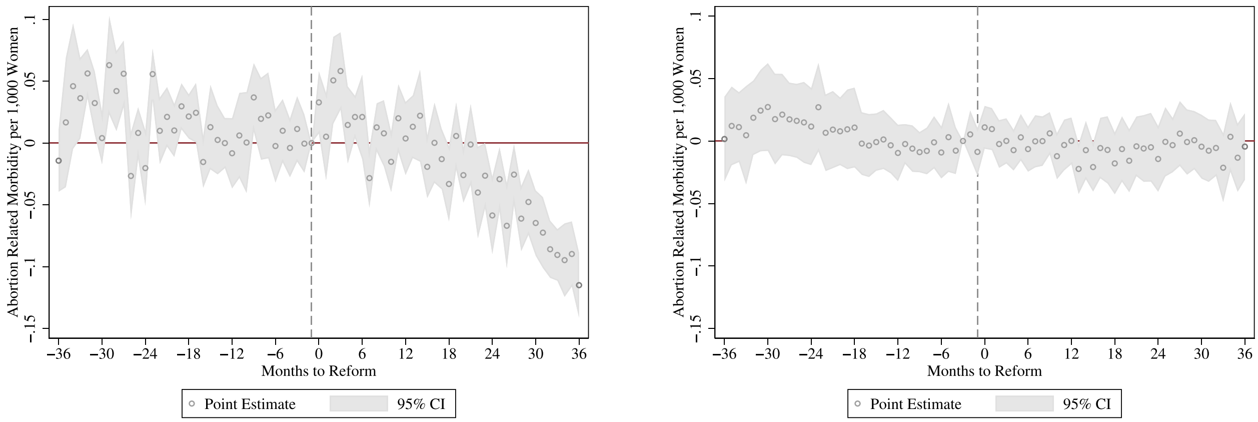


(b) Regressive Abortion Laws (Legal Tightening)

Fig. 2a. Event studies for rates of haemorrhage early in pregnancy.

performed has also improved due to the introduction of mifepristone (combined with misoprostol) in 2011 (Becker, 2013).

We note that these reductions in rates of maternal morbidity in Mexico DF surrounding the passage of the ILE reform are not simply capturing a general improvement in health outcomes in the region. In Appendix Figure A6 we estimate identical models for (late-term) obstetric complications and observe no such improvements in Mexico DF. We also observe evidence to suggest parallel trends throughout the entire period studied between Mexico DF, states implementing regressive reforms, and the rest of the country. Similarly, in Appendix Figures A7 and A8, when considering a much broader class of morbidity outcomes



(c) Progressive Abortion Reform (ILE) (d) Regressive Abortion Laws (Legal Tightening)

Fig. 2b. Event studies for rates of abortion morbidity. Notes: Event studies document the evolution of rates of haemorrhage early in pregnancy (2a) and abortion-related morbidity (2b) per 1000 women surrounding the passage of abortion reforms. Each point estimate refers to the change in rates between treated and untreated states, compared to their baseline differential immediately prior to the reform. The left-hand panel shows the difference between Mexico DF and untreated states surrounding the passage of the ILE reform. The right-hand panel shows the difference between regressive policy changers and non-changers around the (time-varying) date that each reform was passed. Regressions are weighted by the population of fertile-aged women, and the full set of time-varying controls are included.

(such as neoplasms, endocrine diseases and so forth), we once again observe no systematic difference in rates of morbidity surrounding abortion reforms, with the exception of a number of monthly peaks and troughs observed in Mexico DF, indicative of no general improvements across the time period studied.

5.1.2. *Maternal mortality and "the tip of the iceberg"*

Frequently, analyses of the impact of public policies on maternal health focus on maternal mortality, given a lack of access to high-quality morbidity records (such as those available in Mexico). An argument is made that if effects are observed on maternal morbidity, which is the "tip of the iceberg" (see Firoz et al., 2013), then there will logically be considerable impacts on maternal morbidity (the base of the iceberg). However, the much lower frequency of maternal deaths compared with maternal morbidity makes it much harder to precisely estimate impacts of health reforms on maternal mortality. Here we briefly consider how much precision is lost when considering impacts on maternal mortality, comparing our previously estimated impacts on morbidity with those focusing on maternal deaths. Given the much lower rates of maternal deaths, we focus on yearly models. And as these estimates are indeed considerably more noisy than estimated impacts on morbidity, we relegate results to an Online Appendix and discuss broad patterns and lessons for estimating reform impacts on maternal health outcomes.

In Appendix Table A7 we present DD estimates following Eqs. (1) (Panel A) and (2) (Panel B) for both all maternal deaths (columns 1–4), and only maternal deaths originating from abortive causes (columns 5–8). When focusing on the ILE reform, we observe mixed evidence pointing in the direction of negative though imprecisely estimated effects. Both weighted and unweighted estimates suggest a reduction in all maternal deaths following ILE (columns 1 and 2), of approximately 0.5 per 100,000 fertile-aged women (versus a mean value of 4 deaths per 100,000 women in Mexico). Note, however, that when adding time-varying controls in column 3, these estimates are reduced by about one-third and become statistically insignificant at typical levels. Similarly, in the case of abortion-related maternal mortality, we observe significant reductions when using weighted or unweighted simple DD models (with point estimates of around −0.09 per 100,000 fertile-aged women), though these become insignificant with the inclusion of time-varying controls. In the case of regressive reforms we find, across the board, relatively less evidence of any impacts of these reforms on maternal mortality. We do consistently observe negative point estimates of a magnitude approaching that observed in Mexico DF following the ILE reform; however, these are only (marginally) significant in two models. We also note that, as discussed, standard errors are quite wide thus precluding us from concluding that these estimates suggest tightly estimated zero-impacts.

In general these results point to the fact that focusing only on mortality when studying reforms which impact maternal health may considerably understate their importance as a determinant of well-being. Both Appendix Table A7 (as well as event studies presented in Appendix Figures A9a for all maternal mortality and A9b for mortality due to abortion) suggest noisy results with little power to reject a range of nulls. While event studies suggest that reductions in mortality may have been observed in Mexico DF, these are certainly not as clearly observed as the morbidity results presented in Section 5.1.1 of this paper.

5.2. *Benchmarking impacts on birth rates*

While our main focus in this paper is on quantifying the health costs of abortion reform, it is illustrative to also estimate impacts on birth rates. This allows us to consider the magnitude of these reforms compared with a range of other contexts which have been well-documented in the economic literature. As summarised in Appendix Table A8, across studies on abortion legalisation in the US, Nepal, Norway, Romania and Uruguay, we observe a drop in birth rates of between 1.2% and 8%. Studies on the impact of regressive abortion law changes (including parental consent laws and restricted funding of abortions) find considerably more heterogeneous results, with results ranging from significant reductions in birth rates (Kane and Staiger, 1996), insignificant impacts (Levine et al., 1996), and increases in rates of birth (Lahey, 2014).

Our results from the Mexican abortion reforms suggest broadly similar impacts on birth rates to those observed in other settings following the elimination of abortion restrictions. In Table 3 we present DD estimates of the impact of reforms on birth rates, weighted and unweighted by the number of fertile-aged women in each state by month cell. In general, across specifications, results are quite stable in suggesting a significant reduction in births in Mexico DF following the ILE reform. Depending on estimation weights, we observe a reduction of between 0.53 and 0.64 births per 1000 women per month, which is the equivalent of a reduction of between 7% and 8.5% in birth rates compared with pre-reform levels in the state. Our preferred estimates are those including population weights with full time-varying controls, which suggest a reduction of 0.64 births per 1000 women of fertile age in the years following the ILE reform, which is the equivalent of an 8.5% reduction in birth rates in Mexico DF. In the case of states passing regressive laws altering their penal codes or state constitutions related to abortion, we find some evidence to suggest smaller reductions in birth rates in these DD specifications. Depending on the model, point estimates vary from −0.19 to −0.27 births per 1000 women per month, equivalent to a 2.5% to 3.6% reduction in birth rates.[20] We return to examine the nature of these legal reforms in more detail in Section 5.3, revisiting the smaller estimated impacts on birth rates.

We provide full event studies corresponding to the passage of progressive and regressive reforms in Fig. 3a. Given the considerable seasonality and even monthly variations in the quantity of births, we additionally present event studies by year in Fig. 3b (and by trimester in Appendix Figure A10), which allows for some smoothing of sharp monthly changes. In the left-hand panel of both monthly and yearly event studies, we observe a reduction in birth rates in Mexico DF when compared with all non-reform states, which becomes consistently observed from around 7–10 months (or one year) post-reform (2008) onwards. This is in line with lags in birth rates expected to be observed approximately 7–9 months following the passage of abortion reforms due to the gestational period and limits on gestational length when undertaking abortion. Estimates in the pre-reform period are not completely flat. If anything they suggest evidence of a slightly upward-sloping trend or an inverted U-shaped trend a number of years before the adoption of abortion law reforms in Mexico DF.

The impact of prevailing trends in outcomes which are not parallel between treated and untreated areas can be formally examined using “Honest DD” methods proposed by Rambachan and Roth (2019). These methods propose a robust estimation and inference technique assuming that trends in the post-event period do not diverge ‘too much’ from those in the pre-event period.[21] We document how our estimated event study coefficients would vary under a range of assumptions using these “Honest DD” techniques in Appendix Figures A11, A12 and A13. Given that these techniques require bounding *each* figure in the event study, we conduct these methods with yearly analogues of the monthly event studies (confidence intervals for baseline yearly event studies are presented in blue in these plots for comparison). Importantly, as well as allowing for varying violations of parallel trends, they document that in each case, if trends had remained constant in the post-event period, the true estimate would be both more negative and consistently statistically significant. In Appendix C we also document the stability of these estimates when compared to a judiciously chosen synthetic control group (Figure A26).

The right-hand panel of Fig. 3a documents the same point estimates and confidence intervals for states altering their constitutions or criminal codes to increase legal sanctions on abortion. In the case of regressive reforms, while we observe a gradual reduction in estimated coefficients following legal changes, the effects are generally not statistically distinguishable from zero in monthly (or trimesterly) event study models, although a significant impact is observed 4–5 years post-treatment in more aggregated yearly models. In this case, prevailing trends in the pre-reform period are observed to be somewhat downward sloping, with little evidence of a trend break following the implementation of regressive reforms. These event studies suggest that, if anything, the DD estimates presented in Fig. 3a may over-state the impact of legislative tightenings. Given recent advances in work on the interpretation of two-way FE models, it is important to note that in the case of regressive abortion laws which are rolled out in a time-varying way, these event study models which suggest insignificant effects (in monthly and trimesterly models

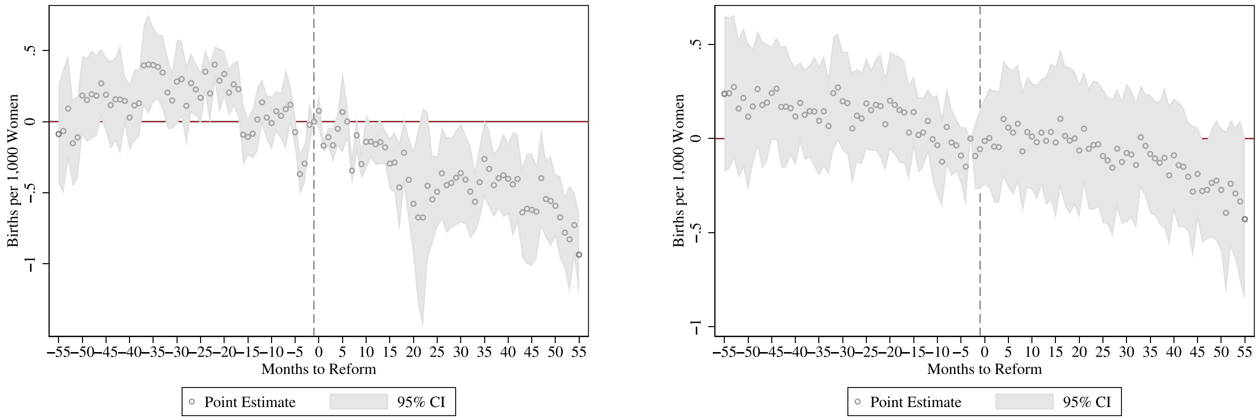
[20] As we will discuss below, we note that these estimates are significant only in the case of two-way FE models and not in event study models. In general, in the case of the regressive reforms which are rolled out in a time-varying way, two-way FE models potentially mis-estimate the true nature of the ATT, and so we find event study estimates more credible. Nonetheless, we do note that in some literature discussed in Appendix Table A8 reductions in fertility are observed following legal restrictions on abortion access, and this could potentially be explained if increasing sanctions on abortion act to discourage marginal births. A theoretical model of such a case is provided in Ananat et al. (2009).

[21] Specifically, these methods are not based on parallel trends assumptions but rather impose a limit on the degree to which parallel trends can fail in the post-treatment period. Following Rambachan and Roth (2019), in the tests conducted in this paper we limit the degree to which counterfactual trends between outcomes in the ILE and non-ILE areas are allowed to deviate from linearity. We present bounds estimates based on various different assumptions related to the maximum possible divergences from linearity between each time period, allowing us to observe the sensitivity of inference to possible failures of parallel trend assumptions.

Table 3
Monthly difference-in-differences estimates of abortion reforms on fertility.

	Births per 1000 women			
	(1)	(2)	(3)	(4)
Panel A: ILE versus non-reformers				
Post-ILE Reform (DF)	−0.531***	−0.636***	−0.537***	−0.608***
	(0.075)	(0.106)	(0.110)	(0.106)
Observations	2028	2028	2028	2028
Mean of Dependent Variable	7.302	7.302	7.302	7.302
Mean of Dependent Variable (Mexico DF)	7.433	7.433	7.433	7.433
Panel B: Regressive reforms versus non-reformers				
Post Regressive Law Change	−0.191**	−0.269***	−0.202**	−0.228**
	(0.089)	(0.097)	(0.085)	(0.112)
Observations	4836	4836	4836	4836
Mean of Dependent Variable	7.266	7.266	7.266	7.266
Mean of Dependent Variable (Regressive States)	7.434	7.434	7.434	7.434
State and Year FEs	Y	Y	Y	Y
Population Weights		Y		Y
Time-Varying Controls			Y	Y

Each column displays a difference-in-differences regression of the impact of abortion reform on birth rates. Birth rates are measured as the number of births per 1000 fertile-aged women each month. Time-varying controls are documented in Section B.2. All standard errors are clustered at the level of the state using a wild clustered bootstrap procedure.
** $p < 0.05$.
*** $p < 0.01$.



(a) Progressive Abortion Reform (ILE) (b) Regressive Abortion Laws (Legal Tightening)

Fig. 3a. Monthly event studies for birth rates.

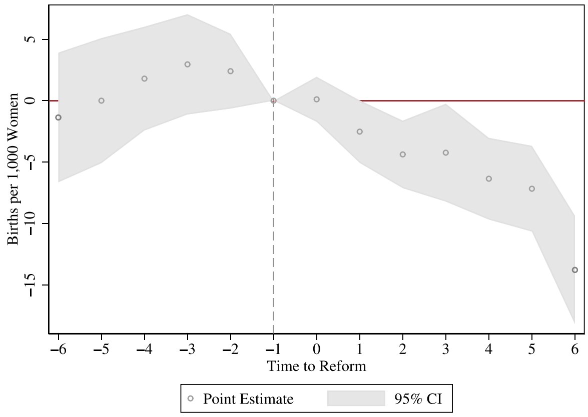
documented in Appendix Figure A10) should be preferred over the models presented in Table 3. This is because the weighting in the two-way FE models may result in estimates which do not truly capture the ATE. We return to this point in the next sub-section.

5.3. Understanding impacts of abortions laws

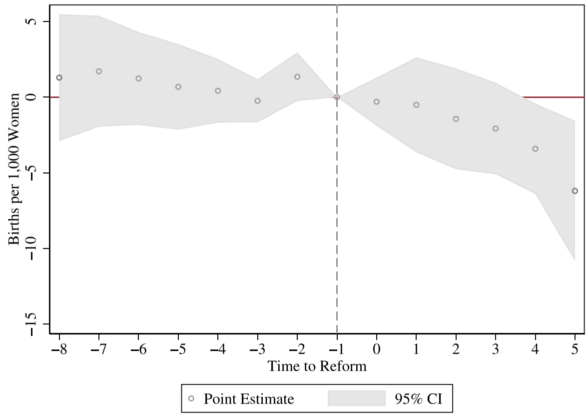
5.3.1. Why do we observe changes in women's health?

In Section 2.4 we discussed three conceptual channels through which abortion reform could impact maternal health: the "quality of care" channel, the "demand" channel, and the "selection" channel. We consider which of these channels could explain the observed impacts of abortion reform on women's health laid out in Section 5.1.

The selection channel suggests that the composition of women giving birth may change because of selective interaction with abortion laws. We consider this directly in Appendix Table A9 where we use the same two-way FE set-up to examine how maternal and paternal characteristics vary surrounding both the ILE reform as well as regressive reforms. In general, we observe that parents who give birth following the ILE reform are both older and more educated. We *also* see some change in the composition of parents following regressive reforms with parents more likely to be married, which potentially indicates that there are impacts on the types of women conceiving when sanctions on abortions are raised. While this suggests that a *selection* channel may be occurring, these results do not allow us to sign the direction this channel works in, as they do not directly consider the interaction of these changing characteristics



(c) Progressive Abortion Reform (ILE)



(d) Regressive Abortion Laws (Legal Tightening)

Fig. 3b. Yearly event studies for birth rates. Notes: Event studies document the evolution of birth rates per 1000 women surrounding the passage of abortion reforms. Each point estimate refers to the change in rates between treated and untreated states, compared to their baseline differential immediately prior to the reform. Fig. 3a presents event studies based on monthly birth rates while Fig. 3b is based on yearly birth rates (trimesterly figures are displayed in Appendix Figure A10). In each case, the left-hand panel shows the difference between Mexico DF and untreated states surrounding the passage of the ILE reform. The right-hand panel shows the difference between regressive policy changers and non-changers around the (time-varying) date that each reform was passed. Regressions are weighted by the population of fertile-aged women, and the full set of time-varying controls are included.

on health outcomes. We test this directly in Appendix Table A10 where we consider the same two-way FE models examining reform impacts on health outcomes; however, in this instance additionally controlling for variables which capture parental selection (namely, all outcomes considered in Appendix Table A9). This allows us to ask whether the estimates of the impact of abortion reforms on health that we observe are completely explained by the changes in the composition of parents.

The results of Table A10 suggest a number of important interpretations. First, in the case of abortion-related morbidity, many of the observed impacts of the ILE reform *do* appear to flow from changes in the characteristics of parents. And, in particular, the fact that our significant estimates are attenuated towards zero conditional on these controls suggests that those giving birth following the reform are selectively healthier (at least in terms of the likelihood of having a miscarriage or other types of abortion-related morbidity). However, in the case of haemorrhage early in pregnancy, we observe no such change in the coefficients. This suggests that the observed impacts are not simply a compositional impact but rather likely owe to the demand channel or the quality of care channel. In the case of Mexico, it seems likely that the quality of care channel explains the large reduction in haemorrhage in early pregnancy. In clandestine settings, where misoprostol is used without adequate access to information, haemorrhage and resulting hospitalisation is likely to occur, something which can be avoided when care is sought through ILE providers who give both treatment as well as information related to post-abortion care. However, without data on the usage of clandestine abortion prior to the ILE reform it is difficult to assess the degree to which legalising abortion may have resulted in the avoidance of marginal births. Thus, in principle, while the evidence suggests that the observed impacts of legalised abortion on rates of haemorrhage early in pregnancy do not owe to the selection channel, and while it seems likely that the quality of care channel is a main determinant, without data on clandestine abortion we cannot formally rule out the demand channel.[22]

5.3.2. *De jure versus de facto legal reforms*

In general we find no impact of regressive law changes on resulting morbidity and small impacts on birth rates. One potential explanation for this is that, although *de jure* changes were made to state constitutions, the *de facto* implementation of laws and penal codes remained unchanged. As we document in Appendix Table A1, in many cases, while constitutions were altered—generally to declare that human life begins at conception—this did not always translate in concrete legal changes in the criminal sanctions imposed on women. This has been similarly noted in legal analyses of the reform (Singh et al., 2012). And even in cases where criminal sanctions were increased, it may be the case that state-level judiciaries do not alter the likelihood of imposing sanctions on abortion.

We examine whether there is evidence of changes in the likelihood of being sentenced to prison for undertaking an abortion or in the length of prison sentences received, based on the passage of the abortion laws examined in this paper. These data cover all individuals in the country, and here our outcomes are focused explicitly on whether an individual is sentenced to prison for undertaking an abortion. DD results following specifications (1) and (2) are displayed in Table 4. We observe, first, that

[22] Given the expansion of the medical abortion regime (i.e., the use of misoprostol and mifepristone) in Mexico DF (Becker, 2013), complications due to the abortion procedures themselves following the ILE reform appear to be low (Grimes et al., 2006). It is therefore unlikely that channel 2b, from Section 2.4, explains large movements in maternal health.

Table 4
Difference-in-differences estimates of abortion reforms on judicial outcomes.

	Number of prison sentences		Length of prison sentences	
	(1)	(2)	(3)	(4)
Panel A: ILE versus non-reformers				
Post-ILE Reform (DF)	−4.018***	−4.050***	2.251**	2.150**
	(0.294)	(0.340)	(1.032)	(1.063)
Observations	117	117	56	56
Mean of Dependent Variable	1.427	1.427	3.606	3.606
Panel B: Regressive reforms versus non-reformers				
Post-Regressive Law Change	−0.648	−1.362**	4.144XX	5.220**
	(0.464)	(0.607)	(2.361)	(2.439)
Observations	279	279	171	171
Mean of Dependent Variable	1.806	1.806	3.749	3.749
State and Year FEs	Y	Y	Y	Y
Population Weights		Y		Y

Difference-in-difference models illustrate how abortion reforms correlate with prison sentences handed down by the judiciary and the length of these prison sentences in years. The total number of sentences and the average length of prison sentences are generated from administrative records captured in Mexico's Judicial Statistics on Penal Matters. This is the universe of judiciary decisions in the country based on the first legal judgment and so does not include any subsequent appeals. Analysis of the length of prison sentences presented in columns 3 and 4 is conditional on any prison sentences being handed down in each state and year. Prison sentence lengths are calculated from a categorical variable capturing bins of between six months and two years, and in each case we record the total years (or fractions of years) based on the midpoint of each bin. Bins are consistently used in the period displayed here. All standard errors are clustered at the level of the state and calculated using a wild bootstrap procedure. Identical models using population-standardised values for dependent variables are presented in Appendix Table A11.

XX $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

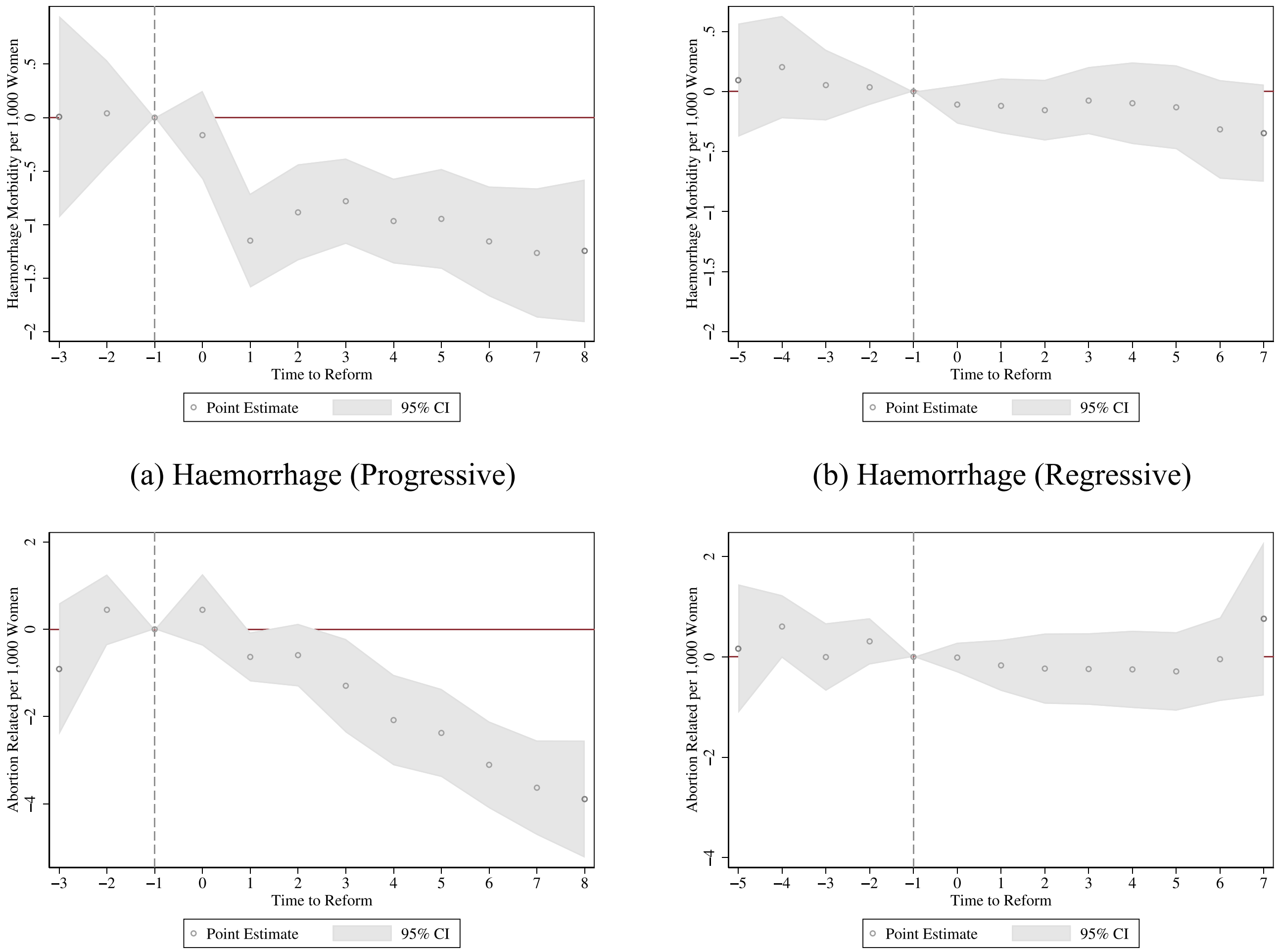
there is a sharp reduction in the number of prison sentences for undertaking an abortion in Mexico DF following the reform (in line with the legalisation of abortion); however, this was observed alongside an estimated increase in sentence lengths.[23] Importantly, we observe evidence of a dual impact in regressive states. We observe mixed evidence pointing to a slight reduction in the number of prison sentences handed down, falling by 1.36 cases in weighted regressions (compared with a mean number of sentences per state and year of 1.806). In the case of the length of sentences, we observe a considerable increase of between 4.1 and 5.2 years, depending on the specification estimated. In the case of weighted estimates, we observe an average increase of 5.2 years, which is considerable, even at the lower end of the 95% confidence interval, when compared with the mean sentence length of 3.7 years. While the increase in the length of sentences is in line with harsher sanctions, the observed reduction in the number of criminal sanctions is surprising. There are, however, a number of potential explanations which could be offered such as prosecutors focusing on more serious crimes or reduced rates of sentencing if women temporarily leave their state to access abortion in other states with less stringent sentencing practices. Thus, these results suggest that while the changes in law did not necessarily always prescribe a change in prison sentences, there is a detectable increase in the length of prison sentences observed in administrative data, conditional on being sentenced to prison. This increase in average sentence length is observed to hold in event study analysis, with significant impacts observed from one year post-reform onwards (Appendix Figure A15).

5.3.3. *Considering the universe of health records*

Considering the universe of the public health system. In principal models, we have used month × year × state measures of outcomes, given that as documented in Table A2, a wave of legal restrictions were put in place in varying months of 2008 and 2009. However, as discussed in the Data Section of this document, only hospitals administered by Mexico's Secretariat of Health (available to all individuals) report month of hospitalisation in administrative data. In order to determine whether a similar pattern is observed across the entire public health system, we additionally conduct analysis pooling by hospitalisations in hospitals administered by the Secretariat of Health, as well as those administered by social security providers. In this case, given the lack of monthly records in social security hospitals, we can only estimate models by year. In Fig. 4 we present yearly event studies for the main morbidity outcomes considered, extending to this full sample.

In all cases, we observe broadly similar results in each case. In the case of haemorrhage morbidity, we observe a sharp fall of around 1 per 1000 cases per year immediately following the reform in Mexico DF (Panel (a)), with no similar results observed in states undertaking reform tightenings (Panel (b)). Similarly, reductions in abortion-related morbidity are observed in Mexico DF, appearing gradually

[23] Note that in Mexico DF, while abortion was legalised by the ILE reform it was only the case for abortions realised up to 12 weeks of gestation. Thus, in theory, custodial sentences can still be handed down for abortion when not meeting this condition. In practice, a non-zero number of sentences was only observed in Mexico DF in 2011 (refer to Appendix Figure A14 for trends over time). In particular, the significant increase in the length of custodial sentences is observed as very few cases were prosecuted, with prosecuted cases likely being relatively serious offences outside the scope of the ILE reform.



(a) Haemorrhage (Progressive)
(b) Haemorrhage (Regressive)
(c) Abortion Related (Progressive)
(d) Abortion Related (Regressive)

Fig. 4. Yearly event studies based on all public data. Notes: Event studies replicate those from Figs. 2a and 2b ; however, using yearly administrative records based on Secretariat of Health hospitals *and* social security hospitals. All details follow those indicated in notes to Figs. 2a and 2b ; however, we now work with the universe of hospital visits in all public hospitals. DD models are provided in Appendix Table A12 (and Appendix Table A13 for birth rates).

over time as observed in monthly event studies with no similar changes observed in regressive states (panels (c) and (d)).

Leakage to the private health system. One potential alternative explanation of the observed morbidity results in all public hospitals is that, rather than being driven entirely by the abortion reform, they may reflect changes in usage of the health system, with a larger number of women opting to use the private healthcare system. This argument cannot explain the impact on fertility and maternal mortality as these outcomes are based on the complete records of births and deaths in the country. However, it could partially explain the impacts observed on morbidity as our administrative data records inpatient stays in the public health system (refer to Section 3 for additional discussion).

While we cannot consistently merge public and private health data at the most disaggregated level of morbidity causes, we *are* able to consider all causes of abortion-related morbidity in the private healthcare system.[24] In Appendix Figure A16 we plot rates of abortion-related morbidity in the universe of private hospitals (left-hand panel) and the universe of public hospitals (right-hand panel). These descriptive plots suggest that, if anything, results in the private system will only strengthen our estimates, as we observe a sharper reduction in abortion-related morbidity in private hospitals than we observe in public hospitals. In the case of morbidity due to haemorrhage early in pregnancy, we are unable to observe this as a sole cause in the private health records, but we are able to observe the class in which this cause falls and again we observe a considerably sharper reduction in morbidity fol-

[24] Note that as documented in Appendix Table A3, this mapping captures all ICD-10 codes O00–O08, while typically abortion-related morbidity is calculated from codes O02–O08. In Figure A16 we plot comparisons using precisely the same aggregated codes in public and private hospitals.

lowing the reform in the private health system than we observe in the public health system (Appendix Figure A17).

5.3.4. Reform spillovers and heterogeneity

Reform spillovers. As outlined in Section 2.2, the ILE reform was not strictly limited to residents of Mexico DF. Recent evidence from the US documents a willingness to travel a significant distance to access abortion providers (Lindo et al., 2019; Venator and Fletcher, 2019). In Appendix Table A14 we provide summary figures of the state of precedence of users of abortion services in Mexico DF based on administrative data for 2007–2015. While the majority of users (72.5%) are women from Mexico DF, women residing all throughout Mexico have access to ILE. The largest non-DF population comes from nearby Mexico State (24.2%). In general, users of the ILE reform are clustered in states geographically close to Mexico DF. A descriptive plot is presented in Appendix Figure A18. Residents in Mexico DF have by far the highest rate of abortion, at 5.8 abortions per 1000 women aged 15–49, followed by Mexico State, at 1 per 1000, and then two nearby states of Hidalgo and Morelos, with rates of 0.1 per 1000. Remaining states have rates which are an additional order of magnitude lower than this.

Despite some evidence of very localised geographic spillovers, we do not observe clear evidence of changes in birth or maternal health outcomes in nearby states. In Table A15 we estimate DD models augmenting specification (1) with a post ILE $\times$ spillover state indicator, where spillover states refer to Mexico State, Morelos and Hidalgo (the three states with most considerable abortion usage per population). In no case do we observe statistically significant reductions in fertility, morbidity, or mortality; if anything, we observe weakly positive impacts. Additional discussion is provided in Appendix C.

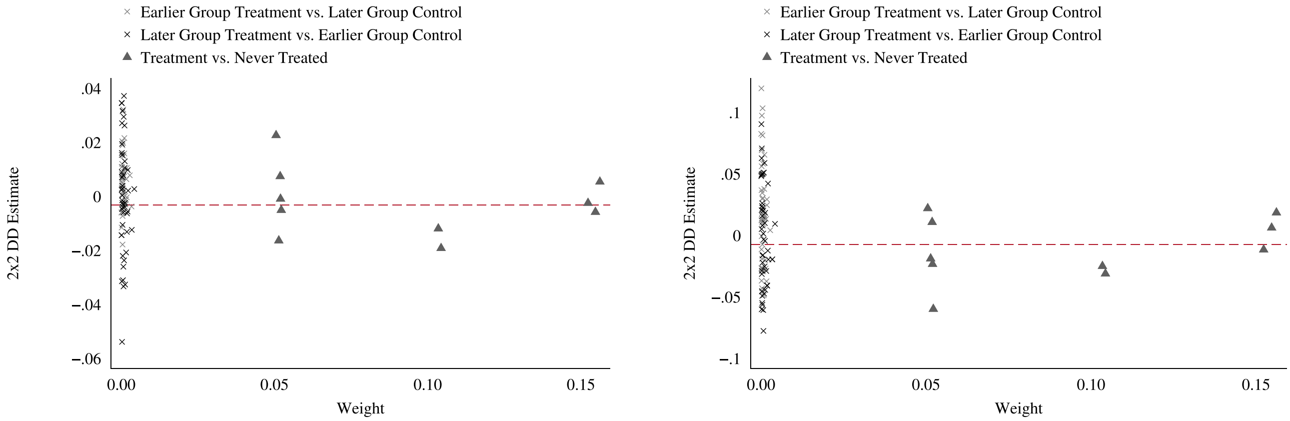
An alternative model which captures both the impacts of the reform in Mexico DF as well as any reform spillovers to the rest of the country replaces the ILE variable in Eq. (1) with the intensity of treatment in each state. This intensity measure is captured as the rate of abortion per 1000 women via the ILE program (documented in Appendix Table A14) in the post-reform period in each state.[25] This information is reported in official ILE reports, but it is only available at the level of the year, and as such in this case we estimate yearly models as in Section 5.3.3. If outcomes per 1000 women are regressed on abortion usage per 1000 women, we are provided with a back-of-the-envelope calculation of the elasticity of outcomes with respect to the availability of a legal abortion. For example, if each additional legal abortion results in 1 fewer birth, we will estimate a coefficient of -1 in this model, suggesting full pass-through of legalised abortion to birth rates. We note, however, in our case that the figures on abortions refer *only* to those abortions provided by official ILE providers. To the degree that private providers additionally provide access to legal abortion, these estimates should be seen as an upper bound estimate on the magnitude of pass through. We estimate models of this type in Appendix Table A16. In general we observe that, using the full data on abortions across Mexico resulting from the ILE reform, impacts per abortion are considerable suggesting approximately 0.9 fewer births per every abortion provided, 0.16 fewer cases of abortion-related morbidity, and 0.14 fewer cases of morbidity due to haemorrhage in early pregnancy. These values must be viewed in line with the caveat above that any abortions accessed from private providers will reduce the actual magnitude of pass-through (likely to be considerable given the relevance of the private sector), and as such these are upper bound estimates.

Individual heterogeneity in reform impacts. Effects discussed up to this point have focused on average outcomes over all women aged 15–49. However, given evidence documenting heterogeneous impacts of abortion reform in other contexts (Ananat et al., 2007), and heterogeneity in access to abortion in Mexico (Friedman et al., 2019), we consider heterogeneity in a number of measures here. In Tables A17 and A18 we examine the impacts of the reform on abortion-related morbidity and on haemorrhage early in pregnancy by quinquennial age groups and by an individual's insurance coverage. While we would like to consider impacts by individual income level, the category of insurance coverage is one of the closest proxies of income which we can observe in hospitalisation microdata.[26] In the case of both outcomes we observe three broad stylised facts. First, health impacts are observed across the age distribution; second, these impacts are largest in size among younger women, peaking in the 20–24 age group; and, third, that results appear to be driven by individuals who *do* have insurance coverage. This final fact suggests that impacts may be larger among higher-income women; however, it is important to note that this classification by insurance status is very crude because not having formal insurance likely signifies a considerable disconnection from the public health system.

Finally, in Appendix Figure A19 we document impacts more finely by age, specifically focusing on teenaged and younger women. Given that minors require parental consent, we may expect that impacts on these women are

[25] We thus capture rates of abortion using the ILE program in Mexico DF among residents of all states in the country. Non-residents of Mexico DF are able to access abortion through the ILE program without restrictions provided that they travel to an ILE clinic in Mexico DF.

[26] Microdata on patients treated at state-run hospitals, in particular, records whether they have insurance coverage through any social security providers (such as the Mexican Social Security Institute), state governments, private providers, or alternatively no insurance through these channels. In all years, at least 15% of patients in public hospitals are recorded as having no insurance. This proportion did not change sharply around the passage of the ILE reform or subsequent regressive reforms (and access to ILE is free for residents of Mexico DF regardless of individual insurance status). It is important to note that all hospitalisation records we observe depend on individuals actually presenting at hospitals. Thus, to the degree that lack of insurance coverage acts to dissuade individuals from seeking care, this will result in under-estimates of the true magnitude of complications. However, in the absence of access to formal insurance, such incentives are likely similar in both pre- and post-reform periods, and as such we do not expect insurance cover categories to directly interact with the reform coefficients estimated here. Nonetheless, as insurance coverage depends on a broad range of both observable and unobservable characteristics, we treat this heterogeneity analysis as broadly descriptive, and only a rough proxy for income.



(a) Haemorrhage (b) Abortion Related

Fig. 5. Goodman-Bacon (2018) Decomposition based on 2 × 2 difference-in-difference models. Notes: Figures document the Goodman-Bacon decomposition into a series of 2 × 2 difference-in-differences models depending on the type of comparison unit. Here the 'treatment' refers to the passage of a regressive abortion law, and the outcomes refer to rates of morbidity per 1000 women. The passage of the ILE reform occurred at a single moment in time and, as such, decompositions need not be performed. The global decomposition is given in Table A19.

reduced, in line with reduced rates of usage. In Appendix Figure A19 we observe single-coefficient DD estimates for each age, where we estimate models (1) (presented as hollow circles) and (2) (presented as black squares) separately in each yearly age group. Here, while we do observe a general increase in the magnitude of coefficients when increasing from women aged 15–18 and above, we do observe that even among younger adolescents (e.g., 16-year-olds), significant impacts of ILE are observed on both rates of haemorrhage and abortion-related morbidity.

Temporal heterogeneity in reform impacts. An additional concern related to the timing of adoption of the regressive reforms is that single-coefficient DD estimates (Eq. (1)) may be capturing heterogeneity and variation in law implementation rather than the ATT itself (Goodman-Bacon, 2018). We may be concerned that small effects, in particular, are being driven by the incorrect use of already treated units as controls in future periods. This will bias towards zero any effects of the laws if these impacts are growing over time. We thus follow Goodman-Bacon (2018) in explicitly decomposing the single-coefficient DD model into its component parts of a pure "treated versus never-treated" effect, and effects owing simply to the variation in timing of the passage of laws.[27] We note that this decomposition is only of concern in the case of regressive laws given that ILE was adopted in a single moment of time (and a single state). This decomposition is displayed graphically in Fig. 5 following Goodman-Bacon (2018), Goodman-Bacon et al. (2019). We plot the full set of "2 × 2" DD estimates of the impact of regressive reforms on morbidity outcomes,[28] where these models come from all variation in treatment timing and all possible control groups. We observe that estimates are largely clustered around zero, particularly for the Treated versus Never-Treated comparisons of interest. We present the global decomposition in Appendix Table A19, and observe—reassuringly—that in each case the majority of the weight in the single coefficient DD estimate comes from the Treated versus Never-Treated comparison (around 92%) and, in general, estimates even within the timing-only groups are similarly small when compared to the Treated versus Never-Treated effect.

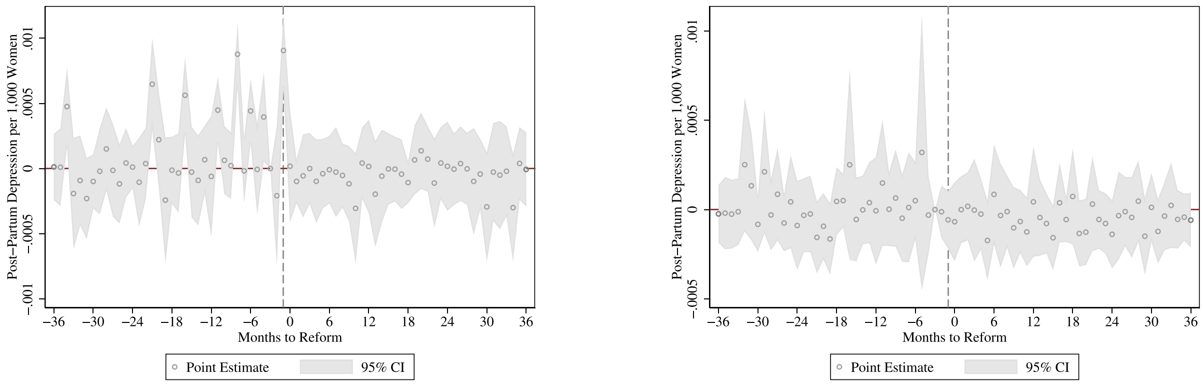
5.3.5. *Broader impacts of the ILE reform*

The ILE reform was a significant change, and was noteworthy across Latin America (Sánchez Fuentes et al., 2008). Evidence from literature in economics, such as Chiappori and Oreffice (2008), Oreffice (2007), suggests that abortion reform can have significant impacts beyond health and fertility outcomes, extending to broader spheres within households and individual well-being. Here we briefly discuss a number of possible additional impacts of abortion reform on women's mental health and family outcomes. Given challenges in measurement and reporting,[29] this

[27] Goodman-Bacon (2018) notes that a single-coefficient DD estimate is a weighted combination of each possible pair-wise DD estimate comparing treated states with (a) non-treated states, (b) states which are not yet treated but are treated later, and (c) states which do not change their treatment status in a given period but that have been previously treated. In the case of time-varying treatment effects, this third comparison can potentially lead to mis-signed treatment effects. The decomposition we conduct here consists of plotting the individual DD estimate from each possible state-by-state comparison, as well as the weight it is given in the global single-coefficient DD estimate resulting from Eq. (1).

[28] Estimates for birth rates are provided in Appendix Figure A20.

[29] When screened for, as well as other mental health issues related to childbirth, maternal depression is a common condition both antenatal and post-partum with estimates suggesting a prevalence of between 13–18% in Mexico (Albuja et al., 2017). However, as in most countries, the prevalence of maternal depression is difficult to establish as maternal depression is often under-diagnosed and under-reported (by both health-care professionals as well as patients) (Anokye et al., 2018). Our ability to capture post-partum depression (and similar conditions) is limited given that we do not have access to outpatient or pharmaceutical data. Being hospitalised for post-partum depression is rare and hospitalisations are



(a) Progressive Abortion Reform (ILE)

(b) Regressive Abortion Laws (Legal Tightening)

Fig. 6a. Monthly event studies examining post-partum depression.

work should be considered as exploratory and a possible subject of future research.

To examine the impact on maternal mental health, we consider the predominant mental disorders in this domain, namely post-partum depression and other mental and behavioural disorders associated with the puerperium (World Health Organization, 2015). We use the same administrative health records as used previously recording all hospitalisations classified as owing to mental and behavioural disorders associated with the puerperium, including post-partum depression (ICD-10 code F53).[30]

In Fig. 6a and 6b we estimate impacts of abortion reform following Eqs. (1) and (2) on rates of post-partum depression. While Fig. 6a suggests no consistently observable impacts on rates of post-partum depression around the passage of abortion reforms (for both progressive and regressive reforms), there is some evidence suggesting a reduction in rates of depression in Mexico DF compared with other untreated states when examining yearly estimates in Fig. 6b. No such clear evidence is observed in the case of states undertaking legal tightenings.

Finally, while in this paper we are focusing principally on the health impacts of the adoption of abortion laws, there are potentially much broader impacts of such policy. For example, Lauletta (2019) provides evidence of potential reductions in domestic violence flowing from these reforms, in line with economic theory suggesting improvements in women's bargaining power flowing from birth control reforms.

5.3.6. *Mechanisms: availability, education, or behaviour*

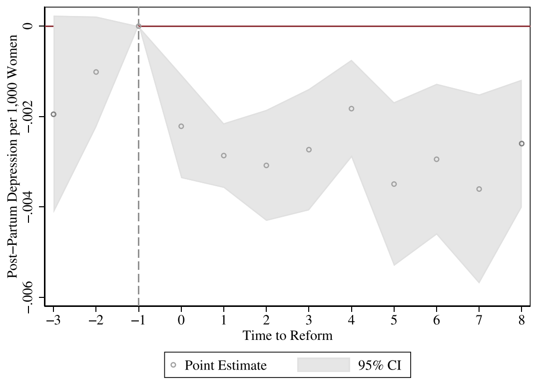
Along with the law change legalising abortions, the ILE reform included additional components relating to sexual education and disbursement of additional contraceptives in clinics (refer to Section 2.2). To examine the channels through which the reform affected birth rates: whether it be only access or a combination of access with behavioural change, we use the MxFLS data which follows women over time and has survey rounds both before and after the abortion reforms of interest. To examine the potential effect of the other aspects of the reform (sexual education and contraceptives), we estimate a version of Eq. (1) at the level of the individual, which allows for individual-specific fixed-effects given the panel nature of the MxFLS data.

We examine the effect of abortion reform on all available measures of contraceptive use (using any contraceptive or using modern contraceptives), the number of reported sexual partners, and whether the respondent reports having knowledge of modern contraceptive methods. These results are presented in Appendix Table A20. In general, we find very little evidence to suggest that the results of the abortion reform flow from an increase in *other* contraceptive knowledge in reform areas or change in risky sexual behaviour as a result of the reform. We find quite close to zero effects for change in contraceptive use and knowledge, and an insignificant reduction in the number of sexual partners reported. In all cases, these results are insignificant at the 10% level[31] suggesting that the ILE reform's effect is largely due to the sharp increase in utilisation of abortion services rather than alternative contraception or information channels. Similarly, we do not find that regressive changes in abortion laws cause women to seek additional information, to be more likely to use contraceptives, or to change sexual behaviour as proxied by the number of sexual partners compared to areas which were not subject to a regressive reform. We do note in the case of the number of sexual partners that while we cannot reject that the impact is significant even at a 10% level, we cannot rule out economically meaningful effects given the reasonably inexact point estimate.

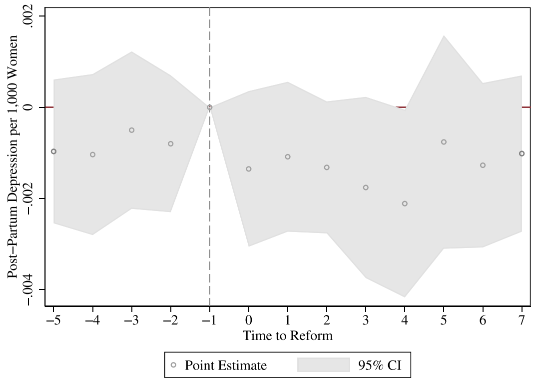
likely to capture the most serve cases of mental illness while the majority of women are most likely treated elsewhere if diagnosed.

[30] Another relevant mental health outcome is suicide attempt. However, this is an extreme measure of poor mental health. Moreover, in the ICD system suicide attempts are recorded separately as external causes, which are not consistently available in all microdata over time.

[31] Summary statistics are provided in Appendix Table A21, and similar results are observed using a repeated cross section of women (Appendix Table A22).



(c) Progressive Abortion Reform (ILE)



(d) Regressive Abortion Laws (Legal Tightening)

Fig. 6b. Yearly event studies examining post-partum depression. Notes: Event studies document the evolution of rates of post-partum depression per 1000 women surrounding the passage of abortion reforms. Each point estimate refers to the change in rates between treated and non-treated states, compared to their baseline differential immediately prior to the reform. Fig. 6a presents event studies based on monthly rates of post-partum depression (using data from Secretary of Health hospitals only), while Fig. 6b presents event studies based on yearly rates of post-partum depression using data from all public hospitals (both Secretary of Health and social security providers). In each case, the left-hand panel shows the difference between Mexico DF and untreated states surrounding the passage of the ILE reform. The right-hand panel shows the difference between regressive policy changers and non-changers around the (time-varying) date that each reform was passed. Regressions are weighted by the population of fertile-aged women, and the full set of time-varying controls are included.

6. Conclusion

In this paper we examine the impact of abortion law on women's health. We consider a context in which considerable heterogeneity in legislative reform is observed. In Mexico in the late 2000s both a substantial loosening and a series of tightenings of abortion policies were undertaken at the sub-national level. Using comprehensive vital statistics data on maternal health outcomes, we observe that safe legal abortion available in the first trimester of pregnancy in Mexico DF resulted in a sharp drop in maternal morbidity due to haemorrhage in early pregnancy and a slower decline in abortion-related morbidity, perhaps in line with the gradual adoption of recommended abortion techniques by public health clinics. These declines were of substantial importance suggesting 8600 fewer inpatient visits in the post-abortion years in Mexico DF. In general, we observe quite weak effects of the tightening of *de facto* sanctions on abortion, even though, as we show, these sanctions did lead to changes in the length of sentences handed down to women.

We document that the impact of Mexico DF's ILE reform on birth rates, at around an 8% reduction, is in line with impacts estimated in other settings (e.g., the US in the 1970s). We observe generally weaker effects of regressive reforms on birth rates, although we note that in the case of Mexico these state-level reforms may have *reduced* fertility by around 1–2%. When examining the impacts of abortion reforms on rates of maternal death, our estimates are considerably noisier than those for maternal morbidity. This is important given that a range of papers examining the impact of abortion on women's health limit analyses to maternal death because of a paucity of high-quality health records. Our results suggest that this focus on "the tip of the iceberg" may lead to less convincing results than when focusing on maternal morbidity. While focusing on surviving child birth should be an absolute minimum when designing public policies to protect maternal and women's health, maternal morbidity is of considerable importance when quantifying lifetime well-being and avoiding a considerable health burden leading to chronic conditions.

The results of this paper are becoming relevant once again as a number of countries revisit abortion legislation and attempt to make considerable changes to constitutions and penal codes. Among others, legislative reforms have been undertaken or attempted in Ireland, Argentina, Australia, Chile and New Zealand between 2017–2020 focusing on legalising abortion in certain circumstances, and increasing restrictions have been enacted or proposed in Poland and a number of US states. This paper documents that these policies are likely to have a considerable impact on women's health and well-being.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jhealeco.2020.102413.

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